

Anthropology, History,
and American Indians:
Essays in Honor of
William Curtis Sturtevant

WILLIAM L. MERRILL
and
IVES GODDARD
EDITORS

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Anthropology, History,
and American Indians:
Essays in Honor of
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EDITORS



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A B S T R A C T

Merrill, William L., and Ives Goddard, editors. *Anthropology, History, and American Indians: Essays in Honor of William Curtis Sturtevant*. *Smithsonian Contributions to Anthropology*, number 44, 357 pages, frontispiece, 86 figures, 13 tables, 2002.—This collection of 31 essays and one bibliographic compilation is presented as a festschrift for William Curtis Sturtevant. Since 1956 a research anthropologist, and, since 1965, a museum curator, at the Smithsonian Institution in Washington, D.C., Sturtevant is one of the world's leading scholars of the cultures, languages, and histories of the indigenous peoples of the New World. Over the course of his career, he has also served as general editor of the *Handbook of North American Indians*, president of four of anthropology's major professional organizations, university professor, consultant, and public lecturer. He has contributed in myriad ways to the development of contemporary anthropology and to the research endeavors of scores of anthropologists and scholars in many other disciplines.

The volume is organized into six sections. The first begins with recollections of Sturtevant's childhood and early adulthood by his younger sister, Harriet Sturtevant Shapiro, followed by an overview of his professional career and a compilation of his writings from 1952 through 2001. The second section offers a range of perspectives on the history of anthropological and historical research on themes related to Native Americans, and the third examines the transformations that have occurred in their lives and circumstances from the time of European contact to today. The fourth section considers the relationship of anthropological collections and repositories to the development of the field and the shifting significance of museums, archives, and universities as the settings where anthropological research has traditionally been conducted. The fifth section presents the results of a series of research projects focused on museum and archival collections, and the sixth explores the complex interconnections between the cultural and natural worlds.

The essays provide an indication of the variety of topics and approaches represented in North Americanist studies at the turn of the twenty-first century. Together they address issues central to current scholarly debate: the political implications of cross-cultural research; the transcending of traditional disciplinary boundaries; the impact of colonialist and post-colonialist projects on native peoples and their responses to these projects; the relevance of anthropological repositories and collections to research; and the linkages among material and nonmaterial dimensions of human existence. Reflecting the scope of Sturtevant's own research, they stand as testimony to his intellectual breadth and to the extent of his influence on contemporary scholarship.

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Preface

Of all its major figures, North Americanist anthropology today is perhaps most closely identified with William Curtis Sturtevant. The last North Americanist hired by the Smithsonian Institution's Bureau of American Ethnology—long the center of North American Indian studies in the world—Sturtevant has provided continuity and leadership in a field that has undergone radical changes in the decades since he began contributing to it. Through his research and writings, his editorship of the *Handbook of North American Indians*, and his participation in professional societies and conferences, he has been a guiding force in the development of North Americanist anthropology, both in the United States and abroad. Few North Americanists have failed to have at least passing contact with him, and most can recount occasions on which his willingness to share his knowledge and insights has been of enormous benefit to them and their research. His lifelong fascination with and respect for the native peoples of the Americas, his unwavering commitment to North American Indian studies and to anthropology in general, and the quality of his work have inspired several generations of North Americanists and have set the standard against which they have measured their own careers.

This volume is intended to recognize William Sturtevant's contributions to North Americanist research and to the field of anthropology as a whole. The authors of these essays—representing at least five generations of Americanist scholars—have all been influenced in one fashion or another by Sturtevant, and most are linked to him in multiple ways, as former professors or students, as collaborators on research projects, as Smithsonian colleagues, or as interlocutors at professional conferences. All are leading scholars in their own right who have made, and continue to make, substantive contributions to scholarship. The essays prepared for this volume necessarily reflect only certain aspects of their contributors' work, but together they suggest the range of topics that have been of concern to Americanists over the past half century and the different approaches that have been adopted to explore them. The essays also reveal the breadth of Sturtevant's interests and the impact he has had on the field.

We have grouped the essays into six general sections. The first section provides an overview of Sturtevant's development as a scholar and an assessment of his contribution to anthropology and other disciplines. The second treats the history of anthropological and historical research among North American Indians, with particular emphasis on the political factors that have affected this research and the presentation of its results. The consequences of European colonialism and the responses of native people to the profound changes that have taken place in their worlds over the past five centuries is the subject of the third section. The fourth section deals with the development of anthropological research collections and repositories and the evolving roles that they and the scholars associated with them have played in the anthropological endeavor. The value of these collections to anthropology and the challenges facing collections-based research is demonstrated in the fifth section. The final section explores the complex linkages between cultural and natural worlds as exemplified through case studies from various parts of North America as well as Europe.

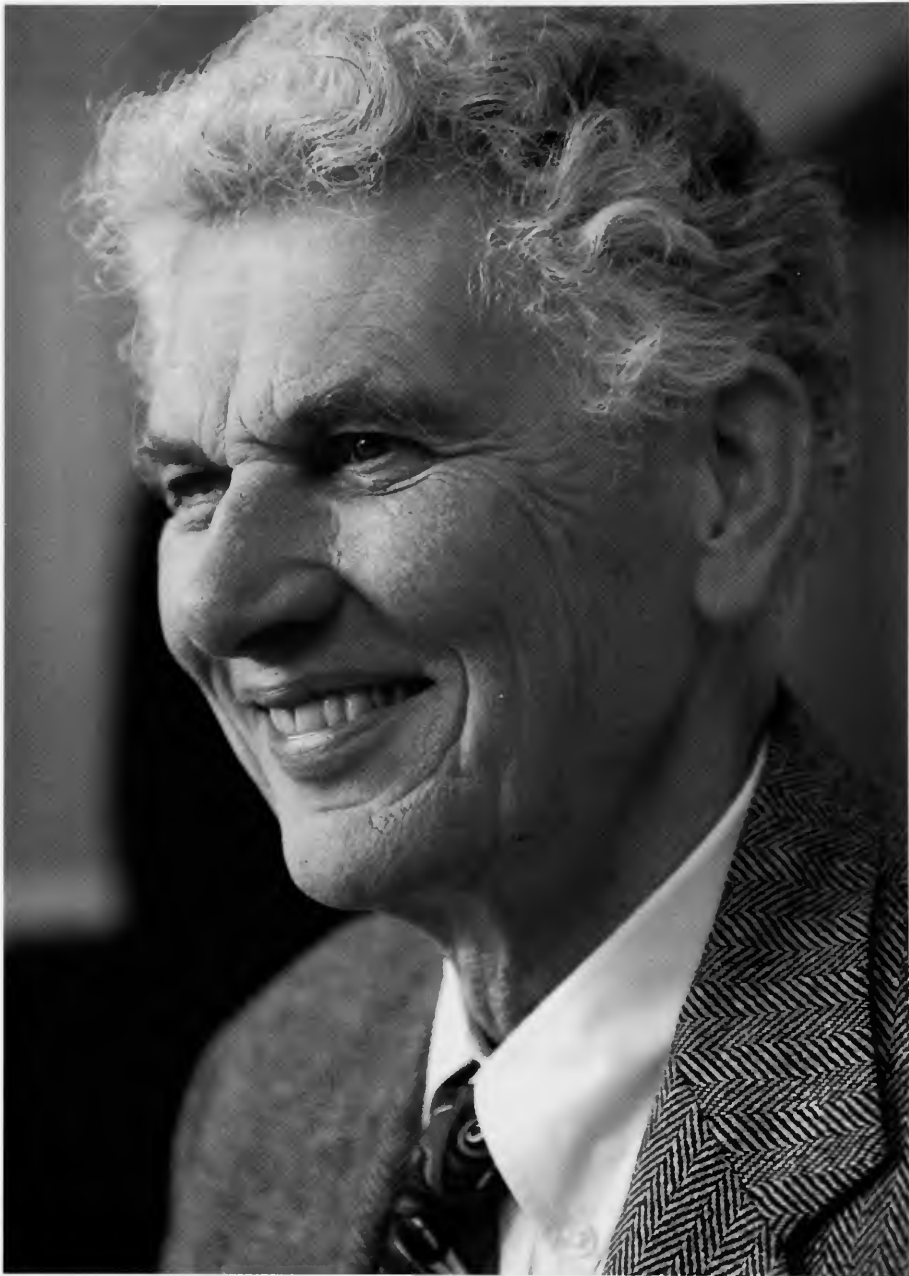
This particular organization should not obscure the fact that the essays presented in one section of the volume often are linked to the themes of others. Also, the essays contribute to the development of a number of additional topics not explicitly recognized in this organization. These include the history of anthropology in general and its relationship to other disciplines, particularly history and the natural sciences; American Indian linguistics and literacy; the connections between material and nonmaterial dimensions of culture; ethno-

science and cognitive anthropology; world systems; and the anthropology of food—all topics that Sturtevant has addressed in his own work.

We would like to express our appreciation to the scholars whose essays appear herein for their collaboration in making this volume a reality and to the administration and staff of the National Museum of Natural History and its Department of Anthropology for their support of this project. We are particularly grateful to Joyce Sommers and Kim Waters for their assistance in logistical matters, to Victor Krantz for preparing prints from the Sturtevant family album, to Marcia Bakry for producing the map that accompanies David Damas's essay, and to the readers of an earlier version of this manuscript for their insightful comments.

This volume is very much a festschrift in the original German sense of "celebratory writing." We offer these essays to William Sturtevant to celebrate his work and to honor him for his accomplishments. We hope that he will find them entertaining and will recognize in them the admiration and affection that we have for him.

William L. Merrill and Ives Goddard



FRONTISPIECE.—William Curtis Sturtevant. Photograph taken on 22 November 1997 in Washington, D.C., at the annual breakfast of former presidents of the American Anthropological Association. Photograph by Chester Simpson (page 7734, frame no. 31).

I. William Curtis Sturtevant

Sibling Review

Harriet Sturtevant Shapiro

When the editors asked me to write an article about Bill's childhood, my first reaction was that if they wanted an article on "Growing Up With A Great Man In The Family," I could do one, but the subject would be Father, not Bill. On further consideration, it seems that this is not a bad way to start. Because the family was so centered on Father, and all his children have taken him as a role model for how to be an adult, it is necessary to understand Father in order to understand any of his children.

Father, Alfred Henry Sturtevant II, was the youngest of six children by a substantial margin. He was born in Jacksonville, Illinois, where his grandfather, Julian Sturtevant, was one of the founders and an early president of Illinois College, a small Congregational church school that still exists. Julian was clearly a great man in that community and apparently was quite autocratic. Julian's youngest son, Alfred Henry Sturtevant I, taught mathematics at the college, but when Father was seven (in 1898), Alfred moved with his family (except the eldest, Edgar, who was away at college by then) to Kushla, Alabama, where he farmed and recovered turpentine. The family led an isolated, rural existence. Until he reached high school, Father attended a one-room school taught by his father's maiden sister, who lived with his family. Father never considered himself a Southerner. He remembered vividly that he fought the Civil War throughout his school years.

When Father graduated from high school, his brother Edgar, who by that time was teaching linguistics at Barnard College, suggested that Father could live with Edgar and his family while Edgar paid Father's tuition at Columbia University. This offer surprised Father, who did not expect to go to college. Edgar eventually became a professor at Yale and was an authority on the Hittite language. Although he was some 16 years older than Father, he was a great favorite with us children because he told wonderful tall tales of his supposed adventures. Father continued the tradition started by Edgar when Hope Tisdale, the daughter of another Sturtevant sibling, reached college age. He paid for her tuition, and she lived with him and his new wife in their New York apartment while she got her undergraduate degree at Barnard. Cousin Hope became a distinguished population statistician, and she in turn financed the college education of one of her own nieces.

At the time that Thomas Hunt Morgan was laying the foundation for modern genetics, Father was in the only beginning biology class taught by Morgan at Columbia. Father was interested in the pedigrees of horses, and he realized that the records of their genealogies and coat colors provided a useful opportunity to test the fundamental patterns of inheritance being investigated by Morgan. Father's paper working out that relationship was published while he was still a college sophomore; more important, it won him a place with Morgan's research group: a desk in the "fly room" where the early work on the genetics of *Drosophila* (the fruit fly) was done. For many years thereafter, the nucleus of the fly-room group consisted of Morgan, Calvin B. Bridges, and Father, with a shifting contingent of graduate students and visiting scientists.

Father remained at Columbia, aside from brief service in the Medical Corps in World War I, until 1928, when Morgan took the group to Pasadena, California, to start the Biology Division at the California Institute of Technology (Caltech). Morgan received the Nobel Prize for Medicine in 1933 for the research on genetics done by the fly-room group. In recognition of the contributions of his colleagues, Morgan divided the prize money equally among his, Father's, and Bridges's children for their education. It is a source of pride to me, and, I think, my siblings, that our college and graduate educations were financed substantially by Nobel Prize money.

Mother, Phoebe Reed Sturtevant, was, like Father, one of six children, one of whom died in early adolescence. Mother grew up in New Jersey and graduated from Mt. Holyoke College, where she majored in art. A few years after graduation, she was hired to illustrate the papers and books prepared by the fly-room group. Mother was very visually oriented, and although she left her fly-room job after she married Father in 1922 and did not thereafter continue with her drawing, she remained actively involved in various crafts throughout her life. While her children were young, this involvement was largely expressed in her attention to interior decoration, in knitting, and in sewing for herself and me. She also attended carpentry class at night and made a wonderful large table and a set of cubby holes that were used together as a desk in the living room. Many years later, she went back to carpentry class and built a very successful dump truck for her grandchildren. She also wove; Father proudly wore a jacket made from wool she had woven.

Mother was unusual in the number and age range of her friends. She often befriended the wives of Father's graduate stu-

Harriet Sturtevant Shapiro, 108 Primrose Street, Chevy Chase, Maryland 20815-3325, USA.

dents, several of whom regularly used our washing machine because none was available in their quarters. Her circle of friends also included many of her fellow members of the League of Women Voters and many Caltech faculty wives. For one of the latter who was especially devoted to her poodle, Mother spun wool from the poodle's hair and knit it into a coat for the dog.

I think as far as we children were concerned, Mother's besetting fault was her lateness. Dinner was not started until everyone was really hungry, and she could never get anywhere on time. Her children usually were the last to arrive at parties or other events and almost invariably were the last to be picked up.

Bill was born on 16 July 1926, at Mother's family home in Morristown, New Jersey. Many years later, when Bill wanted to know the hour of his birth for the preparation of a Burmese horoscope, Father was able to provide that information by con-

sulting the notes he had taken on an ant war he was observing while waiting for the birth. I was born in 1928 and was named Harriet Morse after Father's mother. The rest of Morgan's laboratory group moved to Pasadena while the family stayed behind to await my birth because Father felt I had a right to be born in the East; I made the trip to California in a basket. A family story is that Bill, on waking in the morning as the train traveled through the Midwest, inquired "whobody cut down all them trees?"

A younger brother, Alfred Henry Sturtevant III, is the only one of the three of us to have been born in California, in 1931. For three generations, our family has named its youngest sons Alfred Henry Sturtevant, but I don't think there was any conscious family tradition to that effect. In any event, Henry, whom we called "Fritz" until he insisted on "Henry" shortly



FIGURE 1.—Passport photograph of the Sturtevant family before leaving for England, 1932.

before World War II, is the only one of us not to have named his last child Alfred.

I have always thought that the fact that there were three of us, with me being both the middle child and the only girl, meant that the inevitable childhood alliances followed no set pattern: There were instead three natural alliances: the boys, the two younger siblings, or the two older ones. Henry, however, remembers it differently: he thinks that the usual alliance was Bill and I against him. I would have said that, if any alliance dominated, Henry and I tended to gang up to try (unsuccessfully) to unseat Bill's natural position of authority as the eldest. In any event, the more or less constant intersibling squabbles were trivial compared to our basic compatibility and affection for each other. There was no real feeling of rivalry among us. I think we were then, as we certainly are now, unusually close.

The family spent a year in England, in 1932 to 1933, while Father taught at various universities as a visiting professor of the Carnegie Endowment for International Peace. I remember almost nothing of this time, and Bill has no connected memory of it. It was here, however, that he learned to print, rather than to use cursive, a habit he still retains. Other than that period, our childhood was spent in Pasadena, California, with summers at Woods Hole, Massachusetts.

There was a very clear division of responsibilities between Mother and Father: Mother was responsible for child raising and running the household. Nevertheless, the household revolved around Father, not because he was in any way auto-

cratic, but simply because that was the way Mother wanted it. There was never any doubt that Father's wishes were the rule, and his convenience was paramount. This principle worked because Father never abused it. He considered Mother his full partner in all respects, and he had a fundamental regard for individual rights that extended to his children. Father once told me that "don't do unto others as you would not have them do unto you" was a better general standard of conduct than the usual positive formulation, because it is less likely that what you want, others also want, than it is that what you don't want, others don't want either. The motto he wrote in my autograph book was "try all things, and hold fast that which is good." This was advice to a 10- or 12-year-old daughter, which reflects in part the more innocent pre-war age but which also was entirely consistent with his confidence in the fundamental good sense of his children. At the time, I was more surprised by his use of a biblical quotation than by the sentiment. Father never talked down to us and was regularly available for serious conversation, one-on-one, while he burned the trash in the back yard in the morning or while he washed the dinner dishes, as a child dried them, or during the three meals a day that the family ate together.

The house was full of talk, although introspection was not encouraged, nor was the overt expression of emotions. Mother and Father talked about his work and Mother's activities in the League of Women Voters. Both were enthusiastic gardeners, so they also discussed their plans for the garden and Father's efforts in hybridizing irises, applying genetic principles to de-



FIGURE 2.—Harriet, Bill, and Fritz Sturtevant in Newcastle, England, 1933.



FIGURE 3.—Fritz, Harriet, and Bill Sturtevant in their yard in Pasadena, California, ca. 1936.

velop specific flower colors and forms. National and international affairs were another, less cheerful, topic. Father visited Germany while we were in England in 1933, and he was appalled at the conditions there. From that time on, he was very pessimistic about the state of the world, and he was convinced that war was coming and that the United States would have to be involved. Perhaps for this reason, during most of our childhood Father was a rather somber figure.

Despite Mother's wide range of personal friends, family social life was mostly with other members of the Caltech Biology Department and with visiting professors. Many of these came from overseas, sometimes for rather extended stays. They and their children gave us some appreciation of the styles of other countries.

There was no religion. Father was a committed atheist who believed religion was responsible for many of the world's ills, and Mother was largely uninterested in the subject. Education



FIGURE 4.—Bill Sturtevant photographed in Woods Hole, Massachusetts, ca. 1938.

was a very high priority, for its own sake, not as a means of advancement. In fact, ambition was not a value. We were told that happiness lay in deciding what we enjoyed doing, learning to do it well enough to earn a living at it, and being satisfied with the living we earned. We all went to a private school, Polytechnic Elementary, because Mother and Father lacked confidence in the education we would get in the public schools. They were correct in expecting that we would get a solid academic foundation at Poly; the unintended effect was to make us feel somewhat marginalized, although probably we would have felt the same way in public schools, too. We were quite different from the rest of the students—we were Democrats, atheists, much less well-off, and nonathletic—but I don't think any of us interpreted those differences as inferiority, just the reverse. Our parents expected the best of us academically, and they were pleased, but not surprised or at all demonstrative, when they got it. Similarly, it was simply assumed that we would read a lot. Although we were regularly taken to the library until we were old enough to go on our own, nobody ever got particular credit for reading. Indeed, if Mother found us reading during the day, she was very likely to find a chore for us to do, or

at least to send us outside to play. We soon learned that daytime reading was most safely performed in the seclusion of one's room. Even after supper, reading in the living room was likely to be interrupted by talk. Reading was, in short, a pleasure to be cherished, not an activity that won praise.

In general, it was very bad form to boast about your children, and praise was to be used with caution, for fear that they would develop "swelled heads." On the other hand, we never had any doubt that each one of us was in fact a cherished and important member of the family; considerable effort was devoted to encouraging our individual interests. Henry was mechanical, and from an early age he was the one the family turned to for fixing whatever broke down. He has undergraduate and advanced degrees from Caltech and went on to design spectrophotometers and equally complex machines to analyze blood. I was the girl in the family, so I sewed, cooked, and knitted. Although I was encouraged in (and enjoyed, as I still do) stereotypical feminine roles, and it was expected that I would (with luck) be a devoted wife and mother, it was always assumed that I was fully as competent intellectually as my brothers, and I was held to the same standards. I completed my undergraduate degree at Wellesley College and then law school at Columbia, where I was editor-in-chief of the law review. After graduating, I was hired by the Atomic Energy Commission and later moved to the United States Department of Justice, becoming the first woman lawyer in the Office of the Solicitor General, the group responsible for representing the federal government in the Supreme Court. Like that of my brothers, my professional work

has been intellectually stimulating, and I have taken great pleasure both in my legal career and in my family.

I don't remember any similar concern for encouraging Bill's particular bent; that was quite unnecessary. Bill was a collector from an early age. His collection was on bookcase shelves along one side of his room; the lower shelf held old *National Geographic* magazines. Although there must have been American Indian items in the collection, I cannot remember any specific such items. It did contain some ground glass from the mirror for the 200-inch telescope being made across the street at Caltech (though Bill would never tell us how he got it), some fossils and geological specimens collected from the trash receptacles at the Caltech Geology Department, and a horned toad and a bat that Bill had preserved. His taxidermy also extended to the preparation for my doll house of several mouse-skin rugs, of which I was inordinately proud. He must have made these when he was about 12 to 14. They were whole-skin rugs, wonderfully soft and (at least the later models) flexible, and no one else had anything like them. Bill also collected (at about age 10) enough Ralston cereal box tops to get a pair of heavy leather cowboy (Tom Mix) chaps that were the envy of his younger siblings. He had the usual stamp collection, as well as a scrapbook full of newspaper clippings concerning the English royal family, which covered the death of George V and the coronation and abdication of Edward VIII.

For as long as I can remember, Bill has been interested in American Indians. More than the rest of us, Bill inherited Mother's artistic abilities, and he painted several murals reflecting this interest for his room. On the wall over his bed, he



FIGURE 5.—Bill Sturtevant in his room in Pasadena, California, 1945. Above the headboard is his copy of a copy by Covarrubias of a design on a Haida house front.



FIGURE 6.—Bill Sturtevant at his desk beside his rendition of the Aztec day signs, Pasadena, California, 1945.



FIGURE 7.—The Sturtevant family at lunch in Pasadena, California, 1945.

painted a large reproduction of a Northwest Indian bear design, and his closet door was covered with colorful Aztec calendar symbols (Henry remembers the legend as “The 20 day signs of the Aztec Calendar,” with their names around the edges.

The layout of our rooms also provided an early lesson in the exercise of, and limitations on, territoriality. Although we each had our own room, we all three shared a bathroom that was lo-

cated between Bill’s room and mine, with access from those two rooms but not from the hall. This meant not only that Fritz was entitled to go through my room to get to it (the most direct route, though I felt some resentment that the easement was not through Bill’s room), but also that if the door was left locked on one side, the child whose access was blocked was entitled to go through the other’s room. The trick was to do this as noisily

and disruptively as possible, while not lingering so long or deviating so far from a direct route as to justify complaint by the invaded party.

Besides our parents, the other most important adult family member in our lives was Mother's mother, Granny, our only surviving grandparent. Granny spent several winters with us in California, escaping the New Jersey weather. She was a somewhat formidable character, in part because, like many grandparents, she didn't really think her grandchildren were being properly brought up; she never hesitated to criticize our manners and general deportment. She was a voracious reader and a great source of information and stories. She would even occasionally let us listen to her radio, a contraption that was otherwise banned from the house until just before the war. Granny and Father got along very well. They respected and admired each other, and they shared interests in genealogy and Democratic politics.

Bill was named after Granny's father, was her eldest grandchild, and was born in her house; he was clearly a favorite. She was the source of most of his early American Indian artifacts. Granny, like Mother, had a very good eye for artistic quality and sometimes purchased American Indian pieces when they were available. In addition, Granny's brother, Lloyd Curtis, was a naval officer who, during a tour to Alaska in 1881–1882, acquired a number of items from Alaska Natives on the advice of his friend Lt. George Emmons, an amateur anthropologist and himself a collector of Alaskan Indian artifacts. Many of the items Uncle Lloyd acquired found their way to Granny and then to Bill, in light of his interest and special place in Granny's affections. When Uncle Lloyd died, while Bill was in college, his widow gave several more American Indian items to Bill.

Father's family too was a source of artifacts, most notably two ceremonial adzes from the South Pacific that came into Father's family through the seafaring brothers of Father's grandmother. The provenance of the adzes is established by letters that those brothers wrote to their family in Connecticut and later Illinois. One brother settled in Honolulu around 1835; copies of his letters home until he died in 1850 are in the Hawaiian National Archives (Bill has the originals). For all of our childhood, the adzes, as well as an Alaskan Indian basket from Uncle Lloyd and a tapa cloth (probably purchased by Mother), decorated the living room in Woods Hole.

Bill's friends were always somewhat mysterious to me. I couldn't even reproduce their assembling call, which was made by blowing air, flute fashion, across an opening in one's cupped hands; Henry did master it. With the son, also named Linus, of the chemist Linus Pauling, Bill explored the storm drains under Caltech (unbeknownst, surely, to Mother and Father). They also built a tree house in the vacant lot across the street, which was, of course, off limits to the younger Sturtevant and Pauling siblings by order of the builders. Later, when Bill was in high school, there were regular meetings of his mah jongg club. The group's interest in the game derived, I believe, from the Chinese member of the group of friends. The game

pieces and the box (which Bill still has) were entrancing—the pieces are carved ivory on bamboo backs, and the box is an elegant brass-bound, highly polished wooden construct—but the game itself was totally mysterious. I don't remember wanting to learn how to play, and the opportunity was certainly never offered. That was just something that Bill did.

We spent our summers in Woods Hole, on Cape Cod, while Father worked at the Marine Biological Laboratory. The house in Woods Hole that our parents built in 1927 now belongs to the three of us; Bill's grandson Alex is the fourth generation to have a strong sentimental attachment to it. Our family traveled to and from Woods Hole by car after we children were old enough to make the trip that way (and outgrew reduced train fares). Due to Mother's congenital lateness, we never started the trip before the end of the day scheduled for departure; this did not seem to bother Father. The aim was to travel three hundred miles a day, allowing frequent stops at likely looking places so Father could collect flies using a modified butterfly net made by Mother. The trips took about ten days' travel time. They were not direct, but involved stops to visit Father's colleagues at other institutions, as well as his family in Alabama and Mother's in New Jersey. Father planned the route, trying to take a different one each time and ultimately to visit each of the then forty-eight states.

The trips were rather arduous, with the five of us in close quarters in an unairconditioned car. I particularly remember the smell of Father's pipe in the closed car before breakfast. Nevertheless, the trips did provide a chance to have the undivided attention of our parents, who would join us in playing games (Father was unbeatable at "I Packed My Grandmother's Trunk") and collecting Burma Shave jingles (My brothers' favorite, because it got a rise out of me, was "Ladies jump from fire escapes to get away from hairy apes," "Harriet" being a ready substitute for "hairy apes"). Mother, who had a small repertoire of college and folk songs, would sing with us; she rarely sang anywhere else. We also had rituals for the trip, such as the method for the hourly change of drivers: the whole family piled out and ran once around the car before taking new places. These places were rigidly rotated: the non-driving parent always sat in the middle in the back, to reduce squabbling. When that did not suffice, the offending child (usually, as I remember it, Bill) was put out to walk for a while before being picked up further down the road. Although that discipline backfired once when Bill found a whole dollar by the roadside, it may help explain his current aversion to walking. The walking strategy was dropped after my best friend, Jane Lancefield, and I were put out to walk on a day trip to Martha's Vineyard, and hid when the car drove past. We didn't realize why that particular trick so upset our parents until we ourselves had children.

Woods Hole itself was pure delight. This was primarily because the Lancefields also summered in Woods Hole. Rebecca Lancefield was a noted bacteriologist; her husband, Donald, was a former colleague of Father's at Columbia. They were longstanding and very close friends of Mother and Father, so

their daughter Jane and I were destined to be best friends. When we came to Woods Hole, our family essentially expanded to include the Lancefields. Becca and Donald became second parents, and for a few months I had the sister I always wanted. All four of us were in and out of both houses constantly. We swam every day at least once, and went barefoot all summer, so that the soles of our feet became almost like hooves, and by the end of the summer our shoes were covered with green mold and felt incredibly cramped. For many years we took classes in the mornings at the science school associated with the Marine Biological Laboratory. These classes involved such things as field trips for collecting specimens, dissections of marine creatures, and studies of the varied flora and fauna of Cape Cod. When we were teenagers, the two families jointly owned one of the first fiberglass sailboats, an untipable center-board we called the Glass Cat, in which we took day trips down the Elizabeth Islands off Woods Hole.

Bill's collecting tendencies evidenced themselves in Woods Hole at an early age by his Tootsie Toy layout, which consisted of his collection of metal Tootsie Toy cars (the forerunners of Matchbox cars, slightly larger) in a town he constructed. Perhaps this was also an early manifestation of his mapmaking tendencies. Somewhat later, he collected a complete deer skeleton from the islands and carefully labeled each bone in ink with his characteristically neat printing. Many years later, it was fun to watch Bill, his elder son, and his grandson spend a rainy afternoon reconstructing the skeleton on the living room floor.

In his early teens, Bill made a fortune (perhaps as much as \$15) one summer by making and selling house signs consisting of the owner's name spelled out in twigs, meticulously split, cut, and nailed onto a board. He also spent a whole summer laboriously harvesting cattails and constructing a reed boat in imitation of the Peruvian Indian ones, only to have it sink ingloriously when it was finally launched. Henry, the engineer (who has worked in Peru), says that it did not in fact sink, "It just floated really low; you got wet sitting in it. Cattails don't have the same low density as the reeds around Lake Titicaca." Bill nevertheless still believes that his failure was due to his inability to duplicate the design of the Peruvian boats. Despite this early disappointment, Bill and Henry later successfully built a more conventional boat as a small tender for the Glass Cat. That boat, although it also rode low in the water, served to carry us relatively undampened to and from the Cat's mooring until a real rowboat was scavenged from the islands after a hurricane. The built boat was called "the Whale," because of the American Indian design Bill painted on its front.

After Bill's freshman year at the University of California at Berkeley, he went to summer school in Mexico. He wanted to visit Yucatan after the summer-school term ended, but Mother and Father told him not to, and they refused to send him money



FIGURE 8.—Bill Sturtevant in the Navy, 1945.

for that trip. Bill cashed in his return plane ticket, budgeted the resultant funds carefully, and made the trip anyway. He returned to Los Angeles by bus with just enough money for one phone call home to Pasadena to ask to be picked up. He was drafted into the Navy soon thereafter.

Although I think we all assumed we would live forever and be happy and successful adults, we surely didn't expect to reach 75 or to be eminent. Henry reports that when his eldest child was in college at the University of California at San Diego, she was asked whether she was related to "the Dr. Sturtevant." Mother was not at all amused when it turned out the reference was to Bill, not Father. Henry and I are sure that Father would have been amused, and pleased. We are certainly ourselves pleased and perhaps amused, but not surprised, at Bill's eminence. We offer him our affectionate congratulations.

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William Curtis Sturtevant, Anthropologist

William L. Merrill

William C. Sturtevant is widely regarded as one of the leading North Americanist ethnologists of the second half of the twentieth century. He has dedicated his career, spanning half a century, to extensive research on the cultures and histories of the indigenous peoples of the entire North American continent as well as those of much of the rest of the Western Hemisphere. The breadth of his interests and knowledge distinguish him from most other North Americanists of his and subsequent generations, who have tended to focus their research on specific societies or regions of North America.

Sturtevant's first publication was a review of a popular book on the Seminole Indians of Florida (Sturtevant, 1952). In the decades since, he has produced over 200 scholarly publications on New World anthropology and on other themes as well, most notably the history and philosophy of anthropology as a discipline and the importance of museums and material culture to the anthropological enterprise. At the same time, he has devoted considerable time and energy to the activities of several of anthropology's major professional organizations and has curated one of the largest museum collections of North American Indian materials in the world.

Sturtevant's entry into anthropology coincided with World War II, a watershed event in the history of anthropology in the United States. Before the war, most American ethnologists conducted their research among North American Indians, with Franz Boas and his students dominating the field. After the war, the focus of American ethnology became more global, and the percentage of ethnologists working in North America declined dramatically. Despite his interest in world ethnology, Sturtevant chose to concentrate his research on North American ethnology and to build upon the extensive knowledge about American Indians accumulated by his North Americanist predecessors.

This decision is but one of several instances in which Sturtevant has gone against the grain of trends in anthropology. He adopted a comparativist approach in his research during an era of growing particularism. He promoted a historical perspective in anthropology when the dominant theoretical approaches were largely ahistorical, and he advocated research on material

culture and museum collections when such research was decidedly out of fashion. He also explored basic issues related to European images of non-European people and their worlds long before anthropologists considered these issues to be interesting or recognized the value of the images themselves to anthropological research.

He has not taken these positions simply to be contrary but because they follow logically from his vision of what anthropology should be and from the insights he has gained from the pursuit of his own particular research interests. His views on these matters have often placed his work "out of sync" with the shifting trends of anthropology, but more often than not this work has anticipated, in some cases by decades, the directions that subsequent anthropological theory and research have taken.

Berkeley and the United States Navy, 1944–1949

Sturtevant began his formal education in anthropology in the spring of 1944, when he entered the University of California at Berkeley (Cal) as a second-semester freshman. His interest in anthropology, however, extends back to his grammar-school days. He remembers first hearing of anthropology while he was a third grader at Polytechnic Elementary in Pasadena, California. One afternoon, after a class on American Indians, he asked his father what kind of people study Indians, and his father replied, "Anthropologists." Sturtevant decided then that he would make anthropology his career.

In the preceding essay, Sturtevant's sister, Harriet Sturtevant Shapiro, provides an engaging portrait of their family and their childhood years together (Shapiro, 2002). Here I will add only that Sturtevant completed his secondary education at McKinley Junior High School and Pasadena Junior College (now Pasadena City College), the latter offering both high school and the first two years of college. Because he had taken extra credits, he was eligible for graduation from high school by the end of his junior year, but he did not realize the fact until the fall semester of his senior year. He took several college-level courses that semester at Pasadena Junior College, and in the spring semester of 1944, he began classes at Cal.

Sturtevant chose Cal over other universities because it was relatively close to home, and its anthropology department was among the best in the country. During his first semester there, he concentrated on studying foreign languages, which had long

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held a fascination for him, taking courses in Chinese and in Spanish.¹ To gain some firsthand experience in a foreign culture and to continue his education in anthropology, he traveled in the summer of 1944 to Mexico City, where he attended summer school at the Universidad Nacional Autónoma de México. There he took courses on Mexican archaeology and South American ethnology taught by Robert Barlow and George Engerrand, explored Mexico City, and visited archaeological sites nearby and in the Yucatan. He also turned 18 and registered for the draft at the United States Embassy in Mexico City.

A bout with hepatitis prevented Sturtevant from returning to Cal in the fall and delayed his being drafted until March of 1945, when he entered the United States Navy. After boot camp in San Diego, he was assigned to the hospital corps and was stationed at the Naval Air Station in Calexico, California, where he was given the task of treating sailors for venereal diseases they had contracted during visits across the Mexican border. He was at Calexico when the war ended, and shortly afterward he was transferred to Guam. There he spent most of his time on night duty, sitting idle in an ambulance on the edge of an airstrip. To break the tedium, he prepared a map of Guam. He also made a small surface collection at an archaeological site at Tumon Bay, which he later donated to the Peabody Museum of Natural History at Yale, and joined a discharged Navy officer who was a botanist in collecting plants on different parts of the island. This botanical work gave him the opportunity to visit briefly with native Guamanians, whose settlements were otherwise off-limits.

Sturtevant's military service ended on 30 September 1946, at San Pedro, California, where he received an honorable discharge with the rank of pharmacist's mate third class.² He immediately resumed his studies at Cal. Alfred Kroeber, the dean of American anthropology of the era and for decades the dominant figure in the Department of Anthropology at Cal, retired in 1946, before Sturtevant's return. Kroeber began spending the academic year at other major anthropology departments in the United States, and although he returned to Berkeley in the summers, he gave no classes. Sturtevant took courses offered by the department's senior anthropologists, including Robert Lowie and E.W. Gifford, but his closest contacts were with the younger anthropologists, primarily Robert Heizer, John Rowe, and David Mandelbaum.

Heizer and Rowe provided Sturtevant with his first formal introduction to American Indian studies and convinced him of the fundamental connection between ethnology and archaeology. Along with Mandelbaum, Rowe was also an important influence in the area of anthropological theory. Sturtevant recalls as especially useful and challenging Rowe's reading course on the history of anthropology. Rowe would give each student a topic, expecting a report a week later, which he would then read and comment on to the class. One week he assigned to Sturtevant Herbert Spencer's magnum opus, the five-volume *Principles of Sociology!* Sturtevant was impressed by the breadth of Rowe's knowledge, his dedication to his research,

and his respect and high expectations for his students. Rowe served as Sturtevant's undergraduate advisor and guided him in the preparation of a research paper on the origins and history of Chinook jargon.

In part because of the influence of his uncle Edgar Sturtevant, a renowned linguist, and in part because of his keen interest in the subject, Sturtevant also enrolled in several linguistics courses. At the time, linguistics was not being offered in the Department of Anthropology, so he took courses with professors in other departments: phonetics and phonemics from Murray Emeneau in the classics department, and advanced linguistics from Mary Haas, a linguist in the Department of Oriental Languages. Haas was especially important in encouraging Sturtevant to develop a competency in linguistics and, later, after he began research among the Seminoles, in providing him with guidance in his analysis of Muskogean linguistic materials, one of her areas of specialization.

During his undergraduate years at Cal, Sturtevant gained his first experience in anthropological fieldwork. He interviewed Japanese-American students who had been interned in relocation camps during the war, and he attended the University of New Mexico's 1947 summer field school in Chaco Canyon, New Mexico, where he learned the basics of archaeological fieldwork and visited briefly among the Navajos and Rio Grande Pueblos. In June of 1949, he spent one weekend with Clement Meighan, Francis Riddell, and Tullio Tentori among the Eastern Pimos in northern California (Meighan and Riddell, 1972). He also joined fellow student William King on a short archaeological surface survey of the California coast from San Diego south well into Baja California. Apart from King, Sturtevant's closest friends among the undergraduate anthropology majors at Berkeley were Henry Nicholson and Donald Lathrop.

Sturtevant was inducted into Phi Beta Kappa during his junior year, and in the fall of his senior year, he began considering where to apply for graduate school. Apart from the University of California, which encouraged its undergraduates to pursue their graduate educations elsewhere, the leading anthropology graduate departments at the time were at the University of Chicago, Columbia, Harvard, Michigan, Pennsylvania, University of California at Los Angeles, and Yale. John Rowe regarded Yale as the best of these and recommended that he apply there. Sturtevant was impressed by the quality of the faculty at Yale and by Yale's reputation as a center for innovative theoretical work in anthropology, especially in the area of culture and personality studies. He was particularly attracted by the prospect of studying with Ralph Linton, whose work on acculturation and, more recently, culture and personality theory, combined with his writings on anthropology in general, had established him as one of the most important anthropologists of the day. He decided to apply only to Yale, a decision reinforced by the fact that his uncle Edgar was a member of the linguistics department there.

The University of California gave Sturtevant academic credit for training that he had received in the Navy Hospital Corps. Combined with credits he had earned at Pasadena Junior College, he was able to graduate early, receiving a bachelor's degree with highest honors in anthropology in January 1949. He stayed in Berkeley to audit several courses during the spring semester of 1949. The following summer, he studied French and German in preparation for examinations in reading comprehension in these languages that he would be required to take upon entering Yale in the fall. By remaining in Berkeley, he passed up an opportunity to attend summer school at the University of Oslo. He regretted his decision after taking the examinations, because they turned out to be less challenging than he had expected.

Yale, 1949–1956

Yale, like most other departments of anthropology in the United States, promoted a comprehensive approach to the study of humanity, which required an understanding of what have become identified as the four principal subfields of the discipline: ethnology, archaeology, linguistics, and physical anthropology. Sturtevant's undergraduate and graduate training in this approach established the vision of anthropology that he has maintained throughout his career. He clearly expressed his view of the connections among the four subfields in his 1969 article "Does Anthropology Need Museums?" After acknowledging that each of the subdisciplines has its special interests and perspectives as well as linkages to separate, nonanthropological disciplines, he commented:

But anthropology remains a single subject, with sub-divisions. Some observers believe that it will not (and sometimes that it should not) remain so, that increasing specialization will lead to fragmentation. But this specialization often overlaps sub-field boundaries, so that the discipline may well become a network rather than a rigid set of four pigeonholes. I believe that the sub-fields will (and should) continue to offer more to each other than to outside disciplines. (Sturtevant, 1969a:630–631)³

A number of major figures or rising stars in anthropology were on the faculty at Yale while Sturtevant was a graduate student there, and he took courses from most of them: social structure, cultural processes, and culture and personality from Linton; cultural dynamics from George Peter Murdock; New World and Asian ethnology from Cornelius Osgood, Wendell Bennett, and Sidney Mintz; archaeology from Irving Rouse; and linguistics from Floyd Lounsbury. He also participated in a seminar directed by George Kubler on the analysis of Mixtec codices and took several linguistics courses from Bernard Bloch.

His professors provided him with a solid foundation in anthropological theory and method as well as a good background in more specific areas, like structuralist linguistics and New World ethnology and archaeology, which he put to good use in his subsequent research. However, once he had firsthand experience with the theoretical approaches in vogue or development at Yale at the time, such as culture and personality theory pro-

moted by Linton and others and Murdock's cross-cultural statistical studies, he was not convinced of their value. Also, like many other students and professional anthropologists, he found Linton a difficult person to deal with (Sturtevant, 1980a). He served as Linton's teaching assistant in courses on social organization and introduction to anthropology, but he was not eager to complete his dissertation under Linton's guidance. Floyd Lounsbury, who completed his doctorate at Yale and joined the faculty the same year that Sturtevant arrived, became his doctoral advisor.

As is typical in graduate school, Sturtevant found that he learned as much about anthropology from other graduate students as from his professors. His cohort included Stefan Borhegyi, Harold Conklin, Philip Dark, William Davenport, Charles Frake, Peter Goethals, William Mangin, John Musgrave, Leopold Pospisil, Donald Robertson, Douglas Schwartz, Annemarie Shimony, Councill Taylor, Johannes Wilbert, and Stephen Williams. In long conversations in Yale's Hall of Graduate Studies, he and his fellow graduate students developed their ideas about anthropology, exchanged opinions about the work of their professors and other anthropologists, and defined the goals and plans for their own research.⁴

Sturtevant's perspectives on anthropology were strongly influenced by his fieldwork among the Seminole, which he began in the summer of 1950, at the conclusion of his first year of graduate studies. Although many of his friends were planning research outside the United States, Sturtevant's long-term commitment to North American ethnology never wavered; however, he had not yet decided on the region of North America where he would focus his research. Rouse, a specialist on Caribbean archaeology who had also published on Florida archaeology, suggested he consider the Seminoles of south Florida. By the end of his first fieldwork season, Sturtevant was convinced that the dearth of ethnographic information about these Seminoles and their status as one of the least acculturated of all North American Indian societies justified ethnographic research among them and offered the possibility of making an important contribution to North American ethnology.

In 1950 the Florida Seminoles lived in a number of small communities in and around the Everglades. The members of these communities were the descendants of the minority of Seminoles who had successfully avoided deportation to Indian Territory by the United States government during the nineteenth century. These and the Seminoles who were deported derived primarily from distinct groups of Muskogean-speaking Indians who had migrated or had been displaced progressively southward from their homes in southern Georgia and Alabama during the eighteenth and nineteenth centuries. Reflecting the diverse origins of their ancestors, the Florida Seminoles spoke two related but mutually unintelligible languages: Mikasuki (a dialect of Hitchiti) and a dialect of Muskogee (Creek) now sometimes called Creek Seminole (Sturtevant, 1971a).

The limited ethnographic research on the Seminoles that had been completed before 1950 focused primarily on the Musko-

gee-speaking Seminoles, so Sturtevant decided to concentrate on the Mikasukis in his own work. In the summer of 1950 and again in the summer of 1951, he traveled from New Haven, Connecticut, to south Florida, his field expenses covered by Yale's Caribbean Anthropological Program, directed by Os-good, which in turn was supported by funds from the Wenner-Gren Foundation for Anthropological Research. He had concluded that the best approach to beginning ethnographic fieldwork among the Mikasukis and to establishing rapport with them would be to study the language. Accordingly, he devoted most of his time during these two summers to collecting and analyzing Mikasuki words, phrases, and texts, focusing more on phonology than on morphology or syntax.⁵ The superintendent of the Dania (now Hollywood) Reservation recommended that he hire Joseph Jumper to work with him. At the time, Jumper was a high school student on the Dania Reservation; when he graduated, he became one of the first Seminoles to obtain a high school degree. Jumper proved to be of invaluable assistance in the project, and by the end of the second summer, they had worked out the phonemics of Mikasuki.

Through this research, Sturtevant established a basic understanding of the Mikasuki language, which allowed him to record Mikasuki terms accurately, analyze their etymologies, and evaluate the translations of Jumper and other interpreters upon whom he relied in his interviews with non-English-speaking Seminoles. He also gained some familiarity with the Muskogee language as well as a Mikasuki nickname, roughly translatable as "Language Asker." He soon discovered, however, that his growing competency in the Seminole languages did not provide the entrée to Mikasuki society that he had hoped for. Most Mikasukis were suspicious of outsiders, and they were reluctant to interact with him on more than the most superficial level.

It was thus good fortune that, near the end of the summer of 1950, he met Josie Billie, a Mikasuki doctor and medicine man about whom Sturtevant (1954b:4) wrote in his doctoral dissertation, "no greater expert on Seminole culture is now alive" (cf. Sturtevant, 1960c). In the 1940s, Billie, along with about one-third of the Florida Seminole population, had converted to the Southern Baptist religion, but he continued to practice traditional Seminole medicine. He was different from most other Mikasukis, including his fellow Christian converts, in being more open to outsiders and more willing to discuss details of Seminole culture with them. Sturtevant chatted briefly with Billie on the Dania Reservation in 1950 and, in the summer of 1951, visited him at his home on the Big Cypress Reservation. When he returned to Florida the following year to complete his doctoral research, he moved into an abandoned trailer on Big Cypress and began working intensively with him.

During the summer of 1950, he met another person who was to have a significant impact on his Seminole research and his perspective on the anthropological endeavor as a whole: John Goggin, a major figure in Florida anthropology. In his obituary

of Goggin, who died of cancer in 1963, Sturtevant described their relationship:

He was very much interested in all aspects of Seminole ethnography. My own friendship with him began when he visited me during my first fieldwork as a graduate student in 1950 and 1951, decided that I was serious about Seminole ethnography, and thereafter at every opportunity encouraged and helped me, sharing his knowledge of Seminole history and culture (including his own field notes, photographs, specimens, and large newspaper clipping file), extending his hospitality to me, and introducing and sponsoring me among his many friends in Florida. (Sturtevant, 1964b:389)

Goggin's interests and expertise extended across much of North America, the Caribbean, and Latin America and encompassed most of the subfields of anthropology. Although primarily an archaeologist, he was firmly committed, in Sturtevant's words, "to a unified approach to anthropological materials, particularly the interconnections of archeology, ethnology, history, and natural history" (Sturtevant, 1964b:389). Goggin's view of anthropology reinforced Sturtevant's own conviction of the value of the comprehensive approach in which he had been trained. Goggin also impressed upon Sturtevant the importance of material culture to anthropological research and inspired him to explore museums, archives, and the extremely varied published literature for information to complement his ethnographic field data.

In the fall of 1951, Sturtevant attended the annual Conference on Iroquois Research at Red House, New York, in the company of Harold Conklin, who had developed an interest in the Iroquois while a high school student in New York. Sturtevant and Conklin had met while both were anthropology undergraduates at Berkeley, but they did not become good friends until Conklin entered graduate school at Yale in the fall of 1950, a year after Sturtevant. At the conference, William Fenton introduced them to some local Seneca people who invited them to attend the Seneca Midwinter ceremonies to be held the following January and February on the Allegany Seneca Reservation, near Salamanca in western New York State. In the intervening months, they decided that the purpose of their visit would be to study Seneca musical instruments, one of the few categories of traditional Iroquois material culture still in existence, but one that had been little studied.

They spent 11 days at Allegany, during which time they worked out the Seneca classification of musical instruments, recorded detailed information on their construction and use, and acquired examples of several of the musical instruments for the Yale Peabody Museum, which sponsored the project. Sturtevant was pleased that the Senecas, unlike the Seminoles, treated him as an individual rather than as just another white outsider and that they welcomed his questions about their culture and history. This visit marked the beginning of his now decades-long research among the Iroquois.

They reported the results of their research in a coauthored article (Conklin and Sturtevant, 1953). In its careful recording of Seneca terms, close attention to Seneca perspectives, and thorough coverage of the relevant literature, this article exemplifies the high standards of scholarship that characterize all of their

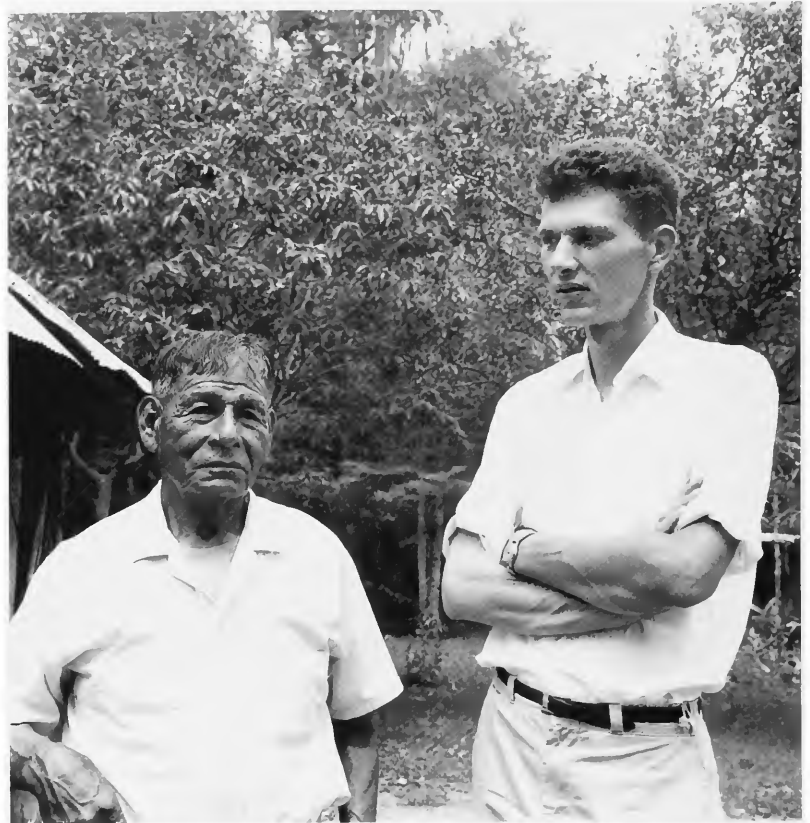


FIGURE 1.—Mrs. Charley Cypress demonstrating grating of coontie (*Zamia*) roots at her camp, Big Cypress Reservation, Florida, 1 February 1957. Junior Billie (left), interpreting; William Sturtevant (right), taking notes (posed). Photograph by John M. Goggin. Courtesy of the National Anthropological Archives, Smithsonian Institution (neg. no. 44,464-a).



FIGURE 2.—William Sturtevant with Solon Jones, Cattaraugus Reservation, New York, 29 June 1957 (posed). Photograph by Theda Maw Sturtevant.

FIGURE 3.—Josie Billie and William Sturtevant, Big Cypress Reservation, 28 March 1959. Photograph by Harold C. Conklin.



subsequent research and writing and foreshadows their development, in collaboration with several others, of the approach to ethnography commonly known as ethnohistory.

In May 1952 Sturtevant resumed his fieldwork among the Florida Seminoles. Two months later, he left for his family's summer home in Woods Hole, Massachusetts, where, on 26 July, his twenty-sixth birthday, he and Theda Maw were married. Maw, from Burma, was a graduate student in history at Yale when they met in 1950; they became engaged in the spring of 1952. Following the wedding, they returned together to Florida, where Sturtevant continued his field research.

Sturtevant focused this research on Seminole ethnography rather than on linguistics, attempting to gather as much information on as many different aspects of Seminole culture as possible. He also collected some physical anthropological data on such things as Seminole blood-type frequencies, handedness, color blindness, and dentition, and he made a small collection of ethnographic materials for the Yale Peabody Museum to supplement those deposited earlier by Goggin and others. His original plan had been to produce a general ethnography of the Seminole, but he decided to focus on Seminole medicine because this was the area of Seminole culture about which Billie was most interested and knowledgeable. A few other Mikasuki people shared bits of information with him, but only Billie was willing to discuss Seminole culture in any detail. Although he was aware of the methodological problems of

relying so heavily on a single informant, and he recognized that Billie was somewhat marginal to traditional Mikasuki society, he had no other options.

Sturtevant worked regularly with Billie through the fall and winter of 1952 and into early 1953. Their discussions focused on Seminole medicine, worldview, and religion, but they also covered Seminole history, inter-ethnic relations, material culture, economy, kinship, and social organization. Together they collected hundreds of plants, for which Billie provided Mikasuki and often Muskogee names as well as detailed information on their use. Sturtevant was also able to make wire and later tape recordings of 18 medicinal spells and songs, a central but previously undocumented component of Seminole medical practice, which Billie and other Mikasuki doctors recited or sang for him. Also of great importance was the information he gathered on Seminole medicine bundles, the significance of which was poorly understood at the time. In investigating these bundles, he built upon the research of Louis Capron, a longtime resident of south Florida who over the years had established amicable relations with several Muskogee Seminoles. He had access to the galley proofs of Capron's (1953) study entitled "The Medicine Bundles of the Florida Seminole and the Green Corn Dance," and he used Capron's findings to elicit information from Billie about Mikasuki bundles and their place in Seminole culture.

In late February 1953, the Sturtevant family left Big Cypress to return to New Haven, and Sturtevant devoted the next 18 months to analyzing his notes and recordings, reviewing the relevant literature, and writing his dissertation.⁶ He submitted the dissertation in September 1954, and it was approved before Christmas; he was officially awarded the Ph.D. at the end of the spring semester of 1955.

As is often the case with doctoral dissertations, the title he gave his dissertation—"The Mikasuki Seminole: Medical Beliefs and Practices"—is misleadingly specific. In it, he examined not only these beliefs and practices but the many other areas of Mikasuki culture linked to them, and he produced one of the more detailed, systematic ethnobotanical studies available for any American Indian society. He also compared his findings among the Seminole with data from Indian societies across the Southeast and in other regions of North America and considered some more general theoretical issues, such as the validity of broad, cross-cultural typologies of diseases and psychological explanations of the efficacy of curing practices. The result is the most thorough ethnography of the Mikasuki ever written and is a significant contribution to a comparative ethnology of North America. By discussing in detail the difficulties he encountered in his research among the Mikasuki and the limitations of his data, he also provided an important portrayal of the complexities of anthropological fieldwork and a model of anthropological candor.

Sturtevant incorporated into his doctoral dissertation only about one-third of his field data and an even smaller proportion of the extensive materials on Florida Indian ethnography and history that he had gathered from other sources. While he was writing his dissertation, he drew on this additional information to produce three articles. In one of these, he provided a detailed comparison of his data on Seminole medicine bundles and busks with data collected by Capron (Sturtevant, 1954a). In the other two, he focused on Florida Indian history, evaluating in the process the reliability of native oral history and emphasizing the importance of taking into account Indian perspectives on their own history (Sturtevant, 1953, 1955a). He also prepared a paper that reviewed the ethnohistorical and ethnographic evidence for cultural connections between the Indians of south Florida and of the Antilles, which he presented in December 1954 at the annual meetings of the American Anthropological Association. This paper was a companion piece to a presentation by Rouse and supported Rouse's view, based on archaeological evidence, that the influence of Caribbean cultures on those of Florida had been negligible.

By 1954 Sturtevant's research, publications, and participation in professional meetings had marked his transition from graduate student to professional anthropologist. This status was further confirmed in March 1954, when he submitted a statement to the United States Congress opposing a so-called withdrawal bill, which would have terminated federal supervision of the Florida Seminoles (Sturtevant, 1954c). Nearly all the Florida Seminoles also opposed the bill, and Congress did not pursue its passage. In the same year, he was hired by Yale

University as an instructor in the Department of Anthropology and as an assistant curator of anthropology in the Yale Peabody Museum.

The Bureau of American Ethnology, 1956–1965

In 1956, when Yale did not renew his contract, Sturtevant began looking for a permanent position elsewhere. He received two offers. One was from Brown University, to establish a department of anthropology and to direct the newly created Haffenreffer Museum of Anthropology. The other was from the Smithsonian Institution's Bureau of American Ethnology (BAE), to serve as an ethnologist in the position vacated by Philip Drucker, who left the BAE and anthropology in 1955 to become a rancher in Mexico (Stirling, 1957:5; Lantis, 1991).

William Fenton, who had left the BAE a few years earlier to take a job at the National Research Council, strongly encouraged Matthew Stirling to consider Sturtevant for the position. Stirling had directed the BAE for three decades, but despite his own energetic research program, he had failed to prevent the unit's steady decline. By 1956 its permanent research staff consisted only of Stirling and two other archaeologists, Frank Roberts and Henry Collins. Despite these problems, the BAE position was in many ways a "dream" job. Sturtevant found it more attractive than the position at Brown because it offered greater freedom and time for research and because it housed an incomparable library and archives of photographs and manuscripts on North American Indian subjects. Also, together with the staff and collections of the Anthropology Department of the United States National Museum, part of the Smithsonian Institution, it continued to be a major center of North American Indian studies.

The offer from Brown was the first of several from universities—including the University of Arizona and the Berkeley and San Diego campuses of the University of California—that Sturtevant received, and declined, over the next decade. In retrospect, he feels that his decision to remain at the Smithsonian throughout his career was the correct one, but in a conversation of June 1996, he expressed to me some regrets:

I still think that one keeps up better and tends to be broader if one is teaching, for one is then forced to keep up with changes in the field. And I sometimes thought that if I had been teaching I would have sent people to do fieldwork in the Southeast and maybe there would have been more ethnographic fieldwork done in the Southeast than there has been.

Sturtevant officially joined the staff of the BAE on 29 March 1956 (Stirling, 1957:22), but he did not actually begin working in Washington, D.C., until the following summer. His principal responsibility, like that of the other members of the research staff of the BAE, was research and writing. The BAE had no material culture collections, so its staff had no curatorial duties, but they were expected to respond to the numerous requests for information on American Indians received every year from various organizations and especially those from the general public, a function that the BAE had fulfilled throughout its ex-

istence (Hinsley, 1981). To respond efficiently to these requests, the staff of the BAE had prepared over the years a series of bibliographies and information leaflets, to which Sturtevant was expected to contribute. Between 1956 and 1962 he compiled bibliographies on diverse topics: the Seminole and other Indians of eastern North America, the Cherokee language, maps related to American Indians, the contemporary situation of Indians in the United States, American Indian songs and dances, basketry, wars and warfare, clothing, medicine and health, and languages and language families. He also prepared a leaflet entitled "Anthropology as a Career" (Sturtevant, 1957). Regarded at the time as the best portrayal of the discipline for nonspecialists, the leaflet was distributed widely and was included in a major collection of readings in anthropology (Fried, 1959, 1:6–14, 2:581–587).

Producing these materials broadened his already extensive knowledge of North American Indian ethnology, linguistics, and history. He further expanded this knowledge through reading, research in museum and archival collections, field research, and short visits to Indian communities in various parts of the United States, Canada, and Mexico. In July 1957 he traveled to Rock Hill, South Carolina, to collect linguistic data from Sam Blue, the last member of the Catawba tribe to have maintained some competency in the Catawba language. While there, he made a small collection of Catawba pottery for the United States National Museum. In 1957 and 1958 he spent seven weeks continuing his research among the New York Seneca, and in 1959 he returned for a few months to Florida to follow up on his previous fieldwork, focusing especially on Seminole ethnobotany. He also collected ethnographic materials, especially objects made for the tourist market, which he deposited in the United States National Museum. In July and August of the following year, he visited 17 European museums to examine early ethnographic examples and possible European prototypes of eastern North American Indian material culture, research that complemented his growing familiarity with museum collections in the United States. In the summers of 1961 and 1962, he conducted five weeks of basic ethnographic fieldwork among the Seneca-Cayuga in Oklahoma and paid a short visit to the Six Nations Reserve in Ontario, Canada, during October of the latter year to attend a ceremony and to do a bit a fieldwork among the Seneca and Cayuga there. He also continued to participate regularly in conferences on Iroquois history and culture and, in 1964, joined Stanley Diamond and Fenton in preparing a memorandum to the Subcommittee on Indian Affairs of the United States House of Representatives, protesting the construction of the Kinzua Dam, which later flooded a large part of the Allegany Reservation and forced the relocation of many Seneca people living there (Diamond, et al., 1964).

Sturtevant combined this active research program with a number of writing projects. In fact, his nearly nine years at the BAE proved to be one of the most productive periods of his career. He continued writing up the results of his Seminole re-

search and prepared several articles on Seminole history, mythology, ritual, and material culture (Sturtevant, 1955a, 1956a, 1956b, 1956c, 1962a, 1963b, 1967f) as well as a biographical essay on Billie, which appeared in a collection of 20 portraits of anthropological informants (Sturtevant, 1960c). He also offered a detailed assessment of the state of ethnological research in Florida (Sturtevant, 1958a) and, with Goggin, analyzed diverse archaeological and historical data to reconstruct the culture and society of the Calusa, one of the principal Indian societies in southern Florida at the time of Spanish contact, and one of the few stratified, nonagricultural societies in all of North America (Goggin and Sturtevant, 1964). He applied his growing knowledge of Caribbean history and ethnology to analyze the precontact agricultural system of the Taino Indians of the Greater Antilles and to expand his unpublished 1954 paper on the lack of influence of Antillean cultures on the cultures of the Indians of the southeastern United States (Sturtevant, 1960d, 1961b).

The expanded study appeared in 1960, in a collection of essays on the Caribbean compiled by Sidney Mintz and dedicated to Cornelius Osgood, Sturtevant's former professor at Yale (Sturtevant, 1960d). In it Sturtevant pointed out that ethnological traits taken as evidence of a direct connection between the Antilles and the Southeast often were widely distributed in the circum-Caribbean and adjacent regions, and that in some cases, their presence in the Antilles was assumed rather than actually documented in the historical and ethnographic record. Moreover, the relatively shallow time depth of this record precluded arriving at any definitive conclusions about the direction of influence among neighboring societies, which could be determined only within a broader chronological framework, typically derived from archaeological research. In the case at hand, the available archaeological data indicated that the only Southeastern Indians who interacted with Antillean societies were those located in south Florida, and that the influence between them had flowed primarily from Florida to the Antilles rather than the reverse. This essay thus offered a systematic critique of the use of culture-trait distributions based on data gleaned from historical and ethnological sources alone to reconstruct New World culture history and was an important contribution to the methodology of cultural historical research in general. Completing this research project also reinforced Sturtevant's view, which he shared with Goggin and many other anthropologists at the time, that archaeology and ethnology are best regarded not as totally separate subfields of anthropology but rather as complementary endeavors within cultural anthropology (Sturtevant, 1964b:389).

His writing during this period was not restricted to Florida and the Caribbean. His growing reputation as a North Americanist resulted in invitations to prepare entries on the Haida, Huron, and Aleš Hrdlička for the *Encyclopedia Hebraica* and on the Creek, the Five Civilized Tribes, and the Seminole for the *Encyclopaedia Britannica* (Sturtevant, 1960a, 1960b, 1961a, 1963a, 1964a, 1964d). He also produced a detailed

overview of Spanish-Indian relations in the Southeast (Sturtevant, 1962b) and wrote brief articles on Carolina Indians in the early historic period, on American Indian linguistics, and on field methods (Sturtevant, 1958b, 1959, 1960e, 1965b). In 1959 David Quinn invited him, on the recommendation of Wilcomb Washburn, to analyze the ethnographic content of the John White watercolors of coastal North Carolina Indians, which Sturtevant (1976:443–444) later characterized as “perhaps the most interesting and important sixteenth-century illustrations of Indians from both an ethnographic and artistic point of view.” His work on these watercolors and on early illustrations of Northeastern Indians, published between 1964 and 1967, marked the beginning of what would become a central focus of his research in the following decades (Sturtevant, 1964c, 1965a, 1967a).

It was also during this period that he wrote “Studies in Ethnoscience,” the article that has proven to be his most controversial to date (Sturtevant, 1964e). He presented the original version of this article at a conference on “Transcultural Studies in Cognition,” sponsored by the Social Science Research Council and held in Mérida, Mexico, in the spring of 1963. Although Sturtevant had not applied a strict ethnoscience approach in his own research, he was an appropriate choice to provide an overview of ethnoscience. Many of the ideas that formed the basis of this approach had emerged out of conversations among Conklin, Frake, Lounsbury, and Sturtevant while they were together at Yale in the 1950s, and Conklin and Frake had presented their views of ethnoscience in a lecture series that Sturtevant co-organized for the Anthropological Society of Washington in 1960 to 1961 (Conklin, 1962; Frake, 1962). Yet it was somewhat happenstance that Sturtevant wrote the article. He believes that Conklin—one of the earliest practitioners of ethnoscience and the person who, in Sturtevant’s opinion, was most instrumental in the development of its methodology—would have been invited to prepare the presentation for the conference had he not been conducting fieldwork in the Philippines at the time.

Sturtevant’s goal in writing the article was not to provide a history of the development of ethnoscience but to present, explicitly and systematically, its perspectives and goals, explore its fundamental theoretical and methodological principles, review how these principles had been applied in specific research projects, and suggest areas where an ethnoscience approach might be profitably employed in future research. He defined ethnoscience as a general approach to ethnography focused on discovering and describing the conceptual models that the members of different societies employ to organize their experiences and to orient their activities in the world. Although he recognized that this focus continued a long tradition in anthropology of emphasizing the importance of understanding native points of view, he noted that most ethnographic work failed to explore native perspectives exhaustively and tended to distort them by forcing them into general categories, like religion or kinship, which were mistakenly as-

sumed to exist in more or less equivalent form in all human societies. In this regard, he commented, “It has long been evident that a major weakness in anthropology is the underdeveloped condition of ethnographic method. Typologies and generalizations abound, but their descriptive foundations are insecure” (Sturtevant, 1964e:100).

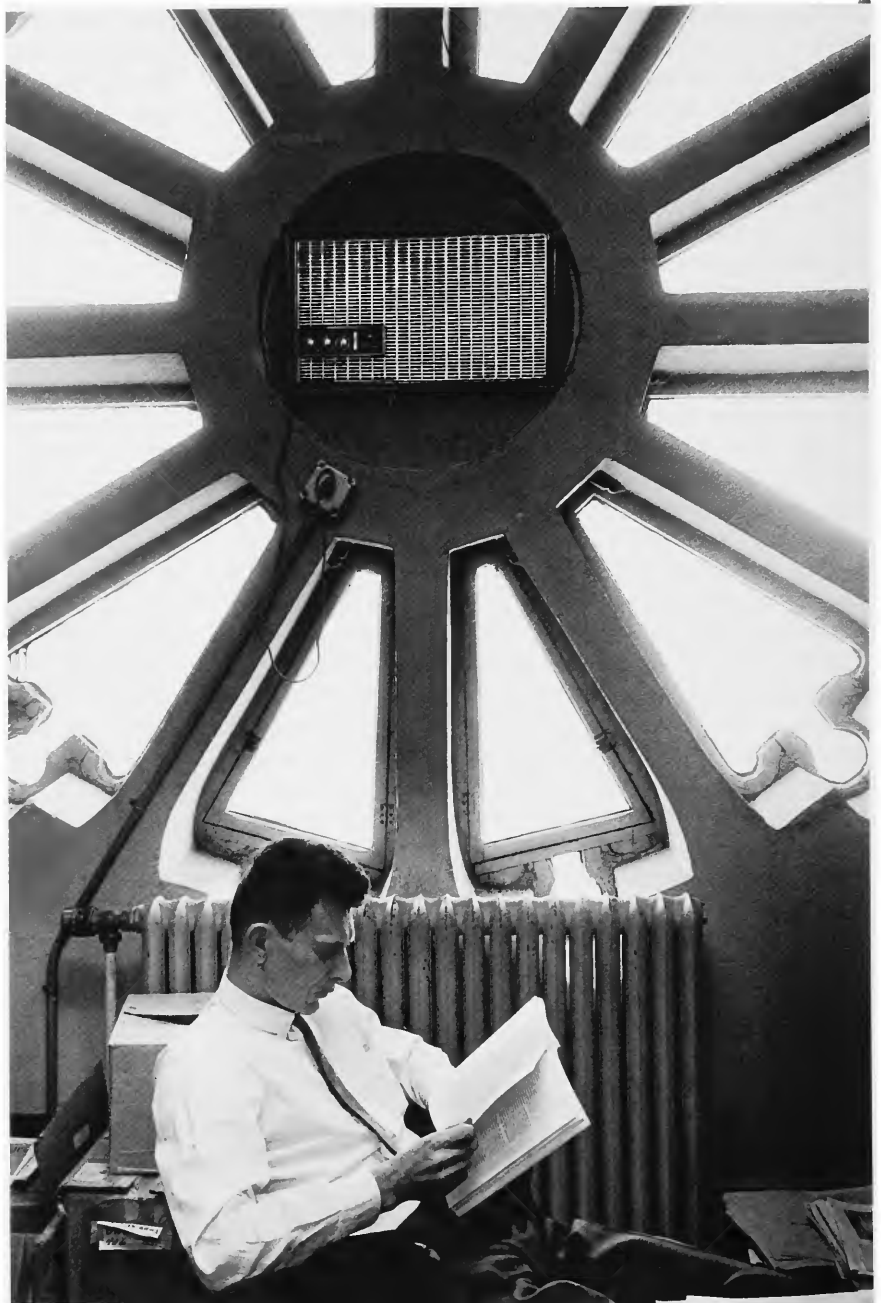
From Sturtevant’s perspective, ethnoscience offered the rigorous methodology that would enable ethnographers to develop a more sophisticated understanding of the conceptual models of other societies and would allow a comparative anthropology to move forward on a firmer empirical footing. He referred to these conceptual models as “classifications,” in the broad sense of being orderings of experience, and argued that the ethnoscience approach could be adopted to explore any cultural domain, regardless of the degree to which it was structured by more specific classificatory principles, like taxonomic inclusion, or the extent to which its contents were explicitly labeled in the native language. At the same time, he noted that ethnoscience research conducted to date had concentrated on taxonomic classification and that the analysis of terminological systems had provided the principal avenue for gaining access to native perspectives.

Despite anthropology’s lip service to the importance of learning native languages, few ethnographers gained more than a basic competency in the languages of the people with whom they worked. Ethnoscience insisted, quite logically, that an understanding of native perspectives was possible only if ethnographers acquired a solid command of native languages. They also emphasized the importance of collecting information in the normal contexts of everyday life and of following procedures that approximated local cultural approaches to gaining knowledge. By making these procedures explicit, they hoped that the results of one ethnographer’s research could be checked in the future by others.

The publication of “Studies in Ethnoscience” in 1964 stimulated considerable interest in the approach but also generated sharp criticism. In a general review, Marvin Harris (1968: 568–604) faulted ethnoscience, as well as most other approaches in anthropology, for privileging native perspectives over those of outside observers and for perpetuating mentalist perspectives at the expense of more materialist ones. Because of its emphasis on native terminologies and its adaptation of some methods and concepts originally developed in descriptive linguistics, he and others dismissed ethnoscience as a misguided attempt to reduce culture to language and to impose linguistic models on largely nonlinguistic cultural phenomena (Berreman, 1966; Keesing, 1972). Another common critique was that the results of ethnoscience research were “trivial,” or as Harris (1968:592) expressed it, “the net contribution to substantive theory is less than what usually results from equivalent labor in-puts.”

Nonetheless, the methods and perspectives of ethnoscience were widely embraced by anthropologists in the 1960s and 1970s and have endured in several research specialities, such as

FIGURE 4.—William Sturtevant in his Bureau of American Ethnology office in the Smithsonian Castle, Room 406, January 1965. Photograph by Stewart Brand.



ethnobiology and cognitive science. The impact of the standards for ethnographic research established by its early practitioners is also evident, although seldom acknowledged, in much of contemporary ethnography. For his part, Sturtevant considered most of the critiques of ethnoscience to be wrong-headed, being based on misunderstandings of its aims and underlying principles, but he never responded in print to any of them. By the time they appeared, he was heavily involved in other, mainly ethnohistorical, research projects and had already

anticipated and addressed many of the critiques in his essay. For example, he had no illusions about the amount of time and resources required to implement the ethnoscientific approach in ethnography, commenting,

Ethnoscience raises the standards of reliability, validity, and exhaustiveness in ethnography. One result is that the ideal goal of a complete ethnography is farther removed from practical attainment. The full ethnoscientific description of a single culture would require many thousands of pages published after many years of intensive field work based on ethnographic methods more complete and more advanced than are now available. (Sturtevant, 1964e:123)

Ethnographic research could proceed more rapidly only by lowering the standards of good ethnography or by underestimating or ignoring the vast complexity of human cultures. Sturtevant regarded both alternatives as untenable if anthropology's expressed commitment to understanding humanity was to be taken seriously.

In 1962 Sturtevant began planning a year-long research project in Burma to complete an ethnoscientific analysis of clothing in the Pegu District northeast of Rangoon. He intended the project to provide a counterpoint to his research on Seminole clothing as well as an opportunity to develop ethnographic methodology within the framework of ethnoscience. Burma was an appropriate place to conduct the project because it was one of the few countries in the world whose citizens had not adopted European-style clothing, but his decision to focus his project there was motivated primarily by his wife's desire to visit her family and to have their children learn about her country. She had not returned to Burma since the summer of 1955, when she introduced her husband and their infant daughter, Kinthi (born in 1954), to her family. In the interim, they had had two more children—Reed, born in 1956, and Alfred, born in 1958—and the country had undergone a number of radical political changes. The army had taken over the government in 1962 and was in the process of transforming Burma, which they renamed Myanmar in 1989, into a pseudosocialist state.

In 1963 Sturtevant received a grant from the National Science Foundation to undertake the project and secured permission from the BAE to take a one-year leave of absence. In May of that year, his close friend and mentor John Goggin died after an eight-month-long illness with cancer. Sturtevant (1964b) prepared an obituary, which was published in the *American Anthropologist* the following year. Earlier, realizing that Goggin's illness was terminal, he had joined Charles Fairbanks and Rouse to organize a collection of Goggin's writings, also published in 1964 (Fairbanks et al., 1964).

On 4 October 1963, the Sturtevant family left Washington, D.C., arriving in Rangoon on 24 October. On the surface the country seemed little changed from their 1955 visit, but the new political climate made fieldwork impossible. Sturtevant was unable to get permission from the Burmese government to spend any significant amount of time outside the capital, and government officials, infused with antiforeigner sentiment, were suspicious of him and kept close tabs on his movements and on the people with whom he associated. He was tempted to try to work in the Pegu District without official approval, but he feared that the government would revoke his visa, thereby cutting short their visit, or that it would take reprisals against his wife's relatives, whose political situation was already tenuous. Her father, Dr. Ba Maw, had served between 1937 and 1939 as Burma's first premier after the British established Burma as a colony separate from India and had also served as Burma's head of state between 1943 and 1945, during the Japanese occupation. Although Maw did not support the 1962 military coup, he had not yet been identified by the government as

an enemy. His was one of the few politically prominent families in Burma whose members did not form part of the military government and had not yet been jailed.

Despite these difficulties, Sturtevant was able to visit neighborhoods in Rangoon and villages in the surrounding countryside, examine photographs in several archives, study the Burmese language, and read extensively about the country's history and culture. He also became quite interested in Burmese drama and began attending performances, especially of the Indian epic *Ramayana*, but he felt that he lacked the requisite language skills and background knowledge to make these performances the focus of his research. In the process, he assembled extensive notes on Burmese clothing and many other aspects of the culture, took hundreds of photographs, and made a large collection, 386 objects in all, of clothing and other objects for the Smithsonian.

He also had the opportunity, in 1964, to visit Inle Lake in the Southern Shan States southeast of Mandalay, where he examined local approaches to artificial island agriculture. Agricultural systems had been an important focus of his work in Florida and the Caribbean, and he had observed another example of this unusual approach to agriculture—in which earth is moved to water rather than the opposite—during a brief visit in 1960 to the “floating gardens” of Xochimilco, in the suburbs of Mexico City. In 1968 he collected data on a similar system in Kashmir, and he presented a paper on the topic that year at the Eighth International Congress of Anthropological and Ethnological Sciences, held in Tokyo and Kyoto (Sturtevant, 1970). This research established that artificial island agriculture emerged independently in different parts of the world and lent support to the conclusion that early Spanish observations of this form of agriculture in the Valley of Mexico had been accurate.

About five months after arriving in Burma, Sturtevant was shocked to learn that the BAE had become the focus of a reorganizational plan being developed by S. Dillon Ripley, who on 1 February 1964 assumed the duties of Secretary of the Smithsonian Institution. Ripley was concerned by the general state of anthropology at the Institution and was being drawn to the conclusion that merging the BAE and the Department of Anthropology would make for a stronger program. The department in various organizational guises had formed part of the Smithsonian since the Institution's founding in 1846.⁷ Its staff was responsible for curating the Institution's anthropological collections, predominantly from North America but including important materials from other parts of the world. The BAE was created in 1879 as a separate research bureau focused on North America. Over the years, the two units developed a cooperative relationship—among other things, the department curated the enormous collections amassed by the BAE's researchers—but in recent decades the department had been able to increase its research staff and budget, whereas those of the BAE had declined. By 1964 Stirling had retired from the BAE, and Roberts retired in the spring of that year. Its research staff

then consisted only of Sturtevant, Collins, and Robert Laughlin, who had been hired in 1962.

To determine the future of anthropology at the Institution, Ripley consulted with anthropologists in the BAE, in the department, and outside the Smithsonian. In a series of letters written from Rangoon, Sturtevant provided Ripley with his perspective on what he believed should happen. He argued that the BAE should remain an independent research unit, but that it should be transformed into “a bureau of ethnology in the modern sense” by increasing its staff to include 10 to 15 research positions and expanding its focus from the Americas to the entire world.⁸ Ripley was not unsympathetic to Sturtevant’s vision, but he believed that the interests of anthropology would be better served by merging the BAE and the department into a new unit that could then serve as the foundation for a separate museum of anthropology.

The Department of Anthropology, 1965–1997

The BAE was officially abolished on 1 February 1965, and its staff, library, and archives were subsequently moved from the Smithsonian Institution building (the Castle) across the National Mall to the Department of Anthropology’s recently expanded space in the National Museum of Natural History (NMNH). The new unit created by the merger of the BAE and the Department of Anthropology was named the Smithsonian Office of Anthropology (SOA). Its position within the organizational structure of the NMNH was higher than that of the other departments in the museum but was lower than that formerly held by the BAE, which had been an independent bureau within the institution.

Waldo Wedel, a senior scientist in the museum who specialized in Plains archaeology, was named the first chairman of the SOA, but he was soon replaced by Richard Woodbury, a southwestern archaeologist. A few months later, Secretary Ripley decided that the SOA needed additional, more dynamic leadership and invited Sol Tax to become its head, apparently on the recommendation of Collins. Ripley believed that Tax—a professor of anthropology at the University of Chicago and a major figure in international anthropology at the time—would transform the SOA into one of the most important centers of anthropology in the world (Tax, 1988; Stanley, 1996). Tax was intrigued by the possibilities, but he did not want to resign his position at the University of Chicago, so Ripley asked him to spend a few days each month at the institution as a special advisor for anthropology. Tax accepted this revised offer and began familiarizing himself with the Smithsonian’s anthropology program and developing ideas for the future of the new unit.

Tax was immediately concerned that the research staff of the SOA had failed to develop programs that went beyond their personal research projects. He asked them to prepare descriptions of their work and to offer their perspectives on new programs that could be developed, organizing a meeting in late January 1966 to discuss their ideas. Among the alternatives

considered was the proposal that a new edition of the two-volume *Handbook of American Indians North of Mexico*, originally published by the BAE in the first decade of the twentieth century, would be appropriate. Everyone supported the idea, and a second meeting was held a short time later to decide on the general format of the new *Handbook of North American Indians* and to determine who would be in charge. A few of the curators felt that a dictionary format like that of the original *Handbook* would be best. Sturtevant proposed instead that a more ambitious, multivolume collection of detailed essays would be more useful, on the lines of the *Handbook of South American Indians* published by the BAE between 1946 and 1959 and the *Handbook of Middle American Indians* then in preparation at Tulane University. The majority of the curators concurred with Sturtevant’s position, and he volunteered, and was designated, to coordinate the project.

Sturtevant was attracted by the prospect of becoming general editor of the new *Handbook* for several reasons. No comprehensive, scholarly overview of American Indians was then available except for the original *Handbook*, which was badly out of date. A new *Handbook* would make available to a broad audience the significant advances in knowledge about North American Indians that had been accomplished during the previous 60 years. He was also becoming concerned that his inclination to pursue somewhat disparate research topics and to focus on rather specific issues would preclude his ever producing a major synthesis of North American ethnology. He believed that organizing the new *Handbook* would provide him the opportunity to make a major contribution to the field.

With characteristic energy and enthusiasm, Tax began elaborating his vision of a new Smithsonian anthropology, but he soon encountered difficulties. Because he was reluctant to leave Chicago for more than a few days at a time, Tax could not maintain contact with the Smithsonian staff at the level required to implement his plans, even after the Smithsonian hired his former student Samuel Stanley to keep Tax informed and to coordinate the programs on a daily basis. A more serious problem was the lack of support for and, in some cases, opposition to his plans on the part of several members of the SOA research staff. Some were concerned that Tax’s ambitious vision ignored the basic responsibilities of the staff to the museum’s collections and that its implementation would overwhelm the staff with new duties. Others resented Tax’s attempts to direct the activities of the SOA from a distance or feared that his plans would have a detrimental impact on their personal research programs.

By November 1967 Tax was convinced that this opposition would preclude moving ahead with his plans for the SOA, and he proposed to Secretary Ripley that the new programs that he and a few members of the SOA were developing should be organized within a distinct unit. An outside committee, appointed to review the status of the SOA in January 1967, concurred with Tax’s suggestion, and on 1 July 1968—just three years and six months after he abolished the BAE—Secretary



FIGURE 5.—The staffs of the Bureau of American Ethnology and the River Basin Survey (RBS) at the time of the BAE's merger with the Department of Anthropology, on the steps of the NMNH, September 1965 (left to right): Front row: Matthew W. Stirling (director, retired), Jessie Shaw (administrative assistant), Carl Miller (RBS), Florence Morgan (secretary); center row: Evelyn S. Anderson (secretary), Robert L. Stephenson (RBS), Robert M. Laughlin (ethnologist), Karlana Glemser (secretary), Henry B. Collins (archaeologist); back row: William C. Sturtevant (ethnologist), Edward G. Schumacher (illustrator), Harold A. Huscher (RBS), Margaret C. Blaker (archivist), Rachel Penner (archival assistant). Missing: Frank H.H. Roberts, Jr. (archaeologist, retired). Courtesy of the National Anthropological Archives, Smithsonian Institution (neg. no. 1530B).

Ripley created the Center for the Study of Man, with Tax as its acting director and Stanley as its program coordinator. Three months later, the Smithsonian Office of Anthropology was demoted to the status of a department. Sturtevant and other members of the SOA who supported Tax's plans were invited to join the Center along with a few other people from within the Institution and several leading scholars from the international anthropological community. The *Handbook of North American Indians* project was also designated as one of the Center's programs.

Before the merger of the BAE and the Department of Anthropology, Sturtevant regularly joined members of the department's staff for lunch and regarded many as his friends. The merger, however, generated conflicts, especially between him and Clifford Evans, a Latin Americanist archaeologist who had been one of its more active proponents. Sturtevant resented

Evans's role in bringing about the demise of the BAE and his tendency to resort to heavy-handed and, from Sturtevant's perspective, underhanded tactics in departmental and institutional affairs. For his part, Evans considered Sturtevant's opposition to many of his plans to be unreasonable and was frustrated by his inability to neutralize this opposition or to convert Sturtevant into an ally. In contrast, they seldom disagreed on intellectual matters, the main exception being Evans's view that New World culture history had been significantly affected by trans-Pacific contacts, which Sturtevant rejected for both methodological and factual reasons.

Sturtevant was not the only member of the department who opposed Evans, but Evans also had his allies. By 1969, two factions had emerged, known among the staff as "the Sturtevant faction" and "the Evans faction."⁹ In October of that year, Richard Cowan, director of the NMNH, asked the research

staff to provide him with names of possible candidates for the chairmanship of the department. The supervisors of the four research divisions of the department responded:

We are convinced that the factions in this department are so firmly established, so polarized, and so pervasive, that there is no member of the staff who is sufficiently neutral (or likely to remain so if appointed) to serve effectively as departmental chairman....The only permanent solution to this dilemma, we believe, will come with the establishment of a Museum of Man with a new Director (from outside) with a strong mandate and several additional positions to dilute the existing factions and radically change the administrative, social, and personal environment which has thus far supported the factions.¹⁰

Given this situation, most staff members were surprised and several were appalled when Cowan appointed Evans as chairman, first on a one-year trial basis in 1970 and then for an additional four years, starting in 1971. His appointment escalated the conflicts, and the associated factionalism dominated departmental affairs throughout the 1970s, lingering on even after Evans's death in 1980. William Fitzhugh, who succeeded Evans as chairman of the department in 1975, worked hard to improve relations within the department, as did Douglas Ubelaker, who succeeded Fitzhugh in 1980. Their efforts were facilitated by the hiring of several new curators in the late 1970s and early 1980s to replace curators who had retired, and by the mid-1980s the factionalism had disappeared entirely.

When Sturtevant was transferred from the BAE to the Department of Anthropology, he assumed curatorial responsibility for all of the North American ethnology collections. The other North Americanist ethnologist in the department was John C. Ewers, a noted Plains specialist hired by the department in 1946. In the 1950s, Ewers had been responsible for renovating the American Indian exhibition halls in the United States National Museum, and in the 1950s and early 1960s, he had played a major role in creating the National Museum of History and Technology (now the National Museum of American History) (Ewers, 1956, 1959). In recognition of this service to the institution, he had been given a largely research position, with the title of "senior scientist." Sturtevant and the other curators tried to shield him from day-to-day curatorial concerns, consulting with him only on issues about which he had special expertise or which they believed would be of interest to him.

Although Sturtevant had never intended to be a museum curator, he was not averse to becoming one. He had long maintained an interest in material culture, had conducted considerable research in museums, and had taught a course and prepared a bibliography on the subject when he was an instructor and assistant curator at Yale (Sturtevant, 1955b). He had deposited ethnographic and archaeological collections from the United States, Mexico, and Guam at the Yale Peabody Museum. He also made large ethnographic collections for the Smithsonian from the Seminole and from Burma as well as smaller collections of Catawba pottery and Tarascan laquerware, the latter collected during a brief visit to Uruapan, Michoacán, Mexico, in January 1960. In the 1950s he had worked with Ewers in planning the Seminole case for the museum's North American Indian exhibition halls, and in subsequent

years he served as a consultant on a number of large exhibitions at the Smithsonian and other museums.

In his new position, Sturtevant emerged as a major advocate for the view that museums had an important role to play in the anthropological enterprise. In two articles (Sturtevant, 1969a, 1973) he explored, within the framework of the history of anthropology museums and material culture research, what this role should be, and he prepared a "Guide to Field Collecting of Ethnographic Specimens," which he hoped would "improve the quality and research usefulness of collections of ethnographic materials" (Sturtevant, 1967d:1). Beginning in 1964, he was a member of the American Anthropological Association's Committee on Anthropological Research in Museums, which was supported from 1965 to 1974 by the Wenner-Gren Foundation for Anthropological Research. On 25 February 1970, Sturtevant and some other committee members wrote to Nelson Rockefeller, then governor of New York, and to the regents of the University of the State of New York explaining why wampum belts currently housed in the New York State Museum in Albany should not be returned to the Onondaga who had requested them. Because they represented themselves as a committee of the American Anthropological Association but failed to clear their statement with the Association's executive board, in 1971 the Association severed its connection to the committee. The following year, the American Ethnological Society decided to sponsor the committee, which continued to enjoy the support of Wenner-Gren. In 1974 the committee evolved into the Council for Museum Anthropology (Freed et al., 1977). Sturtevant was the council's first vice president, serving two terms between 1974 and 1978, and was its president from 1978 to 1981.

Immediately after Sturtevant was officially designated as the editor of the new *Handbook of North American Indians*, in 1966, he and Stanley began preliminary planning, developing a general outline of its contents, preparing lists of potential contributors, analyzing the coverage of the old *Handbook*, and working out budgetary and personnel requirements for the new *Handbook* and its staff. More detailed planning of the *Handbook*, however, did not get underway until 1969. Between 1965 and 1969 Sturtevant wrote several articles on Indian agriculture (Sturtevant, 1965b, 1965d, 1969b), and, with Stanley, compiled an overview of contemporary Indian communities in the eastern United States (Sturtevant and Stanley, 1968). He also prepared entries for the *Encyclopaedia Britannica* on "mutilations and deformations," "tattooing," and "scalping," which summarized ethnographic and historical information on these practices from around the world (Sturtevant, 1965c, 1965e, 1967e). The entry on tattooing provided the point of departure for a more extensive overview of the subject (Sturtevant, 1971c), and later he expanded his entry on scalping into a full-length essay, prepared in collaboration with James Axtell, which convincingly refuted the idea that scalping had been introduced to the New World by Europeans (Axtell and Sturtevant, 1980).

During this period, Sturtevant produced one of his most important studies of Seminole material culture. Presented in 1966 at the annual meeting of the American Ethnological Society and published the following year, this study focused on Florida Seminole men's clothing to trace the evolution of their clothing styles during the period of their greatest isolation, from the mid-nineteenth to the mid-twentieth centuries. Although Sturtevant (1967f:160) characterized the paper as giving "the preliminary results of a larger study," it is a mature work, the culmination of over 15 years of field, museum, archival, and library research, in which he had collected information on all major and most minor museum collections of Seminole artifacts, compiled a corpus of over 1000 illustrations of the Seminoles from the eighteenth to twentieth centuries, and consulted extensively with Seminole people on the interpretation of these materials. In addition to providing a chronological typology of certain elements of this clothing and detailed information on their construction, he examined their relationship to the material culture of both Europe and other areas of North America and their role as identity markers among the Seminoles and other North American Indians. He indicated that the Florida Seminoles employed a "reconstructed older-style Seminole costume" (Sturtevant, 1967f:173), sometimes mixed with modern-style clothing, to distinguish themselves from both non-Indians and other Indians alike, but that Indians in other parts of the United States, both Seminoles and non-Seminoles, had adopted some of these same items to mark a generalized, pan-Indian identity. He also demonstrated how certain methods associated with ethnohistory—in this case, native classifications and componential analysis—could be profitably applied to the study of material culture.

In addition to his research and writing, Sturtevant became increasingly involved in the activities of several professional organizations. He had begun participating as an officer in such organizations in the previous decade, soon after he was hired by the BAE. In 1957 he began a three-year term on the board of governors of the Anthropological Society of Washington, and from 1959 to 1960 he was a member of the executive committee of the Florida Anthropological Society. In the latter year, he joined Thomas Gladwin of the National Institute of Mental Health to organize the annual lecture series of the Anthropological Society of Washington, for which they invited nine speakers from anthropology, linguistics, and psychology "to take a critical look at a variety of strategies available for the study of human behavior in a cultural context" (Gladwin and Sturtevant, 1962:vii). The essays were published in a volume titled *Anthropology and Human Behavior* (Gladwin and Sturtevant, 1962). From 1962 until 1968 he worked as the book-review editor and associate editor of the *American Anthropologist*, and in 1969 he began serving on the American Anthropological Association's Committee on Archives. His membership on this committee was especially appropriate, not only because of his commitment to consulting archival materials in his own research but because he had been instrumental in

establishing the National Anthropological Archives—created through the merger of the archives of the BAE and the Department of Anthropology—as a major repository of anthropological materials from around the world.¹¹

He also devoted considerable time to the development of the American Society for Ethnohistory, serving on its executive committee in 1959 and as its president between 1965 and 1966. This society began as the Ohio Valley Historic Indian Conference, and then, around 1958, changed its name to the American Indian Ethnohistoric (later "Ethnohistorical") Conference. In 1966 Sturtevant convinced the majority of the members that the society should have a global rather than a strictly North American focus and that its name should be the "American Society for Ethnohistory" rather than the "Society for American Ethnohistory," which many preferred. At the same time, he explored ethnohistory as an intellectual endeavor, providing in his essay "Anthropology, History, and Ethnohistory" (Sturtevant, 1967b) a definitive analysis of the relationship between history and anthropology and of the relevance of historical data and methods to anthropological research.

In June 1967 Sturtevant briefly visited the Seminoles in Florida. The following month he and his family left Washington, D.C., for England where he spent a year, at Rodney Needham's invitation, as a Fulbright scholar and lecturer at Oxford University's Institute of Social Anthropology. He returned to the United States in September 1968, traveling first to Germany to attend the International Congress of Americanists, then to Kashmir to collect data on artificial island agriculture, and on to Japan to present a paper on the topic (Sturtevant, 1970).

After settling back in Washington, he became active in the anti-Vietnam war effort, signing petitions, attending demonstrations, and supporting anti-war motions at the business meetings of the American Anthropological Association. He also helped draft an advertisement, published in 1968 in the *American Anthropologist* (70:1311–1317) and signed by over 800 members of the association, protesting an advertisement from the United States Navy for anthropologists to participate in psychological warfare in Vietnam, which had been published in the same journal two issues earlier.

During this period, Sturtevant began devoting increasing amounts of his time to planning the new *Handbook*. Faced with the size of this task and the pressures of a number of unfulfilled writing commitments, he found it difficult to accomplish anything. He discussed the problem with Tax, who recommended that he "wipe the slate clean" by cancelling all his commitments except the *Handbook*. He followed Tax's advice and soon was able to move ahead on his various projects.

By 1970 Sturtevant had established, after extensive consultation with a number of North Americanists from the United States, Canada, and Europe, that the *Handbook* would be organized into 20 volumes. Eleven of these volumes would focus on specific North American culture areas, whereas seven would explore general topics from a pan-North American perspective. The remaining two volumes would be devoted to an



FIGURE 6.—William Sturtevant collecting botanical specimens on a floating agricultural island at Dal Lake, Kashmir, 1968. Photograph by Manmohan Durani.

introduction and index of the entire collection. Editors and planning committees were selected for each volume, and an editorial staff was organized within the Center for the Study of Man. In November 1970 a general planning meeting for the *Handbook* was held in Chicago, and planning for each volume continued into 1971.

Manuscripts began arriving at the *Handbook* office in December 1971, and by the end of the following year they filled several file cabinets. Sturtevant expected the contributions to be accurate and up-to-date, and he insisted that they be accompanied by detailed bibliographies and well-researched photographs, line drawings, and maps. He gave each manuscript a close reading, preparing detailed comments for authors and often filling in gaps and checking the accuracy of information himself. If the original authors were unable to complete their manuscripts, he worked with the volume editors to find alternates. When a manuscript was revised, he read it again, and a third time when it was in galley proofs. A tremendous amount of work was involved, but his meticulous editing and insistence that the contributions conform to his high standards ensured the quality of the volumes.

Despite his *Handbook* duties, Sturtevant continued to be active in professional organizations, especially the American Anthropological Association, the Council for Museum Anthropology, the Anthropological Society of Washington, the American Association for the Advancement of Science, and the American

Ethnological Society. In 1977, he became president of the American Ethnological Society, for which he prepared a new constitution and set of by-laws. In 1992 he was elected president of the Anthropological Society of Washington. Between 1976 and 1982 he served three terms on the board of trustees of the Museum of the American Indian-Heyle Foundation and was appointed to a fourth term between 1984 and 1986. During his tenure, he worked with the other board members to resolve the serious fiscal and management crises that confronted the museum and helped to convince them to appoint Vine Deloria and George Abrams as trustees, the first American Indians to serve in the capacity.¹²

He also began to increase his interaction with students. He served as the principal advisor to a number of Smithsonian pre- and postdoctoral fellows and summer interns and as a member of several thesis committees. In 1974 he accepted an appointment as an adjunct professor in the newly created Department of Anthropology at Johns Hopkins University, a position he held until 1989. There he offered courses on American Indians, linguistics, and technology and material culture as well as on less traditional topics, which he labeled “graphic systems,” “pictorial ethnohistory,” and “social archaeology.” He also joined Sidney Mintz to teach a course on the anthropology of food and eating.

His involvement in these activities left him little time to devote to his personal research. Nevertheless, he maintained a

prodigious level of writing and even was able to visit briefly, in 1975 and 1977, Seminole Maroon communities in northern Mexico and the Bahamas. Among the impressive number of publications that he produced during this period are several articles on Southeastern ethnography, history, material culture, and ethnobotany (Sturtevant, 1971b, 1978a, 1979a, 1979b, 1979c, 1979d); a synthesis of early Seminole history (Sturtevant, 1971a); an overview of the hole-and-slot heddle in the New World (Sturtevant, 1977c); a chapter on the Seneca-Cayuga of Oklahoma for the *Northeast* volume of the *Handbook* (Sturtevant, 1978b); and essays reporting the results of his extensive research on early European representations of American Indians (Sturtevant, 1975, 1976, 1977a, 1977b, 1978c, 1979b, 1980b, 1981b, 1981d, 1982). He also edited a collection of essays for the catalog of a Smithsonian exhibition on Northwest Coast material culture (Sturtevant, editor, 1974) and, with Jerald Milanich, edited a seventeenth century Franciscan confessional that included some of the most detailed ethnographic and linguistic information available on the extinct Timucua Indians of northern Florida (Milanich and Sturtevant, 1973). Because of his now well-established reputation as one of the leading North Americanists in the world, he was invited to supervise the preparation of general ethnographic and linguistic maps of North America for the *National Atlas*, the *National Geographic*, and the *Times Atlas of World History* (Sturtevant, 1967c; Sturtevant, consultant, 1972; Sturtevant et al., 1978). He also served as the principal consultant and author of the chapter on the Indians of eastern North America for a popular volume on North American Indians published in 1974 by the National Geographic Society (Billard, 1974; Sturtevant, 1974).

In 1976 Sturtevant's work on the *Handbook* began to be shared by Ives Goddard, hired that year as a curator by the Department of Anthropology. Goddard had served as the linguistic editor of the *Handbook* since 1970, responsible for evaluating the information on American Indian languages submitted by authors and for coordinating consultations with other linguists on these materials. After 1976, in addition to his work preparing linguistic sections for the *Handbook*, he increasingly fulfilled other duties related to the general management of the project.

The first volume of the *Handbook*—volume 8, *California*, for which Robert F. Heizer served as the volume editor—appeared in 1978 and immediately received rave reviews. The comments of Claude Lévi-Strauss are representative:

Here is the first product of a monumental enterprise in gestation for more than ten years under the general direction of W. C. Sturtevant, who brings to the project his organizational talent, his immense erudition, and—one appreciates this on reading the pages where he introduces the whole project—the bold inventiveness and originality that characterizes it. . . . It is hardly necessary to add that the new *Handbook of North American Indians* promises to be what it already is for the California area: an absolutely indispensable tool that should be found on the shelves of all libraries, public and private alike. (Lévi-Strauss, 1979: 77–79)¹³

The original production schedule for the *Handbook of North American Indians* envisioned that all 20 volumes would be

published by 1976, in conjunction with the bicentennial celebration.¹⁴ Everyone involved, however, had underestimated the time required to complete such a large and complex project. A second volume, on the Northeast, was published in 1978, followed a year later by the first of two volumes on the Southwest. Between 1980 and 1990, volumes were published at intervals of between one and three years: volume 6, *Subarctic*, in 1981; the second volume (10) of the *Southwest* in 1983; volume 5, *Arctic*, in 1984; volume 11, *Great Basin*, in 1986; volume 4, *History of Indian-White Relations*, in 1989; and volume 7, *Northwest Coast*, in 1990.

Several strategies designed to increase the rate of production were implemented—the general supervision of the *Handbook* was taken over by the associate director of the NMNH and later by the chairman of the Department of Anthropology;¹⁵ managing editors were hired to direct the editorial staff; volume editors were contracted to come to Washington to supervise the completion of their volumes—but to little effect. In the 1990s, a series of fiscal, logistical, and organizational problems combined to slacken the pace of production further and no volumes were produced until 1996, when volume 17, *Languages*, edited by Goddard, appeared.

Despite these difficulties, Sturtevant's commitment to the *Handbook* remained firm. He continued to fulfill his duties as general editor, but around 1980 he began to focus more of his time and energy on other activities. He served as president of the American Anthropological Association from 1980 to 1981 and then spent part of the spring semester of 1981 at the University of California at Berkeley as a Regents Lecturer. In 1982 he began a six-year term on the board of directors of Survival International, which overlapped with his fourth term (1984–1986) on the board of trustees of the Museum of the American Indian-Heyle Foundation. Between 1981 and 1985 he sponsored four Smithsonian postdoctoral fellows and served on two thesis committees at Johns Hopkins. In 1980 he and Theda Maw separated, and they divorced in 1986. He spent the academic year 1986–1987 as a Smithsonian Fellow at Oxford University's Worcester College, and in 1990 he married Sally McLendon, a linguist and anthropologist at Hunter College and the graduate school of the City University of New York.

Sturtevant's curatorial duties changed somewhat in September of 1980 when I was hired as an associate curator of anthropology and North American ethnologist to fill the position vacated by Ewers, who had retired in 1978. Sturtevant and I agreed to divide the responsibility for the North American ethnology collections according to our areas of expertise: he would curate the collections from eastern North America, the Arctic, Subarctic, and the Northwest Coast, while I would curate those from the Plains, Great Basin, Plateau, California, Southwest, and northern Mexico. Apart from standard activities like reviewing loan requests and proposing new accessions, two major curatorial matters dominated our attention in the 1980s and 1990s. The first was the move of the entire ethnological and archaeological collections of the Department of



FIGURE 7.—William Sturtevant at work in his office in the Department of Anthropology, NMNH, September 1980. Photograph by Sheila Hicks.

Anthropology from the attics and hallways of the NMNH, on the National Mall, to a state-of-the-art storage facility in Suitland, Maryland, in the Washington, D.C., suburbs. This move was largely completed for the North American ethnology collections by 1992. The second matter was responding to requests from American Indian tribes for the return of objects, usually of religious significance, from the collections. We received the first major repatriation request from the Pueblo of Zuni in 1981 and responded to several other, smaller requests in the following years.¹⁶ With the passage of federal repatriation legislation in the 1990s, repatriation-related activities have taken priority over all our other curatorial duties.

Throughout the 1980s and 1990s, Sturtevant continued to pursue his long-term research interests. He worked with Seminole people in both Florida and Washington, D.C., on several different projects, including the creation of a new exhibit on the Seminoles in the permanent North American Indian halls of the NMNH. He also compiled two anthologies on Creek and Seminole history and culture (Sturtevant, editor, 1987a, 1987b) and published articles on a wide range of topics, including anthro-

pological museums and archives; American Indian art, material culture, and political organization; and Eastern North American Indian ethnology, history, and linguistics (Sturtevant, 1980c, 1981a, 1981c, 1983b, 1985, 1986a, 1986b, 1991a, 1991c, 1994a, 1994b; Meltzer and Sturtevant, 1983; Krech and Sturtevant, 1992, 1995; Archambault and Sturtevant, 1996a, 1996b). In the last category were two major studies of Iroquois culture, one providing a detailed overview of Seneca masking, the other a thorough analysis of modern Iroquois ritual as practiced in the 15 principal Iroquois communities in Ontario, Quebec, New York, and Oklahoma (Sturtevant, 1983a, 1984).

In addition to these diverse projects, he devoted considerable time to his investigation of early European illustrations of the New World and its native inhabitants, the main focus of his research during this period. When he began this project in the 1950s, his primary goal had been to locate illustrations of eastern North American Indians produced prior to the emergence of photography in the mid-nineteenth century. Soon, however, his research encompassed the entire Western Hemisphere as well as other parts of the world and became a central compo-



FIGURE 8.—William Sturtevant with Claude Lévi-Strauss in his office at the Collège de France, Paris, 24 November 1981. Photograph by Josef Koudelka.

ment of his efforts to understand native New World cultures and societies in historical context.

He considered such illustrations to be an invaluable complement to early written descriptions of New World people because they could portray certain kinds of ethnographic data not readily described in words. The information they contained, however, like that found in historical documents, could not be accepted uncritically. To evaluate their ethnographic reliability, he began comparing the ethnographic content of the illustrations with information preserved in historical documents, the archaeological record, museum collections, and modern ethnographies. He soon discovered that European illustrators often incorporated elements into their portrayals of the New World that they derived from the compositions of their predecessors, in some cases originally depicting scenes, people, or objects from Africa, Asia, or other parts of the Old World. He encountered numerous examples of what he called “recurring visual images” (Sturtevant and Quinn, 1987:99), in which the same or similar image appeared in a sequence of illustrations, frequently attributed to quite distant areas of the New World at distinct points in time.

To sort out the “genealogical” linkages among these illustrations and to establish the corpus of illustrations and other information available to artists when they produced their works, he reconstructed the chronology of European illustrations and written accounts of the New World and the history of their dif-

fusion within Europe. To the same end, he investigated the history of New World people and artifacts in Europe, whose presence would have afforded artists the opportunity to produce illustrations from life without actually traveling to the New World. He also examined the intended audiences of the illustrations, prevailing conventions of composition and aesthetics, and European stereotypes of the New World, not only to determine the impact of such factors on the accuracy of the illustrations but to explore the social and cultural contexts within which they were produced. From early in his research, he had realized that these illustrations frequently revealed as much about the societies and cultures of the artists as they did those of their subjects.

By the 1990s he had devoted nearly four decades to this work and had located and analyzed several thousand relevant drawings, paintings, and sculptures. In addition to developing the methodology required to undertake this kind of research, he filled a number of gaps and corrected many mistakes in the historical ethnography of the New World. The results of his work have had substantial impact not only on historical anthropology and New World ethnology but on European and American history, American studies, and art history. His detailed overview of the “First Visual Images of Native America” (Sturtevant, 1976; see also Sturtevant, 1991b), has been particularly important; its significance to the development of a new focus of anthropological inquiry is comparable to that of his earlier essays on ethnohistory and ethnohistory. Another

notable contribution is his careful analysis of the history of the transformation of the vertical feather headdress from an article of limited distribution in the New World at the time of European contact into the preeminent symbol of American Indians for Europeans and Indians alike (Sturtevant, 1988, 1990, 1992). In the course of this and other illustrations-based research, he has provided crucial insights into the evolution of European conceptions of the non-European world, the impact of these conceptions on non-Europeans' images of themselves, and the more encompassing processes of interactions between Europeans and American Indians.

Conclusion

The development of Sturtevant's research and writing over the course of his career has followed a pattern that most scholars, reflecting on their own research careers, will find familiar. His undergraduate and graduate training channeled an early, general interest in anthropology and American Indians into a concentration on more specific areas, which became the focus of his initial research. This research served as the point of departure for a series of projects on related themes as his work on one topic led him to investigate others. The direction of Sturtevant's research also shifted as he began exploring other, often quite different topics and responded to invitations from colleagues to participate in their own projects, and its progress was periodically delayed as he fulfilled growing obligations to his institution and profession.

What sets Sturtevant's research apart is its broad topical and geographical coverage, ranging from ethnobotany and material culture to art and the history of anthropology and encompassing much of the Western Hemisphere as well as other parts of the world. His knowledge of a number of languages—mainly French, Spanish, and German, but also some Latin, Portuguese, Dutch, Italian, Greek, Russian, Chinese, Burmese, Mikasuki, and Creek—and the collegial relationships he has established with scholars from other parts of the world, especially Europe and Latin America, have given him access to information and perspectives not available to most American anthropologists. Because of the breadth of his interests, he has contributed to the advancement of scholarship on a number of different topics, with the majority of his work ultimately linked to the development of knowledge within two general areas: the historical ethnology of the New World and the definition and practice of anthropology as a scholarly endeavor.

As a result of his fieldwork, Sturtevant has established the ethnographic foundation upon which all subsequent research on the Seminole will be based, and he has also provided important insights into neglected aspects of Iroquois culture. Convinced of the need to understand the results of his contemporary ethnographic research within a broad cultural and historical framework, he has contributed significantly to the development of a comparative ethnology and history of the Indians of eastern North America and the Caribbean and extended the ethnographic record for these and other regions of the New

World back to the time of the arrival of Europeans. This work and his extensive interaction with the scholarly community have stimulated much important new research on American Indians. As general editor of the *Handbook of North American Indians*, he has also been instrumental in the creation of what many regard as the most important general overview of North American Indian culture and history ever produced.

Sturtevant has developed his ethnological and historical research in tandem with his work on defining the aims and scope of anthropology and his exploration of how the practice and results of anthropological research can be enhanced. He has elaborated a vision of anthropology as a discipline devoted to understanding humanity in all its dimensions and diversity, in which both the unique interests of each of the subdisciplines and their interdependence are recognized. He has also situated anthropology within the framework of scholarship in general, examining the features that distinguish anthropology from other scholarly disciplines as well as its linkages to them.

Sturtevant has focused much of his research at the interface between anthropology and other disciplines, mainly history, art history, and the natural sciences. Through this work, he has demonstrated the value of a multidisciplinary approach in which data drawn from sources traditionally associated with many different disciplines are brought to bear on particular research problems. In the process, he has worked out the methodology required to employ these materials in anthropological research and has played an important role in the development of several disciplinary-bridging specialities within anthropology, like ethnohistory and ethnobotany. He has also argued for decades that material culture and museum collections should be incorporated into the mainstream of anthropological research, but only recently have anthropologists and scholars in many other disciplines come to share his perspective.

Sturtevant's views of the discipline of anthropology have clearly influenced the approach he has adopted to his ethnological research. Throughout his career, he has regarded ethnological research as a collaborative undertaking in which scholars build upon the work of others to contribute, according to their distinct interests and inclinations, to the accumulation and refinement of knowledge. In "Anthropology as a Career" (Sturtevant, 1957), he described this research as involving three kinds of activities: recording and presenting information about particular societies and cultures (ethnography); comparing these ethnographic descriptions to isolate general processes and cross-cultural patterns of cultural diversity and similarity (comparative ethnology); and explaining these processes and patterns (cultural theory). In this view, the development of cultural theory obviously relies on the results of comparative ethnology and ethnography, but the three activities are interdependent dimensions of a single intellectual process rather than separate, isolated steps.

Sturtevant has presented his theoretical perspectives in both focused theoretical studies and in the reports of the results of his ethnographic and comparative ethnological research. Through



FIGURE 9.—William Sturtevant in the offices of the *Handbook of North American Indians* with the final page proofs and binding mock-up for the *Arctic* volume, 1985. The two posters are from Nationalmuseet, Copenhagen: left, four Greenlanders, painted in Bergen in 1654 by an anonymous artist; right, a “Tapuya man” from Brazil, painted in 1643 by Albert Eckhout. Photograph by Kim Nielsen. Courtesy of the National Anthropological Archives, Smithsonian Institution (neg. no. 85-3235-8).

this work, he has contributed to a more profound understanding of the general structure of cultural systems, long-term cultural and social processes, the complexities of inter-cultural and inter-ethnic relations, the transformation of non-Western art and material culture into commodities, and cross-cultural conceptions of art and aesthetics. He has also devoted considerable attention to examining the nature of ethnography and to developing the procedures required to improve its quality.

Sturtevant’s extensive publication record is presented in the following paper (Merrill, 2002), but these works only partially reflect the extent of his research. Because he holds himself to such high standards of scholarship, he has been reluctant to publish research that he feels is incomplete. Folders and card files of data organized into numerous ongoing projects fill his office and overflow into the hallway outside.

In 1996 I asked him about these pending research projects. A few weeks later he gave me a list, “compiled from memory alone,” of 64 projects related to a wide range of topics: the his-

tory of anthropology; museum collections; material culture; European explorations and representations of the New World; American Indian art produced within the European art tradition; the historical ethnography, comparative ethnology, ethnohistory, ethnobotany, and linguistics of the New World, especially eastern North America; Tupinambá chiefdoms; Burmese dress; and tattooing around the world. He titled the list “projects for the future” and noted, “These are topics for which I have data, in various amounts, as well as ideas about how to write them up (usually after further investigation). Obviously I am unlikely to follow through on most of them, but my files should be helpful for others in the future.” At the end of the list, he added nine topics on which he has fieldnotes but no plans for publication.

Sturtevant’s contribution to anthropology clearly has not been restricted to his research and publications. He has served the discipline in a variety of capacities, including terms as the president of four of its major professional organizations and as a consultant to a number of universities, museums, and grant-

ing agencies. He has been one of the field's leading advocates for museum anthropology and for standards of museum practice now followed around the world. He has devoted considerable energy to making anthropology accessible to a wide audience and to supporting American Indians in their efforts to gain federal recognition and to defend their rights, especially to land and religious freedom. He has also been an enthusiastic collaborator in the projects of his colleagues and an inspiration and guide to many students and young scholars in their own anthropological pursuits.

Visitors to Sturtevant's office, located on an interior corridor in the Smithsonian's National Museum of Natural History, are inevitably struck by the sheer amount of material it contains. Stacks of papers and books, once characterized in a reprimand from the Smithsonian's safety office as "trash on the floor," line the narrow passages through the office and cover his desk and an adjacent table. Hundreds of books and journals run along two walls for the entire length of the office, organized by subject on shelves that rise from floor to ceiling. File cabinets line another wall and fill the center of the office, joined by index-card files filled with the 3 × 5 inch slips on which he jots down new ideas and records bibliographic information relevant to the myriad topics that are of interest to him. But more impressive than the office itself is his willingness to stop what he is doing to discuss whatever is on his visitor's mind. In my experience, surely shared by many others, I have never found him too busy to exchange ideas, and I have never encountered a topic that failed to interest him or about which he had no knowledge.

To recognize his contributions to scholarship and to his profession, on 14 November 1996 Brown University awarded Sturtevant an honorary doctorate in humane letters. During the presentation ceremony, Vartan Gregorian, president of Brown, read the following citation:

For over forty years a distinguished anthropologist, your career has been marked by a long-standing concern for fusing the anthropological and the historical analysis of culture, and for the preservation of the material record of humankind.

Through extended ethnographic fieldwork among the Seminole, Burmese, Seneca, and other indigenous people of North and Middle America and Asia, combined with intensive research in historical archives on several continents, you have been a pioneer in the development of ethnohistory and a contemporary historical anthropology. By example you have led the way not only to a responsible historiography in which languages and histories of indigenous people are constituted, but to scrutiny of the relationship between museums and the public.

As a general anthropologist at the Smithsonian Institution, you have helped bring American Indians into public consciousness, and in congressional testimony you have left a record on behalf of native rights.

As general editor of the multi-volume landmark series, the *Handbook of North American Indians*, curator of North American ethnology at the Smithsonian Institution, author or editor of more than 200 works, and in your many leadership positions, you have been a major stimulus for research on the native people of North America.

We honor you today for these lasting achievements.

Notes

I am grateful to William Sturtevant and Robert Laughlin for sharing with me their files on the Smithsonian Institution's Bureau of American Ethnology, Department of Anthropology, and Center for the Study of Man and to Stewart Brand, Harold Conklin, and Sheila Hicks for permitting me to use photographs from their personal collections. Robert Sayers kindly allowed me to consult tape recordings of interviews that he conducted with Sturtevant in 1996, and Paul Theerman provided crucial information on the history of the Smithsonian. In preparing the final version of this essay, I have benefitted greatly from the insightful commentaries of Harold Conklin, Ives Goddard, Sally McLendon, Harriett Shapiro, and William Sturtevant.

1. Sturtevant studied French in elementary school and studied Latin, Greek, Russian, and Chinese in high school. In his junior and senior years at Berkeley, he also took courses in German.
2. Sturtevant's military service entitled him to the financial aid provided by the GI Bill, which he used, together with the funds from the Nobel Prize award mentioned by Shapiro (2002), to pay for his education at both the University of California and Yale.
3. Apart from emphasizing the necessary interconnections among the diverse specialties and perspectives that make up anthropology, Sturtevant has long believed that anthropology is an intellectual endeavor that should transcend cultural and national boundaries. In this regard, he found particularly appealing the remark of his friend John Murra (1976:4), who, in describing his attitude toward anthropology, commented, "I pretend it is my only ethnic, religious, and ideologic affiliation."
4. Another of Sturtevant's graduate-student friends, Donald Simmons, was a dedicated bibliophile. He introduced Sturtevant to the world of book collecting, in which he has maintained an avid interest ever since.
5. Sturtevant's linguistics professor at Yale, Bernard Bloch, taught his students that they should approach a new language by focusing first on phonology and then on morphology before moving on to syntax and other aspects of the language. Although such a stepwise approach makes some sense for linguistic analysis, it is not the best way to conduct fieldwork in linguistics. Sturtevant feels that he did not master spoken Mikasuki largely because he followed this approach, and Floyd Lounsbury once told him that he blamed Bloch's overly theoretical approach for diverting Sturtevant from a career in linguistics.
6. While preparing his doctoral dissertation, Sturtevant was employed by the Tri-Institutional Pacific Program at Yale. This program was created by George Peter Murdock to undertake comparative linguistic studies of Malayo-Polynesian languages. Sturtevant and George Grace focused on compiling vocabulary lists from these languages derived from published sources. Grace later did fieldwork and published the results of the project (e.g., Grace, 1955, 1961).
7. For a history of anthropology at the Smithsonian, from the founding of the Institution in 1846 until 1910, see Hinsley, 1981. For information on events related to this history that occurred between 1950 and 1985, I have relied on the annual reports of the Smithsonian Institution (after 1965, titled *Smithsonian Year*) as well as on unpublished documents currently in the files of Sturtevant and Robert Laughlin and on conversations with Sturtevant and other members of the Department of Anthropology.
8. Sturtevant to S. Dillon Ripley, 30 Jun 1964, Rangoon, copy in Sturtevant's possession.
9. In a 1969 memorandum to the director of the NMNH, Sturtevant indicated that he was "uncomfortable" being identified as the leader of the anti-Evans faction in the department. Memorandum, William C. Sturtevant to Richard Cowan, 29 Dec 1969, copy in Sturtevant's possession.
10. Memorandum, 14 Oct 1969, from Divisional Supervisors, Department of Anthropology ([Gordon] Gibson, Old World; [Robert] Laughlin, Latin America; [Lucille] St. Hoyme, Physical (Acting); [William] Sturtevant, North America) to Richard Cowan, copy in Sturtevant's possession.
11. Sturtevant was also responsible for naming the archives. The first name adopted was the "National Archives of Anthropology," but he convinced Margaret Blaker, head of the archives, that this name would lead to confusion with the National Archives and that "National Anthropological Archives" would thus be better.

12. The problems facing the Museum of the American Indian were resolved in large part by its incorporation into the Smithsonian Institution in 1990. Most of the events leading up to this development occurred after Sturtevant was no longer a member of the Heye Foundation's board of trustees.

13. The original is as follows: "Voici donc le premier résultat d'une monumentale entreprise en gestation depuis plus de dix ans sous la direction générale de W. C. Sturtevant qui a mis au service de ce projet son talent d'organisateur, son immense érudition et—on s'en aperçoit à lire les pages où il présente l'ensemble—l'audace inventive et l'originalité d'esprit qui le caractérisent.... Il est à peine besoin d'ajouter que le nouveau *Handbook of North American Indians* promet d'être—qu'il est déjà pour l'aire californienne—un instrument de travail absolument indispensable qui devra figurer dans les rayons de toute bibliothèque publique ou privée" (Lévi-Strauss, 1979:77–79).

14. Until 1988, the majority of funding for the *Handbook* came from federal appropriations originally designated for the bicentennial.

15. These changes in the administration of the *Handbook* were linked to some extent to changes in the place of the Center for the Study of Man (CSM) within the administrative structure of the NMNH. Sol Tax resigned his directorship of the CSM in 1976. In October of that year the director of the NMNH took over the direct administration of the CSM, which began being identified with the National Museum of Man, the new museum of anthropology that was being planned then. By that date, the CSM included, in addition to the *Handbook* and several small programs and projects, major programs in immigration studies and anthropological film. Soon afterward, the *Handbook* was established as a separate unit within the NMNH, and then, in the early 1980s, it was integrated into the Department of Anthropology, along with the program in anthropological film (now known as the Human Studies Film Archives). About the same time, the program in immigration studies was abolished, and the CSM itself was closed in 1983.

16. For a summary of the Zuni repatriation request, see Merrill et al., 1993, and Merrill and Ahlborn, 1997.

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The Writings of William C. Sturtevant

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NOTE.—The authors of jointly prepared publications are inserted, in the original order of appearance, within square brackets following the dates of publication.

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II. Anthropologists, Historians, and American Indians

Sleepwalking Through the History of Anthropology: Anthropologists on Home Ground

Laura Nader

Because of his broad experience as ethnologist, general anthropologist, curator, and editor of the *Handbook of North American Indians*, perhaps no one is in a better position to understand the double-edged nature of relations between anthropologists and Native Americans than William Sturtevant. His article on “Spanish-Indian Relations in Southeastern North America” documented the relations of native peoples with Euro-Americans in the different phases of exploration, coexistence, missionizing, depopulation, and diplomacy between 1513–1821 (Sturtevant, 1962), and the history of Indian-white relations, a subject that includes the areas of government and law, is addressed in the *Handbook* (Sturtevant, 1989). In the *Handbook*, the task of the ethnologist is to provide a comprehensive understanding of Native American history, an understanding made all the more central by current Native American legal claims. Nevertheless, because the focus of the ethnologist is in this case on Native American ethnology, there is comparatively less attention given to explicating the position of the anthropologist, in a wider sense, in relation to the knowledge being produced and the uses being made of that knowledge.

Sturtevant directed attention to the production and use of anthropological knowledge in a 1983 essay on “Tribe and State in Sixteenth and Twentieth Centuries” when he discussed the concept of tribe in connection with domains that lie beyond anthropology. He indicated that labeling makes Native Americans eligible for federal programs (or not) and concluded: “Thus it ill behooves anthropologists now to suggest that tribes do not exist” (Sturtevant, 1983:13). Anthropological terms may have a rather ambiguous relation to the real world, and misinterpretation or misuse of social anthropological argumentation by lawyers, judges, bureaucrats, and the sort can introduce confusion. Such confusion may result in adjudicating claims brought by unrecognized tribes who may or may not receive recognition and standing in court.¹

Earlier, Walter Goldschmidt (1979:2) noted the particular difficulties for anthropologists who find that “the problem lies not in the ‘target population’ so much as it does in the sub-cultures of his employers.” He continued:

Where the issues are large, the anthropologist is cast in the role of social critic. ... When practical matters affect established vested interests, matters can become extremely sticky... the anthropologist engaged in research that has policy implications may find that he is at the center of controversy; perhaps it can be said that he himself becomes a part of the problem. (Goldschmidt, 1979:2–3)

Anthropologists have played many roles in their North American work, yet self-reflection, where it is found, is focused narrowly on the relations between the ethnographer and the subjects of research to the exclusion of a wider range of relations, indicating the primary interest of the academic anthropologist. Roles of a political or policy nature are often relegated by academics to the applied anthropology literature (Van Willigen, 1980), or, more recently, to the growing interest in the ethnographic history of anthropology, or to oblivion, rather than to an understanding of ethnography as it is produced by individuals occupying multiple roles and reaching multiple audiences. Such multiplicity would include the roles of scholar, advocate (sometimes for colonizers and sometimes for the colonized), negotiator between parties, translator of world views, educator intent on overturning prejudicial views, politician and citizen forming policy, and witness, as when appearing before the judiciary in court. In other words, we are realizing that our ethnographic contributions must be situated amid our assumptions, intentions, and worldly circumstances more generally in order to understand the grids within which we are working—grids from which it is often not easy to escape without attentive awareness.

In this paper, I consider the role of the anthropologist as ethnographer in the light of this paradox: the seeming contradictions inherent in the story of what it is we actually do and the context in which we do it.

The Importance of Political Context

In a paper entitled “American Anthropologists and American Society,” Eric Wolf (1969) argued that three major phases of American anthropology correspond largely to three phases in the development of American society. Although he granted that his triadic scheme was an oversimplification, he thought it worthwhile to generate an interest in the sociology of anthropological knowledge. His first period spanned roughly the end of the Civil War to the last decade of the nineteenth century; the

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second ranged from the end of the nineteenth century to the onset of World War II; and the third included the military-industrial period of the Cold War, covering a roughly forty-year period from World War II to the present. Wolf argued that American anthropology reflected the historical events of the times. During the first period, Social Darwinist evolutionary theory was elaborated; in the second period, which he calls the Liberal reform period, anthropologists responded with theories that stressed human flexibility and plasticity; and in the present phase, the military-industrial period, anthropologists have responded with uncertainties and equivocations about power. Wolf was attempting to bridge the gap between the anthropologist as an academic and the anthropologist as a political being.

Yet when anthropologists articulate such conjunction between external and internal histories, some dismiss these attempts at a sociology of anthropological knowledge, claiming them to be unconvincing or peripheral to the development of anthropology (Abbinck and Vermeulen, 1992). But how could one explain the writing of Edward Sapir's (1924) article "Culture, Genuine and Spurious" without bridging the gap between histories? Sapir's concept of the spurious culture is one in which ultimate ends are replaced by immediate ones, where human labor is reduced to an extension of a machine, a culture that is neither harmonious, integrated, nor spiritually satisfying—conditions that are far more likely to occur in smaller and preliterate societies, whereas the spurious culture is a direct product of industrialization.

Others relegated the political content to a kind of postscript. In his essay on the institutional censorship of Franz Boas by the American Anthropological Association (AAA), George W. Stocking, Jr. (1968:307) concluded with a list of the many events that he did not treat in his historical attempt to contextualize the event, thereby illustrating the general pervasiveness of the avoidance in American academic life of a recognition of external pressures/power on the discipline. In 1919 Boas published a scathing letter in the *Nation* denouncing the use of "Scientists as Spies," thereby publicly exposing anthropologists who were thought to have used scientific activities in Mexico to further wartime espionage. In 1920 the AAA censured Boas, removed him from the Council of the Association, and forced his resignation from the National Research Council—a sober warning for those who would follow.²

The story about Boas is usually told as an anecdote, something that could not, does not, or has not happened since in a similar or functionally equivalent form. The history of anthropology is still taught as an orderly succession of people and their ideas, and the broad context is missed in such a history. Thus, anthropology appears to proceed autonomously, even though we have documented the opposite for a long time now, especially in the *History of Anthropology* series begun in 1983 by Stocking. To contextualize is to begin to understand how a teaching style and curriculum format evolved.

As a student of anthropology in the 1950s, I learned the history of theory in the model of lineal genealogy: intellectually,

Ruth Benedict, Sapir, and Margaret Mead descended from Boas; Meyer Fortes descended from A.R. Radcliffe-Brown; Edmund Leach, E.E. Evans-Pritchard, and Audrey Richards descended from Bronislaw Malinowski, via the French school.³ The history of anthropology has been primarily what Harvard anthropologist Douglas Oliver once privately referred to as "geriatrics' rituals." The contextualized or ethnographic history of anthropology had yet to come, and, as I was to discover while recently researching the impact of the Cold War on anthropology, what I had learned as a student was surreal by comparison. In my work, the earlier history, neatly ordered, was soon replaced by a nonsequential history. It became a story without order, as in a dream (Nader, 1997).

Apparently Leach felt the same way about these ruptures. In a 1984 publication of the *Annual Review of Anthropology*, he observed:

Differences of social class played a critical role in what happened in British anthropology during the first forty years of this century, yet the two most recent monograph-scale histories of British twentieth century anthropology...distinguish the protagonists only according to the qualities of their research publications, the theoretical attitudes, and their direct academic influence upon one another...nor is the reader given any feel of the major intellectual movements of the period as they were generated by such contemporary Titans as Bertrand Russell and Sigmund Freud. Such deficiencies are serious...I am saying that the sociology of the environment of social anthropologists has a bearing on the history of social anthropology. (Leach, 1984:2-3)

The political environment was key for British anthropologists in Africa, where they were studying peoples who were the products of colonial situations. Max Gluckman spoke about the judicial process among the Barotse—to emphasize their commonalities with European legal systems, specifically Roman-Dutch law—rather than examine more directly the colonial impressions of African law. His message was clear: the natives are not "savages." The same purpose could be found in his well-known piece, "The Reasonable Man in Barotse Law" (Gluckman, 1963), an example of his political need to assert similarity. It took a legal historian, Martin Chanock (1985), to address the construction of customary law as the product of missionary and political colonialism, many years later. From that remove, one could criticize Gluckman for glaringly omitting the colonial policies without understanding the political context and advocacy strategy that he might have been pursuing.

Hugh MacMillan, a historian, wrote an interesting article about Max Gluckman and the Zulu (MacMillan, 1995). He pointed out that after describing the formation of the Zulu Nation, Gluckman analyzed the development of the Zulu-white community. MacMillan noted that although Gluckman was later "criticized for using the word 'community' in this context, and accused of over-emphasizing harmonious interaction, Gluckman was under no illusion as to the nature of the conquest of the Zulu" (MacMillan, 1995:49). Gluckman's definition of the social field and his view on Zulu "tradition," according to MacMillan (1995:51), drove Gluckman "to demonstrate the impossibility of segregation...also to distance South African anthropology from segregation." In examining cultural

movements, Gluckman could see that traditionalism in nationalist movements was not a form of survival but of revival. He first published these ideas in 1940 and 1942, and they were later republished (Gluckman, 1958). MacMillan correctly concluded that the lasting relevance of Gluckman's work is in his identification of two contrasting trends and the controlling processes operative within each—one towards revival and the other towards absorption. Both trends have been continuous in the literature on Native Americans as well.

There is, however, another side to an unreal history of anthropology accomplished by the depoliticizing influence of the scientific method. George Stocking's (1991) piece, "Macay, Kubary, Malinowski: Archetypes from the Dreamtime of Anthropology," presented an interesting tidbit. He quoted from a letter that Malinowski had written in an appeal for graduate student support for Jomo Kenyatta:

Mr. Kenyatta started his work in my Department about two years ago. At that time he had a definitely political bias in all his approaches. This, I think, has been almost completely eradicated by the constant impact of detached scientific method on his mental processes. The highly depoliticizing influence of scientific anthropology has worked a "remarkable change." (Stocking, 1991:57)

Stocking then reminded us that the neutralizing power of social anthropology failed to impress Kenyatta, who went on to become the leader of the independence movement and the first president of the Kenyan nation. It is in such observations that the ethnographic history of the field can develop, with serious attention to anecdotal material as an intrinsic and tightly interwoven part of anthropological developments. Indeed, the same possibilities are open to all of the sciences that generally perceive themselves as autonomous or free from political, economic, or cultural influences more generally (see Nader, 1996).

On Home Ground

Examples of American anthropologists working on Native American ethnography on home ground ("home" referring to the nation state) indicate the challenges of multiple roles and the separate resolutions that are reflected in the ethnographic work and its reception during the past century.

THE EARLY SETTING: JAMES MOONEY AND FRANK SPECK

It is ironic that the recounting of anthropological history should divorce internal politics from external events, because anthropological thought, particularly as it was articulated in North America, was born from a Jeffersonian tradition of heightened engagement between the scientist and the outside world (Mark, 1980:1, 6). Historian of science Joan Mark (1980:5) noted that "Jefferson was interested in the misnamed 'Indians' as well as in the African Americans and Europeans who had come or been brought forcibly to the New World." To this end she quoted Jefferson as writing in *Notes on the State of Virginia* in 1785: "To our reproach it must be said that though for a century and a half we had had under our eyes the races of

black and red men, they have never yet been viewed as subjects of natural history" (Mark, 1980:5). Jefferson (1955:101) added: "It is to be lamented... that we have suffered so many of the Indian tribes already to extinguish, without our having previously collected and deposited in the records of literature, the general rudiments at least of the languages they spoke." Under Jefferson's leadership the Philadelphia-based American Philosophical Society became an important repository for such studies, and by means of government expeditions, a large amount of data was collected during the pre-1840 period (Mark, 1980). Not all of these expeditions were driven by scientific purpose alone.

Following Jefferson's lead, many of the first ethnologists and "proto-anthropologists" (Eddy and Partridge, 1987) were compelled to action by the complexities of an expanding frontier, especially as they pertained to the contemporary "Indian Problem" (Murra, 1976). New and prolonged contact with the Other, as some now refer to the peoples we study, secured a place for these early scientists in government discussions about Indian land settlement and resettlement as well as assimilation issues.

Of course, which ethnologists were to gain the ears and, ultimately, the money of administrators and politicians at this time depended upon what these ethnologists thought about how the Indian population should be controlled, and subsequently, to which theories they subscribed. Lawyer and anthropologist Henry Lewis Morgan's progressive evolutionism was used to justify the continuous resettlement of Indians throughout the end of the nineteenth century and into the twentieth century by the movers and shakers of Theodore Roosevelt's time. Progressive evolutionism was also useful to the research-supporting institutions of the time, mainly to the Bureau of American Ethnology (BAE), which was placed under the already established Smithsonian Institution. John Wesley Powell, who founded the BAE (as the Bureau of Ethnology) in 1879, was determined to use ethnologists' work to empirically and inductively further Morgan's theories as they related to Indian populations and to secure the BAE as the informational arm of the Congress and the American people. The bureau provided the first permanent, government-supported research for anthropological work (Hinsley, 1979).

Curtis Hinsley (1979) wrote about the contradictory ways in which Powell thought about the BAE's role and noted that in his recommendations to Congress, Powell was bolstered by new evolutionary arguments about the transition to agrarianism. That Powell underestimated the complexities of the role of anthropologists in this regard is present in the historical record. The work of those BAE ethnologists who did not agree with Morgan's progressivist theories may have been suppressed by Powell, beginning a long history in which anthropologists' confrontations with dominant social beliefs would be avoided or guided into muddy waters by other anthropologists, publishers, or government agencies. For example, ethnologist Charles Royce's studies of *Indian Land Cessions in the United States*

(Royce, 1899), completed in 1885, passionately argued, through the use of cultural relativity theories, for the removal of whites from Indian territory. Powell promised publication, but the work lay untouched until 1895 and finally appeared in 1899. Some might argue the delay reflected financial problems, but a statement by Powell himself indicates differently:

Despite the pitifully frequent cases of personal and temporary injustices to the weaker race, the general policy has been guided by a deep-grounded recognition of the principles of justice and right on the part of both peoples...the justice shown the red man is more richly tempered with mercy today than during any earlier decade. (quoted in Hinsley, 1979:23)

Compare Powell's statement with a statement made by Royce (1887:371): "For two hundred years a contest involving their very existence as a people has been maintained against the unscrupulous rapacity of Anglo-Saxon civilization."

Powell also appears to have been uneasy with the work of other ethnologists. These included Frank Hamilton Cushing, who called into question the ethics of condoning the living conditions on Zuni reservations (Tozzer, 1935), and James Mooney. In the 1890s Powell provided Mooney with funds to conduct fieldwork among the Sioux. This BAE research, combined with Mooney's own research, culminated in his extraordinary work, "The Ghost Dance Religion and the Sioux Outbreak of 1890" (Mooney, 1896). In it, Mooney detailed the Ghost Dance movement among many Native American tribes in the last decades of the nineteenth century, which culminated in the massacre of over 200 Sioux at Wounded Knee in 1890. He documented the greatest aboriginal revival the country had ever seen, a religion that promised a return to a time without the white man. With empathy and compassion, Mooney, an early proponent of the cultural deprivation school of thought, made the connection between religious revivalism and the alienation, cultural decay, tragic conditions, and desperation of a people who had suffered enormous losses at the hands of the white invaders of their land. In his research on peyote he maintained that the peyote religion combined the soul of Indian culture with a modified form of Christianity, and he advocated the Indians' right to use peyote. Native peoples, in Mooney's view, had their own logic. Mooney's work marked the beginnings of a revitalization theory.

Hinsley described Powell's position as that of the nonpolitical, detached, unemotional scientist and pointed out that "Mooney caused Powell constant headaches...for being present—and active" (Hinsley, 1979:23). The Smithsonian's response as a government agency was censorial, prompted by fear that Mooney's work would alienate both the Bureau of Indian Affairs and the Congress, with which the Smithsonian needed to work. Mooney's supervisors at the Smithsonian wished that he had avoided comparisons with European religions and had not made connections between the Ghost Dance and the conditions of misery of native peoples, who longed for a prophet to give them a means of redemption. In this introduction to the annual report, Hinsley noted, director Powell tried to repair the damage:

It may be observed that caution should be exercised in comparing or contrasting religious movements among civilized peoples with such fantasies as that described in the memoir; for while interesting and suggestive analogies may be found, the essential features of the movements are not homologous. (Hinsley, 1979:24)

Mooney's American Indians were not heathens or barbarians; they were part of the human race, including the "civilized" human race. His work spanned the chasm of time, space, and mental development in a period when unilineal evolution was not solely a question of anthropology but of everyday political and religious-political practice (Moses, 1984). Later on Mooney was barred from further research on the reservations by the commissioner of Indian affairs, who did not appreciate the ethnologist's references to religious freedom and scientific truths as justification for his peyote research (Moses, 1984). Mooney was ethnologist, advocate, and citizen. Powell published his work, but Mooney was regarded as a political time bomb.

Many times there was real reason to teach a fictitious or partial history of anthropology in order to protect the scientific ideal within which anthropological researches were cast. Harvey Feit's article on Frank Speck's 1915 publications encompasses such reasons in his title: "The Construction of Algonquian Hunting Territories: Private Property as Moral Lesson, Policy Advocacy, and Ethnographic Error" (Feit, 1991). Feit reviewed the issue of private property as it has developed before and since the work of Eleanor Leacock (1954). Leacock's conclusion that "there are now strong reasons to think that it was erroneous to claim that Algonquian territoriality ever was, or was becoming, a kind of private property system" (Feit, 1991:110), invited a reexamination.

According to Feit, Speck's case for the existence of private property in Algonquian territories was the product of a local colonial situation of land ownership issues in North America, which included government policies and settler claims, and social science advocacy. In his work, Speck spoke against the policies of United States President Theodore Roosevelt, who was presiding over the further dismantling of Indian lands at the time and who discussed, as an evolutionist, American Indian nomadism. The powerful evolutionists of the time spoke for the mainstream. When Speck shifted to an anthropological audience, he spoke against Henry Lewis Morgan and his brand of unilineal evolution, although Morgan himself, as an advocate of American Indians, understood the transformation from communal to private property and its implication for the destruction of Indian ways of life. Feit recorded that Roosevelt and his friends depicted Indians as wanderers and beggars unattached to private property, a thin observation used to justify government policy.

Mooney and Speck were not exactly examples of retreat from controversy, and their roles as citizens and anthropologists had impact on their ethnographies, albeit in different ways. Speck compared the conditions of the Algonquian hunting peoples to that of colonial peoples elsewhere, and he countered government policy by arguing the case for the family

hunting ground, a position at the time being argued by Indians themselves. In the end, Speck took his policy advocacy into theory. Feit (1991) illustrates the origin of such ethnology in political advocacy.

CONTEMPORARY ISSUES

Times have changed for Native Americans since the days of Mooney and Speck, largely due to the emergence of the Red Power movement in the 1960s and to new economic developments on Indian reservations over the same period, namely gambling and the employment of Native Americans as the overseers of toxic waste (see Tsosie, MSa; Ou, 1996). Times have also changed for American anthropologists, due to a more critical evaluation of the ethnographic project and the political realities in which the ethnography is embedded. Two young anthropologists have been drawn to contemporary issues in a somewhat revolutionary manner that reflects the changing relationships between anthropologists and Native Americans and between anthropologists and various systems of control.

Michael Tsosie, an anthropologist and Native American, has initiated a study of Indian gambling among the Mojave of Fort Mojave Indian Reservation and the Colorado River Indian Reservation, in Arizona and California. In a recent manuscript, "Avi MiYou'a—Indian Gaming and the Struggle for Power and Control in Two Mojave Communities," Tsosie (MSa) focused his attention on the internal dynamics of the development and operation of a casino facility in a reservation-based community, a subject that has generally not been attended to by early analyses of arguments for and against gambling on reservations. Tsosie linked opposition to gambling as a tribal economic activity to fear among the reservation political elite of the potential of gambling to reconfigure the existing balance of power. Indeed, once the casino facilities opened, multiple power struggles did emerge. Tsosie argued against essentializing and against the use of binary terms such as "traditional" and "progressive," approaches that often obscure the power dynamics in Native American communities, and thereby produce ethnographic error.

Tsosie's discomfort with restrictions on Native American scholarship posed by anthropological concepts was also directed at the spurious restrictions imposed by other Native Americans. The use of the traditional culture concept has become an effective means of censoring discourse in both tribal and pan-Indian settings. In a second manuscript on traditional culture, Tsosie (MSb) outlined a description of the use of traditional culture to exclude competitors by essentializing identity to a finite trait list. He cited Peter Worsley in *The Three Worlds: Culture and World Development*:

Cultural traits are not absolutes or simply intellectual categories.... They are strategies or weapons in competitions over scarce social goods. What is mistakenly often seen as tradition—attachment to the past as a value in itself—is better viewed as a way of maintaining title to power, wealth and status in the present, or as a nostalgic spiritual contrast to present disprivilege. (Worsley, 1984:249)

Tsosie also cited Richard Clemmer's work on the Hopi (Clemmer, 1994), again corroborating Worsley's argument. Clemmer questioned the usual categories of traditional versus progressive, while noting and making use of symbolic uses of tradition for the purposes of legitimation. Certainly these are not the first observations of such usage. The application of tradition and culture concepts to pan-Indian militancy stresses the importance of such concepts, not as intellectual tools, but as tools for garnering power, which is critical to drawing boundaries around Native Americans and establishing their claims legally. By use of the temporal dimension they are able to establish authority, an authority that confounds the configuration of federal, state, and corporate involvement.

"Native Americans and the Monitored Retrievable Storage Plan for Nuclear Wastes" (Ou, 1996) is a novel ethnographic example, different from Mooney, Speck, or even Tsosie, because the scope of the work is broader and is ethnographically more inclusive of the power dimensions. The ethnography in this instance is being used to explain the "way in which structural relationships between the federal government, Native American tribes, and the nuclear industry have shaped and directed the nuclear waste negotiation process" (Ou, 1996:32). Thus, the subject of ethnographic practice includes an analysis of actors who were previously in the shadows, or out of the ethnographic picture altogether. The constrictive nature of earlier ethnographic work made an understanding of intense struggle and violence incomprehensible as part of root causes. C. Jay Ou's approach, on the other hand, is inclusive.

Ou examined critically the politics of economic blackmail through the use of culture and political representation. The story is about how federal officials and native advocates actively try to find linguistic, ethical, moral, cultural, and political traditions that could be used to sell nuclear waste disposal to Native Americans—a case of ethnographic knowledge being turned back on Native Americans in the form of subtle persuasion.

In his example of the Mescalero Apache, Ou (1996) described a coercive and incremental process of consensus building and documented the sociolinguistic construction of tribal perceptions of ethics and morality. The means of control that Ou outlined are implicit and are closely related to the language use of particular speakers at specific times. In analyzing a critical dialogue held at the Cheyenne Mountain Conference Resort in Colorado Springs, Colorado, Ou broke down the dialogue process as manifested in the use of Indian spirituality, Western reason, history, nuclear wastes, ethics, morality and patriotism. He examined particular forms of power relations as expressed in speech acts, drawing relations between each person's institutional ties and their speech acts. In his analysis of selling, limiting, and maintaining control of the agenda, he outlined the boundaries of acceptable discourse. In several cases, dialogue was controlled by a facilitator and was transcribed by two court reporters. The underlying intention of the facilitator was to delineate the idea of tribal perceptions of ethics and morality so as to make a fit with the interests of nuclear waste storage on

reservations. The dialogue between critical-minded Indians and pro-waste apologists was kept at an abstract level, and in this way a general feeling of social harmony was found at the level of the common good. As Ou noted, "Indian identity was up for grabs. . . . Those in positions of power benefited from it" (Ou, 1996:75).

The facile cultural maneuvering was a striking use of cultural knowledge. In the end, the facilitator portrayed the nuclear waste crisis in terms of a collective crisis:

The nuclear waste issue is just a microsymbol of the challenges that we face as human beings living on a single living organism, the earth. To find a way to live together as Black Elk said, as children of a single mother and a single father. (Ou, 1996:78)

As Ou said, "there is a fine line between coercion and informed consent. . . . The question of economic development is closely linked with the MRS [Monitored Retrievable Storage] program, and it in turn intricately related to the notion of tribal sovereignty" (Ou, 1996:80).

C. Jay Ou and Michael Tsosie are correct in calling attention to the political structure of the local community, because it is at the local level that intergroup agreements are being reached. The institutional elements of federal, state, and reservation politics include the nuclear waste industry, a player that has been on the back burner in ethnological reports in the past. In the case of gambling, intergroup politics has been oversimplified by the use of concepts such as traditional and progressive. The point in both of these examples is that the inclusion of all the players in the ethnographic work makes it unnecessary to commit the ethnographic errors by which earlier ethnographies are now characterized, or to separate academic ethnography from applied or policy concerns, or to separate us from them. In other words, the perils of studying only up or only down (Nader, 1969) might be avoided by a vertical slice approach (Nader, 1980).⁴

Discussion

There are a number of issues in Native American anthropology on home ground, an anthropology that has been dealing with the political complexities of Native American life since Europeans came to the New World. Starting with an issue close to the defining factors in American anthropology, it is clear that particular conditions in North America shaped the discipline here. Sidney Mintz put it simply: "A profound difference between the history of our discipline in Europe, on one hand, and in the Western Hemisphere on the other, inheres in the simple fact that our subjects of study, our 'primitive' peoples, were our neighbors—our ill-treated, indeed often persecuted, neighbors. In this instance, as in others, the anthropology we did and have done is conditioned by the history and social complexion of the society from whence we come" (Mintz, 1996:290).

One need only glance at the papers about fieldwork by British anthropologists in *Anthropology at Home* (Jackson, 1987) to realize to what Mintz was referring. In the discussions of

fieldwork in Britain, the authors are heavily concerned with the anthropologist and the informant, with reflexivity, with rural life as seen by the academic eye for readers of their work, and with self-knowledge and what "home" really means. Although some American anthropologists, especially those who work away from the United States and within a Eurocentric tradition, may share the flavor and interests of Jackson's volume, those working at home with Native Americans have different experiences. The historical base of American anthropology was at home. For the British, the historical base was away from home.

It is apparent in the brief commentary on Mooney, Speck, Tsosie, and Ou that the social field, as Gluckman defined it for South Africa, long ago went beyond looking at communities as if they were isolated. Writing before Boas, Mooney made connections. Speck also made connections. Both knew their work had consequences beyond the anthropologist/informant dyad. Lives were at stake, land was at stake, sovereignty was at stake, and what's more, power was central. The younger anthropologists Michael Tsosie and C. Jay Ou understand the wider political and economic uses (among others) made of anthropology. Culture traits, tradition, essentializing, pan-Indianism, nuclear waste—all this has consequence for turning Native American reservations into gambling casinos or toxic waste dumps. It also has consequence for anthropology.

In conclusion, I would like to address the implications this essay might indicate for anthropology's ethnography. If the study of "them" does not include the study of "us," we are only encouraging, as in the case of Frank Speck, ethnographic error, at the same time that we are forgetting early pioneers in functionalist theory, such as James Mooney, a man forgotten in favor of remembering the decades of liberal Boasian anthropology, and another heroic figure. What contemporary anthropologists have shown is that the critique of traditional anthropology, which was followed by a focus on the fieldworker in ethnographies, can move toward a systematic inclusion of an examination of power holders. As we learn to operate the wide-angle lens that Malinowski envisioned, we begin to better comprehend the concerns that Sturtevant has raised in his own work about the consequences, and therefore the value, of our ethnographic work beyond academic circles. Anthropologists located outside of academia have already begun to outnumber academics (Basa, 1994).

As the primary site of anthropological activity shifts once again, the time is ripe for us to have the best of many worlds, but the optimism inherent in having the best of all worlds needs to be tempered with a comment on the perils of studying up. In 1969 I wrote that anthropologists like to like the people that they study. When anthropologists began to study up, their efforts often replicated earlier models, that is to say earlier ethnographies that were truncated and detached from studying down or sideways. Later (Nader, 1980), I wrote about the vertical slice as a method to call attention to the observations that anthropologists who were studying up were still conceiving of communities like government agencies, industrial sites, and ge-

netic engineering firms, as if they were isolated island societies without connections to the wider world. More important perhaps, the need for anthropologists to like the people they study becomes an obstacle when their informants are powerful, not powerless, as has been the case in Native American communities. In situations of unequal power, intellectual co-optation of critical insight will mar the quality of our ethnographies, much as it did at the turn of the nineteenth century.

Notes

I acknowledge with gratitude the indispensable help of Rachel Stryker in the preparation of this paper and the out-of-the-ordinary library support of Suzanne Calpestri. Errors and omissions are my responsibility.

1. Elizabeth Colson's article, "Political Organization in Tribal Societies: A Cross Cultural Comparison," is one of the clearest expositions of the problem that Sturtevant raises about "tribal" and "tribe" and correctly concluded that anthropologists have and are studying "political units which take their defini-

tion and their meaning from their position within the much larger political system of nation-state" (1986:17).

2. David Price has recently published a sequel to Boas's piece ("Anthropologists as Spies," *The Nation*, 20 Nov, 2000) bringing earlier concerns up to date, noting that the American Anthropological Association has removed explicit condemnation of secretive research from their code of ethics.

3. One only need look at histories of anthropology available in the 1950s to realize how narrowly history was conceived. The broadening of our understanding was to come later, in works by historians of science and ethnographers who sought to conceptualize political processes like industrialism, colonialism, World War II, or the Cold War. Works by George Stocking, Hugh MacMillan and L.G. Moses are examples of the excellent contributions from history.

4. In "Up the Anthropologist" (Nader, 1969), I argued that a more inclusive anthropology needed to study not only the powerless (down) but also the powerful (up), as well as the horizontal networks (sideways), in order to link power with consequences. I later introduced "the vertical slice" to underscore the need to study the powerful not as isolated elites, and in this way to connect their structures as they extend to the less advantaged, while acknowledging that in some situations, we are all disadvantaged.

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Charlatan, Scientist, or Poet? Frank Hamilton Cushing's Search for a Language of Experiential Knowledge

Curtis M. Hinsley

In Cushing files in SW Mus lib, a photo of Cushing, full-length, standing in front of Navajo blks, arms folded, dressed as a Zuni. . . . Presented to SW Mus by F W Hodge, Mar 13, 1950. On back of print, in Matilda Coxe Stevenson's hand (acc to Hodge note) in pencil, appears the following: "Frank Hamilton Cushing in his fantastic dress worn while among the Zuñi Indians. This man was the biggest fool & charlatan I ever knew. He even put his hair up in curl papers every night. How could a man walk weighted down with so much togery?"

In 1973, at my first meeting with him at his Smithsonian Institution office, Bill Sturtevant handed me the above notation, which he had made at the Southwest Museum, in Los Angeles, a few years earlier, and thereby introduced me to the problems of Cushing, anthropological style, and scientific reputation. I have been indebted to Bill ever since for his generosity with his materials, his insights, and, above all, his constantly questioning mind. This paper, about style, specifically discursive style in the history of anthropology, is a small payment on a large obligation.

My central arguments in this paper are that Frank Cushing, beginning in 1879, developed a unique, poetic style of reporting; that this style inhibited the acceptance of his archaeological work on the Hemenway Southwestern Archaeological Expedition in the late 1880s; and that this gulf in turn accounts for the historical perception of his archaeological work as a scientific failure.¹ This paper analyzes the genesis and elements of that style, as seen especially in Cushing's marvelous journal or "Itinerary" of the expedition, which was written intermittently between 1890 and 1892 and which now will finally be published (Wilcox and Hinsley, in press).

Cushing's writings from the Hemenway Expedition fall into three parts: first, his "Preliminary Notes" (Cushing, 1890), the well-known programmatic statement of the expedition, which he prepared for the International Congress of Americanists in Berlin in August, 1888, but which was not actually finished in time for the meetings and was ultimately published in 1890; second, the unpublished Itinerary, which was written a few years later and is discussed below; and third, an obscure but significant set of notes entitled "Commentary of a Zuñi Familiar" (Cushing, 1893), written as an appendix to *Song of the An-*

cient People, a *Hiawatha*-type poem by the Boston poet Edna Dean Proctor.

Taken together, these writings between 1888 and 1893 provide a continuum of Cushing's style, ranging from a serious attempt to communicate with an international audience of scientific peers to acquiescence in the recognition that so-called scientific discourse was simply inadequate to the expression of his ethnographic and archaeological knowledge. As Cushing wrote in a letter to Edward Everett Hale as early as 1882:

The life and language, the religion of the Zunis, are intensely poetic. With no amount of poetry to which my words or pen may aspire, can I hope to give to the world as I feel it, in listening to the rituals, folklore, or even councils of this innocent people, their imagery and their poetry and their quaintness. . . . I have, thus, inadequately told the truth about my Zunis; yet I have told the truth so far as my abilities enable me to.²

Here as elsewhere Cushing used the term "poetry" quite loosely, but his term and his sense of the problem substantially overlap the two senses in which I am using "poetics" here: first, the structures—syntax, vocabulary, figures of speech—embedded in forms of communication; and second, poetry more specifically, namely, the concentrated language forms we employ, relying on tone, sound, metaphor and metonymy, to communicate a reality that eludes more literal and denotive expression. The problem of Cushing's "scientific poetics" was the challenge of creating a form of communication that both conveyed Cushing's interlocutive knowledge and conformed to the discursive practices that were increasingly, at the Smithsonian's Bureau of American Ethnology and elsewhere, being called "scientific."

Cushing began to develop his authorial voice during his years with the Zunis (1879–1884), and it first emerged with clarity in his brilliant "Zuñi Breadstuff" series of 1884–1885; however, he carried his problems of communication into his archaeology for the Hemenway Expedition. In the paper prepared for the 1888 International Congress of Americanists, Cushing (1890:151–152) announced that he had come to believe that "a people carry through all succeeding environments—relatively unmodified—the impress or the *Idea* of the earliest environment which affected their culture." Therefore, the tasks of the anthropologist are, he suggested, "the ascertainment of . . . what *Idea* possessed" a people, the origin and history of the idea, and how it influenced or modified all other ideas and institutions of

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the culture. Cushing (1890:151–152) had various terms for this dominant cultural idea: the “Culture-soul,” the “living soul,” or the “dominating or all-fashioning Idea” of a people.

Whatever the name, the search for the idea within a cultural configuration involved two stages, the first of which Cushing felt he had already accomplished at Zuni pueblo: he had established the seven-fold (septenary) nature of Zuni life and had isolated the search for the middle (or center) of the world as the organizing principle of Zuni culture. It now remained for him, through his archaeology for the Hemenway Expedition, to trace both the Zunis and their central cultural idea back through time and space to their origins. This was the central focus of his expedition for Mary Hemenway, the wealthy Boston patron of the expedition. He sought a specific historical and migratory sequence, paying special attention to the relationship between people and the physical environment through time. It is important to emphasize, however, that Cushing argued for the tenacity of deep, original ideas—his entire method depended on an assumption of their perseverance. Far from being repeatedly shaped by environment and altered by historical experience, the mental structures of peoples, he wrote, give the framework to the world as they perceive it: “A primitive people, once having conceived an Idea, rather modify (nominally) a changed environment to suit it, than modify it to suit their changed environment” (Cushing, 1890:191).

Such were, to use Cushing’s own terms, his “working hypotheses” for his first sustained archaeological work in the Salt River Valley, near present-day Tempe, Arizona. But what were his methods in this pursuit? To answer this question, consider two passages from Cushing’s Itinerary, that strange amalgam of travelogue, landscape sensibility, excavation descriptions, and interpretive ruminations.

The first is Cushing’s account of the afternoon of 3 March 1887. The crew had been digging at the site Cushing named *Los Muertos* for two days with rich initial finds, and Cushing was about to begin laying out the grounds for detailed excavation. He wrote that he would ask the artist of the expedition, his sister-in-law Margaret Magill, to be present for on-site sketching, that he wanted to get a camera, and that he would push the work hard for several weeks. Abruptly, though, Cushing’s narrative voice shifted as he remarked on the idyllic climate and purple mountain rims to the south and west. He continued in this vein:

The sky, save for a few white and brown clouds hastily gathering today, so soft a blue, yet so bright that it seemed more than tinted—alive with liquid turquoise as the ocean sometimes is—alive as a face is with blood. Then the great valley was raggedly green with mesquite groves, or olive gray with broad stretches of sage-brush level as lakes, and the whole plain seemed to wave tremulously, yet very slowly, in the tremendous but always soft flood of light, as though this light were in some sort of luminous gauze, and were swayed from side to side by the ceaseless winds. The two Mexicans sang at their eager work . . . and the covies of quail, scudding about everywhere under the bushes and across bare spaces, sang also for all the world like the fowls of an Eastern farmyard in springtime, but with a more gladsome and constant plaint. (Cushing, Itinerary, entry for 3 March, in Wilcox and Hinsley, in press)³

Cushing’s reverie was interrupted at this point, though, by a sudden, violent late afternoon monsoon, which ended as abruptly as it started, after which a rainbow appeared:

a splendid, dazzling rainbow, double (and at one point triple), and connected, yet cut into sections, by radiating bars of prismatic color and light. There it blended, on hands and feet (so it seemed to Weta [Cushing’s Zuni companion]), over the whole great mountain-centered valley, between us, seemingly, and the flying clouds, and all the rain ceased far and near. Never before, except in Zuniland, had I seen anything approaching this in gorgeousness and sublimity—nothing ever so spectacularly swift! (Cushing, Itinerary, entry for 3 March, in Wilcox and Hinsley, in press)

The rainbow seems to have stirred in Cushing images of the Zunis’ great Man-Worm of the Skies devouring the rain clouds. He now pursued this new connection:

The relation of such fleeting scenes and incidents as these to the primitive history for which we were delving after the material data is sharply illustrated in a discovery made long afterward by me in the Maricopa Mountains, just under where the sun shone so brightly that day, for there I found a triple arch crudely depicted . . . two lower segments of which were connected by radiating lines precisely as had been the remarkable rainbow we saw on our way. Further elaboration there was none, but the presence underneath this of an emblem or figure of the sea-serpent . . . so sharply undulated that it expressed the rapid fleeing away of that which, of course, in this connection it was merely designed to symbolize, namely, the rain-clouds and rain—clearly showed that the Ancient City Dwellers, like the Zunis of today, held to an identical mythical interpretation and belief regarding the God of the Rainbow and associated phenomena. (Cushing, Itinerary, entry for 3 March, in Wilcox and Hinsley, in press)

Cushing’s narrative then shifted back to that evening’s events in camp, but the significance of the storm-rainbow reverie should not be missed. Cushing had inserted the thread of authority which runs through his entire account: seemingly immersed in excavations, he experienced a series of natural phenomena; drawing on his Zuni knowledge-experience, he “ceases to wonder,” that ancient Zunis understood and depicted the rainbow as they did; this thought in turn reminded him of a rainbow petroglyph discovered nearby—after the 3 March experience but before the Itinerary writing—thus providing proof, to Cushing, of connection between peoples’ present and past. Through a kind of triangulation, Cushing had established his position of authority between past and present, between archaeology and ethnography, by reference to immediate events on the expedition. Through the interplay—and the play—between his imaginative sensibilities and those events Cushing created an ethnographic past. The thread of his authority runs, then, through three critical points: his field data and observations (e.g., the petroglyph); the events of the moment (rainbow/clouds); and his presumed knowledge of life at Zuni pueblo (the myth of the God of the Rainbow). From these positions Cushing derived an interpretation, as he moved from earlier suspicion or wonder—two of his most common self-descriptions—to explanation and conviction.

Two weeks later, in the face of rumors of Apache violence, Cushing and two companions undertook a reconnaissance of the Superstition Mountains. Exploration in the face of danger is vintage Cushing, and the account of this two-day adventure occupies a significant segment of the Itinerary. It centers upon

one eternally long, cold night of wakefulness in the mountains. As Cushing watched the shadowy mountain rims while his companions slept, his imagination slipped its moorings. In the Itinerary he described the experience as follows:

I constantly scanned, through the long hours that succeeded, all these cliffs and forms, expecting that at any moment others would rise up from amongst them and, if unseen in so doing, cheat my closest scrutiny by their mimetic motionless. No change in those outlines occurred, however, until their nature was fully revealed, and in such stately magnificence that description is belittling. Yet when the moon had fully risen, there even it stayed, fixed like the stars, and with all this wondrous change no change seemed to have been wrought save only in my own looking; all the world seemed as dead frozen and stony as before, I only saw more, and more than ever it seemed as if Time would never return,—never! No wonder that the Ancients of that wondrous land tell of the countless destructions of men, monsters and cities, whole countries and regions,—changed to stone everlasting! No wonder they sought such scenes as this for their mystic midnight rites and sacrifices, deeming these latter as lasting as the mountain they were laid in! And this thought became so suggestive, as with the growing light my dream of a dead and timeless nature became less dominant, that I felt already amply rewarded for all the toil and risk of our journey by this new light on our researches, dawning in my mind. (Cushing, *Itinerary*, entry for 15 March, in Wilcox and Hinsley, in press)

The key term in Cushing's account of night in the Superstitions is "wonder"—"No wonder... they tell... No wonder they sought." And his reflections eventually, again, dissolved the wonder of ignorance into the certainty of new knowledge. But what are we to make of Cushing's poetics, of his insistent insertion of himself into his narrative of exploration and discovery, his reminding the reader of his own catalytic role in the construction of his archaeological interpretation? At the time, the advisors to Hemenway were not impressed. William T. Harris, her close confidant, wrote to Cushing repeatedly to demand and exhort from him the "scientific" results of the expedition—if he did not produce them, Harris threatened, Cushing would be discredited: people would say that his "personal equation" had run away with him, that he was, as Matilda Stevenson had called him, a charlatan and a fool, perhaps even a madman.⁴

Now, however, we are beyond the nineteenth-century horizons of Harris or Stevenson. In her recent study, *Imperial Eyes: Travel Writing and Transculturation*, Mary Louise Pratt (1992:58–67) explored, among other genres, the natural history and ethnographic accounts of European exploration, analyzing the various linguistic apparatuses, as she called them, of the scientific enterprise that both legitimated scientific authority and globally systematized "nature." The language of taxonomy and, importantly, the presence of an invisible observer—actually an absence of person, but the presence of an authorial voice—were central to this project. According to Pratt (1992:29), "the systematizing of nature... is a European project of a new kind, a new form of what one might call planetary consciousness among Europeans." Following the circumnavigation and mapping of the planet, through the nineteenth century Euro-Americans continued the systematizing process with verbal descriptions of the interiors of regions previously only

exteriorly outlined. Out of the chaos of these interiors the scientist/observer, invisible and objective, presumed to produce order. As Pratt (1992:64) explained, just as the natural historian produced land as landscape and territory, scanning for resources and investment prospects, the ethnographer produced indigenous inhabitants as bodyscapes and resources: "together they dismantle the socioecological web that preceded them and install a Eurocolonial discursive order whose territorial and visual forms of authority are those of the modern state."

A hundred years ago the desert Southwest, from New Mexico to southern California, was in the process of being imaginatively incorporated into the United States—a dynamic that has gone on continuously since, an ever deepening and self-elaborating pattern of tourism and market consumption. In other words, the Southwest was, in effect, an internal colony of the United States national state. The beginnings of this process lay with Cushing's generation, and in important ways he certainly participated. But he also posed a challenge, perhaps even an alternative, to the scientific discourse that Pratt has identified so closely with "imperial eyes": Cushing melded the present peoples of the region with previous populations—the Ancient Ones, as he fondly called them—and connected them both, through mythologies, agricultural practices, religious rituals, and many other ties, intimately to the landscape. In so doing he provided them with a certain legitimacy and continuity in southwestern time and space.⁵ Equally important, from the perspective of the history of anthropology, Cushing presented his archaeological knowledge as the result of an internal conversation that derived in equal parts from indigenous sources (as he understood them ethnographically), his own psychological and emotional capacities, and direct field observations. I believe that he eventually came to trust this internal process completely, but in so doing, and so moving, he experienced scientific "failure."

When Cushing was fired as director of the Hemenway Expedition, in July 1889, he was succeeded by Jesse Walter Fewkes, a Harvard-trained zoologist and college classmate of Hemenway's only son, Augustus. Fewkes knew no Indian languages, and on the train out to the Southwest for the first time he was madly reading Cushing's (1882–1883) "Adventures in Zuñi." Fewkes worked many summers in and around southwestern pueblos in the 1890s, regularly turning out short, descriptive pieces that were as dry as the land around him. But where Cushing failed, Fewkes succeeded magnificently with his scientific points, his factual bits and pieces. The "mania for facts" that David Shi (1995:66–78) has pinpointed as the bedrock of post-Civil War American culture did not, of course, banish so much as it obfuscated subjectivities of all sorts—class, gender, region, race, and other. But the requirements of professional status in fledgling field sciences, such as archaeology, simply did not permit latitude for the internal conversations to which Cushing so freely confessed. He flirted with failure, and, in his own lifetime, he found it.

Notes

1. For background and discussion of Cushing's career on the Hemenway Expedition, see Hinsley and Wilcox (1996).
2. Cushing to Hale, 16 Apr 1882, Zuñi Pueblo, Edward E. Hale Papers, New York State Historical Society, Albany, New York.
3. Several years after the Hemenway Expedition had disbanded, Cushing and his secretary Frederick Webb Hodge prepared a typescript entitled "Field Notes and Journal of Explorations and Discoveries in Arizona and New Mexico during the Years 1887–88." Cushing generally referred to this account as the *Itinerary*, a practice I follow in this text. This document will be published in Wilcox and Hinsley, in press.

4. Harris to Cushing, 9 Nov 1891, Hodge-Cushing Papers, Southwest Museum, Los Angeles, California.

5. For further discussion on this point with reference to Cushing's 1882 trip to "The Nation of the Willows" (Havasupai), see "Authoring Authenticity" (Hinsley, 1990, especially pages 468–470).

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George A. Dorsey and the Development of Plains Indian Anthropology

Raymond J. DeMallie and Douglas R. Parks

Historians of anthropology agree that Franz Boas “was the most important single force in shaping American anthropology in the first half of the twentieth century” (Stocking, 1974:1). Reacting against the evolutionary theorists of the nineteenth century and their outmoded method of classifying cultural and social phenomena from simple to complex, Boas is credited with historicizing and relativizing anthropology. Moreover, by developing anthropology in the context of the university, Boas trained most of the next generation of anthropologists who in turn would teach in ever-increasing numbers at colleges and universities throughout the country. Over time, American anthropology became so closely identified with Boas that a historical amnesia set in: anthropologists who were not trained by or closely associated with Boas came to be forgotten. As a result, the important work done by anthropologists outside the social network of Boas and his followers was forgotten. The intention of this paper is to examine the career of one such individual, George A. Dorsey, focusing on his contributions to the development of Plains Indian anthropology.¹

George Amos Dorsey (1868–1931), who held the first doctorate in anthropology granted by Harvard University (Dorsey, 1894), also taught the first organized class in anthropology at that university. From 1896 to 1915 he headed the anthropology department at the Field Columbian Museum (after 1905 the Field Museum of Natural History) in Chicago, Illinois, and was a major force in anthropology during the first decade of this century. Dorsey was one of the founders of the American Anthropological Association (AAA), and in 1902 he served as its first secretary.² He served as president of the American Folk-Lore Society in 1903 and as vice-president of Section H (Anthropology) of the American Association for the Advancement of Science (AAAS) in 1903–1904. Dorsey was the second member of the anthropology faculty at the University of Chicago, serving under Frederick Starr. After moving to New York City in 1921, “Doc Dorsey” became the most successful popularizer of anthropology of his time. Despite his many professional accomplishments, Dorsey is not well remembered in anthropology today. Nevertheless, his Plains Indian studies, undertaken between 1897 and 1907, represent the first systematic ethnographic survey of the Prai-

ries and High Plains, and the results of those studies, published or unpublished, inspire admiration even today and serve as a cornerstone for the anthropological understanding of the Plains area.

Dorsey’s Early Career

Dorsey was born in the small town of Hebron, Ohio, on 6 February 1868.³ In 1888 he received the Bachelor of Arts degree at nearby Denison University. The next year he went to Harvard University, where he discovered archaeology and anthropology, and in 1890 he received a second bachelor’s degree under Frederick Ward Putnam.

In 1891 Putnam accepted the challenge of heading the archaeology and ethnology department of the World’s Columbian Exposition, held in Chicago, with the intention of founding an anthropological museum there at the close of the exposition. Hastily, he began to send collectors throughout the Americas to procure material for the anthropology exhibits (Dexter, 1966:315–316). Putnam sent Dorsey to South America, where he oversaw excavations and purchased collections in Peru, Ecuador, Chile, and Bolivia (Dorsey, 1900:249).

In 1893–1894 Dorsey completed his studies at Harvard as a Hemenway Fellow and wrote a doctoral dissertation entitled “An Archaeological Study Based on a Personal Exploration of over One Hundred Graves at the Necropolis of Ancon, Peru” (Dorsey, 1894). After he received his doctoral degree, he was appointed assistant in anthropology at Harvard in 1894–1895, and the following year he was appointed instructor in physical anthropology. Among his students who later achieved prominence in anthropology were Roland B. Dixon, Frank Russell, and John R. Swanton. Swanton later reminisced that teaching was not Dorsey’s strength: “His nature was too restless and he was at his best when on the move as a collector and promoter. His lectures were little more than a replica of the textbook which he recommended to us, ‘Précis d’Anthropologie générale,’ by Hovelacque and Hervé [1887]” (Swanton, MS:9). Swanton’s admitted disinterest in physical anthropology, however, may have colored his evaluation.

Putnam’s plan for a permanent anthropological museum in Chicago, to be created out of the collections brought together for the World’s Columbian Exposition, became reality on 26 October 1893 with a founding gift of one million dollars from

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Marshall Field. The Fine Arts building was selected to house the new museum, and, as it was emptied at the close of the exposition, anthropological material from other buildings was consolidated there. Franz Boas, Putnam's assistant throughout the exposition, did the actual work of supervising the anthropological exhibits. On 21 May 1894 the name was changed to the Field Columbian Museum; opening day was 2 June. By then, Putnam, facing too many political problems, had resigned, as had Boas (after learning that he was to be passed over as Putnam's successor), and William H. Holmes, of the United States National Museum (now the National Museum of Natural History, Smithsonian Institution, Washington D.C.), was appointed curator. In 1896 Dorsey was appointed assistant curator of somatology (physical anthropology), doubtless on Putnam's recommendation. When Holmes resigned at the end of September of that year, Dorsey was appointed acting curator of anthropology, and four months later he was promoted to curator. Stephen Chapman Simms, who had been associated with the museum from its beginning, was appointed assistant curator (Field Columbian Museum (FCM), 1894:9–15; Dorsey, 1900:252, 256; Dexter, 1966).

During his last three years at Harvard, Dorsey had devoted himself to the study of physical anthropology. Writing from Cambridge, he offered his services in that field to William R. Harper, president of the University of Chicago, noting that he had taken courses in comparative osteology, comparative vertebrate anatomy, histology, and embryology and had studied anatomy at the Harvard medical school (Dorsey to Harper, 6 Apr 1896, UC-A). He was appointed a lecturer in the University Extension program in 1896 and again in 1897. In 1900 he was appointed professor of comparative anatomy at Northwestern University, and in 1905 he was also appointed assistant professor of anthropology at the University of Chicago, with a promotion to associate professor in 1909.

Dorsey as Field Columbian Museum Collector

With a background in archaeology and physical anthropology, Dorsey was not well prepared to meet the immediate challenges of the Field Columbian Museum. During his first year he was largely engaged in inventorying, cataloging, and labeling specimens, and then reinstalling them. North American Indian ethnological material represented some of the strongest collections: two halls were devoted to the Northwest Coast, two to the Eskimo, and the largest one to the collections donated by Edward Ayer, which primarily represented the Plains and the Southwest. An 1894 photo of Ayer Hall shows what Dorsey was up against: the space was filled with an assortment of vertical, flat-topped, and slant-top cases, salvaged from the exposition, that amounted to open storage, with objects identified by handwritten labels (FCM, 1894:70–75; Figure 1).

Beginning in 1897 Dorsey initiated a series of expeditions in North America to fill gaps in the ethnographic coverage of the museum. He planned to organize exhibits around specific tribes

and to include life groups made from casts and anthropometric measurements of living tribal members. Life-size figures representing various cultures, a popular feature of the anthropological exhibits at the exposition, had been on loan from the Smithsonian Institution and afterwards had been returned to Washington (Kavanagh, 1990). Dorsey now set about replacing them.

On his first expedition for the museum, from May to August 1897, Dorsey was accompanied by Edward Allen, a museum photographer. On 12 May they left Chicago and went first to the Blackfoot, Blood, Flathead, and Kutenai tribes in Montana and Alberta. Dorsey was particularly pleased with their success on the Blood Reserve, where from 20 to 25 May they collected 136 objects, including a ceremonial pipe. Dorsey (FCM, 1895–1900(3):186) wrote, "with this were collected costumes, etc., so that it will be possible to represent one of the scenes in the transfer of the medicine pipe" (see also VanStone, 1992). They then went to the Northwest Coast, where they visited the Haida, Tlingit, and Tsimshian, to photograph and record physical anthropological data for the life groups. By August they were in the southwestern United States, at the Hopi pueblos in Arizona and at Zuni pueblo in New Mexico, where they purchased ethnological material to complement Ayer's collection. At Hopi Dorsey met Rev. H.R. Voth, a Mennonite missionary who had amassed a large collection of Hopi material culture and who was studying Hopi ritual life. In December Dorsey returned to the Hopi with F.B. Melville (a sculptor associated with the Field Columbian Museum's Department of Anthropology) to secure casts to be used for constructing Hopi life groups. On that visit Dorsey collected pottery and other material from Hopi ruins. Most important, he arranged to buy Voth's collection and succeeded in hiring Voth to catalog and install it in the museum (Dorsey, 1901b:219). Voth worked on the project in Chicago from late January 1898 through April 1900, during which time he installed nine altar groups that depicted the annual round of Hopi ceremonial life.

The pace of such a collecting trip left no time for any kind of ethnological study. Dorsey, who had a great sense of urgency for his work, recorded only the name of the object purchased and the price paid; when helpful interpreters were available, he also recorded amateurish renditions of the native terms for the objects. Occasionally he jotted down comments on design symbolism or some other fact about a piece, but his records are frustratingly telegraphic, giving the clear sense that filling the museum with specimens while they could still be obtained was uppermost on his agenda.⁴

After returning from the Hopi in January 1898, Dorsey remained in Chicago for most of the year to oversee the cataloging and installation of both Voth's and his own material (FCM 1895–1900(4):277).

The next year, 1899, Dorsey undertook another far-ranging expedition. In the summer he began by visiting the cliff ruins at Walnut Canyon, Arizona. From there he went to Ukiah, California, where he had arranged to meet J.W. Hudson, a collector



FIGURE 1.—Ayer Hall, Field Columbian Museum, 1894. Courtesy of the Field Museum of Natural History, Chicago (neg. no. 8191).

of California Indian material. Together they visited Pomo communities to make collections for the Field Columbian Museum. Dorsey then went to Tacoma, Washington, where he met Melville and his assistant, who made nine casts “intended for ethnic groups to show the native industries of the people of Puget sound” (Dorsey, 1900:260). The final stop on the trip was Vancouver Island, where additional casts were made for a large Kwakiutl ceremonial group to be installed in the museum. In December he returned to Hopi with Voth, not to collect, but, significantly, to record information for finalizing the exhibits. While there they witnessed and documented the Winter Solstice ceremony (FCM, 1895–1900(6):444; Dorsey, 1901b:220; Dorsey and Voth, 1901). That trip was apparently Dorsey’s introduction to ethnographic fieldwork and represents a significant breakthrough in his career.

In 1900 Dorsey’s summer expedition lasted from May through July. He visited the Fox in Iowa; the Shoshone and Arapaho in Wyoming; the Bannock and Nez Perce in Idaho; the Paiute in Nevada; the Ute in Utah; the Umatilla and Klamath in Oregon; the Assiniboine and Gros Ventre at Fort Belknap Reservation and the Assiniboine and Sioux at Fort Peck Reservation, in Montana; and the Sioux at Devil’s Lake Reservation in North Dakota (VanStone, 1996).

The next phase of Dorsey’s work at the Field Columbian Museum began in January 1901. In a report to F.J.V. Skiff, director of the museum, he presented a collecting plan that would “fill in the gaps” in Ayer Hall and transform it into a systematic ethnographic representation of tribes from the “Plains,” a label he used broadly to encompass five linguistic groupings: the Algonquian tribes, from the Naskapi in the

Northeast to the Blackfoot in the West; the Siouan tribes, from the Santee in Minnesota to the Assiniboine in Montana; the Shoshonean tribes of the Plains, Basin, and Plateau, from the Comanche of Oklahoma to the Bannock of Idaho; the Caddoan tribes; and the Kiowa. The museum had no representation whatsoever from the Caddoan tribes or the Kiowa, and tribal coverage in the other three language families varied from good to nothing. Dorsey proposed a five-year project, during which he would spend three months each year collecting in the field, to be financed at \$12,000 above the current departmental budget.

In the report, Dorsey pointed out that if this plan were to be acted upon, haste was essential for two reasons:

In the first place as each year passes it becomes more difficult to secure good ethnological material. Valuable in the hands of the Indian, representing olden times, it becomes more and more precious in his eyes, and owing to the great interest at present manifested in the Indians throughout the country, which carries with it a desire for Indian things, there is a constant endeavor on the part of tourists, traders, etc., to get hold of this material so that for this reason the sooner the work be done the better. In the second place, this institution is not alone in the knowledge of the fact that there still exists valuable material among the Plains Indians, which if it is to be obtained at all must be secured at once. I need not say that all material retained in the life of the Indian is to a certain extent limited in amount and so it would soon become a question of our having to take what other institutions had left behind, or in a majority of categories of objects[,] getting nothing. (Dorsey to Skiff, 17 Jan 1901)

Justification for the project was that the current collections did not "represent properly and adequately" the range of tribes in this broadly defined region. The very next day Dorsey sent a similar plan to improve the Northwest Coast collections.

For Dorsey, museums were like research libraries: coverage of particular subjects must be as complete as possible for the exhibits to be of maximum value. Ethnological exhibits were valuable to the extent that they represented contiguous tribes in comparable detail.

Dorsey undertook a collecting trip to Oklahoma in May and June 1901, during which he visited the Osage, Pawnee, Wichita, and Cheyenne (he had heard that a Cheyenne Sun Dance was to be performed, but he found it had been postponed). Also during this trip, Dorsey met James R. Murie, a mixed-blood Skiri Pawnee, and Cleaver Warden, an Arapaho, both of whom would become important contributors to Dorsey's ethnographic work.

In November, Dorsey revised the plan he had submitted to the museum director the preceding January. He now proposed to devote more time each year to the Plains work, which would allow collecting to be completed by spring 1903. He would also share the work with Simms (Dorsey to Skiff, 8 Nov 1901). The accelerated time schedule and the revised list of tribes to be covered reflected Dorsey's increasing knowledge of the area as well as the activities of collectors from other museums.

Dorsey as Ethnographer

The new year, 1902, saw Dorsey firmly entrenched in the rapidly professionalizing field of anthropology and enmeshed in an impressive array of projects. First were the projects directly relevant to his position in the museum: overseeing both the acquisition of ethnographic material from throughout the world, especially North America, and the installation of exhibits. Second were ethnographic investigations already begun, most important his collaboration with Voth on Hopi rituals. Third, and increasingly dominant, were his studies of what he called the "social organization" of the Cheyenne and Arapaho and of the Pawnee and other Caddoan tribes. Just as he used the term "Plains" very broadly, Dorsey likewise used the term "social organization" to refer to the entire social and religious organization of tribes, with special emphasis on ceremonies and the myths that underlie them. It was in 1902, Dorsey later wrote, that he "became profoundly interested in the social organization of the Pawnee, Arapaho, and Cheyenne, and used such time as I could spare in making observations on these subjects" (Dorsey to Skiff, 4 Mar 1905). To continue this work, in 1903 he took a leave of absence from the museum on limited pay.

During that leave Dorsey was apparently supported by the Fred Harvey Company and the Santa Fe Railway, and he now had the opportunity to do fieldwork and write without concern for collecting. In a letter to the director of the Field Columbian Museum, Dorsey argued for the need to undertake ethnographic research to support collecting activities: "We have been reproached for not doing our share of the work of investigation. ... In the truest sense of the term, investigation is really a part of the work of collecting, and the two should properly go hand in hand; but on account of the considerable expense involved in pure investigation I have aimed first, to see to it that the Museum side did not suffer" (Dorsey to Skiff, 10 Jan 1903).

Using his rapidly increasing knowledge of American Indian cultures, Dorsey wrote *Indians of the Southwest* (1903c), a popular book promoting tourism along the line of the Atchison, Topeka, and Santa Fe Railway from Chicago, through Kansas and Oklahoma, and continuing across the Southwest to California. Despite his foray into popularization, Dorsey's ethnographic work fully met the standards of the day for professional-quality museum anthropology, which was exemplified in the Plains area by the work of Alfred L. Kroeber and Clark Wissler, both of whom were students of Boas supported by the rival American Museum of Natural History, in New York.

Dorsey's enthusiasm for Plains ethnographic study may have been in large part fueled by the interpreters with whom he worked, notably Cleaver Warden (Arapaho), Richard Davis (Cheyenne), and James R. Murie (Pawnee). All were educated in boarding schools, and each was committed to aiding the efforts of anthropologists to record, preserve, and perpetuate native culture. Professional influences in anthropology may have inspired Dorsey as well. The first part of Kroeber's monograph on the Arapaho, presenting introductory material on social organization and a study of decorative art, was published in 1902

(Kroeber, 1902–1907); however, much of Kroeber's material came from his collaboration with Warden in 1899 (among the Southern Arapaho in Oklahoma) and in 1900 (among the Northern Arapaho in Wyoming). Kroeber and Dorsey worked independently, each going to the Arapaho to collect specimens for their respective institutions, but their studies converged to the extent that both were guided by the interests and perspectives of Warden (Figure 2).

During the five-year period from 1902 through 1907, Dorsey worked feverishly on his North American ethnographic projects, undertaking multiple studies simultaneously and being drawn deeper and deeper into the potential for describing other cultures. In January 1904 Dorsey reported to the museum director that the task of collecting to fill in the gaps in the North American material was nearly completed, and he suggested that the next collecting activity take place in South America and the Pacific (Dorsey to Skiff, 6 Jan 1904). For the next three years Dorsey sought to wrap up his Plains studies so he could move on to other projects; in no sense did he intend the American Indian ethnological work to be a lifetime pursuit. Despite his plans, the Plains studies took more and more time, and finally he abandoned them, leaving many parts undone—some nearly completed, others barely begun.



FIGURE 2.—Cleaver Warden, Washington, D.C., 1895. Courtesy of the National Anthropological Archives, Smithsonian Institution (neg. no. NAA/BAE:94). Photograph by William Dinwiddie.

THE ARAPAHO

On 27 November 1901 Cleaver Warden wrote to Dorsey from Darlington, Oklahoma, announcing that the Indian agent had given the Arapaho permission to have their Sun Dance the next week. Dorsey took the train to Oklahoma, arriving on the reservation on 5 December in time to observe and photograph the entire eight-day ceremony. After the ceremony, Dorsey returned to Chicago with Warden and Hawkan, one of the Arapaho priests, where they “spent two weeks, going over the details of the ceremony, inquiring especially into the symbolism” (Dorsey, 1903a:3–4), which allowed Dorsey to expand his description of the Sun Dance.

Dorsey was pleased with the work he had been able to accomplish with Warden and Hawkan in Chicago, and in January 1902 Dorsey hired Warden on a trial basis, instructing him to record information on Arapaho shields and men's societies (Dorsey to Warden, 27 Jan 1902).⁵

During the summer of 1902 Dorsey visited the Arapaho in Oklahoma and collected material for the museum (Dorsey to S.C. Simms, 14 Feb 1903). He attended the Arapaho Sun Dance on 21–28 August and once again photographed the ceremony in great detail (Dorsey, 1903a:157). Earlier in the year Dorsey's inclination had been to visit the Northern Arapaho in Wyoming with Warden, but he was pleased enough with Warden's work that while in Oklahoma Dorsey proposed that Warden go to the Northern Arapaho independently (Dorsey to Warden, 27 Jan 1902; Warden to Dorsey, 1 Sep 1902). Later that month or early in October, Warden came to Chicago to consult with Dorsey, then he headed for the Wind River Reservation, in Wyoming.

From late January through June 1903 Dorsey was in the field; this was the year of his partial leave from the museum. He was in Chicago early in July but left the museum again for a trip to Oklahoma, California, and Alaska, returning in late September (Commissioner of Indian Affairs William A. Jones to Dorsey, 16 Sep 1903; Dorsey to Jones, 30 Sep 1903). In November he left again, stopping in Oklahoma on his way to the Southwest. Before the end of the month he visited the Hopi, and during the first week of December he was writing letters from Albuquerque on Fred Harvey Company stationery.

Warden continued to work for Dorsey throughout the year. His notebooks record syntheses of information on specific topics rather than interview transcripts, and with the exception of some ritual prayers, they are all in English. The date of his return from Wyoming to Oklahoma is apparently unrecorded, but he reported attending a dance (probably a Clubboard society dance, one of the Arapaho age-graded men's societies) with James Mooney, of the Bureau of American Ethnology, who photographed the event. Warden wrote, “It will take me a little while to straighten the report of the ceremony” (Warden to Dorsey, 1 Dec 1903). Warden's work for Dorsey developed along two lines, writing traditional stories and describing the ceremonies of the men's societies and the sole women's society (all called “lodges” by the Arapaho). Dorsey seems to have felt

an increasing desire to be directly involved in the work. He wrote to Warden from Albuquerque on 3 December 1903: "When I hired you I said that I should probably be able to give you work for at least two years... I am by no means ready to quit work among the Arapaho, but I am anxious to take it up with you personally; that is, I want to camp among your people for some time."

Dorsey remained in the field until the end of April 1904, when he returned to the museum (Dorsey to Skiff, 4 Mar 1905). Throughout the remainder of the year he devoted a portion of his time to the Field Museum's involvement in the Louisiana Purchase Exposition, held in St. Louis.

The second part of Kroeber's study of the Arapaho, "Ceremonial Organization," was published in 1904 (Kroeber, 1902–1907). That summer Dorsey sent a copy to Warden. Kroeber's data had all been recorded in 1899 and 1900, but Warden and Dorsey had continued to work on the same topics since 1902. Dorsey instructed Warden: "Write out everything you know and have not already sent to me... What we want now is additional information" (Dorsey to Warden, 2 Jul 1904).

Following instructions, Warden set about describing the societies. On 1 August he reported that he had finished his account of the women's Buffalo Lodge and had begun writing about the Thunderbird Lodge. He offered this advice: "Mr Dorsey, you have to get a good priest of these topics to [come to Chicago] stay and work out to the bottom of the lodges." On 17 August Warden reported that he had finished his description of the Thunderbird Lodge. Although Dorsey intended to go to Wyoming with Warden that fall, it proved impossible, and Warden again traveled independently to the Wind River Reservation, arriving at the Arapaho subagency in early November (Dorsey to Warden, 12 Oct 1904; Warden to Dorsey, 9 Nov 1904).

Warden continued to keep Dorsey apprised of ceremonial events in the Arapaho communities. Warden wrote on 6 September 1906 to tell Dorsey that the Lime Crazy Lodge was to be held near Calumet, Oklahoma; it would be the first performance of the ceremony in 19 years, and Warden requested that he bring for it the ritual paraphernalia in the museum collections. Dorsey complied with the request and traveled to Oklahoma, taking with him three distinguished visiting European anthropologists: Eduard Seler (Mexicanist from the Museum für Völkerkunde in Berlin), Paul Ehrenreich (Brazilianist from the Royal University of Berlin), and Alfred Cort Haddon (from Cambridge University, leader of the 1898–1899 Torres Straits Expedition, the first multidisciplinary field party in anthropology). They failed to meet Warden, however, and left before the ceremony was held, visiting the Pawnee on their return to Chicago.

The next month Warden wrote to Dorsey to say that the dance, delayed because of opposition from missionaries, had taken place on 6–9 October. "The Indians were very sorry to see you leaving the grounds... It was a wonderful dance." At the culmination of the ceremony the lodge members danced into a bed of glowing coals until the fire was trampled out.

"When they had a fire walk and a circular pass over [i.e., around the camp], it was a grandeur scene!" (Warden to Dorsey, 11 Oct 1906). The Arapaho were grateful for the loan of the regalia, and Warden promised to write an account of the ceremony (Warden to Dorsey, 16 Oct 1906). He sent Dorsey a small notebook on 12 December, suggesting that one or two of the priests be brought to Chicago to fill out the account. Dorsey replied on 18 December that he was too busy to work on the Arapaho but planned to do so at a later date; however, Dorsey never returned to his Arapaho studies.

Nevertheless, he continued to receive letters from Arapaho correspondents. Faustinus White Antelope wrote from the Arapaho Agency in Wyoming in June 1907 to invite Dorsey to the Sun Dance and to enlist his support for it. Dorsey replied that he would like to attend the ceremony but did not know whether it would be possible. He added, "I do not think you will have any trouble with your agent in this matter, as it is none of his business" (Dorsey to White Antelope, 13 Jul 1907). The same day Dorsey wrote a strongly worded letter to Commissioner of Indian Affairs Francis A. Leupp asking him to explain why the Arapaho agent was allowed to interfere in the Indians' religious observances: "The sun dance like other Indian ceremonies will die a natural death, for it has served its day, but it makes me indignant to find certain agents in the West attempting to dictate to the Indian his religious beliefs" (Dorsey to Leupp, 13 Jul 1907).

Dorsey published two volumes based on his Arapaho work. The first is "The Arapaho Sun Dance: The Ceremony of the Offerings Lodge" (Dorsey, 1903a), which includes 137 plates that reproduce 177 photographs of the 1901 and 1902 ceremonies and 10 plates of color drawings of the altar and of body paints of the dancers. The description is based on a day-by-day account of the 1901 ceremony, with comparative notes from 1902. Description of the ceremony is supplemented with the texts of prayers and relevant myths and stories related to the Sun Dance, all given in English translation; much of this material was provided by Hawkan during his visit to Chicago. The ethnographic account, moreover, is personalized by providing the names of all participants in the Sun Dance and their roles. All information was attributed to particular individuals or to Dorsey's own observations, as he did not hesitate to refer to himself in the first person as an active observer of the ceremony. Dorsey was at particular pains to try to understand the minute details of symbolism in the altars, dancers' body paints, and ritual actions, frequently referring to myths for the keys to explain what otherwise seemed to be meaningless detail. Consequently, for ethnographic detail and thorough photographic representation of ritual this volume set a new standard in American anthropology.

The second published volume, "Traditions of the Arapaho," coauthored by Dorsey and Kroeber (1903), includes 146 stories that the two anthropologists compiled independently (but both working with Warden), together with abstracts of each story.⁶ A strong point of this voluminous collection is the inclusion of

numerous variant versions. The stories are arranged in three categories: 15 origin stories relating to sacred bundles and societies, 47 trickster tales, and 84 myths and traditions.

Dorsey's third volume on Arapaho social organization, societies, and art was never completed, although he had commissioned an extensive series of line drawings and exquisite water colors of museum specimens to illustrate it. The publication in 1907 of the final part of Kroeber's (1902–1907) study of the Arapaho, "Religion," which was illustrated with line drawings of artifacts from the American Museum collections, may have dampened Dorsey's enthusiasm for completing the manuscript. Moreover, publication funds at the Field Museum had become harder to obtain.

Today, Dorsey's manuscripts, together with notebooks, drawings, and miscellaneous material from Cleaver Warden, are preserved in the Field Museum's Department of Anthropology archives. They serve as testimony to the two men's collaborative method of working and form valuable resources for future study.

THE CHEYENNE

On 6 July 1901 Dorsey received a letter from Cleaver Warden, informing him that the Cheyenne and Arapaho in Oklahoma had received permission from their Indian agent to hold Sun Dances. Eager to witness the ceremony, Dorsey took the train to Oklahoma in August. The Cheyenne Sun Dance took place on the Washita River near Watonga. Admitting that he was not acquainted with the Cheyenne, Dorsey nonetheless wrote that he "was given authority by a council of chiefs to witness all the rites of the ceremony, and I remained throughout this performance" (Dorsey, 1903a:2, 1905d:xiii). He was permitted to photograph virtually every aspect of the ceremony and to observe the ritual activities day-by-day, with the exception of the secret preparatory rites in the Lone Tipi.

After observing the Sun Dance, Dorsey planned to bring one of the Cheyenne religious leaders to Chicago to revise and expand his field notes, but that proposed collaboration never took place. During the summer of 1902, while Dorsey was working in Oklahoma, he met, or became reacquainted with, Richard Davis, who would become his interpreter and collaborator in the rest of his Cheyenne studies (Figure 3). Dorsey sent \$40 to Davis for expenses, together with instructions for writing out Cheyenne stories in English. He was particularly interested in obtaining everything possible relating to tribal origins, the medicine arrows, and the Sun Dance: "The old men, and especially the blind, are generally the best story tellers. Do not fail to get stories from women as well as from men. Insist upon the stories being completed; the longer the better. When you get hold of a good story teller, get everything he knows before you go to another man. Cover the ground thoroughly and make your money go as far as possible" (Dorsey to Davis, 6 Sep 1902).



FIGURE 3.—Richard Davis, Louisiana Purchase Exposition, St. Louis, 1904. Courtesy of the Field Museum of Natural History, Chicago (neg. no. 15426). Photograph by Edward Carpenter.

In July 1903 Dorsey again attended the Cheyenne Sun Dance, this time accompanied by Field Columbian Museum photographer Edward Carpenter. James Mooney, who was in Oklahoma working on Cheyenne and Arapaho symbolism, was also present. Davis served as interpreter, and the Sun Dance priests, at Dorsey's request, "appointed one of their number, a well-known chief and Sun Dance priest, by the name of Roman-Nose-Warrior, who should devote his time to me. This arrangement proved quite satisfactory" (Dorsey, 1905d:xiv).

Early in the morning on the day following the conclusion of the Sun Dance, a young man underwent an ordeal of self-mortification. Two skewers were inserted into cuts in the skin of his back and two ropes, each tied to a fragment of a buffalo skull, were attached to the skewers. The man dragged the skulls around the inside of the camp circle for 35 to 45 minutes; then

a Sun Dance priest removed the skewers. The incident was photographed both by Carpenter and by Mooney (Dorsey, 1905d:179–180) but was also witnessed by others. When John Seger, the Indian agent, accused the anthropologists of paying the Indian to torture himself to gratify their scientific curiosity, the incident became a media event. Both Dorsey and Mooney wrote affidavits for the Indian Office denying the charge.

In May, Davis informed Dorsey that Roman Nose had pledged to sponsor a Sun Dance during the coming summer: “You understand the situation down here and all Indians desire your assistance and from all other ethnologists who desire to witness this gathering so they [can] perform this ceremony without any trouble from Seger or any other Agent” (Davis to Dorsey, 8 May 1905). By then, however, Dorsey had already completed his Cheyenne Sun Dance book and had sailed for Europe. After he returned, Dorsey wrote to Davis on 21 July to explain his failure to attend the ceremony. On 3 August Davis wrote to Dorsey from Pawnee, Oklahoma, to announce that Agent Seger had resigned and to report that, although Seger had opposed the Sun Dance, he had allowed the Indians to hold it in July. Dorsey sent copies of the Cheyenne Sun Dance book with his reply: “I am very anxious to investigate more thoroughly the various medicine rites, and especially the arrow ceremonies and the ceremonies of the warrior societies among the Cheyenne, but as I say my time will be so occupied that I am afraid I can not get around to it, at any rate for several months” (Dorsey to Davis, 5 Aug 1905).

In June 1906 Dorsey again attended the Cheyenne Sun Dance; Little Chief requested that he bring the buffalo skull and hide from the museum for use in the ceremony (Warden to Dorsey, 26 Jun 1906; Dorsey to Warden, 28 Jun 1906). That he would attend the ceremony suggests the sincerity of Dorsey’s commitment to continuing work with the Cheyennes; nevertheless, he did not return to his studies of that tribe.

The published results of Dorsey’s Cheyenne studies appeared in a single volume, issued in two parts. “The Cheyenne, I: Ceremonial Organization” (Dorsey, 1905c), is organized in two sections: “Ceremonies,” which discusses the medicine arrows, chiefs, and the men’s societies, and “Myths,” which presents five origin stories. Most of the material came from Richard Davis, including the diagrams and drawings, which were reproduced as 17 plates (most in color) and six figures. Dorsey wrote in his introduction: “These notes on the organization of the Cheyenne are given in this form because opportunity for further and more extended observation does not now seem possible” (Dorsey, 1905c:[v]).

“The Cheyenne, II: The Sun Dance” (Dorsey, 1905d) is illustrated by 51 plates and 108 figures, which reproduce 197 photographic images depicting the Sun Dance, 11 diagrams, and 13 color plates that depict body paints and reproduce drawings by Davis. The text describes the 1903 ceremony, with comparative notes from 1901 when relevant. In contrast to the Arapaho study, Dorsey’s account of the Cheyenne Sun Dance is based on the notes he took while in the field and lacks the richness of

prayers and associated stories that might have been recorded had he arranged to bring a Sun Dance priest to Chicago. Dorsey organized the volume to parallel the Arapaho one, thereby facilitating comparison between them. After a discussion of similarities and differences, he concluded that the ceremonies of the two tribes were “morphologically the same,” but he was impressed by how much more forcefully the Cheyenne ceremony enacted the meaning underlying its origin story as “a drama epitomizing creation”—not the first creation, but “a renewal of creation, or of rebirth” (Dorsey, 1905d:185–186). He ended the study with a call for further comparative study to determine the origin of the Sun Dance.

There are no unpublished materials relating to the Cheyenne among Dorsey’s papers in the Field Museum.

THE CADDOAN TRIBES

Dorsey undertook the study of all five surviving tribes of the Caddoan language family: the Skiri and South Band Pawnee (the latter comprising the Chawi, Kitkahahki, and Pitahawirata), the Wichita, and the Caddo—all in Oklahoma—and the Arikara in North Dakota. He planned to collect mythology and traditions from each tribe and to document social organization and religious ceremonies. In his studies of the Caddoan tribes Dorsey depended on the collaboration of James R. Murie, who carried out most of the fieldwork.

Dorsey met Murie during a 1901 collecting trip to the Pawnee. Murie was then collaborating with Alice C. Fletcher on a study of Pawnee rituals. Fletcher and Francis La Flesche, the mixed-blood Omaha with whom she shared her studies as well as her home in Washington, D.C., had known Murie as a student at Hampton Institute, in Virginia. When Fletcher first visited the Pawnee in 1898, Murie had provided entree to tribal religious leaders. In 1899 and again in 1900 Murie had brought to Washington a Chawi Pawnee priest named *Tahirasawica* ‘He Arrives In The Lead’ to record on graphophone cylinders the pipe adoption ritual, to which Fletcher and Murie gave the name “Hako,” and to answer Fletcher’s questions about the ceremony (Fletcher, 1904:14–18; Parks, 1981; Mark, 1988: 274–277).

One can reasonably conjecture that during Dorsey’s 1901 visit, Murie described the methods he and Fletcher had used to record the long and complex Hako ceremony. It also seems likely that Dorsey found their approach well suited to his position as museum curator. Field Columbian Museum administrators frowned on fieldwork that did not enhance the collections, but if American Indians could be brought to the museum, one could accomplish much more than was possible in a few hurried days in the field. Dorsey invited Murie to come to Chicago during the winter and work with him, but, when reporting the incident to Fletcher, Murie noted with apparent satisfaction, “I have refused Dorsey” (Murie to Fletcher, 24 Aug 1901, F-LFP).

Dorsey was determined, however, and wrote to Murie again in March 1902. That letter prompted Murie to seek Fletcher’s advice (Murie to Fletcher, 2 Apr 1902, F-LFP): “Dorsey is

coming in two or three weeks and wants to hire me for two years. If I hire to him I shall let you know. He wants to take the Indians one at a time to Chicago and get all he can out of them. Do you think I had or not?" Also in this letter, Murie revealed the intensity of his desire to see the documentary work on Pawnee ceremonies carried out and his realization that money was the key to success: "O! Miss Fletcher there is lots to learn about these people. If I was rich I could sit down with them and write down what they tell me."

Fletcher wrote to Dorsey about his plans to hire Murie and pointed out that she had been working with him for the past four years on Skiri rituals (Fletcher to Dorsey, 16 Apr 1902). Replying from Wichita, Kansas, on his way to Chicago, Dorsey informed Fletcher that he had taken his plans too far to abandon them and announced that he had hired Murie, who, he said, had been asking him to do so for the past year. He told her that he would be visiting Washington soon and would discuss the matter with her (Dorsey to Fletcher, 4 May 1902).

For Murie, the pragmatic issue seemed to be who had the best rapport with the priests and who had the money necessary to support the work. Finally, in May, he explained to Fletcher his reasons for taking the job that Dorsey offered:

Before Geo. A. Dorsey left Pawnee I had made arrangements to work for him. He had been into several bundle ceremonies and I learn from Indians that he was making headway in gaining some of their secret & sacred ceremonies. I had telegraphed you of my rejecting his offer, which I did—; but he made other offers which I rejected and made some explanations to him thinking he would give me up. He made an offer, and was at my house [so] often, that I could not refuse and if I had not [accepted], he would have got everything so we could do nothing. Leader is now his best friend. I did not write you sooner for he promised to see you & Doctor McKee [WJ McGee, Ethnologist in Charge, Bureau of American Ethnology] and make everything alright.... In one of the ceremonies he was permitted to say something and in his talk to the Indians he spoke of you very highly as doing the same work he was. I was very sorry to have to leave the work with you and go over to Dorsey, for his work is entirely different from yours. (Murie to Fletcher, 28 May 1902, F-LFP)

Dorsey's face-to-face conversation with Fletcher seems to have resulted in a mutual agreement, and in future correspondence they maintained a collegial relationship. Fletcher was 30 years older than Dorsey, and both got their start at Harvard as protégés of Putnam. They agreed that she would bring to a conclusion the studies she had already begun. In August the Hako manuscript was sent to the printer (McGee to Fletcher, 11 Aug 1902), and on 6 September, after taking a long vacation to rest, Fletcher wrote to Dorsey from California inquiring about the progress of his Pawnee work. Dorsey's reply is not extant. By December, he had brought Murie and a Pawnee priest to the Field Columbian Museum, where they began their project (Dorsey to Skiff, 7 Jan 1903). At the winter meeting of the AAAS, held in Washington, D.C., Fletcher delivered a paper entitled "Pawnee Star Lore" (Fletcher, 1902), and Dorsey spoke on "Recent Investigations among the Pawnee" (AAAS, 1903:501).

Dorsey now faced the problem of funding Murie's work. In June 1902 Dorsey learned that he, Holmes, and Boas had been selected as members of the advisory committee on anthropol-

ogy of the newly founded Carnegie Institution of Washington (Holmes et al., 1903; Charles D. Walcott, secretary, Carnegie Institution of Washington, to Dorsey, 3 Jun 1902, CIW-A). Soon after, Dorsey inquired about applying for a grant to subsidize his work with Murie. Boas opposed the request, arguing that the work should be supported by the Field Columbian Museum because that institution stood to benefit from any collections made, but Holmes interceded, and \$2,500 was granted in January 1903 to support the project (Boas to Holmes, 13 Sep 1902; Boas to Dorsey, 23 Sep 1902; Dorsey to Holmes, 12 Jan 1903, CIW-A; Walcott to Dorsey, 27 Jan 1903, CIW-A).

In his proposal to the Carnegie Institution, dated 12 January 1903, Dorsey stated that the object of the grant was "to complete a study of the different tribes of the Caddoan stock, special attention being given to (1) the rituals of the Pawnee ceremonies and sacred bundles, and (2) the mythology and linguistics of all four tribes.... The field work would be done by Mr. James Murie, an educated Pawnee... who is connected by blood relationship with the leading Pawnee priests," while Dorsey would oversee writing up of the material.

On 1 October 1903 Dorsey reported the results of his work with Murie under the grant (CIW-A; Carnegie Institution of Washington, 1903:xv-xvi). Murie had recorded some 70 Skiri myths, which, together with those Dorsey had previously obtained, "form perhaps the largest body of mythology, so far collected from any one tribe in America." Most of them had already been prepared for publication as a memoir of the American Folk-Lore Society. He noted that 35 of the stories included songs but that he had not yet found anyone to transcribe the music. Murie had, moreover, recorded 40 Wichita stories and 80 Arikara traditions. In addition to recording myths and tales, Dorsey also listed an impressive number of Pawnee and Arikara ceremonies that he and Murie had witnessed—sometimes together, sometimes Murie alone.

In 1904 Dorsey was awarded a second Carnegie grant for \$2,500, which allowed him to continue to employ Murie. Dorsey was absent from the Field Columbian Museum until 1 May to continue his study of Caddoan oral traditions in Oklahoma (Dorsey to Skiff, 4 Mar 1905). In a report to the Carnegie Institution dated 1 October 1904, Dorsey noted that the previous fall and winter had been devoted to the study of Wichita mythology and that during the winter and continuing into the spring he had worked among the Caddo (CIW-A; Carnegie Institution of Washington, 1905(3):83). Frederick R. Burton, a musicologist from Yonkers, New York, had come to Chicago to transcribe the music for the songs included in the Wichita stories; in addition, he transcribed about 200 Skiri ceremonial songs (see discussion in Burton, 1909:44-57). Dorsey also reported that, to supplement the volume of Skiri stories just published, he was preparing another volume of those Skiri tales that included songs, together with musical transcriptions (Dorsey, 1904d). He was also working on the collection of South Band Pawnee traditions. Finally, he reported that a vol-

ume of Arikara traditions, recorded by Murie, had been submitted to the Carnegie Institution for publication (Dorsey, 1904c).

Continuing the study of ceremonies, Dorsey brought to Chicago for several weeks a Skiri priest who provided the rituals and songs of the Morning Star and Evening Star ceremonies, medicine men's ceremony, Four Directions bundle ceremony, and scalp offering. Tawakoni Jim, a Wichita ceremonial leader, also visited Chicago, and he began to provide information on rituals (Dorsey, 1904b:[iii]). In the cover letter for his report, Dorsey requested that he be funded at \$3,000 annually for the next two years (1905–1906) to complete his study of Caddoan traditions and ceremonies.

In April 1905 Dorsey requested that the Field Columbian Museum pay rail transportation for Murie to come to Chicago to work with him on the Carnegie Institution project; Murie would also help him prepare the Pawnee material for exhibit at no additional expense to the museum (Dorsey to Skiff, 2 Apr 1905). Murie was employed on the project throughout the year, either in the field or in Chicago, although Dorsey himself spent May and June in Europe (Dorsey to R.S. Woodward, president, Carnegie Institution of Washington, 3 Oct 1905, CIW-A; Carnegie Institution of Washington, 1906(4):53). Murie witnessed a large number of ceremonies and recorded ritual songs, including those of the Morning Star ceremony, and rituals relating to fire and the sun, on more than 500 wax cylinders.

In October, Dorsey declared the Caddoan mythology work complete: he was reading page proofs of the Caddo volume (Dorsey, 1905e) and was revising the South Band narratives manuscript. He then repeated his request for \$3,000 for the coming year: "With this sum I hope to bring to practical completion the accounts of all Pawnee ceremonies, with a full interpretation of their rituals" (Dorsey to Woodward, 3 Oct 1905, CIW-A) While he was studying Pawnee ceremonies, Murie was also actively procuring sacred bundles for the museum, most significantly the Big Black Meteoric Star bundle, which contains a map of the heavens painted on hide (Murie to Dorsey, 16 Dec 1905).

In early 1906 Murie brought to the Field Museum of Natural History (formerly the Field Columbian Museum) the Skiri Pawnee priest Roaming Scout (Running Scout), whom Dorsey characterized as "one of the most influential and perhaps the most learned of the Skiri today" (Dorsey, 1904d:329). They returned to Pawnee, Oklahoma, on 6 March. The next day Dorsey wrote to Mooney that he had "been very busy with them," presumably working on the Skiri religion manuscript, which describes the annual round of ceremonial life. On 6 September Dorsey wrote to Boas to request his help with some linguistic matters, then wrote more specifically on 24 September to say that he would like to come to New York to spend six to eight weeks studying Pawnee linguistics. Boas's reply was less than encouraging, warning that it would take a longer period to become familiar with linguistic methods: "I shall not be able to give more than a certain limited time to you, unless I should want to take up the study of Pawnee myself. . . . Anyway, you

may be sure that I shall do my best to assist you in your endeavors to familiarize yourself with linguistic methods" (Boas to Dorsey, 1 Oct 1906).⁷

Dorsey's seemingly sudden interest in linguistics requires explanation. Up to this point he had been content in all his studies to rely on the translations of his interpreters. His interest in the language itself may have been stimulated in part by the difficulty Murie experienced in translating the archaic and aphoristic ceremonial song texts, but it probably was not coincidental that just at this time, the summer of 1906, William Jones moved to Chicago (Figure 4). A member of the Fox tribe, Jones was trained by Boas in linguistics and received his doctoral degree in 1904 (Boas, 1909). He had already recorded a collection of Fox texts and in 1906 was preparing for publication a collection of Ojibwa texts recorded under a grant from the Carnegie Institution. It thus seems likely that through personal contact Dorsey was attracted to the linguistic side of anthropology, which had not been available at Harvard. At the same time, because both were graduates of Hampton Institute,



FIGURE 4.—Left to right, George A. Dorsey, William Jones (standing), and James R. Murie. Brooklyn Museum of Art Archives, Stewart Culin Archival Collection, Expeditions [2.1], "Report on a Collecting Trip Among the Indians of New Mexico and California, May–September 1907," page 212a.

it seems likely that Murie would have found Jones to be an impressive role model.

In September 1906 Dorsey hosted the visit from Haddon, Selser, and Ehrenreich (see "The Arapaho," above). At the end of the month, after their failure to connect with Warden at the scheduled Arapaho Lime Crazy Lodge, they visited Pawnee, where they witnessed the performance of the Morning Star ceremony (see Haddon's account in Duke, 1989).

The problem of transcribing the recorded Pawnee ritual music remained unsolved. Burton had made a start, but like other musicologists of his day, he harmonized American Indian music for the piano, thereby transposing native musical forms into a western one. Dorsey therefore sought to enlist the aid of Erich M. von Hornbostel, of the Psychological Institute in Berlin, one of the founders of the field that would become known as ethnomusicology. Von Hornbostel's work applied "the concepts and methods of acoustics, psychology and physiology to the study of non-European musical cultures" (Katz, 1980:716). Dorsey wrote to von Hornbostel, inviting him to come to the United States to study Pawnee music. Von Hornbostel responded favorably on 4 June 1906, stating that he could come in October. In his annual report to the Carnegie Institution dated 15 October 1906, Dorsey noted that von Hornbostel was in Chicago transcribing and studying the recorded Pawnee music (CIW-A; Carnegie Institution of Washington, 1907(5):55-56).

In his report Dorsey stated that in June he had submitted for publication the first part of a two-part monograph on Pawnee myths. The first part comprised the South Band stories as well as the Skiri tales that included songs (Dorsey, 1906c). He anticipated that the second part, containing the texts and musical transcriptions of songs that were sung as a part of the stories, as well as his comparative study of Caddoan mythology, would, with von Hornbostel's assistance, be completed by the end of the year. The greater part of 1906, however, had been devoted to working on an account of Skiri Pawnee society and religion, which was about three-fourths written. In a separate application letter, Dorsey asked a fifth time for continued support in the amount of \$2,000: "I should have preferred greatly to have brought my investigation to a close this year and not to have asked for another grant, inasmuch as my Pawnee researches have been consuming each year an increasing amount of time. In fact, my time this year has been almost entirely spent on this subject" (Dorsey to Carnegie Institution of Washington, 15 Oct 1906, CIW-A). The funds were to pay for Murie's continued work, as well as the services of a stenographer and typist.

About the beginning of November 1906, Murie went to Oklahoma with von Hornbostel. Meanwhile, Dorsey reported that he was continuing his linguistic studies: "I have kept old Scout here with me and have succeeded in getting a fine lot of tales, narratives, explanations, etc. on long phonographic cylinders; all of which I hope to bring out in the form of a volume of texts" (Dorsey to Haddon, 13 Nov 1906). He also wrote to Boas about the recorded texts, characterizing Roaming Scout

as "a full blood Pawnee, who speaks no English" (Dorsey to Boas, 13 Nov 1906). It thus seems clear the Roaming Scout texts were recorded in October, after Haddon left Chicago and before Murie left for Pawnee.

The texts fill more than 200 cylinders and amount to over seven hours of narration that cover a range of ethnographic topics—discussions of life, death, the nature of mysterious power, healing, constellations and individual stars, and religious rituals—as well as Roaming Scout's autobiography, reminiscences, and several myths. Later, Murie transcribed the Skiri text of the narratives and then wrote out a literal word-for-word translation of them.

Meanwhile, Dorsey pursued his arrangements with Boas, writing to him again to ask whether he should bring Roaming Scout to New York—"The old Indian I have here who speaks no English"—or whether he should bring his interpreter (Dorsey to Boas, 19 Nov 1906). Boas chose the interpreter. Dorsey and Murie traveled to New York and began work with Boas in January 1907 (Boas to Dorsey, 23 Nov 1906; Simms to F.W. Antelope, 25 Jan 1907). Although there are no documents that record the day-to-day aspects of their linguistic collaboration, the focus seems to have been on phonology and grammar. Writing Boas after returning to Chicago, he commented, "I found my work with you possessing a certain quality of interest that I had not known before" (Dorsey to Boas, 21 Mar 1907).

Throughout 1907 Dorsey focused his attention on the Roaming Scout texts. A typewritten copy of Murie's transcription is covered with Dorsey's emendations in red and black ink.⁸

Meanwhile, von Hornbostel made steady progress on the musical transcriptions. On 5 April he wrote to Dorsey that he had a process for making galvanoplastic copies of cylinders from copper negatives—a process that would allow him to retain copies of the cylinders in Berlin and return the originals to Dorsey. On 3 June he asked Dorsey for typewritten copies of the song texts accompanying the Skiri tales; on 13 August he reported that he had sent the musical transcriptions of the tale songs and had returned the typescript of the song texts. He told Dorsey he could begin on the doctors' songs in September and asked him to send transcriptions of the texts. When Dorsey acknowledged receipt of the musical transcriptions, he reported that he had fallen behind on his end of the project:

Murie returns to Chicago within a few days, and I will take up the matter of filling up the words of the songs, revising the words, etc. preparing the whole thing for publication. I should be glad if you will take the Doctor songs next, but I regret to say that they have not been translated. As a matter of fact the words of the songs have not even been transcribed, and I shall hardly get around to that matter during this year. You will understand that since I took up the linguistic side of the investigation that my big manuscript [on Skiri society and religion] has been pushed to one side and that I have done practically nothing with it and the linguistic work will probably consume my time between now and the first of the year. (Dorsey to von Hornbostel, 11 Sep 1907)

In September, Dorsey submitted an annual report to the Carnegie Institution:

The work of this year has been almost entirely of a linguistic nature. Recognizing the necessity of being able to interpret Pawnee in order to translate prop-

erly the large number of songs and rituals which have accumulated during the past five years, the work of preparing for publication certain material already obtained was suspended and the writer accompanied by Mr. Murie spent ten weeks during the winter in New York, where, under the direction of Dr. Franz Boas, the fundamental principles of the Pawnee language were studied and methods of recording texts, etc., were learned. (Dorsey to Carnegie Institution of Washington, 20 Sep 1907, CIW-A; Carnegie Institution of Washington, 1908(6):167)

After returning to Chicago, Dorsey devoted himself to preparing the Roaming Scout texts for publication, which, according to his report, was a project he hoped to complete within the year: "These have all been transcribed and are now being verified from the records." He planned to introduce the volume with a sketch of Pawnee grammar and to use the texts to revise his manuscript on Pawnee society and religion.

In a letter to Woodward, Dorsey asked for a sixth year of support (21 Sep 1907, CIW-A). Woodward's reply was not hopeful: "We have now reached the normal condition of educational and research institutions, namely, that of poverty" (25 Sep 1907, CIW-A). Nonetheless, Dorsey submitted an application to Woodward in which he asked for \$2,000 to pay Murie for his expenses, his Pawnee consultants, and a stenographer as necessary, but it was denied (Dorsey to Woodward, 2 Oct 1907; Woodward to Dorsey, 24 Dec 1907, CIW-A).

Dorsey's reply reveals no disappointment: "Owing to plans recently formulated by me for a rather comprehensive trip abroad, I am quite satisfied with the action of the Executive Committee. In fact, I was about to write to you that in case the grant were made that I should not be able to use more than a small portion of it [in] the forthcoming year" (Dorsey to Woodward, 26 Dec, CIW-A).

Dorsey was very busy during the fall of 1907. In addition to work on the Roaming Scout texts, he also lectured in anthropology at the University of Chicago and in comparative anatomy at Northwestern University (Dorsey to Boas, 15 Oct 1907). The reason for Dorsey's apparently sudden decision to abandon the Pawnee work in which he was so deeply immersed is not revealed in the correspondence.

The published results of Dorsey's Caddoan project comprise five volumes of myths and tales, totaling more than 1,600 printed pages, that represent each of the four surviving Caddoan tribes. "Traditions of the Skidi Pawnee" (Dorsey, 1904d), published by the American Folklore Society, begins with a substantial historical and cultural introduction and includes 90 stories illustrated with 15 plates depicting some of the narrators as well as bundles and other objects mentioned in the text. This volume includes more extensive notes than others of Dorsey's myth collections, but it lacks abstracts of the individual stories. The narratives were collected between 1899 and 1902 by both Dorsey and Murie, sometimes working independently, at other times working together.

The other four volumes of myths and traditions were all published in a uniform format by the Carnegie Institution; the volumes are not illustrated, but they include story abstracts. "Traditions of the Arikara" (Dorsey, 1904c) presents 82 stories that

Murie recorded in 1903. "The Mythology of the Wichita" (Dorsey, 1904b) presents 60 stories that Dorsey and Murie recorded in 1903–1904, with the assistance of Burgess Hunt; it also includes Burton's musical transcriptions. "Traditions of the Caddo" (Dorsey, 1905e) presents 70 stories that Dorsey recorded in 1903–1905. The final volume, "The Pawnee: Mythology (Part I)" (Dorsey, 1906a), presents 148 stories, 103 told by South Band narrators and 45 by Skiri narrators.⁹ Part two, which was to include the musical transcriptions and comparative study, was never published. The fate of von Hornbostel's musical transcriptions of songs included in stories in part one is not known, and Dorsey's comparative analysis of Caddoan mythology was never written.

Dorsey's only publication of the voluminous ritual texts that he and Murie recorded on phonograph was "A Pawnee Ritual of Instruction," a brief fragment recorded by Woman Newly Made Chief, a Skiri, that presents a text recited to children during the winter and refers to the origin of life, death, and the pipe adoption ceremony (Dorsey, 1906b). Appropriately enough, it was published in the *Boas Anniversary Volume* and was likely a text that Dorsey, Murie, and Boas had worked on together in New York. Inexplicably, the text is presented only in English, omitting the Pawnee.

Dorsey also published a number of other articles on diverse topics relating to his Caddoan work: "An Arikara Story-Telling Contest" (Dorsey, 1904a), "Caddo Customs of Childhood" (Dorsey, 1905b), "One of the Sacred Altars of the Pawnee" (Dorsey, 1902a), "How the Pawnee Captured the Cheyenne Medicine Arrows" (Dorsey, 1903b), "A Pawnee Personal Medicine Shrine" (Dorsey, 1905a), "Pawnee War Tales" (Dorsey, 1906c), "The Skidi Rite of Human Sacrifice" (Dorsey, 1906d), "Social Organization of the Skidi Pawnee" (Dorsey, 1906e), "Hand or Guessing Game among the Wichitas" (Dorsey, 1901a), and "Wichita Tales" (Dorsey, 1902b, 1903d, 1904e).

In addition to the published works, a large body of Dorsey's unpublished Pawnee manuscript material is archived in the Field Museum, the American Museum of Natural History, and Indiana University; the cylinder recordings are preserved in the Archives of Traditional Music at Indiana University. Of these materials, the most significant are the Roaming Scout texts themselves, including the original cylinder recordings, Murie's transcription, Dorsey's revised transcription, and Murie's draft translations. This entire collection of texts has been retyped and retranslated by Parks in collaboration with a contemporary Skiri Pawnee elder (Parks, in prep.). Also significant is the nearly completed monograph entitled "Skidi Pawnee Society and Religion," a detailed account of social organization and the annual round of ceremonies. Brief portions of the manuscript were later published by Ralph Linton (1922a, 1922b, 1923a, 1923b), and the section on social organization was published by Alexander Spoehr (Dorsey and Murie, 1940). A complete, edited edition is planned (Dorsey and Murie, in prep.).

Cumulatively, the published and unpublished materials by Dorsey and Murie provide a more thorough description of the Pawnee than what contemporary anthropologists had accomplished for any other Plains tribe. The volumes of myths and traditions, in fact, remain achievements that few anthropologists or linguists have equaled, and the Wichita and Caddo volumes stand as the only collections of the mythology of those peoples. That the two men did not complete the projects they started is a great loss to anthropology, but the preservation of their research materials has at least allowed future generations to benefit from their work.

Dorsey's Later Career

Dorsey's 1907 report to the director of the Field Museum suggests a sense of completeness to his Plains work. With the purchase of new cases it had been possible to install all the Pawnee and Arapaho collections in the exhibit halls. Two of the three miniature dioramas of Pawnee ceremonies—representing the Morning Star sacrifice, the Medicine Lodge, and the Thunder ritual—were completed, and the third was well underway. (They are still on display today.) Moreover, Simms had returned from a long trip to the Philippines, and now Dorsey was anxious for an extended trip abroad.¹⁰

From January to December 1908 Dorsey's travels took him from Chicago to New York, England, Germany, Egypt, India, Australia, New Guinea, the Philippines, China, Japan, Hawaii, San Francisco, and back to Chicago. He kept in touch with his family by writing a regular letter diary of his experiences, which, upon his return, he showed to the editors of the *Chicago Tribune*. "The object of my journey," he later wrote in the *Tribune*, "was to see several different types of human cultures and to ascertain at first hand the possibilities of ethnological investigation." Impressed with his 1908 journal, the newspaper decided to sponsor Dorsey on "a three-years' tour of the world studying matters of vital interest in the development of and physical destiny of mankind" (Dorsey, 1908–1912). Of particular relevance would be Dorsey's investigation of the sources of American immigration and of political conditions in various places throughout the world.

Dorsey was granted a leave of absence from the Field Museum and from his teaching positions at Northwestern University and the University of Chicago. In early July 1909 he sailed for Europe, arriving in Vienna before the end of the month. In the fall he moved on to Budapest. During 1910 he traveled around Hungary, Italy, and Austria, then went to Egypt and India. After spending the first six months of 1911 in India, he moved on to China. By January 1912 he was in Australia and continued his travels until returning to the United States in October.

In addition to his work as a newspaper correspondent,¹¹ Dorsey was director of the board of the United Photo Plays

Company, based in Chicago, which was exploring the educational uses of motion pictures. On some of his travels Dorsey was joined by photographers who took motion pictures in India, Japan, China, and other exotic locations; they were among the first travel movies ever made (Dorsey, undated memorandum to Stanley Field [1913?]; clipping from *Sigma Xi Quarterly*, May–Jun 1925, in Dorsey, 1925–1926).

Upon his return to Chicago, Dorsey resumed his position as curator at the Field Museum. He seems not to have continued any of his American Indian projects, however, and in August 1915 he resigned ([Laufer], 1915). For several years he was occupied with writing and documentary filmmaking; after World War I he worked with Naval Intelligence, holding the rank of lieutenant commander. In 1921 he moved to New York, where he served as a correspondent for the *London News*. Beginning in 1925 he also lectured at the New School for Social Research.

Just as early in his career at the Field Museum, from 1897 to 1907, Dorsey had embraced ethnography with unbounded energy and enthusiasm, later in his career he embraced popularizing anthropology with the same unflagging energy and zeal. In 1925 he published *Why We Behave like Human Beings*, which rapidly went through many printings and editions. The book offered an evolutionary approach to humanity that sharply differentiated biological from cultural and historical factors. Publicizing the book, the *Boston Sunday Post* (1925) quoted Dorsey as rejecting the significance of race: "There is no such thing as a superior race. We are all just human beings." Likewise, he poked fun at contemporary American gender roles: "The girl doesn't cry for a doll and the boy for a drum, or at least not until the parents have taught them to do it." Written in a brisk, staccato style—one review characterized it as "jazzy" (Dorsey, 1925–1926), befitting the jazz age—the book touched a chord with readers who found in it answers to common human questions.

During the 1920s Dorsey also published several other popular books. His writings culminated in *Man's Own Show: Civilization*, published in 1931. Dorsey had completed correcting the typescript of the final chapter only an hour before his sudden death from an embolism on 29 March 1931 (*New York Times*, 1931). Dorsey's last book is a massive work (nearly 1,000 pages) that examines the interplay of biological and cultural factors throughout history. Dorsey took the broadest possible view, building on the accumulated works of anthropology but taking them to a higher level of generality: "The anthropologist has been swamped in the attempt to find out and describe the vagaries of human culture. He could not see Man for the men, women, and children; he could not see Civilization for the manners and customs; he could not see Life for the skulls and souls" (Dorsey, 1931:xiii). In this work, the richly detailed American Indian ethnography to which he devoted the early part of his career received not even a passing mention.

Conclusion

During the decade that he worked on his Plains projects, Dorsey developed as an anthropologist. He began with an object-oriented approach reflecting the necessity of preparing anthropological exhibits in the Field Columbian Museum. Arranged by culture area and centered around life groups, the exhibits provided context largely based on adaptation to particular environments. Soon Dorsey came to appreciate cultural context and the importance of understanding the significance of objects, particularly those representative of religious ceremonies—an appreciation that led him to the study of social organization, mythology and other traditions, and to the documentation of ceremonies still practiced. Publication of the results of this research served as background for the Field Columbian Museum exhibits that depicted Sun Dance altars, bundle ceremonies, and men's society regalia. Then, in the final stage of his Plains studies, Dorsey came to appreciate the relative superficiality of ethnography written without an understanding of the underlying concepts embedded in the native languages. His year-long study of the Skiri Pawnee language introduced him to the methods and complexity of such work, and to its time-consuming nature.

At the same time, Dorsey pioneered many aspects of Plains Indian anthropology. He developed the first plan for the systematic study of Plains Indian religion and society, a plan that seems to have predated the similar one proposed by Boas for the American Museum and that was subsequently implemented by Clark Wissler. Field Columbian Museum exhibits led the way in combining life groups with exquisitely detailed miniature dioramas. Dorsey's insistence that everything collected should be displayed lent richness to the exhibits. In his publications on the Sun Dance, he set a new standard—one never met again—for detail of photographic illustration. His dependence on educated interpreters to collect and help him organize data was standard practice of the day, but his bringing ritual leaders to Chicago for intensive interviewing and sound recording, though doubtless inspired by Alice Fletcher's earlier work, nonetheless was innovative, practical, and effective. His sound recordings of Roaming Scout, which represent not just traditional narratives but a wide variety of genres, including autobiography, was entirely innovative and unmatched in his day, when most texts were written by interpreters or transcribed from dictation by anthropologists. Finally, his energy to undertake multiple projects simultaneously, all at the same level of intensity and personal involvement, was nothing less than remarkable.

Dorsey failed to complete all the projects he initiated with Plains Indians, but his accomplishments loom larger than his failures. When his remaining unpublished work is finally made available in well-annotated editions for critical study, Dorsey's place as the major contributor to Plains anthropology in the early twentieth century will be secure. If he had not attempted too much, Plains scholarship today would be the poorer for it.

Notes

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1. We have used the following sources for biographical material on Dorsey: Calhoun, 1991; *Chicago Daily Tribune*, 1931; Dorsey, 1908–1912, 1925–1926; Cole, 1931, 1944, 1952; Marquis, 1930; *New York Times*, 1931; and University of Chicago, 1914–1915. We have also cited unpublished correspondence. Correspondence and manuscripts not cited to another repository are archived in the correspondence files of the Department of Anthropology, Field Museum of Natural History, Chicago, Illinois. Other citations of correspondence use the following acronyms:

CIW-A File of correspondence with George A. Dorsey, Carnegie Institution of Washington Archives, Washington, D.C.

F-LFP Alice C. Fletcher and Francis La Flesche Papers, MS 4558, National Anthropological Archives, Smithsonian Institution, Washington, D.C.

UC-A President's Papers, University of Chicago Archives, Chicago, Illinois.

2. As secretary, Dorsey sent letters to some 40 leading anthropologists in April 1902, inviting them to become founding members of the new association and to attend the first AAA meeting, to be held in Pittsburgh in July 1902 in conjunction with the American Association for the Advancement of Science (AAAS) summer meeting (AAAS, 1901:355–357; Dorsey to Boas, 28 Apr 1902 [form letter]; Stocking, 1960:2).

3. For a fictionalized account of Dorsey's childhood, see Dorsey, 1917.

4. Phyllis Rabineau (1981:32–37) has presented a brief overview of Dorsey's collecting activities, and Douglas Cole (1985, chapter 7) has discussed Dorsey's Northwest Coast collecting trips in considerable detail. Disapproving of the museum collecting expeditions of that era, particularly the clandestine collection of skeletal material, Cole was very critical of Dorsey, whom he used as a foil for Boas. Cole characterized Dorsey's work as "superficial," and charged that the 1897 expedition had "the character of a fervid rip-and-run operation" (Cole, 1985:175). Although Cole's views are both biased and historically presentist, his negative portrayal of Dorsey has not been addressed in the literature on the history of anthropology.

5. Dorsey initially hired Warden for two months at \$40 per month plus \$20 per month for travel and expenses of securing information.

6. For commentary, see the introduction by Jeffrey Anderson in the 1997 reprint of Dorsey and Kroeber, 1903.

7. In an often-quoted reminiscence written in 1956, Kroeber recalled a conversation with Dorsey "in the Palace Hotel in San Francisco before the earthquake of 1906," during which Dorsey confided that, for all his trying, he was never able to get close to Boas and remained "wholly shut out." Kroeber added, "For those who remember Dorsey and his energy, self-reliance, competitiveness, and hard-boiled man-of-business manner, this will seem a strangely affecting confession" (Kroeber, 1956:156). Yet Boas's willingness to work with Dorsey privately in order to teach him basic linguistics suggests that Dorsey succeeded in developing the closer relationship with Boas that he desired.

8. American Indian Studies Research Institute, Indiana University, Bloomington, Indiana.

9. For further discussion of this collection, see the introduction by Douglas R. Parks in the 1997 reprint of Dorsey, 1906a.

10. How Dorsey's extensive travels affected his family life is a matter of speculation. In 1893, after returning from South America, he married Ida Chadsey. They had two children, Dorothy Ann and George Chadsey. In 1922 they divorced and in 1924 Dorsey married Sue McLellan.

11. Beginning in August, the *Chicago Tribune* published Dorsey's photo-illustrated 1908 "Diary of a 47,000 Mile Journey" in 74 installments; in December it began to publish his 1909 letters in daily columns.

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American Indian Migrations: A Neglected Dimension of Paleodemography

Dean R. Snow

“Migration” has been a taboo word in the archaeological lexicon for nearly half a century. Reasons for this were discussed at length by Anthony (1990) and Snow (1995), and some of them are quite sound. Yet the general avoidance of discussions of migration in recent decades has hampered our understanding of an important set of paleodemographic processes. This, in turn, has impeded realistic inferences about how human populations constituted themselves and how they deployed themselves on the landscape of the Americas before A.D. 1492; however, new interest in the set of processes covered by migration has arisen in recent years (e.g., Anthony, 1990).

The production of the *Handbook of North American Indians* (Sturtevant, 1978–2001) has played a part in this resurgence of interest. By including both ethnographic summaries and archaeological syntheses, the *Handbook* has reaffirmed the utility of connections between ethnology and archaeology, and it has reasserted the utility of the uniformitarian principle in anthropology. That is, that existing processes, or those of the recorded past, that effect cultural change can be expected to have effected similar culture change in the unrecorded past.

Human demography involves several processes, not the least of which is movement across space and change over time. Everything from small, impermanent relocations to large-scale permanent migrations occur frequently in human societies, and these can involve either individuals or groups of various sizes. Yet until recently, archaeologists have found it convenient to disregard precontact migrations of American Indian populations as an important dimension of adaptation and culture change.

Fuller understanding of why migration is important, why it has been neglected, and what kind of role it can play in future research requires a series of digressions. There are several historical and logical reasons for the current state of affairs, not all of which may be obvious. There are also several conditions attending future research of migration by archaeological means. Finally there are logical ramifications that have to do with our broader understanding of cultural evolution. I will address some of the more important of these digressions in turn.

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Migration in the Era of Naive Inference

Nineteenth century anthropologists generally assumed that American Indians had not been on the continent for very many centuries. Archaeology had not yet shown that human beings had been in North America since the Pleistocene, and it was not until nearly the middle of the twentieth century that this situation changed.

If American Indians had not been long in North America, then there was little reason to be concerned about how hundreds of very different American Indian societies had evolved from one or a few antecedents. Nineteenth century ethnologists were mostly interested in recording what they could of disappearing cultures. It was an era in which most culture change was perceived as loss. Acculturation was seen as the gradual erosion of coherent pristine cultures that had flourished immutably before the shadow of Europe fell across them.

Early archaeologists believed that American Indian cultures were diverse but were neither ancient nor mutable, and they arranged their inferences accordingly. If a local archaeological sequence indicated that change had taken place in the past, then the standard explanation was that cultural replacement had occurred, probably as an immigrating culture replaced the previous resident culture. If an archaeological culture bore no resemblance to any known North American tribe, then it had to be evidence of some extinct culture. In this way were born the Moundbuilders, presumed by many nineteenth century archaeologists to have been exterminated by the recent ancestors of Eastern Woodlands Indians (Silverberg, 1968). If a historical linguist could show a connection between two widely separated languages, then one could begin to speculate about the divergence of specific cultures and their movements into their historically known locations.

Thus, early in the twentieth century, migration was not merely a frequently referenced process in the study of culture change, it was often considered to be in itself an explanation. Human populations could be treated like chess pieces on the landscape and moved around to explicate migration scenarios, which in turn were supposed to account for culture change over time in local archaeological sequences. Migration scenarios abounded, but they were rarely connected to ecological or evolutionary principles, so few of them measured up to anything as lofty as explanation.

The Northern Iroquois case offers a convenient example. In 1916 Arthur Parker proposed that the Northern Iroquoians had originated through some unspecified process around the mouth of the Ohio River. He proposed a scenario that led to some groups crossing the Detroit River and moving northeastward through southern Ontario. Those that moved farthest east, to the vicinity of Montreal, were the Mohawks. Ancestral Onondagas soon moved across the Saint Lawrence to take up residence in Jefferson County, New York. The remainder of the northern branch became the ancestors of the Huron, Petun, and Neutral nations. He supposed that at the same time the ancestors of the Eries, Senecas, Cayugas, and Susquehannocks had moved along the southern shore of Lake Erie to their historic locations. Parker thought that the Iroquoians that Cartier visited along the St. Lawrence River in 1534–1541 were Mohawks. He ignored clear archaeological evidence for a Mohawk presence in the Mohawk Valley prior to the sixteenth century in order to argue that they moved there from the St. Lawrence after Cartier's visit. He appears to have given little or no thought to the processes by which Iroquois culture might have developed or to the probable time depth of those developments.

By 1940 undisciplined speculation about migrations was spinning out of control. Maps with broad arrows of hypothesized movements continued to be developed in ever more complex forms through the 1950s (e.g., Emerson, 1959). The speculative scenarios, however, were often untested and usually untestable, their validity resting on the perceived authority of their authors. Worse, they often ignored the findings of both ethnologists and linguists. Despite assertions about the unity of four-field anthropology, archaeologists often cleaved to their data alone, and interpreted them by their own lights.

Still, migration scenarios were consistent with contemporary Euro-American concepts of cultural development, known historic Indian migrations, and at least some of the details of Indian oral traditions. The problem was the assumption that the practice of working backwards in time from the perceived realities of recorded history would reveal an accurate portrait of prehistory.

Beginning around the middle of the twentieth century, many American archaeologists found good reason to reject the migration scenarios offered up by earlier students of American Indian societies. By that time most of the earlier scenarios were regarded as clearly simplistic in character and tainted as well by older notions about American Indian societies and the ways in which they changed. The fundamental misconception went all the way back to pre-Darwinian ideas about the fixity of species and races. Cultures, too, were regarded by many researchers as well-bounded and fixed in terms of their attributes. The work of Lewis Henry Morgan and other nineteenth-century cultural evolutionists might have led anthropological thought away from the fallacy, but here too there were errors. By encumbering the study of culture change with the ideas that all societies evolved through a common se-

ries of stages and (worse) that progress was an inherent part of the process, the evolutionists ignored some of Darwin's most profound insights and set themselves up to be discredited by twentieth-century anthropologists. Franz Boas led the discipline back to particularistic descriptive studies, and culture change quickly came to be couched in terms of acculturation. Kroeber (1948:425) defined acculturation as "those changes produced in a culture by the influence of another culture which result in an increased similarity of the two." This definition was sufficiently vague and lacking in specific mechanism to be simultaneously almost meaningless and very popular among anthropologists. Barnett (1953) later provided a mechanism derived from Gestalt psychology, but it was largely ignored. The outcome was that American anthropologists, who in the early twentieth century were studying mainly American Indian cultures, were impressed by the dominance of Euro-American culture in the acculturation process. Under the circumstances, American Indian cultures seemed to be doing most of the changing, and change for them was mainly a matter of cultural erosion in the face of the expanding dominant culture.

American archaeologists shared ideas, departments, and office space with cultural anthropologists whose main goals were to recover and describe American Indian cultures before they disintegrated completely. It was not difficult to think of precontact cultures that were by this time nostalgic memories as having once been coherent and vibrant societies. In the rush to record them before they disappeared there was not much interest in exploring how they got to be what they were in the first place. Culture change, after all, was most familiar in its more recent form, which was in turn a very sad story of loss and despair.

The Demobilization of Prehistory

By the middle of the twentieth century it was clear that American Indians had been in North America for a very long time, a dozen millennia rather than a few centuries. It was also clear that complex American Indian societies had evolved from simpler forms and that despite the efforts of diffusion buffs to prove otherwise, they had done so in almost complete isolation from the rest of the world. Finally, it was clear that the environments of North America had also changed dramatically over the course of recent millennia. There had been a long Archaic period between Paleo-Indians and the horticultural societies of late prehistory, and there had necessarily been long sequences of evolutionary change. That being the case, it was no longer possible to explain long archaeological sequences by reference to the migrations of unchanging cultures. The focus shifted to culture change and efforts to relate that change to environmental change.

James Griffin (1944, 1946), of the University of Michigan, was among the first to call for an end to the use of imaginary migrations as pseudo-explanations of the archaeological

record. Griffin and others sought to understand culture change over time, and the notions that prehistoric Indian societies were both largely unchanging and at the same time not stationary were of no help at all. The critics saw no objective means to choose between equally plausible (or implausible) scenarios.

This kind of criticism led archaeologists to focus the burden of proof on migration hypotheses, leaving presumed immobility (rather than simple uncertainty) as the default hypothesis. Griffin trained an entire generation of young archaeologists to think in terms of in-place culture change as cultural adaptation. Irving Rouse (1958) promoted the principle that migration had to be demonstrated case by case and that in the absence of evidence one should always assume that migration had not occurred. Rouse put the burden of proof on any migration hypothesis, insisting that unless certain criteria were met, one must assume that no migration occurred. The main points of his argument were that (1) an in-migrating group must be identified as an intrusive unit; (2) the unit must be traceable to a homeland; (3) it must be determined that all archaeological occurrences of the unit are contemporaneous; (4) favorable conditions for the migration must be identified; and (5) more economical hypotheses, such as trait diffusion, trade, or independent development, must be tested and rejected. The very rigorous nature of these criteria is an indication of the strength of the general reaction against freewheeling migration scenarios by the end of the 1950s. The change was a boon to archaeologists who had labored long and often fruitlessly to control three intractable variables: physical form, space, and time. By eliminating the spatial dimension in all but a few well-demonstrated cases, archaeologists could concentrate on examining variations in artifact forms over time.

The approach prevailed during the decades in which most regional sequences were constructed. Willey and Phillips (1958) codified the approach to American archaeology known as culture history in their well-known book on method and theory. Their framework provided a means of organizing archaeological evidence in local and regional sequences, which was increasingly important as radiocarbon dating emerged to supplement stratigraphy as a primary means of organizing the temporal dimension.

Variation of forms over time became a focus of archaeological attention. Some sequences, such as the main Southwestern traditions, were understood well enough to allow for explication of expansions and contractions in regional space as well, but most regional sequences were presented more simply, in familiar chronological charts like those scattered through Willey's 1966 book on the archaeology of North and Middle America. This approach to culture history came to be so well established that it even survived the revolution of the New Archaeology in the 1960s and 1970s. Yet the ploy of simply ignoring spatial movement has, as David Anthony (1990) put it, discarded the baby with the bath water.

The Conservatism of Processual Archaeology

The discussion of change over time was facilitated by the immobilization of the prehistoric societies being studied. Broadly speaking, the principal dimensions of archaeology are time, space, and form. If the researcher can hold one of these constant, then variation in another can be understood in terms of variation in the third. Freezing the spatial dimension by outlawing most purported migrations accomplished this, and archaeological attention came to focus on local sequences over time.

One can also freeze form in order to look at variation over space and time. Examination of the temporal and spatial variations associated with some specific artifact type is an example of this approach. Curiously, there was less interest in this kind of research at mid-century than there had been in earlier decades.

Looking at spatial distributions of forms at points in time effectively freezes time. This is usually done at the very large scales seen in historical atlases. The processual archaeology of the 1960s and 1970s built upon earlier culture historical approaches, notwithstanding protestations to the contrary from its principal proponents. Local and regional sequences were still necessary first steps. What was new was that archaeologists were concerned with understanding the mechanisms of change over time, not just documenting the key characteristics of that change. Yet attention remained focused on variation over time rather than space. The processual archaeologists, like the prehistoric culture historians, were not much interested in migration or in other kinds of large-scale movement over space.

Indeed, processual archaeologists in this era tended to regard human individuals and even societies as irrelevant to their investigations. The biological characteristics or language of a prehistoric community were not relevant to the processes being examined. Such things mattered little if the archaeologist was trying to understand how the group adapted to a particular environment or how it evolved when that environment changed. People are people, and one could randomly swap skin colors and language families without affecting the basic processes being examined. It should not be surprising that many archaeologists became ever more estranged from their colleagues in cultural anthropology, biological anthropology, and historical linguistics in the era of processual archaeology.

One major problem with this approach was that it denied the importance of information in the form of ideological traditions. The Norse died out in Greenland because they were stubborn Christians who refused to adopt the less civilized survival strategies of their Eskimo neighbors. They obstinately wore woolen clothing rather than fur, and for subsistence they depended on cattle and sheep, which were at their ecological limits even in good years. A few tough years drove them to extinction, whereas the Greenland Eskimos fared quite well. Had the Greenland Norse been able to adapt quickly, as processual archaeologists typically assume a population would do, and as they could have done had their ideology not constrained them, they would have survived.

Ideology counts, and it is sometimes necessary to understand a society's ideological underpinnings to understand why it succeeds or fails in an adaptive crisis. I have found, for example, that it is impossible to understand the adaptational choices made by prehistoric Northern Iroquoians without also understanding their cultural trajectory, the path of allowable change set by prior adaptive successes (Snow, 1994). Adaptive choices made a thousand years ago conditioned others made centuries later, and all of them conditioned the characteristics of historic Northern Iroquoian societies. Thus a knowledge of Northern Iroquoian ethnohistory and archaeology serves an archaeologist better than a knowledge of their archaeology alone.

Much the same can be said about European archaeology and the spread of Indo-European languages. Twenty years ago a processual archaeologist might have argued that the spread of Indo-European languages was a matter of some small peripheral interest but was fundamentally irrelevant to the study of European archaeology over the last several millennia. It is now clear that this is not the case. Renfrew (1987; cf. Mallory, 1989) is almost certainly wrong in arguing that Indo-European languages spread into Europe with Neolithic agriculture. It is very likely that the Indo-European languages spread into Europe after 2000 B.C., carried by mobile equestrians who found it possible to dominate the indigenous agricultural societies (Anthony, 1995). The work of Sokal and others (Sokal et al., 1991; Sokal et al., 1992) has shown that the genetic patterns of Europe have been in place since around 6000 B.C., long before the Indo-European languages could have spread westward, given the archaeological and linguistic evidence presented by Mallory (1989) and Anthony (1995). Therefore, the Indo-European languages must have been established by minority dominant elites after the genetic patterns of Europeans had been largely established, a process that has many historically documented analogues. The case illustrates how archaeology, biological anthropology, and historical linguistics can work together to develop a more complete understanding of a complex process. It also illustrates that it is a mistake to assume a priori that migration is a demographic process that one need not worry about in processual studies (Anthony, 1990). Knowing what we know now about the Indo-European case, it would be fatuous to think that we could understand the processes of European prehistory over the last 10,000 years without reference to human migration.

The Implications of Eastern Woodlands Archaeological Data

The steady accumulation of new data from the Eastern Woodlands has enabled archaeologists to see the patterns of prehistory with increasing clarity. Contract archaeology has in the last 20 years led to surveys of areas seldom investigated in earlier decades. As a result of that work and of the continuing efforts of academic archaeologists, we now have a much clearer understanding of where human populations were concentrated

at various times in the past. Just as important, we have new understandings of where they were absent.

New computer technology, particularly geographic information systems, has allowed researchers to control masses of data as never before and to see both archaeological and environmental patterns at very large scales. David Anderson (1991) has published a series of regional maps showing the distributions of human populations in the Eastern Woodlands in A.D. 900–1100, 1250–1300, 1400–1450, and 1540, as revealed by clusters of archaeological sites. Although data are not included for New England or for some periods in New York, a general Eastern Woodlands pattern is apparent. The home territories of the populations show spotty distributions at each time level. Although it can reasonably be assumed that intervening areas were used for travel and hunting, they were for the most part buffer zones separating population concentrations. Figure 1, which is simplified from Anderson (1991:12–15, figs. 2–5), shows that at each time level only a portion of the whole was made up of home territorial ranges. The spotty patterns in Figure 1 do not correspond well to the impressions of many researchers familiar with the Eastern Woodlands, and that is an important point. Maps compiled to show the distributions of archaeological cultures traditionally have been drawn impressionistically, with poorly known areas often assumed to have been occupied, rather than unoccupied. Anderson's maps are based on a more tightly controlled assessment of archaeological data, including negative data resulting from recent surveys. Whether or not they are correct in every detail, they show a much more patchy distribution of archaeological cultures at all time levels than we have customarily assumed in the recent past.

More important for present purposes, the locations of population concentrations clearly shifted over time. Anderson (1994) has examined this phenomenon at a smaller scale for the period A.D. 1000–1600, showing that the Savannah River basin was occupied in some centuries but essentially was vacant in others. My own work has done the same thing for Northern Iroquoia. Figure 2 shows nearly all of Northern Iroquoia in the same four periods used by Anderson. Each site is represented by a point with a buffer radius of 10 km. Non-Iroquoian site data points are not mapped, but there is good reason to believe that the Iroquoian data are complete or nearly so. Even without site data on adjoining societies, it is clear that there were large buffer zones between villages and village clusters. Even if one increases the sizes of buffers in order to accommodate assumptions about traditional hunting ranges around settlements, large buffer zones remain between village clusters.

Figure 2 provides more detail at a larger scale than does Figure 1. It is clear from both, however, that areas that were buffer zones in one period were sometimes occupied in others, and vice versa. In other words, we cannot account for change over time in the Eastern Woodlands without reference to migration. People must have been moving if so many areas were devoid



FIGURE 1.—Distribution of home territories of eastern North American societies during four time periods (after Anderson, 1991, figs. 2–5).

of population during some time periods but were occupied in others.

If one adds historical linguistics to the picture the argument becomes even more compelling. It was once widely believed that we would eventually be able to match most prehistoric cultures in the Eastern Woodlands to historic languages or to their

reconstructible ancestral protolanguages. Linguists repeatedly attempted to reduce hundreds of North American languages to a handful of families, but hypothesized ancient connections, such as the proposed link between Algonquian-Ritwan and Muskogean, were subsequently abandoned for lack of compelling evidence. Most historical linguists now take the more con-

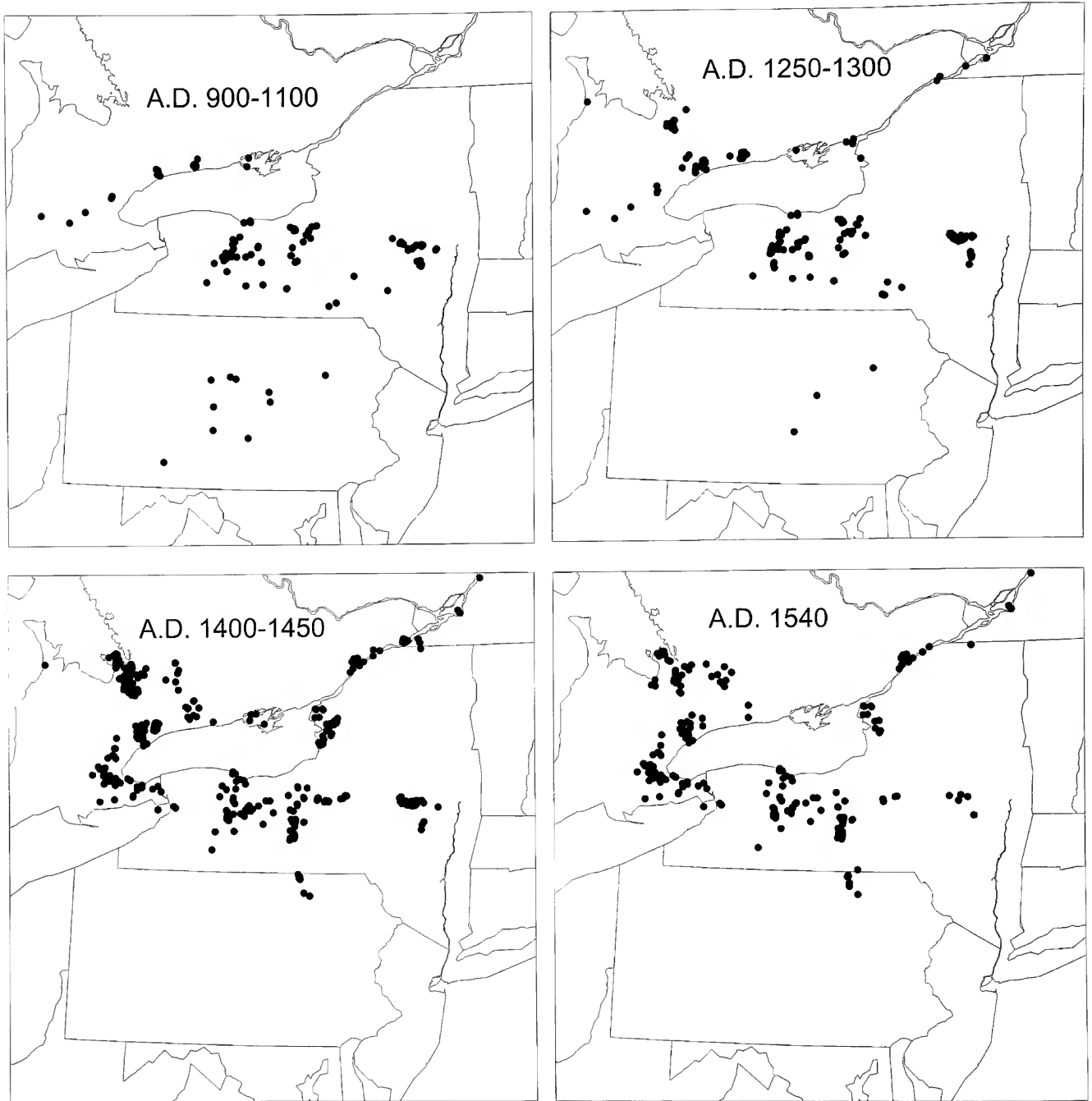


FIGURE 2.—Distribution of home territories of Northern Iroquoian societies during the four time periods examined for Eastern Woodlands human populations by Anderson, 1991.

servative view that we cannot demonstrate the existence of fewer than 21 language families in aboriginal North America (Coe et al., 1986:42–45). Moreover, the reconstructible proto-languages from which each of those families descended appear now to have been spoken within the most recent few thousand years. As archaeology has pushed the earliest appearance of

people in North America further and further back into the late Pleistocene it has become increasingly clear that we cannot hope to match more than a small, recent fraction of prehistory to known or reconstructible languages.

In other words, there must have been many language extinctions in the past in North America. Leaving aside Eskimo-

Aleut and Athapaskan languages, it can be reasonably conceded that all others probably descend from one or a few original languages. The earliest protolanguages were spoken so long ago, however, that linguists may never be able to reconstruct them. More important for present purposes is that the landscape of North American Indian languages, when mapped by language families, must represent the completion of developmental processes that were played out over just the most recent millennia. Earlier processes cannot be reconstructed on the basis of linguistic evidence from recorded languages. We cannot reasonably expect to ever be able to map the distributions of languages spoken around, say, 5000 B.C. in North America.

The known distribution of North American Indian languages suggests that the speakers of many of them must have migrated within the last two or three millennia prior to Columbus. For example, the Cherokee are sufficiently removed from the Northern Iroquoians to indicate that somebody must have moved in the past. The Athapaskan-speaking Navajos and Apaches clearly migrated to the Southwest from western Canada. The Siouan Catawbas and Tutelos of the Southeast must represent some migratory separation from the main block of Siouans in the middle of the continent. The Algonquian Cheyennes and Arapahos (not to mention the California Algonquians) must have migrated westward from the main block of Algonquian languages to the north and east.

There are many more examples, but the point is that like the archaeological map, the language map of the Eastern Woodlands, and indeed of North America as a whole, makes the inference of multiple pre-Columbian migrations unavoidable. We know from documentary history that many migrations took place over the last five centuries as well, but we can no longer justify treating them as special cases related to the disruptions of European contact. As uncomfortable as this may make archaeologists who have grown accustomed to ignoring population movements, it is now clear that simple uniformitarianism compels us to accept that migration was a common demographic process in prehistory.

This evidence is supported in a general way by migration stories preserved by many American Indian societies as parts of their oral traditions. Migration legends exist in oral tradition for reasons other than providing data for modern archaeologists and historians, so it is often hazardous to take them too literally. They must be tested against other lines of evidence, and more than one archaeologist has been embarrassed by a failure to carry out such testing and by taking migration legends at face value. Indeed, such scholarly embarrassments are what led many archaeologists to turn their backs on the investigation of migration earlier in this century. Yet in a general way the number of migration legends alone should be enough to convince us that American Indian populations were fluid on the continental landscape over the long term.

The situation is similar to, and indeed related to, the issue of warfare in prehistory. Although romantics may continue to insist, with no evidence whatsoever, that nonwestern peoples

lived in pristine bliss until contacted by expanding capitalist systems, Keeley (1996) has shown this view to be nonsense. Warfare, too, has been common throughout human history and prehistory. I have no doubt that warfare and migration were often related phenomena in prehistory, just as they have been in history.

The *Handbook of North American Indians* and most ethnographic atlases (e.g., Coe et al., 1986) traditionally show North American Indian tribal (or national) territories as contiguous, their boundaries coterminous. The practice is rational, for one cannot calculate meaningful regional population densities unless all territory is accounted for by one tribal territory or another. But the practice also fails to account realistically for buffer zones and vacant quarters. As archaeological coverage improves and we can say with increasing assurance that population distributions were indeed as spotty as Anderson (1991) shows them to have been, our maps of native America should come to look less like those of European nation states and more like those shown in Figures 1 and 2.

Implications for Cultural Evolution

Diffusion is the transfer of traits across societal boundaries in the absence of migration. This definition includes transculturation, which is trait exchange without the loss of identity, a special form of diffusion (Rouse, 1986:11). It also includes acculturation, which is another special form of diffusion in which a society gradually loses its unique identity through the replacement of endogenous traits by exogenous traits (Rouse, 1986:12–13).

As with migration, these processes involve individuals and various groupings of individuals. I have found that the culture concept is not particularly useful except as a broad covering term for all of these processes. "Society" is a more useful term, for it has a clear dictionary definition as an enduring and cooperating social group whose members have developed organized patterns of relationships through interaction with one another. I define a national society as one constituted by people that share the same cultural norms and have a self-recognized corporate identity. Members of a national society typically share a single common language (whether or not it is also spoken by one or more other national societies) and are autonomous within a well-defined territory. Societies can also be subsets of national societies, such as voluntary associations of individuals for common ends, or a part of a community that is a social unit distinguishable by particular aims or standards of living or conduct. Other complementary definitions are possible, but in all cases the emphasis is on societies as sets (or subsets) of individuals. As Pearce (1996:55) put it, "Societies are people, and culture has no significance unless viewed as something participated in, shared, and transformed by groups of people."

From all of this it should be apparent that a basic assumption is that individuals are the units of transformation (Snow, 1996a). The process of selection is transformational at, but not

above, that level. Moreover, selection operates simultaneously at multiple levels (or scales) in the sense that nested corporate groups may grow as individuals are born or recruited into them, or they may be driven to extinction as individuals die or leave them. Selection also operates at the level of the individual in biological evolution, but in that case the traits of the individual are locked in its genetic code, and the individual does not have the option of changing its genetic code or moving to the breeding population of another species.

A basic assumption is that societies (broadly defined) can propagate or dissolve rapidly as individuals join or abandon them. For individuals, however, several forms of migration entail the abandonment of some or all social memberships and the acquisition of new ones. New societies form as individuals and the cultural elements they carry come together in new, adaptive ways. This is the process of ethnogenesis discussed by Moore (1994), who shows that it is a model that is much superior to any that assumes that human cultures develop in ways parallel to biological speciation. Not surprisingly, one of the best examples of this process comes from Moore's (1996:13–29) own work on the Cheyenne. The Cheyenne reinvented themselves several times as groups merged and split and merged again, at the same time both migrating and taking up new subsistence adaptations. In the seventeenth century they were wild rice gatherers living among the lakes of northern Minnesota. In the eighteenth century they were maize horticulturists living along the prairie rivers of southern Minnesota and the Dakotas. In the nineteenth century they were mounted nomadic buffalo hunters living on the plains of South Dakota and Wyoming.

From this and many other examples it is clear that ethnogenesis and migration are more often than not tightly bound up with each other. The Cheyenne case is probably not unusual. The complex patterns we see in the distributions of languages, tribes, and archaeological cultures in North America result from the complex interactions of these processes, not some

simpler process that we might all prefer. I have argued that these complex processes account for the appearance of Iroquoian peoples in the Northeast around A.D. 900 (Snow, 1995, 1996b). The founding population was probably small. More than that, it was probably made up of migrating people carrying a new innovative adaptation that was a departure from that of the population from which they were derived. The founders of later Northern Iroquoian nations may have retained only a modified version of the donor culture.

What archaeology can gain from or contribute to all of this remains to be seen. The Cheyenne of 1800 did not look much like their ancestors of only a century earlier, and archaeologists will probably have some difficulty demonstrating the connection on artifactual grounds alone. The Northern Iroquoians of a millennium ago may not have resembled their immediate ancestors any more closely in archaeological terms, but we are able to see that heritage through osteological evidence (Langdon, 1995), and it is possible that it would be detectable through molecular genetics as well. I judge that we have entered a period that promises (or at least will allow) a reintegration of ethnology, history, historical linguistics, biological anthropology, and archaeology. Great progress is currently being made in the study of the European Dark Ages, mainly through the integrated use of documentary sources, comparative linguistics, molecular genetics, osteology, and archaeology. We can hope for similar progress in North America.

Uniformitarian principles do indeed apply to anthropology. The processes of the recent past are in fact keys to the understanding of the processes of the more remote unrecorded past. Ironically, while recent history has shown that archaeology can go it alone politically, it cannot do so intellectually. The good news is that having demonstrated that migration is a nearly constant feature of human societies and its subsets, we have reason to hope that the subdisciplines of anthropology will find their way back to each other.

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From Ethnohistory to Anthropological History

Shepard Krech III

In a career spanning five decades, William Sturtevant has focused his scholarly attention on diverse aspects of American Indian culture and history. His contribution to a more profound understanding of American Indians has been especially notable in the area commonly known as ethnohistory. Here his work is distinguished by a willingness to consider as historical evidence artifacts, visual imagery, and reconstructions of the past derived from both native testimony and the often oblique and layered texts written by nonnatives.¹

The essay having the longest-lasting impact is “Anthropology, History, and Ethnohistory,” which appeared in two versions in the late 1960s, first in 1967 in *Ethnohistory* and then two years later in an edited volume serving as a general introduction to cultural anthropology (Sturtevant, 1967a, 1968a). A discussion of “the intersection of anthropology with history” (Sturtevant, 1967a:1), this essay ranged broadly, methodologically and theoretically, through the shared interests of anthropologists and historians and their convergence in ethnohistory; the critical use of written sources; the relativity of historiography, folk history, and oral tradition; and the relevance of biology, linguistics, and archaeology to historical work. An important statement at the time on ethnohistory, Sturtevant’s essay serves as a point of departure for my thoughts herein on more recent work in ethnohistory—or, as I prefer, anthropological history—that has a bearing on the analysis of the native North American past. In the remarks that follow, which extend far beyond North America to consider contemporary historiographical issues, I draw on several recent works (Krech, 1991, 1997a, 1997b).

In considering ethnohistory, or anthropological history, it is important to remember that in the decades before Sturtevant’s essay, many influential anthropologists kept their distance from history. In 1915 Robert Lowie decided that one cannot “attach to oral traditions any value whatsoever under any circumstances whatsoever” because “we cannot know them to be true” (Lowie, 1915:598), and in the next decade, A.R. Radcliffe-Brown (1958:40) argued that history, for the most part, “does not really explain anything at all.” Radcliffe-Brown’s objectives were no less than a science of society, and in his view, historical description simply did not add up to explanation.

Historians were no better about anthropology, and for years they maintained healthy skepticism about anything but narrative, event-based histories in which indigenous people played minor, passive roles—if they figured at all. Some historians seemed determined to dig in and resist the idea that the majority of people on earth had their own histories. The most infamous example—within a year of Sturtevant’s opposite sentiments—was Trevor-Roper’s (1965:9) startlingly ethnocentric remark that in Africa, only the history of Europeans was worthwhile—“the rest is largely darkness, like the history of pre-European, pre-Columbian America. And darkness is not a subject for history.”

Statements like these came from mainstream history as long as it remained uninfluenced by anthropology. And in fact the chasm between the disciplines remained wide until recently. Most twentieth-century historians have been historicists, the intellectual descendants of nineteenth-century idealists who argued so strenuously with the positivists of the day. They have sought to understand an age in its own terms, but they have given practical priority to politics and great men and have produced idiographic, unique-event-based, particularist, atheoretical, intuitive, factual and truthful narrative histories (Barraclough, 1979). Despite their separate and specific national-based interests, and despite their flirtation with historical issues at several distinct moments, ethnology and cultural and social anthropology have together had a different course from history, and they have a shared interest in theory and the implications of comparative analysis (Keesing, 1974; Krech, 1997a).

Decades ago, however, American anthropologists interested in acculturation and ethnohistory and British social anthropologists interested in history and social change began to build bridges to history. Their effort was not one-sided, however, as certain French historians reacted in especially forceful ways to historicism. First came arguments that history was scientific not intuitive, comparative not particularist, analytical not narrative, and theoretical not atheoretical—all sounding very much like anthropology of the day. Some started to emphasize that history, rather than being confined to the politics of a few great men or to discrete political events or wars, was, instead, the story of all people in society; and some saw history as rooted in long-term geographic and climatological structures as well as in shorter-term economic and demographic changes. In recent years, various brands of symbolic anthropology have flourished in French history, and today a symbolic or interpretive

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approach rules in so-called cultural history (Stone, 1977; Barracough, 1979; Kammen, 1980; Hunt, 1989; Krech, 1991; Krech, 1997a).

These trends affected historical writing worldwide. At the time, they were not without influential critics in history, who correctly pointed out that “every social science is a rapidly moving frontier” (Stone, 1977:19) difficult to keep abreast of. Despite such criticism, the convergence between anthropology and history has been pervasive. Once widely made, the argument that history is atheoretical, particularist, idiographic, and moralistic and that anthropology is theoretical, generalizing, nomothetic, and value-free can no longer be sustained. The idea that scholarly inquiry can proceed without theory, or that theory can be opposed to ideology, today seems naive. Once, anthropology focused mainly on exotic people, and history, in contrast, dealt primarily with the nonexotic west. Today, as daily the people among whom they traditionally worked become less exotic and remote, anthropologists conduct ethnographic work throughout the world, including the urban west. Concurrently, historians are interested in popular culture and history from the bottom up or from below; Bernard Cohn (1980:214) once wickedly called this “proctological history.” Rigidly separate at one time, anthropology and history are no longer. In anthropological theory, history qualifies as a “key symbol” of work in the years since Sturtevant’s influential essay appeared, and in the last decade alone literally hundreds of anthropological works incorporating history have appeared (Ortner, 1984:158).

These new directions changed ethnohistory. When the label first came into use—decades before Sturtevant’s essay—ethnohistory was the history of indigenous, usually small-scale, non-European societies, and its sources were mainly documents. The American Society for Ethnohistory embraced this understanding of ethnohistory, as did the journal *Ethnohistory*, which in 1955 devoted itself to research on “the documentary history of the culture and movements of primitive peoples, with special emphasis on the American Indian.” By “primitive” was meant small-scale societies. “Documents” proclaimed the privileged form of evidence. In this journal (and elsewhere), ethnohistory meant using documentary sources to talk about the past of the Miami, Shawnee, and other societies located mainly in North America. That ethnohistorians preferred the writings of literate nonnatives over oral, indigenous sources was due not to slavish adherence to Lowie’s earlier positions but to the fact that in the 1950s many ethnohistorians conducted research in the formal, legal context of land-rights cases before the United States Indian Claims Commission (Krech, 1991; Tanner, 1991).

Sturtevant (1967a, 1968a) was among the many who sought to define ethnohistory in the 1960s and 1970s, decades when interest in American Indians built steadily. For him, ethnohistory was “(the study of) the history of the peoples normally studied by anthropologists” (Sturtevant, 1967a:6–7). Many scholars stressed the catholic use of data obtained in the field, archive, and museum in order to write what John Ewers

(1961:268) called “a thorough, delicately balanced tribal history” and what Wilcomb Washburn (1961:45) called “history in the round”; or, as James Axtell (1979:2) put it, in order to “gain knowledge of the nature and causes of change in a culture.” From history, some said, came “cautious accuracy,” and from anthropology came “imaginative theorization” (Washburn, 1961:45) as well as culture “defined by ethnological concepts and categories” (Axtell, 1979:2). Ideally, these qualities fused in the ethnohistorian. For Nancy Lurie (1961:90) it was important that the ethnohistorian take proper precautions against the antiquarian virus and seek “valid cultural and social generalizations, ‘laws’ if you will.” The principal products of ethnohistory were historical ethnography, or synchronic reconstruction of a culture or society at some past moment; folk history, or historiography in a nonliterate society; and specific history, or diachronic ethnohistorical study (including the archaeologist’s direct historical approach) achieved by working (and projecting) “upstream” from the present to the past or “downstream” from the earliest to the most recent time period (Hudson, 1967; Sturtevant, 1967a, 1968a; Carmack, 1972).

Today, in an era caught up by postmodernism, the picture is quite different than it was 20 and 30 years ago. One problem is that what was conventional then in anthropology and history is no longer conventional today; in a nutshell, the convergence of the two disciplines has caught up to ethnohistory. The result is disagreement over everything other than that ethnohistory is a method, not a discipline (Trigger, 1982, 1986; Simmons, 1988; Krech, 1991). For example, when one compares historiography on different continents, it is quickly apparent that one person’s ethnohistory is another’s history, social history, cultural history, or ethno-ethnohistory. In Africa, history is rarely called ethnohistory, perhaps because decades ago Jan Vansina (1960:53) stated that “history in illiterate societies is not different from the pursuit of the past in literate ones....And there is therefore no need to coin a special term, such as ethnohistory just for this reason.” Writing about New Guinea, Edward Schieffelin and Deborah Gewertz (1985:3) expressed similar reservations, arguing that to qualify as such, ethnohistory “must fundamentally take into account the people’s own sense of how events are constituted, and their ways of culturally constructing the past.”

Today, much self-described ethnohistory, especially in North America, is chronological narrative history of a kind now considered old-fashioned by many scholars influenced by postmodernism. What distinguishes a work as ethnohistorical rather than merely historical? Typically, it is only the focus on the Cherokee or another ethnos, or on a region, in which anthropologists traditionally have had stated vested interests. But some find fault in the concept of ethnos. They argue that ethnohistory has still not shaken its tribal referent—for classical Greeks, ethnic features were barbarian (Chapman et al., 1989)—and that it is exclusionary. Why is the history of small-scale indigenous ethnic groups ethnohistory, not history? Why is the history of minority groups usually history, not ethnohistory? And why is the history of people like the Germans or English

always history and never ethnohistory? Henri Brunschwig and Hubert Deschamps, both writing about Africa, were the first to forcefully raise this problem. In a searing critique, Brunschwig (1965:291, 300; my translation), likening ethnohistory to a “weed,” denied that there were (as others would have it) “people without history,” and posed the unsettling question, “Is it then ‘people without written history’ that is meant by ‘people without history’, and will Blacks, wounded by this title, be relieved to be named people with ethnohistory?” Deschamps’s (1968:1434; my translation) answer: reserving ethnohistory for people considered “primitive” would be “resented as an unjust discrimination by Africans.”

Similar voices have also been raised in North America: Bruce Trigger (1984:22) has wondered if ethnohistory was not “patronizing,” and James Merrell (1989b:115) has wondered if it was not “pernicious,” both for essentially the same reasons articulated by Brunschwig and Deschamps. This problem is not trivial. Will ethnohistory ever consistently or logically be applied to western, urban, history-possessing ethnic groups, or will it forever be restricted to particular groups like American Indians? One would have to be unconcerned with the politics of historiography or with moral criticism to ignore the problem (Krech, 1991).

One way out of this dilemma is to extend the label “ethnohistory” to any ethnic-group focused history, but this reduces ethnohistory to history and most historians would probably not go along with it. A second way is to make ethnohistory a cognate of other ethno-terms, such as ethnobotany or ethnoscience, and reserve it for indigenous historiography, but this would shut out many self-proclaimed ethnohistorians. A third way is to use some other label for what amounts to the combination of method and theory current in history and anthropology, and the focus on history or historiography in or of some ethnic group. Anthropological history or historical anthropology, both of which remove the stigma of ethnos, would suffice. Which discipline is nominal and which is dependent might well be a matter of taste. I prefer anthropological history because, as with social or cultural history (or ethnohistory), it is history that is being written (Krech, 1991, 1997b).

To generalize about the directions that the anthropological history of native North America has taken in recent years, one can refer to the same types, or genres, identified by Sturtevant and others several decades ago: historical ethnography, or the reconstruction of a culture or society at some past time; folk history, or historiography in a nonliterate society; and the historical study of a group written either from past to present or, rarely, from present to past. Most common—the genre is robust—is the last mentioned, which may be narrowly focused in time and geography or may unfold through biography. Typically, it takes the form of a narrative. Often a straightforward and chronological narrative that presents the facts, it offers no pretentious engagement with theory and is seldom complicated by theoretical musings. This is not the same as saying that it lacks theoretical assumptions, but these assumptions are

almost always implicit. The aim of the historical narrative is to chronicle a people’s (tribe’s, nation’s) past, or part of that past. Interethnic relations, particularly relations between Indians and whites, are often detailed, and an abiding, if implicit, sense of a frontier is characteristic. More often than not, the narrative is based on archives rather than on oral testimony, and on the assumption that an author can authoritatively “release” a descriptive chronological narrative from documents. It often displays sensitivity to native culture and society but not necessarily to native perspective. It sometimes emphasizes the cultural distinctiveness of historical tradition or, drawing on oral and documentary evidence, details a people’s history but not historiography.

Driven by data, not theory, the historical narrative is written by anthropologists and historians and is considered a contribution to American Indian history, studies, or ethnohistory. The best of this category is based on comprehensive anthropological and historical research. It is also marked by strong narrative style as well as by judicious reflection upon and careful weighing and interpretation of facts, not simply their presentation. Recent examples—there are many, written by anthropologists and historians alike (see Krech, 1991)—include James Merrell’s (1989a) *The Indians’ New World: Catawbas and Their Neighbors from European Contact Through the Era of Removal*, which is a sensitive exploration of the convergence of Catawba and white culture and society. Others include James Axtell’s (1981, 1985) essays, especially *The Invasion Within*, a sustained inquiry into what he calls contests of cultures.

Because the focus of most narrative-based specific histories of native America is on presenting the facts rather than on articulating the theory in the light of which certain events or data are selected as historical fact, many theorists influenced by postmodernism consider such histories problematic. Some anthropological historians prefer instead to engage openly with theory. In their histories the analysis of culture or of native American-European immigrant relations is explicit and is informed by a strong, clearly articulated interest in theoretical issues. In today’s postmodern climate, theory in anthropological history derives not just from the two nominative disciplines but from cultural studies, literary criticism, political economy, and other fields. Today no single paradigm holds sway unless it be a set of general understandings about postmodernism. More than ever, anthropology and history are open to influence from outside, but as in the past, discussions rage over “what constitutes the grounds of credibility for the community” (Krieger, 1989:x). As others have remarked, the major debate has been the age-old Manichean one over what is variously called, in anthropology and history, positivism and idealism, rationalism and historicism, or objectivity and subjectivity. In both disciplines each of the sides is complex. Historical objectivity, for example, “is not a single idea, but rather a sprawling collection of assumptions, attitudes, aspirations, and antipathies” (Novick, 1988:1). Recent fragmentations in anthropology are so sharp that anthropologists are sometimes far better at linking their

theoretical interests to nonanthropologists than to each other. Theoretical perspectives may range, crudely speaking, from an extreme world-systemic positivism to an extreme perspectival relativism; but in their nuances they also belie the notion that they must be one or the other, positivist or idealist, objective or subjective (Ortner, 1984; Weiner, 1995).

The effect of these intellectual trends on anthropological history is seen in analyses informed by a clearly articulated interest in theoretical issues. Some are founded on staunchly positivist interests rooted in ecology, economy, and political economy and investigate mercantilism, the market economy, the fur trade, the world-system, control of productive resources, underdevelopment, internal colonialism, dependency, ecology, demography, modes and relations of production, and commoditization. Many authors are sympathetic with the anthropologist Eric Wolf's emphasis on the analysis of processes linking societies to each other within a world-system. Wolf (1982) spoke more compellingly than most about what he called "a manifold of social and cultural processes at work," or the "fields of force" and "chains of causation and consequence," which in the past have affected or embraced societies, and continue to do so today.

These works ultimately derive their intellectual inspiration from Karl Marx and owe much to critical theoretical engagement with not only Wolf but with Immanuel Wallerstein and Andre Gunder Frank, among others, who represent a variety of schools, strands, and strains of Marxian analysis. The extent to which local culture mediates or structures outside forces or to which human agency overrides systemic power varies in part with how much each is read into the Marxian agenda and its interpolations, in which the importance ascribed to culture and the individual as well as the articulation of idealism with materialism have altered through time. For native North America these ideas have appeared prominently in sociological histories like *Social Change in the Southwest* (Hall, 1989), a sweeping analysis of economic transformations, variations in incorporation and types of periphery, and the ways that global, regional, and local changes are mutually determinative in the American Southwest; and in *The Roots of Dependency* (White, 1983), which is a comparative analysis of the impact of market economies linked to Europe on traditional native American economies, and the subsequent development of dependency of the Choctaw, Pawnee, and Navajo. Anthropologists and historians contribute in roughly equal numbers to these and other analyses. The degree to which they engage explicitly with theory remains a matter of disciplinary preference. Anthropologists tend to be self-conscious in their comparative and explicit analytical interests, whereas historians tend to privilege narrative and embed analysis in text and notes to keep it from interfering with the telling of the story (Krech, 1991).

Other theoretically explicit anthropological historians have gone in directions more compatible with the antiessentialist aims of postmodernists who seek to locate meanings in texts produced by individual authors; they have gone toward culture

and agency. The interest in agency stems from dissatisfaction with political economic schemes that regard emergent relations between center and periphery as unidirectional and overly systemic; as Ortner (1984:143) has remarked, history does not simply "arrive like a ship." The force fields so exhaustively explored by Wolf (and others) meaningfully expose technoeconomic change, political economy, and political and economic differentiations as functions of capitalist development. But histories of the impact of west on nonwest, center on periphery, or metropolis on satellite are ultimately unsatisfactory. Resistance and other aspects of the local scene have always affected capital expansion. To write histories of the nonwest, historians and anthropologists, taking cues from a number of thinkers, have put indigenous people as active agents into the histories they help make. Worldwide, anthropological historians explore not just oppositions to colonial relationships, but agency and how institutions and cultures are "created, sustained, and displaced" (Worsley, 1984:174; see also Asad, 1987; Roseberry, 1989). Individuals are returned to center stage as culture-constructing makers of history or as producers of structured texts whose metaphors and tropes need to be read. Interests shift to individuals and past cultural worlds and to the structure of history and/or the historiographical conventions of indigenous historians. To a degree perspectival, they evoke the tension between anthropological history as indigenous historiography and as a more or less conventionally (viz., in a positivist sense) conceived history of an ethnos (for discussion, see Krech, 1991).

Outside native North America this type of anthropological history has been actively pursued by Renato Rosaldo, Richard Price, Marshall Sahlins, and others. Rosaldo's (1980a, 1980b) analysis of Ilongot historical consciousness and how the past is embedded in ethnogeographical knowledge had an early influence on cultural approaches to history. Price's (1983, 1990) analysis of Saramaka historical thought is a perceptive account of the presentist, perspectival, guarded, dangerous fragments Saramakas possess about the past. The Saramaka preserve and encapsulate their past in different rhetorical genres, which Price used in conjunction with other forms of evidence, all of which he manipulated with different typefaces to preserve different voices, in order to image various Saramaka pasts. In provocative but debated work on the encounter of Europeans and native people in Hawaii, Sahlins (1981, 1985, 1995; see also Obeyesekere, 1992) has influenced historiography more than Price and Rosaldo have. He has forced extensive discussion and debate with his emphasis on the conflicting interests, interpretations, and schemes brought to particular structures of conjunctures (like the death of Captain Cook); his forceful analysis of the cultural structure of Polynesian societies; and in the idea that cultural structure is either open or closed to history and that each orders the other.

Worldwide, the analysis of a people's historical thought or historiography is common. This literature raises numerous issues having to do with indigenous historiography, conceptions of time, inventions of culture, ideological hegemony, relations

between history and self-consciousness, the cultural specificity of ways of making history, and the cultural and historical contexts of presentist histories—whether in the insular Pacific, colonial India, or nineteenth-century European intellectual circles. Related issues are how to understand the production of hegemonic discourse and how to capture an authentic native voice in texts reflecting ambivalent colonial relations. Debates occur over authenticity and over what constitutes history. The perspectival nature of cultural interpretations is often stressed (e.g., White, 1973; Prakash, 1990).

Many of the works that explore culture and agency are deeply reflexive and return us importantly to one of Sturtevant's early initiatives. In an influential essay on narratives of native North America, Edward Bruner (1986) spoke of narratives that have dominated whole eras to meaningfully and powerfully structure the telling of American Indian history. One of the first tasks of the anthropological historian is to develop awareness of these narratives as well as of one's own. One must first appreciate the different ways that people "imagine the past" in order to decipher historical thought (Breen, 1989). Others have arrived at similar conclusions, expressed, for example, as the importance of understanding how history "is both a metaphor of the past and a metonym of the present" (Denning, 1988:2). In some circles today, there is greater appreciation than ever before for the perspectival and contested nature of history, for "how knowledge is arrived at" (Tompkins, 1986:76-77), and for the invention of tradition (Hobsbawm and Ranger, 1983; Hanson, 1989; Keesing, 1990).

Sturtevant has long held an interest in this type of approach, but not to the exclusion of other types of anthropological history (here he departs from others like Schieffelin and Gewertz, mentioned previously). Over 30 years ago, in a collection of essays dedicated to ethnohistory and the "new ethnography," Sturtevant (1964:100) spoke of ethnohistory as "the conception of the past shared by the bearers of a particular culture, rather than (the more usual sense) the history (in our terms) of 'ethnic groups.'" Ethnohistory is here regarded as cognate with ethnohistory, ethnobotany, ethnozoology, and so on. Perhaps because nobody paid much attention to the implications of this approach, Raymond Fogelson in exasperation coined "ethnoethnohistory" and then championed taking seriously the historically and culturally constructed worlds of Cherokees and other native Americans (Fogelson, 1974, 1984, 1985, 1989:134). His determination to privilege cultural meanings has been pivotal both in his own and his students' work, and it is mirrored in the analyses of other scholars who have equal concern for privileging culturally constituted meaning and relating changes and continuities in native symbolic systems to specific historical conditions (see Krech, 1991, for discussion).

This kind of work is clearly heading in the direction of an indigenous historiography, in the promotion of which anthropological historians have been in the forefront. Some claim privileged understanding of the cultural worlds of native people, on which basis emerge explanations of why native people partici-

pated in the fur trade (Martin, 1978), or analyses of the metaphysics of Indian history (Martin, 1987a, 1987b), and others claim an ethnointellectual basis for the analysis of Indian-white history (Miller, 1985). But though these works pose important questions about cultural interpretations, they are highly dubious if not wrong-headed on methodological and theoretical grounds, raising questions about whether the cultural schemes were ever located in indigenous societies, as the authors propose, or were merely in the author's own minds (Peterson, 1988; Krech, 1981, 1990). This is precisely the complaint raised long ago about ethnohistorical analysis in anthropology. More promising, perhaps, in revealing native voices are works that draw extensively on native consultants in order to construct an ethnosociological account of historical events and processes (Whiteley, 1988) or that both draw on native testimony and are written by native historians (e.g., Sando, 1983). The unproblematized, narrativized version of events in the last-mentioned, however, elides questions about hegemony.

Indigenous historiography has been at the heart of many of the debates of the 1990s. In this decade, the writing of anthropological history has become contentious as battles have raged over the control of culture and history and over who has the right to control what appears in print in books, museum exhibition text, and other media. The Columbus Quincentenary was a battleground for many of the debates as controversy swirled about almost every aspect of a day at best commemorated, not celebrated, and by many vilified. Political correctness and polemics ruled the production of history for fully two years building up to the day of the Quincentenary. Of the many books published on the encounter (not discovery) by Columbus of the New World, in what many described as a veritable Columbus industry, some revisionist examinations were accused of an extreme political correctness. In contrast, others were attacked because they ignored both sea changes undermining the master narrative and objectivism in history and the plurality of voices in postcolonial discourse.

One work widely noticed, in part because its author played a key role in the development of the new historicism, was Stephen Greenblatt's (1991) *Marvelous Possessions: The Wonder of the New World*, a series of essays on the multiple meanings of Columbus's taking possession of discovered marvels. It received lengthy, critical, and thoughtful reviews. One was related directly to the matter of indigenous historiography. The reviewer, Roberto González Echevarría, denigrated Greenblatt's history compared to the "infinitely richer understanding of Columbus and other colonial writers" in the "troubled retellings" by Gabriel García Márquez, Alejo Carpentier, and others. "This is so," González Echevarría stated, "because these writers have more at stake; they see the discovery and conquest of the New World as part of their own history. No matter how repugnant these may be from a modern perspective, they are a part of Latin American history. Facile condemnations from the safety of the American academy thus seem condescending" (González Echevarría, 1992:23; see also Krech, MS).

González Echevarría is saying two separate things. First, that it is patronizing to condemn facily the encounter, or discovery of the New World—with which one can but agree. Second, that an infinitely rich understanding of the past is possible only if the past is one's own. This is contentious and contestable. It is also ethnocentric if in fact he is demanding, as he seems to be, that one must share either culture or genes, or both, with those about whom one writes in order somehow to produce a more correct history. The unstated assumptions underlying this contention include an inherent presentism, which at the very least must be exposed.

There is no debate over the context for González Echevarría's remarks: in the broadest intellectual sense it is formed by postmodern historiography and the new historicism, with their questions about authority, power, contingency, and truth, which are themselves the products of a specific historical moment of postcolonial reflexivity. In the early 1990s the issues were played out in sweeping discussions concerning objectivity in historical methodology. Even though the argument is hardly a new one in history, the most compelling recent comments can be found in Peter Novick's (1998) *That Noble Dream*, and in the reviews and forums that followed the publication of that work.²

Descending from lofty debate to historical practice, there has been a growing tendency in the 1990s to regard history (defined herein as the past and writing about the past) as intellectual property to be "repatriated" along with human remains or material artifacts in the collections of museums and historical societies. Some indigenous people demand control over the interpretation of the past as a native right. None deny that it is a form of empowerment. Disputes range widely and affect anthropological history. For example, the most visibly disputatious case of interpretation—that pitting Marshall Sahlins (1981, 1985, 1995) against Gananath Obeyesekere (1992) over the meaning of Captain James Cook in Hawaiian history—is contentious over the matter of who may speak for whom: Sahlins, the American intellectual whose knowledge of historical sources surrounding the death of Captain Cook may be unparalleled but who dared to malign nineteenth-century Hawaiian chiefs in another work; or Obeyesekere, the Sri Lankan intellectual who suggested that his own birth and his country's history made him a better candidate to comprehend Hawaiian history. Several Hawaiian activists unhesitatingly appropriated Obeyesekere to their postcolonial critique of Europe and the West, despite misgivings about his treatment of sources (Borofsky, 1997).³

The American Indian scene is similar. Here the debates turn narrowly, and at times bitterly, over how to imagine Columbus, measure the influence of the Iroquois on the United States constitution, or gauge the European role in the transformation of an earth-mother metaphor into a Mother Earth deity and its subsequent spread. Or they involve historiography at its broadest and the identical issue about who possesses the right to speak for others (Biolsi and Zimmerman, 1997).

In these and other struggles, some who possess the right credentials (after González Echevarría and others) ignore contrary interpretations and call for their own evidentiary rules and other historiographical conventions (Biolsi and Zimmerman, 1997). In this context, ethnohistory or anthropological history can be perceived as part of a colonial agenda to control the telling of the past, or (to recall ethnohistory in the 1950s) to determine legal cases whose outcomes have had significant political and economic consequence for native people. For some it is not enough to dismiss nonnative interpretations. Even certain native interpretations may be suspect because their authors in "confusion" at times "identify with the wrong people, the wrong things, the wrong tradition" and as a result are incapable of understanding (Churchill, 1993:404). Only natives with acceptable ideological postures are said to speak legitimately for native people or native history, to qualify as revealers of truth. According to American Indian scholar Annette Jaimes, what is needed is a historical "consciousness necessary to realize the liberation of North America from the grip of its nazi heritage" (Jaimes, 1992:9).

Although the reasons for the anger expressed here can and should be acknowledged, this does not begin to answer the need for sensitive explorations of the past and its many meanings. In his review of the "objectivity question" in history, James Kloppenberg (1989:1030) concluded that although "the indeterminacy of truth and historicity of reason are now widely conceded," there exists nevertheless a "terrain of pragmatic truth" to which one can aspire in one's musings or writings on the past. With the caveat that truth is always deeply embedded in culture, Kloppenberg's optimism points the way toward a reasonable middle ground for interpreting the past(s). For models for indigenous North America, perhaps one can return to Sturtevant's aforementioned interest in ethnohistory as "the conception of the past shared by the bearers of a particular culture" and to the many who have followed in his wake, together with today's more mature approaches to problems of colonialism, hegemony, and subalternity in writings about the Indians of Asia's subcontinent (Prakash, 1990; Chakrabarty, 1992). This demands acknowledging the sources—cultural, ethnic, psychological, and so on—of the assumptions (the biases) with which one operates. It demands, in other words, a reflexive awareness of one's historiography. All that is needed now is for anthropological historians, nonnative and native alike, to propel discussion onto this plane.

Notes

1. Sturtevant's contributions to the study of the American Indian past have reached the widest audience through his role as general editor of the *Handbook of North American Indians*, of which 12 volumes of a projected 20 have appeared; as the author of entries in the *Encyclopaedia Britannica*; and as principal consultant on a *National Geographic* map on Indians of North America. The breadth of his work is reflected in publications on the Seminole, among whom his ethnographic field research has been the longest. These publications range widely, from an account of a key informant's life (as well as of his own long relationship with that man) to a typology and history of men's

clothing and other analyses (e.g., Sturtevant, 1953, 1956a, 1956b, 1960, 1967b). His analyses are distinguished by a command of both standard European and indigenous languages, exquisite detail, and reticence for unwarranted generalization. Often Sturtevant is concerned with comparative analysis; for example, the informant-anthropologist relationship or the visual arts cross-culturally. The strengths are often in the details; in revealing, for example, the origin, two centuries before and a continent away, of Lafitau's engraving of Huron agriculture, which many had wrongly read literally (Sturtevant, 1968b). Or in reexposing as fraudulent an engraved mammoth tusk. Offered in the nineteenth century as evidence of man-mammoth contemporaneity in North America, the tusk was soon dismissed as a fake. But the dismissal was forgotten, and the artifact, rediscovered in the late twentieth century, was newly presented as genuine (Meltzer and Sturtevant, 1983; Griffen et al., 1988). Sturtevant's conclusions—as when he remarked, after scrutinizing the

origin of Lafitau's image of Huron farmers, that "Illustrations are historical documents which must be criticized in a manner comparable to that required by written documents" (Sturtevant, 1968b:94)—are always cautionary and sometimes didactic. For more information, see William Merrill (2001), "The Writings of William C. Sturtevant."

2. Discussion of *That Noble Dream* has been extensive: see Novick, 1988; Kloppenborg, 1989; Cmiel, 1990; Haskell, 1990; Gordon, 1991; Hexter, 1991; Hollinger, 1991; McGill, 1991; Novick, 1991; Ross, 1991; and Himmelfarb, 1992.

3. Of the several sources, including the two protagonists, cited by Borofsky and the commentators in the Current Anthropology Forum on Theory in Anthropology (Borofsky, 1997), Kame'eiehiwa (1994) and Trask (1991) are especially revealing of the fault lines in historiography of indigenous people of the Pacific.

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Editing a Cambridge History in a Postmodern Context

Bruce G. Trigger

I was fortunate to serve my editorial apprenticeship working for William C. Sturtevant as volume editor of the *Northeast* volume of the *Handbook of North American Indians* (Trigger, 1978). This gave me a welcome opportunity to gain a detailed, up-to-date regional perspective on the Iroquoian peoples I was studying, while helping to create a major reference work. I appreciated the chance to benefit from Sturtevant's vast knowledge of Native Americans and Native American studies as well as from his editorial skills. That experience was an important factor predisposing me to accept a geographically even more challenging task, coediting the *North America* volume of *The Cambridge History of the Native Peoples of the Americas* (Trigger and Washburn, 1996).

Recruitment

Frank Smith, the executive editor for the social sciences for Cambridge University Press, visited McGill University in October 1984 to ask me to consider editing this volume, which would be part of a three-volume series that would trace the history of the aboriginal peoples of North America, Mesoamerica, and South America from earliest times to the present. I was then finishing *Natives and Newcomers* (Trigger, 1985), and my research interests already were turning to investigating the history of archaeology. Despite initial reluctance to commit a large block of time to editing this volume, I agreed to do so because I believed that there was a pressing need for a history that would transcend disciplinary boundaries, that would begin with the arrival of the first aboriginal people and not the Europeans, and that would embrace all the native peoples who lived north of Mesoamerica. Although there were already adequate separate histories of the native peoples of Canada and the United States, and the histories of individual groups were being treated very thoroughly in the *Handbook*, there was no comprehensive history of the native peoples now living within the boundaries of those two countries and Greenland. I also believed that research on native history was playing a significant role in combating prejudices against native people, especially as such findings became incorporated into school textbooks and the presentations of the mass media. I hoped that an accessible,

scholarly reference work covering all of North America might play a special role in helping to counteract a variety of stereotypes and prejudices.

Being asked to edit a Cambridge history is not being given freedom to do whatever one wishes. Since the early twentieth century, Cambridge histories have sought through collaborative authorship to create major reference works that synthesize the current monographic literature relating to the history of a region, country, or people in a manner that meets the highest professional standards but remains accessible to educated nonspecialists. Even relatively young fields, such as Native American history, have produced a literature that is extensive and often forbidding to outsiders, even those who are academics and accustomed to reading widely. In recent years, as scholarship has grown increasingly chaotic, fractious, and competitive, these problems have increased, making responsible guides to this literature all the more important. In addition to being reference works, Cambridge histories are read around the world by students, professional historians working in other fields, and people who are interested in the history of a particular region or people. Thus, as a volume editor, I ensured that every sentence would be understandable to a German or Chinese reader who knew little about the history of North America and its peoples.

Each volume in a Cambridge history series is edited by a specialist broadly familiar with the field, and each chapter is written by someone well versed in the subject matter of that chapter. Chapters tend to be relatively long and therefore few in number. Their ample length gives the authors scope, but their broad range means that selectivity and concision remain essential. Coverage is supposed to be balanced, not partisan; the style authoritative, not polemical. Illustrations are minimal and are usually limited to maps, although some concession was made to including other types of illustrations with the archaeology chapters in the *North America* volume because that reduced the need for verbal description.

Smith had decided that each volume of *Native Peoples of the Americas* would be edited jointly by a historian and an anthropologist chosen after informal consultation with experts in the field. Wilcomb E. Washburn, director of the Office of American Studies at the Smithsonian Institution, Washington, D.C., and editor of the *History of Indian-White Relations* volume of the *Handbook* (Washburn, 1989), was appointed as coeditor of the *North America* volume. This meant that the coeditors were

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also an American and a Canadian, which it was hoped would facilitate recruiting authors and editing manuscripts. Finally, the editors' political views were far from identical. Since we were jointly responsible for planning the volume and also had to agree on the authors for each chapter, our selection by Cambridge University Press might be viewed as a further guarantee against specific forms of political or academic bias. Yet, while this arrangement may have inhibited potentially innovative risk-taking, which is not part of the tradition of Cambridge histories, it probably resulted in views being expressed in this work that are more diverse than those in most other aboriginal histories. As a result of working with Washburn, I found myself being exposed to unfamiliar and challenging arguments, and I acquired a greater awareness of the diversity of views concerning aboriginal history and among aboriginal people themselves. Hence, my editorial collaboration proved to be a valuable learning experience. Although we never had a chance to discuss the matter, I believe Washburn regarded our collaboration in the same way.

Both of us discovered that editing a Cambridge history was very much a cottage industry compared to editing a volume of the *Handbook*. We lacked the team of expert editorial assistants, coordinators, bibliographers, illustrators, and cartographers that Sturtevant had assembled at the Smithsonian, and whose strenuous efforts relieved us of much routine hard work and anxiety about the accuracy of the material going into our volumes. Knowing and working with these people became itself a pleasure. As coeditors of a Cambridge history, we were ourselves responsible for performing these tasks, and this in turn resulted in more sustained communications with the authors of the various chapters. On the other hand, the more narrowly focused theme of our volume posed a much greater challenge of ensuring that each chapter covered its assigned topics and that there were no serious gaps or discrepancies within or between them. Editing a volume of the *Handbook* was like editing an encyclopedia, whereas the *North America* volume was an integrated narrative.

Washburn and I worked closely on all aspects of the volume, although, after 1994, I took over more editorial responsibilities so that Washburn could complete writing the final chapter. Although later he sometimes seemed to lack his usual energy, he never complained of illness or failed to carry out his editorial responsibilities. It came as great shock to me to learn, not long after the volume was published, that he had died of cancer.

Planning

Cambridge University Press decided that, unlike the usage in the *Handbook*, the border between North America and Mesoamerica in this series would run along the present United States-Mexico frontier rather than along the northern limit of aboriginal high civilization farther south. This was decided soon after the editorial teams for all three volumes had been appointed, during a general division of responsibilities, and in-

involved consultation between Cambridge University Press and all six volume editors. Because of these consultations, the whole of Mexico was assigned to the Mesoamerica volume. Although it was recognized that modern national borders were irrelevant before European contact and that Texas, the American Southwest, and California had been part of Mexico until the mid-nineteenth century, it was judged that the more recent development of pan-tribal institutions and identities within the context of national states justified this arrangement. It also seemed worth exploring what resulted from using a division different from the one that had been employed in the *North American Handbook* and in the *Handbook of Middle American Indians* (Wauchope, 1964–1976). Although one archaeological contributor objected strongly to this arrangement, it permitted later chapters of the *North America* volume to focus on the experiences of native North Americans in relation to the United States, Canada, and Denmark.

Washburn and I finalized our proposed chapter outline of the volume on 10 November 1984 at the annual meeting of the American Society for Ethnohistory held in New Orleans. We then also had a chance to discuss our ideas with other ethnohistorians. Our main challenge was to present a comprehensive, balanced, and well-structured history of the native peoples of North America within our contract's stipulated limitation of a maximum of 16 chapters, none of which should exceed 20,000 (this was later expanded to 25,000) words. Our second concern was that *North America* should not be a one-volume condensation of the historical sections of the *Handbook*. That meant avoiding a tribal history approach and instead trying to explore more general themes in Native American history, in particular ones relating to changing patterns of adaptation and identity. The chapter outline, with accompanying annotations, that was drawn up that day, and which Cambridge University Press sent to 10 readers for comment, was followed in the published volume (Table 1), except that we had intended chapter 5, dealing with prehistoric Mississippian chiefdoms, to be followed by a chapter assessing the significance of historical linguistics for understanding Native American prehistory. As a result of negative comments from several prominent linguists who maintained that the current state of historical linguistics did not justify separate treatment, this chapter was dropped. Its essential elements, however, as well as pertinent information about the linguistic prehistory of North America, were incorporated into the chapters that dealt with native history before European contact, especially the chapter by Dean Snow. The loss of this chapter has since been made good by the publication of Michael Foster's (1996) magisterial survey of native North American historical linguistics in the *Languages* volume of the *Handbook*.

The final ordering of the volume reflected the coeditors' awareness of, and concerns about, the current postmodern pre-occupations of history and the social sciences. It therefore was decided to begin with a chapter, by Peter Nabokov, surveying native views of history, to be followed by a second chapter, by

TABLE 1.—Contents of *The Cambridge History of the Native Peoples of the Americas*, volume 1, *North America*.

List of illustrations

Editorial preface

1. Native views of history
Peter Nabokov, University of Wisconsin, Madison
2. Native peoples in Euro-American historiography
Wilcomb E. Washburn, Smithsonian Institution
Bruce G. Trigger, McGill University
3. The first Americans and the differentiation of hunter-gatherer cultures
Dean R. Snow, Pennsylvania State University
4. Indigenous farmers
Linda S. Cordell, University of Colorado, Boulder
Bruce D. Smith, Smithsonian Institution
5. Agricultural chiefdoms of the Eastern Woodlands
Bruce D. Smith, Smithsonian Institution
6. Entertaining strangers: North America in the sixteenth century
Bruce G. Trigger, McGill University
William R. Swagerty, University of Idaho
7. Native people and European settlers in eastern North America, 1600–1783
Neal Salisbury, Smith College
8. The expansion of European colonization to the Mississippi Valley, 1780–1880
Michael D. Green, University of Kentucky
9. The Great Plains from the arrival of the horse to 1885
Loretta Fowler, University of Oklahoma
10. The greater Southwest and California from the beginning of European settlement to the 1880s
Howard R. Lamar, Yale University
Sam Truett, Yale University
11. The Northwest from the beginning of trade with Europeans to the 1880s
Robin Fisher, University of Northern British Columbia
12. The reservation period, 1880–1960
Frederick E. Hoxie, The Newberry Library
13. The Northern Interior, 1600 to modern times
Arthur J. Ray, University of British Columbia
14. The Arctic from Norse contact to modern times
David Damas, McMaster University
15. The Native American Renaissance, 1960 to 1995
Wilcomb E. Washburn, Smithsonian Institution

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Washburn and myself, examining changing representations of native people in Euro-American historiography. Washburn and I agreed that these chapters were necessary to ensure that readers were aware of the socially constituted nature of history and of the varied political uses and misuses that are made of it. In our own chapter, while admitting the inevitability and desirability of alternative interpretations of the past, we sought to counter extreme relativist assertions that there is no objective way to assess the truth claims of mutually exclusive interpretations of the past. We attempted to illustrate how fresh evidence frequently undermines accepted interpretations, even when these enjoy considerable ideological support, and how differing viewpoints often give rise to interpretations that are complementary rather than antithetical. Nabokov demonstrated that there was considerable variation in traditional views of the past from one aboriginal group to another, thereby challenging the adequacy of evolutionist claims that all native peoples shared a

similar mythopoeic outlook (Martin, 1987), and he also showed how these views have changed and multiplied as successive generations of aboriginal people have sought to accustom themselves to new experiences.

The next three chapters were to deal with Native American history before European contact. Our plan for these chapters was strongly criticized by a number of assessors for promoting an evolutionary view of native cultures, which they thought was ethnocentric and even racist. It is true that different types of cultures were to be examined in the historical order in which these types had first appeared, but this did not imply inevitable, unilinear development. Chapter 3 discusses what is known about the arrival of native people in the New World (alternative aboriginal views are presented in chapter 1) and traces the history of hunter-gatherer cultures in each region until European contact. It is made clear that big-game hunting and collecting cultures flourished over large areas of North America for more than 12,000 years and that these cultures had their own histories of change and development. Hunter-gathering is not treated as evidence of social or cultural inferiority or as nothing more than an ephemeral prelude to agricultural societies. Chapter 4 deals with the agricultural societies that developed in the Southwest and East beginning in the second millennium B.C. Its authors stress that farming economies developed along fundamentally different lines in the two regions, reflecting their disparate origins and environmental settings. Chapter 5 discusses the complex chiefdoms that developed in the Southeast after A.D. 900. With the exception of the Natchez, these societies disappeared, largely as a result of European epidemics during the sixteenth century, but before then they were the most complex societies evolved by any aboriginal North Americans.

Although the archaeological chapters offer a generally familiar view of the past, all three stress more than most older syntheses do how even those native peoples who relied the most heavily on agriculture knew a great deal about wild plants and animals and continued to use them to a considerable extent for food and clothing. This orientation helps to explain bizarre but persistent nineteenth-century Euro-American claims that all native peoples were hunters, whether or not they relied on agriculture. As shown in chapter 8, cultural prejudices prevented Euro-Americans from acknowledging that women could play a significant role in agriculture and induced them to classify North American Indian economies on the basis of male activities. Hence, the nineteenth century witnessed an ethnocentric Euro-American effort to “civilize” Indians by turning male hunters into farmers and female farmers into housewives.

The third and longest section of the book (chapters 6–12) traces the history from 1500 to 1960 of those parts of North America that at least since the late nineteenth century have been settled mainly by people of European origin. This includes all of the United States south of the 49th parallel and Canada south of the boreal forest. Chapter 6 documents the highly varied contacts that took place between Indians and Europeans before the end of the sixteenth century, mainly in

southern and eastern North America. Chapters 7 to 11 survey what happened to aboriginal peoples in the areas of North America that were dominated by European settlement between 1600 and the 1880s. Two of these chapters cover the Eastern Woodlands, and one each covers the Great Plains, the Southwest and California, and the Northwest Coast, southern Alaska, and Aleutian Islands. Each of these regions had distinctive native cultures and was colonized by Europeans of different nationalities (Spanish, French, and English) and at different times. The cultures of the native peoples of the Plains were transformed by the arrival of the horse, which had been introduced into the Southwest by the Spanish, well in advance of any significant direct contact with Europeans. The final chapter in this section examines the history of all native peoples living south of the North American boreal forest from 1880 to 1960. During this period most recognized Indians in the United States and southern Canada were confined to reservations and were subject to the tutelage of the United States and Canadian governments. Yet, despite its repressiveness, the reservation period was when these Indians acquired a more detailed knowledge of Euro-American ways and began to use expanding Euro-American communication networks to forge pan-Indian solidarity and to create political movements that allowed them ever more effectively to resist encroachment and begin to regain control over their own lives.

Key elements of chapters 6 to 12 are Michael Green's account of the development of United States Indian policy from 1780 to 1880 and Frederick Hoxie's survey of United States and Canadian Indian policy from 1880 to 1960. Green chronicled the shift in United States official policy in the 1820s from stressing assimilation to justifying removal. Indian resistance was tenacious, resourceful, and often well informed about Euro-American intentions, but Euro-American pressures to acquire Indian land were remorseless and cumulative. Largely by letting Euro-Americans and Indians of the time speak for themselves, Green revealed the injustices of this period. Hoxie demonstrated that these attitudes did not change significantly until the 1930s. Although the Indian Reorganization Act of 1934, prompted by then United States Commissioner of Indian Affairs John Collier, did not greatly enhance the living standards of native people, it created conditions that promoted both legal recognition of native self-government and the staffing of the United States Bureau of Indian Affairs by aboriginal Americans. This assisted native people in their ongoing struggles to protect and enhance the freedoms that they had never willingly surrendered.

Chapters 13 and 14 chronicle the history from first European contact to the present of the aboriginal peoples who continue to live north of the main zone of European settlement. The Subarctic and Arctic regions are treated in separate chapters, the Arctic chapter being a history of the Inuit. These peoples retained control of their destinies until recent times, but the problems that small numbers, isolation, and the cost of travel pose for education, employment, and social action have made the

challenges northern peoples face as they become increasingly dependent on a world economy significantly different from, and in many ways more serious than, those confronting aboriginal people farther south. The most significant divide among native people today does not run along the Canada-United States border but along the southern edge of the boreal forest.

A final chapter, written by Washburn after Nancy Lurie, a senior anthropologist noted for her strong support for the struggle for Indian empowerment, was unable to accept our invitation to do so, surveys cultural, political, and economic developments among Native Americans since 1960 on a continental scale. It continues Hoxie's documentation of how aboriginal peoples have made progress in their struggle to regain control over their own lives. Considerable attention is paid to artistic, literary, and scholarly achievements. At the same time it indicates the changing but persistent external pressures with which native peoples still must contend as they seek to assert their tribal and pan-Indian identities within the North American cultural mosaic. Especially within those aboriginal communities in the United States that have successfully implemented self-government, there has been a growing dichotomy between those Indians who reject the communal ideals of the past as an impediment to economic progress and those who view embracing a free-market economy as a betrayal of their traditional community ideals. There are no easy solutions, especially for smaller native groups that as a result of the colonial encounter lack the resource or revenue base to support their legitimate aspirations.

Although the general topics that had to be covered in each chapter were noted in brief descriptions prepared by the editors, the tradition of Cambridge histories is to select appropriate experts to write each chapter and allow them to develop their material as they see fit. Washburn and I briefly considered the possibility that chapters might be submitted to native groups for comment or approval, but we decided not to do this. Each chapter, as a result of its broad scope, deals with many different peoples, all of whom in fairness would have had to be consulted and who often did not share the same view of their common past. Under these circumstances, consultation would have been an extremely lengthy and problematical undertaking.

There was, however, some editorial intervention relating to terminology. The terms "tribe" and "band" were used only as, respectively, United States and Canadian government administrative designations; otherwise the terms "peoples," "nations," "communities," or "groups" were used. The term "prehistory" was banished because it is ethnocentric and patronizing to imply that history began only with the arrival of Europeans (Axtell, 1988:42–43). We do not deny, however, that, because of the absence of written records, the study of native history before the arrival of Europeans is quite different from the study of native history after that time. We followed Francis Jennings (1975:viii) in avoiding such expressions as "blacks," "whites," and "redskins" (except in historical quotations) because such references to physical appearance clearly embody outmoded racial stereotypes. And finally, the names of native peoples

were pluralized (Hopis, Cherokees), as other nationalities habitually are (Germans, Russians). This was done in response to James Axtell's (1985:xi) complaint that the "anthropological singular" wrongly treated Indian groups differently from other peoples.

Recruiting Authors

Washburn and I began to select authors to write chapters in the late 1980s; however, when we started to assemble a list of names, we encountered a major problem. To ensure the unity of each chapter, we were determined that it would be written by no more than two coauthors. All of our original proposals were for single authors, with a second author being added only at the request of the original one. This usually happened when the essay divided naturally into two parts on a geographical basis. Yet, even in these cases, each author had to cover a broad topic. We had failed to appreciate the extent to which Native Americanist scholars are specialists. The vast majority seem to focus on the history and ethnography of particular aboriginal peoples, on the study of intergroup trade in a particular region during a specific century, or even on more specific topics, such as Hopi rain dances or Iroquois warfare. Often, researching such specialities becomes their life's work. I am not certain why this is the case. Similar specialization has been avoided in other areas that have a similar complexity of ethnographic subject matter. Moreover, it appears to be a specialization of long standing, albeit one that has been accentuated in recent decades by a declining interest in a comparative perspective. It is an attitude that perhaps is encouraged by the geographical proximity of many researchers to the groups they are studying and also by a belief, which is often incorrect, that the more general studies have already been done.

Whatever the reason for this problem, it was difficult to recruit generalists to produce the broad syntheses we required. A likely source appeared to be other volume editors of the *Handbook*. This worked well in the case of David Damas on the Arctic, but three others either declined our invitations or were forced to withdraw by the pressure of other work. Six authors in all failed to complete their assignments, and one chapter lost two sets of authors, who each failed to produce an acceptable draft, before being written in record time by a third recruit. This slowed the completion of the volume and produced a crisis that was overcome only when the present slate of authors was settled on as part of a major reorganization of editorial responsibilities in May 1994. At that juncture Smith deployed his editorial skills well beyond the call of duty by working as a de facto third member of the editorial team until new authors were recruited and the production of the volume was back on schedule. Almost all the authors, although selected for their extensive knowledge, were required to research unfamiliar topics in order to complete their chapters. We therefore needed people who were not only experts but who were also willing and able to cope with challenges.

This need for extensive and detailed knowledge, rather than narrowly focused specialization, did not leave the editors much choice. When we discussed possible authors with colleagues across North America, they frequently objected that the obvious choice was undesirable because he or she probably did not have anything new to say. But our interlocutors frequently had to admit that the alternatives they were proposing were either less informed about the subject or their judgement was less mature. Fortunately, we did discover, often when we were looking for late replacements, younger generalists who on short notice produced excellent papers. So we did not end up depending entirely on old war horses. The volume was written by an excellent mix of scholars who varied in age and academic rank from beginners to postretirement.

Another problem was the 49th parallel. In general we found that anthropologists and historians who were well informed about native people in the United States knew little about native people in Canada or about Canadian institutions, whereas Canadian specialists similarly knew little about matters south of the border. Some authors rose above this problem in exemplary fashion. Neal Salisbury already had a comprehensive knowledge of Indian relations with Spanish, English, and French colonists in eastern North America, and Damas drew upon his experience editing the *Arctic* volume of the *Handbook* to survey the history of that region from Greenland to Alaska beginning in A.D. 900. Using the exceptional textual and human resources of the Newberry Library, Hoxie produced a magnificent study of the reservation period in the United States and Canada. His chapter contains probably the most detailed comparison currently available of Indian policies in those two countries between 1880 and 1960. Chapters 2 and 6, both of which had to deal equally with Canada and the United States, each were coauthored by an American and a Canadian.

More serious problems came with chapters that were geographically centered within one modern nation state but extended into another. The authors of chapters 4, 8, and 9 were less familiar with Canadian than with American sources. In these instances I found myself, as editor, suggesting the addition of material relevant to southeastern Canada or the Canadian Plains. On the other hand, the authors of chapters 11 and 13 were more familiar with Canadian than with United States material. Robin Fisher knew the ethnohistorical sources relating to British Columbia and coastal Washington and Oregon thoroughly but was less familiar with the Plateau area and coastal Alaska. Arthur Ray, as a consequence of his intensive studies of the Hudson's Bay Company, had paid less attention to the Indians of interior Alaska than he had to those who lived in what is now Canada.

Although each author tried hard to provide a balanced coverage, it was clearly impossible to discuss everything relating to the history of aboriginal North America in fifteen 25,000-word papers. Despite its shortcomings, this volume of *Native Peoples of the Americas* offers a series of more balanced and comprehensive regional syntheses of the history of the aboriginal

peoples of North America than does any previous work. It also provides a needed basis for starting to compare the relations between Canadian and United States governments and native peoples over the past several centuries.

Washburn and I did not score well in recruiting women and aboriginal authors. Only two of our 16 contributors are women: Linda Cordell (chapter 4) and Loretta Fowler (chapter 9). I also regret that less attention has been paid to women's issues in this volume than should have been. There does not, however, appear to be a correlation between such attention and either the gender or the age of authors. Some of the most extensive and insightful coverage of gender issues is provided by Green in chapter 8 and Ray in chapter 13.

We did ask a number of prominent aboriginal anthropologists and historians to write chapters, but they declined, usually because they were too busy. There is much interest among native people in studying aboriginal history, but for good reasons much of it is directed toward the investigation of specific peoples or issues. This orientation is not compatible with the more synthetic goals of a Cambridge history. We might have tried to attract more native contributors as coauthors, but we feared that might be viewed as patronizing.

Assessment

I am the first admit that this is a "white man's" history. Yet, without being smug or apologetic, I would suggest that it may be appropriate, as well as historically inevitable, that the *North America* volume of *Native Peoples of the Americas* has been a work of Euro-American scholarship. In chapter 2 the coeditors documented how early Euro-American historiography reflected and reinforced a popular image of native people as inferior to European settlers and hence justified their marginalization, spoliation, and abuse. This situation did not begin to change until 1930, when Harold Innis (1930) paid serious attention to the important role that Indians had played in the fur trade. In the course of subsequent ethnohistorical research, Euro-American anthropologists and historians have collected a vast amount of data that have challenged negative stereotypes concerning native people that their predecessors had helped to create. It is now recognized not only that the history of North America is incomplete without knowing what has happened to native people but also that it is impossible to understand many aspects of Euro-American history fully and accurately in the absence of such knowledge (Trigger, 1985). The *North America* volume constitutes a significant milestone in overcoming and repudiating the most pernicious myths that previous generations of Euro-American scholars had fabricated. This is largely the cumulative result of the responsible collection and deployment of data that have gradually limited the scope for certain kinds of pernicious wishful thinking and the abuse of history for political ends.

Another unexpected benefit of this volume has been the opportunity it affords to assess the preoccupations that currently

dominate the academic study of aboriginal history. Although the racism and ethnocentrism that dominated earlier Euro-American treatments of Native American history largely have been eradicated, new interpretations reflecting currently fashionable commitments to political correctness, cultural relativism, and idealistically privileging subjectivity have taken their place, introducing new biases into the study of aboriginal history. Because, in general, theoretical issues loom larger as the level of synthesis increases and the use of facts becomes more selective, it might be expected that this volume would mirror currently fashionable attitudes. Yet, because the authors come from intellectually diverse backgrounds and have a detailed understanding of their subject matter, their chapters are more factually constrained than is often the case when a single individual writes a general survey of aboriginal history. Far from mirroring current prejudices, the historical accounts in this volume document a diversity of aboriginal behavior that defies simple classification or explanation.

Recent scholarship has made it clear that in the past the impact of European diseases upon aboriginal Americans was grossly underestimated (Dobyns, 1983; Thornton, 1987). Beginning in the first half of the sixteenth century, vast numbers of Native Americans perished from European diseases against which they had no acquired immunity. The deaths of vast numbers of native people led in some instances to political and cultural collapse and to the disappearance of entire peoples, and they weakened the ability of most native groups to resist European expansion. The vast overall population decline also encouraged an erroneous impression that North America had been an empty continent before European settlement. Many Euro-American scholars now treat this decline and the recent demographic recovery as the principal factors that account for Indian–Euro-American relations. Some native scholars have also begun to argue that their ancestors were vanquished by diseases and not by European settlers (Sioui, 1992). The authors contributing to the *North America* volume clearly acknowledged the importance of microbial disasters, but they also demonstrated that epidemics occurred under specific circumstances and at different times and had varied impacts in different regions of North America. Epidemics alone cannot explain the course of Indian–Euro-American relations. Europeans were not obliged, except perhaps by their cultural values, to take advantage of Indian population decline in the ways they did. Too much emphasis on demographic factors diverts attention from the political, economic, religious, and cultural factors that were also involved in Indian–Euro-American relations. Invoking epidemics to explain what happened to native people bears an uncanny resemblance to attributing it to Divine Providence, as Euro-American historians frequently did between the seventeenth and nineteenth centuries. If the volume helps to reverse this trend, it has achieved something important.

A second currently popular theme in the study of aboriginal history is resistance. It has become fashionable to argue that native history has placed too much emphasis on documenting

exploitation, suppression, and annihilation. What is needed is to pay greater attention to how native people struggled to preserve their ethnic identity and valued aspects of their traditional ways of life (Merrell, 1989). There is no doubt that aboriginal people, as individuals and groups, displayed great resourcefulness in resisting colonization. Sometimes they struggled effectively to maintain a traditional way of life intact, as the Hopis have done. In other cases they adopted many aspects of European social and political organization in the hope that this would permit them to resist European expansion. The Creeks and Cherokees provide excellent examples of that strategy. But it is evident that every native group that has survived had to struggle against terrible odds to do so, and they suffered major losses as their freedom was restricted by growing numbers of European settlers who were intent on controlling the resources of North America. The brutal, land-hungry Euro-American response to the efforts of the "five civilized tribes" of the southeastern United States to adapt to the new order makes this all too clear.

The *North America* volume provides many examples of aboriginal peoples struggling against immense odds and in many different ways to maintain their freedom, but it also bears witness to the failure of many of their struggles and to the terrible losses that all North American native peoples have suffered at various points in their history since 1492. For a long time Indian groups had great difficulty in uniting to oppose European settlement, and these problems became more serious as the settler population increased and the native population declined. All too often native people aligned themselves with European settlers against rival native groups. It is instructive, for example, to learn about the important role that Plains Indian scouts, working for the United States Army, played in forcing other Plains Indians onto reservations. European groups also aligned themselves with Indian allies against European rivals, but the political consolidation of European settlers almost always outstripped that of native peoples. European success was based not only on superior technology but also on greater social discipline and on an ideology that encouraged greater self-discipline and aggressiveness. The miracle is that so many aboriginal groups managed to survive until eventually they learned to work together to expand their freedom in a sometimes very hostile and always very demanding political environment. Pan-Indianism, although sometimes dismissed as a betrayal of more authentic or genuine forms of aboriginal identity, has in fact played an important role in native peoples' quests for dignity and freedom. The volume bears witness in equal measure to aboriginal peoples' will for collective survival and to the limitations that Euro-Americans have placed on their freedom and the losses and injustices they have suffered. Native history cannot be properly understood unless both these aspects are considered.

There are also heated, theoretically grounded disagreements about the early impact that European contact had on aboriginal cultures. It has been argued that new sources of wealth often

led to the economic and religious florescence of indigenous cultures. Although many examples of this can be cited (Trigger, 1976), other native societies soon disintegrated as a result of epidemics, loss of territory, or spiralling warfare resulting from growing economic competition among native peoples. An especially bitter academic dispute relates to the impact that European technology had on native peoples. Some scholars argue that native people quickly appreciated the value of European metalware and became technologically dependent on it. Their inability to produce iron led to increasing reliance on European suppliers, which encouraged intergroup warfare over trade routes and raw materials to trade with Europeans and which in the long run undermined their political independence and adherence to their traditional belief systems (Trigger, 1985). Others have argued that native groups long valued European goods not for their utilitarian properties but because of the meanings they assigned them in relation to their traditional religious beliefs (Miller and Hamell, 1986). It has also been claimed that native groups in eastern North America were so little influenced by European technology that they were able to determine their own destinies in the midst of European colonial rivalries until at least 1760 (Eccles, 1986; Heidenreich, 1986; Steele, 1994). Those who uphold this view maintain that Indian warfare during the early historical period was mainly a continuation of longstanding conflicts, the scale of which was perhaps enhanced by the acquisition of European firearms and a growing need to replace escalating population losses from European diseases by incorporating increasing numbers of prisoners into their societies (Richter, 1983). Like the nineteenth-century historian Francis Parkman, though without his racism (Trigger, 1985:10–14), these analysts believe in a wholly idealistic fashion that after, as well as before, European contact Indian behavior was determined almost entirely by their own particularistic and unchanging cultural norms. Little attention is paid to the role played by rational calculations of individual political and material self-interest, whether such interest is defined in terms of either universal or culturally specific criteria (for a discussion of universal criteria, see Brown, 1991).

The chapters in the *North America* volume document that some aboriginal societies diminished and disappeared as a consequence of lethal epidemics, whereas others that were equally affected managed to hold their own and even to prosper. European contact resulted in cultural florescence for some native societies and in cultural decline for others. It also resulted in relative autonomy for some societies but growing dependence on Europeans or on neighboring aboriginal groups for others, in resistance for some but varying degrees of collaboration with Europeans for others, and in what can only be described as the victimization of many native peoples by Europeans. These examples, drawn from comprehensive sources that guarantee their representativeness, suggest that the range of variation in relations between aboriginal societies and European intruders was far greater than any ideologically driven formulation might suggest. If any overall pattern is discernible,

it is that epidemiological, demographic, technological, social, and ideological factors gave European colonists an advantage that allowed them to gain control of the more fertile areas of North America in the 400 years following 1492, but that many aboriginal peoples managed to survive within the context of Euro-American society and today have regained more freedom to express themselves and determine their own destiny than they have had since the 1870s or earlier. This was not an outcome that would have been predicted even by the most sympathetic anthropologists at the beginning of the twentieth century. The overall pattern does not, however, diminish the variety of ways in which different aboriginal groups under very difficult and dissimilar circumstances sought to assure their survival in the face of Euro-American competition.

Concluding Observations

I do not accept the view that only members of an ethnic group may rightly study that group's history. On the contrary, the study of English, Japanese, and German history has been enriched as a result of being examined from multiple ethnic perspectives. What makes the difference between polemic and productive academic discourse in such exchanges is a shared commitment to ascertaining the truth and to maintaining the highest disciplinary standards. Nevertheless, it is peculiar and unacceptable when a people's history is studied exclusively, or even primarily, by outsiders. In all studies of relatively recent history, it is normal that the greatest academic interest should come from those people whose own past and cultures are being investigated. They bring to such studies a commitment and perspectives that are important ingredients of historical research

everywhere. To observe that the root cause of the lack of native representation in professional historiography is broadly social, rather than specifically academic, does not excuse academics from playing a major role in resolving it by trying to recruit more native people to study Native American history and anthropology. Although I would oppose nonnative scholars being excluded from contributing to the next edition of the *North America* volume of *Native Peoples of the Americas*, I would like to see it edited and largely written by a new generation of native professional historians and anthropologists. That native people do not yet play the leading role in the professional study of their own history and anthropology is an anomaly that requires correction. Only when this anomaly has been eliminated will the legacy of colonialism and ethnocentrism truly be exorcised from these professions. The present generation of non-native ethnohistorians has managed to rescue Native American history from some of the egregious misunderstandings of previous generations of anthropologists and historians. Ahead lies the challenging task of writing history from a more authentic Native American perspective. That can be done only after more native people have become professional historians and anthropologists.

Note

This paper is drawn mainly from a talk titled "Conflicts and Blind Spots: Can Non-Native Scholars Write a History of the Native Peoples of North America?" which I gave as the Sixth Seminar of the McGill Institute for the Study of Canada, 28 March 1995. I thank Desmond Morton, director of the institute, for inviting me to give this talk, and I thank Frank Smith of Cambridge University Press for commenting on a draft of the paper. The paper was finished while I was the recipient of sabbatical leave from McGill University.

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A Historian Among the Anthropologists

Wilcomb E. Washburn

When I came to the Smithsonian Institution in 1958 as a historian interested in the American Indian, I soon found my way to Bill Sturtevant's crowded office in the Natural History Museum. I have traced that route many times since on missions involving collaboration or merely to exchange views on issues of the day. My associations with Bill have included participation in the abortive Center for the Study of Man project, directed by Sol Tax, who came in regularly from the University of Chicago; editorship of volume 4, *The History of Indian-White Relations* of the *Handbook of North American Indians* under the general editorship of Bill; participation in the meetings of the Anthropological Society of Washington, the annual meetings of the American Anthropological Association (AAA), and international meetings, such as the one at the University of California, Los Angeles, that resulted in the publication *First Images of America* (Chiappelli, 1976), and the one in Moscow out of which came *Pre-Columbian Collections in European Museums* (Hocquenghem et al., 1987); even shared projects, such as our joint article "The First Americans" (Sturtevant and Washburn, 1976) for the catalog *A Nation of Nations*, which accompanied the exhibit of that name in the Museum of History and Technology (now the National Museum of American History) in 1976, and more recently an abortive attempt to do a joint review of an exhibit on Pocahontas at the Virginia Historical Society, in Richmond, Virginia. These associations have fostered a warm relationship that I value most highly among all my Smithsonian relationships.

Nevertheless, despite our shared assumptions and good working relations, I have always remained a historian among the anthropologists. The relationship has been basically friendly, but it has sometimes been antagonistic. The antagonistic debates have occurred principally in three areas, which I want to deal with in this paper: United States government policy toward the American Indian; anthropological participation in ideological debates within our society; and anthropological theories, such as cultural relativism. Often my critical attitude stimulated anthropologists, like my friend Nancy Lurie, to ask me rhetorically, "Where did you do your fieldwork?" Nancy asked me this at a meeting at the Asilomar Conference Center, in California, where I gave a paper on "Ethical Perspectives in

North American Ethnology" at a meeting of the American Ethnological Society (Washburn, 1985). In my paper I had attacked Tax and the "action anthropology" with which both he and Nancy were closely identified. My good-natured reply to her needling was, "Among the anthropologists."

The Project Camelot affair (an effort by the United States Army to enlist social science research for counterinsurgency) became a turning point for anthropologists in 1965, when their desire to be listened to by government was overmatched by their concern over the ultimate purpose for which their expertise was being sought. Anthropologists raised nasty accusations against fellow anthropologists perceived to be working for the United States government against the interests of the peoples being studied. Vietnam in particular and Southeast Asia in general became the first great test of whether anthropologists had any legitimate business working on projects designed to help the United States government in such areas. I thought that the AAA, and Bill himself, overreacted in trying to draw a cordon sanitaire between the government and anthropology. Of course, given that both Bill and I worked for the Smithsonian, a quasi-governmental institution that receives most of its support from appropriated funds of the United States government, there was a reason to be concerned that the work of anthropology not be used against the interests of the peoples being studied.

As a Marine Corps Reserve officer who had been in uniform during World War II and the Korean War, I occupied a particularly vulnerable position. The Standing Rock Sioux author and educator Vine Deloria and others, when they sought to oppose me in scholarly meetings, would often refer to me as "colonel," or as a marine, as a way of instantly identifying me as hostile to the aspirations of men of good will. As a member of the Commission on the Study of Peace of the International Union of Anthropological and Ethnological Sciences, I have continued to observe how wide the breach is between those connected with the military and those who, like a later president of the United States, "loathed" the military.¹ In the commission's newsletter, *Human Peace*, were to be found learned essays, such as the article "Witchcraft and the Cold War," in which anthropologist Paul R. Turner attempted to apply his knowledge of witchcraft in the small Indian village of Santiago in the state of Oaxaca, Mexico, to the relationship between the United States and the Soviet Union. Turner conceded that his "attempt to explain the Cold War in terms of witchcraft is simplistic and does not do justice to the complexity of the situation," but he

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concluded that one could legitimately make the analogy (Turner, 1989:72). Turner's use of his work on witchcraft to explain the Cold War was expanded upon by others, such as the anthropologist Alice Kehoe, in the next issue of *Human Peace*. "Peace Studies" scholars, particularly among political scientists and historians, seemed to me to present arguments that were ideological, idealistic, or condescending in character. Like Herman Kahn, founder and director of the Hudson Institute, one of whose seminars there I had attended while on military reserve duty, I found most peace studies irrelevant if not unreadable.

Anthropology in the 1960s and 1970s moved recklessly to the defense of regimes that I felt deserved critical scrutiny if not outright condemnation. Anthropologists habitually denounced United States government policy with regard to Communist regimes in the Soviet Union, the Peoples Republic of China, Cuba, Angola, Nicaragua, and the like. At the business meetings of the AAA, I often participated in debates on resolutions on international questions (such as the condemnation of the United States for the invasion of Grenada, on which issue I constituted the sole vote opposed to the resolution) and also on domestic questions involving American Indian issues. Indeed, I realized I had achieved a certain degree of notoriety when Bill, beside whom I was seated at one business meeting, asked if I minded if he sat somewhere else because people were beginning to suspect that he might be corrupted by my proximity. When I reminded him of this incident many years later, he questioned whether it actually happened, but I believe I remember the incident accurately, and I have enjoyed repeating the story to my conservative friends over the years.

In addition to finding myself at odds with my anthropological colleagues on international issues, I diverged on questions relating to the government's American Indian policy. Often the issue pitted elected tribal governments against self-selected groups, such as the American Indian Movement (AIM), or factional elements within tribes. Anthropologists tended, in my opinion, to be swayed too readily by the rhetoric of the radicals and to join too easily in condemning so-called puppet tribal governments.

In the *Indian Truth*, a publication of the Indian Rights Association, I wrote that the wild charges made by unrepresentative American Indian groups and individuals before the Fourth Russell Tribunal, held at Rotterdam from 24 to 30 November 1980, trivialized the real injustices suffered, squandered white support for the American Indian, and undermined the cause of expanding Indian self-government (Washburn, 1981). I listed the accusers who brought the charges, a motley array: the Independent Hopi Village of Hotevilla for the Hotevilla Hopi; Herbert Blatchford for the Big Mountain Dine; the Lakota Treaty Council for the Lakota; the Indian Law Resource Center on the general issue of land rights of Indians in the United States and the situation of the Iroquois, Lakota, Hopi, Seminoles, and Shoshone; Rosemary Christensen, Minneapolis Public Schools, for the urban Indians of Minneapolis; the Grand

Council of the Iroquois Confederacy for the Haudenosaunee (Iroquois); Bruce Ellison, lawyer, for Leonard Peltier; Indian Law Resource Center for the Ganienkeh Territory (Mohawk); the Hupa Survival Group for the Hupa; and the Western Shoshone Sacred Land Association for the Western Shoshone. Although anthropologists were not prominent among the complainants, they tended to support the initiatives of the activists.

None of these entities, I pointed out, was an official tribal body, none was designated by tribal governments to represent them, and none was elected by their tribal members to represent them. Typical of the charges was that of the organizing committee of the tribunal itself, which asserted that "sterilization of Indian women without permission or request has been alleged against many countries, but has been fully documented and proven with respect to the United States by a report of the General Accounting Office of the United States Congress" (cited in Washburn, 1981:8). Of course this was a pure invention and deliberate falsification of the evidence. I was later to run into this type of accusation abroad. At a meeting at a university in Rome in the 1980s, where I shared the platform with the Tewa anthropologist Al Ortiz, the Creek medicine man Philip Deer, and the AIM activist Bill Means, I had to face the wrath of the left-leaning student body when I pointed out that the charges of sterilization and even of genocide, which Deer and Means had asserted, were nonsense and they knew they were nonsense. I was greeted with shouts of disbelief and outrage. "We know it is true!" the students shouted. I could not get Ortiz to join me in refuting the charge and had to congratulate Deer and Means at dinner that night on their successful rhetorical achievement.

At another meeting in Italy, after I had defended the sincerity of John Collier, commissioner of Indian affairs from 1933 to 1945, in attempting to re-create American Indian tribal sovereignty through the Indian Reorganization Act of 1934 (see Washburn, 1984), the room had to be ritually cleansed by a presumed religious leader, James Youngblood Henderson, before the next session could go on!

Perhaps the most divisive ideological issue involving American Indians stemmed from the activities of the AIM, an urban Indian phenomenon that obtained enormous press coverage throughout the 1970s and 1980s because of the charismatic character of its various leaders, most notably Russell Means and Dennis Banks. The most publicized controversy involving AIM was the take over at the village of Wounded Knee, South Dakota, in 1973. AIM radicals, led by Means, demanded a change in the democratically elected (and allegedly puppet) Oglala tribal government. The occupation was eventually ended, and Means failed to overthrow the elected government, but one of the ugly by-products of the siege was the execution-style killing, in 1975, of two FBI agents, for which Leonard Peltier was convicted and sentenced to jail, where he still remains. In the meantime, millions of people around the world (twelve million in the former Soviet Union alone) have signed petitions demanding his release. In books such as Peter Mat-

thiessen's (1983) *In the Spirit of Crazy Horse*, authors have written that the evidence against Peltier is insufficient to establish his guilt. Matthiessen and others concede that Peltier was a member of the group that killed the agents, but Matthiessen asserts that it was another individual in the group who actually fired the fatal bullets. On elite campuses around the country support groups were set up in which the Wounded Knee issue was presented as a conflict between an oppressive United States government and victimized American Indians.

Academic anthropologists were generally supportive of such groups although they often recognized the tenuousness of the radicals' claims. Only one professional anthropologist (Carol Talbert) took part in the takeover of Wounded Knee, and she defined her role, in a conversation with me shortly afterward, not as an anthropologist studying a crisis situation but as a committed activist, ready to do the bidding of those in charge. In a note in the *American Anthropologist* (Washburn, 1980), I challenged Karl Schlesier's assertion that because the militants were the true representatives of the Oglala people, while the elected tribal government was by definition illegitimate, more anthropologists should have supported the faction taking over Wounded Knee. I pointed out that anthropologists should be faulted not for failing to enlist as ideological guerrillas under the AIM leadership against the Oglala Establishment but for failing to give us an accurate account and explanation of the event, as James Mooney, of the Bureau of American Ethnology, did with regard to the Ghost Dance movement of 1890. Fay Cohen (1976) and Steve Talbot (1979) have written accounts of the AIM but were not participants at Wounded Knee as Talbert was. The claims of the AIM radicals about the injustice done to Peltier have been undermined in recent years by accounts such as Scott Anderson's (1995) "The Martyrdom of Leonard Peltier," in *Outside Magazine*, and it may be significant that in his autobiography, Means (1995) mentions Peltier only twice, and there is no index reference to Peltier in the book.

Bill and I did not always disagree, particularly when we ventured outside anthropological realms. I remember vividly how we reacted with shared irritation to the first version of the exhibition *The West as America* at the Smithsonian's National Museum of American Art in 1991, which became a cause célèbre in the battle against political correctness. I had arranged to take the students in my graduate seminar on Material Aspects of American Civilization to the National Museum of American Art to look at the exhibition and to discuss it with the curators at the museum. The students got little opportunity to express their own views as Bill and I repeatedly challenged the curators about the character of their interpretation of the American West. One of the labels, for example, showed the same Indian girl before and after entering a Bureau of Indian Affairs school. The contrast in her clothing before and after putting on her school uniform was striking, but her impassive face was the same in both photographs—except in the label, which enjoined us to note how sad and despondent she became after being

forced into the white man's school! There were even more extraordinary labels, such as that describing Frederic Remington's painting, *Fight for the Water Hole*, in which a couple of desperate cowboys are preparing to defend themselves against a circling band of hostile Indians, as expressive of capitalism threatened by a resurgent labor movement. *Washington Post* columnist Charles Krauthammer (1991) keenly observed, "Thirty years ago in Moscow, I imagine, a party museum might have staged an exhibit of American art depicting the Western frontier as a chronicle of racist capitalist rapacity and called it 'The American West: The Origins of Imperialism.'" But, as Krauthammer went on to note, "Alas, they don't do it that way in Moscow any more. But they do do it that way in Washington." In the comment book placed in the exhibition, Daniel Boorstin, librarian of congress emeritus and previously director of the Smithsonian's National Museum of American History, wrote: "A perverse, historically inaccurate, destructive exhibit," and Simon Schama, then of Harvard University, wrote: "A relentless sermon, phenomenally condescending to both the painters and the painted."²

The growing activism among anthropologists often seemed to me to leave anthropologists committed not to some scholarly conception of truth but to an uncritical support of "my people, right or wrong." Sometimes these issues came up in the course of scholarly debates. For example, when Joseph Jorgensen and Richard Clemmer (1978) and Jorgensen (1978) critically reviewed *The Indian in America* (Washburn, 1975), their principal criticism, referring to the Indian Reorganization Act of 1934, was that Collier had "made up his own statistics, and perpetrated a good deal of deception in order to make it seem as though the Hopi were seeing things his way, when they were not" (Jorgensen and Clemmer, 1978:70). I was faulted for being fooled by Collier and for reporting in my book that the Hopi had legitimately accepted the act and the Hopi Constitution.

It was a simple matter to disprove the calculations of Jorgensen and Clemmer by referring merely to the published materials that they had carelessly used and incorrectly cited. But I went on to examine the manuscript record of the Hopi Census of 1934 in the National Archives in order to demonstrate how scandalously inadequate was what they had presented as their research. I then went on to analyze the work of Clemmer, in particular his doctoral dissertation (Clemmer, 1972), in which he conceded that he worked actively as a partisan for a small faction of the Hopi tribe, which he grandiosely referred to as the "Hopi Resistance Movement." In his dissertation, Clemmer also conceded some self-doubt about his role but brought himself "to an activist position that I could hardly justify not taking." Why? Because "if the anthropologist's goals are more than simply cultural, then his activist affiliation with a particular faction or ethnic group is hopefully a function of his goals on a more cosmic level and he becomes culture-bound only as an interpreter of certain universal laws or values" (quoted in Washburn, 1979:97). Clemmer's overt inter-

ference in the affairs of the Hopi caused me to cite his conduct to the Ethics Committee of the AAA as being incompatible with the guidelines established by the profession against such political involvement. As might be expected, I was not able to obtain a formal hearing on the subject on the grounds that my dispute with Clemmer was already being waged in the scholarly journals.

My challenge to Clemmer and Jorgensen was followed by other challenges to anthropologists who insisted on identifying minority radical groups within American Indian tribes as the true representatives of those tribes and dismissed any Indian tainted by the electoral process as somehow a tool of the Bureau of Indian Affairs.

My concern with the growing commitment of anthropologists to ideological radicalism led me to introduce a resolution at the annual business meeting of the AAA in Chicago, on 18 November 1983. My resolution followed a debate over the Yellow Thunder Camp, occupied by radicals in the Black Hills, South Dakota, in which individuals who were not Sioux professed to speak for the Sioux, and the only Sioux present noted that she was not authorized by her tribe to express an opinion. My resolution was as follows:

Be it resolved that any motion or resolution submitted by a member of the AAA for consideration by the membership either at its annual meeting or by mail ballot that professes to speak for the wishes of members of an American Indian tribe or nation include within its text or in an accompanying document the evidence on which the claim to speak for the wishes of members of the tribe is based. If the evidence does not include an affidavit or resolution from the legally constituted tribal government, indicating support for the proposed motion or resolution, then the evidence submitted with the AAA member's motion or resolution must include a statement indicating which individual Indians and or unofficial groups of Indians support the member's motion or resolution.

The resolution was fiercely debated and was defeated. David Aberle, of the University of British Columbia, was one of the principal opponents of the resolution.

At the same 1983 meeting at which my resolution was rejected, a motion opposing relocation under the Navajo-Hopi Land Settlement Act of 1974 was approved. That resolution was offered by 11 individuals, all of whom (as Bill pointed out to me) had worked with the Navajo rather than the Hopi in their fieldwork. The AAA subsequently established a panel on the Hopi-Navajo land dispute, which issued a series of reports in the *Anthropology Newsletter*; reports that I considered generally banal and unrealistic when they were not one-sided. Professors Thayer Scudder (California Institute of Technology), David Aberle, and Joseph Jorgensen (University of California, Irvine) were particularly concerned about how distressful relocation would be to the Big Mountain Navajo forced to move under the act. Appropriate answers were provided to their learned commentaries by United States Senator James Abourezk, who noted that "we did not need an anthropologist to tell us [that] almost without exception people resist forced relocation" (cited in Washburn, 1989:740), and by Abbott Sekaquaptewa, chairman of the Hopi negotiating committee, who noted that "the Navajo Tribe has yet to explain why great

psychological harm will be done if they move off Hopi lands, and similar harm is not done to them when they uproot their homes and families to move in on Hopi territories" (cited in Washburn, 1989:741). In a report on "Anthropological Advocacy in the Hopi-Navajo Land Dispute" (Washburn, 1989), I attributed the one-sided anthropological support of the Navajo to anthropology's instinctive concern with present injustice and methodological unconcern with historical injustice.

As a historian I have always looked with suspicion on anthropology's eager embrace of frequently changing theories. My greatest concern was with cultural relativism, which has been a mixed blessing for anthropology. On the positive side it has corrected some of the narrow, ethnocentric observations associated with those ignorant of other countries and convinced of the superiority of their own mores. But on the negative side it has led to increasing embarrassment in its failure to distinguish between cultural traits that violate the rights of other groups in the assertion of the validity of its own.

When I heard Clifford Geertz's Distinguished Lecture at the 1983 meeting of the AAA, entitled "Anti Anti-Relativism," I began to think about the dilemma cultural relativism posed for anthropology. I began to write a response to the Geertz piece, which I originally entitled "Anti Anti Anti-Relativism," a title that was replaced by the editors of the *American Anthropologist* by the title "Cultural Relativism, Human Rights, and the AAA" (Washburn, 1987b). Postulating that cultural relativism derived its continuing power as much from cultural anthropologists' hostility to the values of their own society as from any inherent theoretical logic, I began to play the historian again. In the National Anthropological Archives of the Smithsonian, I examined the materials relating to the "Statement on Human Rights" submitted to the United Nations in by the Executive Board of the AAA (see AAA, 1947), and I incorporated the powerful attack on Western civilization from that statement into the article that I submitted to the *American Anthropologist*. I will not rehearse the argument except to point out that the statement asserted that individuals must be free to participate in "the only right and proper way of life that can be known to them, the institutions, sanctions and goals that make up the culture of their particular society" (AAA, 1947:543). The thought was expressed more directly elsewhere in the statement that "man is free only when he lives as his society defines freedom" (AAA, 1947:543). Where such a philosophy leaves an individual in societies such as Stalin's Russia, Mao's China, Castro's Cuba, or Ortega's Nicaragua can well be imagined. I discussed the issue at greater length in an invited paper delivered at the 1995 meeting of the AAA in Washington, D.C., entitled "Cultural Relativism Versus Universal Human Rights."

The radicalism of anthropology extended even to archaeology as I learned during my association with the Society for Historical Archaeology. (Indeed, Colonial Williamsburg archaeology director Noel Hume and I attribute the spelling of the last two words in the society's name to our involvement in an organizational meeting to set up the society. I insisted that "historical"

was the right word and not "historic," as the fashion at the time had it, and he that "archaeology" was infinitely superior to "archeology.") At one of the meetings of the society, University of Maryland archaeologist Mark P. Leone and his students gave papers that attempted to demonstrate how the archaeology of the colonial city of Annapolis, Maryland, revealed the pattern of oppression characteristic of stratified capitalistic societies. The streets of Annapolis were seen as "ideology...masking a significant and potent reality" (cited in Washburn, 1987a:544), and gardens concealed the ideological reality of class conflict. Leone's definition of the critical method in archaeology stated that "critical theory is a set of varied attempts to adapt ideas from Marx to the understanding of events and circumstances of 20th-century life that Marx did not know" (Leone et al., 1987:283). I vigorously attacked Leone and his views in "A Critical View of Critical Archaeology" (Washburn, 1987a).

Perhaps an account of an incident in which I came face to face with the paradoxical and ironic character of the relationship between scholars and activists may serve to conclude and throw light on this difficult relationship. I was scheduled to give a talk at the University of California, Davis, one evening in the 1970s. As I always did when I was in the vicinity, I paid a quick visit to the AIM-run D-Q University, which had appropriated an abandoned military installation near Davis. Its head was then Dennis Banks. I was always accused of trying to demonstrate that the university was really a Potemkin village because I would always come in unannounced to see what, if anything, was going on. In any event, in this instance, Banks invited me to participate in a "sweat": a religious ceremony in which a group gathered in a small tent while hot stones, introduced from outside, produced a steam bath within. I was seated at the spot furthest from the opening of the tent, which was occupied by Banks. All around me were Indians of various tribes tightly packed together. I was the biggest person there and hence closest to the hot stones, which virtually seared my skin. As prayer after prayer was said, and stone after stone was intro-

duced, I realized that Banks was playing a trick on me, trying to get me to cry "Uncle" because of the discomfort. So I gritted my teeth and avoided any such remark. Finally a Navajo beside me said, "I can't stand it any more! I've got to get out!" Banks responded, "Put your face down to Mother Earth!" The edge of the tent provided what little air was available in the small space, other than through the opening controlled by Banks. "Fuck Mother Earth!" the Navajo responded, and bolted for the opening. This broke up the ceremony, and I was able to get out and go to Davis for my lecture. I have always remained an admirer of Banks, and impressed by his political skills. When I was in the Ainu village of Nibutani in Hokkaido, Japan, in 1995, I discovered Banks had recently been there and continued to exert an influence on foreign perceptions of the American Indian.

The battle to create the image of the American Indian continues to be waged among Indian radicals and Indian conservatives, as well as among anthropologists and historians of both radical and conservative persuasion. I hope my brief account will give some picture of the struggle as it was fought in the years in which Bill Sturtevant and I shared so many fond memories.

Notes

[These notes were added by the editors after the author's death.]

1. It became known during the 1992 presidential campaign that Bill Clinton had written in a letter of 3 December 1969 to Col. Eugene J. Holmes, Professor of Military Science and commander of the ROTC program at the University of Arkansas: "I am writing too in the hope that my telling this one story will help you to understand more clearly how so many fine people have come to find themselves still loving their country but loathing the military" (*Atlanta Constitution*, 13 Feb 1992:A9). This statement was widely referred to in the form of the paraphrase "I loathe the military," first by Clinton's critics and eventually in the mainstream media (for example, Tom Brokaw on *Hardball*, CNBC, 21 Dec 1998).

2. Photocopy of the comment book from *The West as America* exhibit, Smithsonian American Art Museum/ National Portrait Gallery Library.

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III. Worlds Transformed

“A Very Great Harvest of Souls”¹: Timucua Indians and the Impact of European Colonization

Jerald T. Milanich

The last several years have witnessed the appearance of new information and interpretations regarding the Timucua Indians who lived in what is now northern Florida and southern Georgia during the period 1513–1763 (e.g., Johnson, 1991; Worth, 1992, 1995, 1998a, 1998b; McEwan, 1993; Milanich and Hudson, 1993; Bushnell, 1994; Milanich, 1995, 1996; Hann, 1996). New details about the geography of Timucua-speaking chiefdoms allow us to recognize better who the Timucua were and to trace more successfully the respective histories of specific groups within the colonial period. Particularly important is our greater understanding of the tempo of population decline and the responses to it by the Spaniards in the seventeenth century, the period of the Franciscan missions.

In the early sixteenth century, native people who spoke dialects of the Timucua language occupied most of the northern one-third of peninsular Florida, apparently not including the Gulf of Mexico coast. The Timucua also inhabited southeastern Georgia as far north as the Altamaha River (Figure 1).

This is a large area, about 19,200 square miles (49,728 km²), of which 63% is in present-day Florida and 37% is in present-day Georgia. Before contact with people from Europe, approximately 150,000–200,000 Timucua lived in that region, a population density of 7.8–10.4 people per square mile. The higher density figure is almost the same as that derived by Keegan (1992:162–163) for the native population of the Bahamas at the time of contact with Europeans, and, as Keegan noted, it is very close to the density figures recently calculated for indigenous societies on Hispaniola and suggested for other chiefdoms.

The Timucua were never organized as a single political unit. Instead the people we refer to as the Timucua consisted of 25 to 30 small chiefdoms whose populations spoke dialects of the same language. These simple chiefdoms were organized around small groups of five to 10 villages. Each chiefdom was led by one dominant village chief who inherited his or her position through the ranking matrilineage. Documentary evidence suggests that in addition to hierarchies of civil chiefly officials, hierarchies of war chiefs were also present.

In several instances, chiefdoms formed alliances that served both offensive and defensive military purposes and probably had other functions as well. Alliances must have been present in precolumbian times—Hernando de Soto observed some in 1539 (Milanich and Hudson, 1993:177–178, 184)—and others may have formed in the early colonial period in response to the European military presence.

During the 50 years following Juan Ponce de Leon’s initial voyage to Florida, in 1513, Spain and France explored the coasts and portions of the interior of the southeastern United States, the land Ponce de Leon had named *La Florida*. In that period contacts between Europeans and Timucuan chiefdoms differed widely across the Timucuan region, resulting in dissimilar impacts in terms of disease introduction.

Shortly after the founding of the Spanish colonial settlement at St. Augustine, in 1565, the Timucua entered a new phase in their interaction with people from Europe. In 1573 Pedro Menéndez de Avilés, founder and governor of the La Florida colony, arranged for Franciscan missionary friars to minister to the Timucua. Missions would save the souls of the Indians while transforming the native people into loyal Catholic subjects of the Spanish crown who could be forced to labor in support of the colony.

After two decades of little progress, Franciscan efforts took hold in the 1590s. The mission friars systematically missionized each chiefdom, working through chiefly political structures while essentially assuming the roles once held by native priests (Worth, 1998a:44–78). By the 1630s nearly all of the existing Timucuan chiefdoms had received missions.

From the Spanish point of view, colonization of the Timucua was a success, even though the La Florida colony would never produce the profits envisioned by its founders. For the Timucua, however, colonization can hardly be deemed positive.

New perspectives on the Timucua are presented here, especially information concerning their geography and history and the tempo of the demographic disaster that befell them. These data are used as a backdrop for examining how the Spanish colony of La Florida sought to maximize exploitation of the Timucua Indians while contending with a continually dwindling population. The Franciscan mission system was intimately entwined with forced labor, directed culture change, and resettlement. Rather than being a benign system to save souls, missions were the right arm of colonialism.

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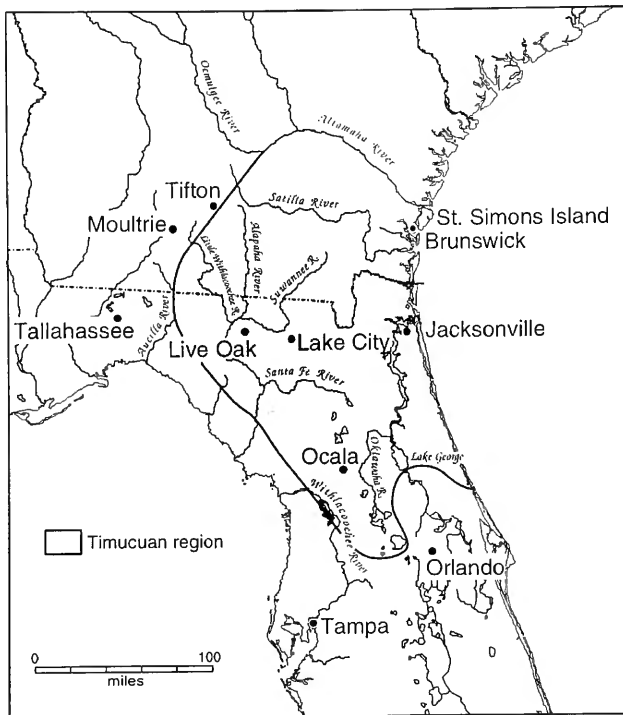


FIGURE 1.—The region of the Timucua.

Defining the Timucua

The people we today call Timucua are defined on the basis of language: Timucua Indians spoke dialects of the Timucua language. Delineation of Timucuan dialects comes almost entirely from the writings of Father Francisco Pareja, a Franciscan missionary friar who arrived in La Florida in 1595 and served Timucua Indians at the mission of San Juan del Puerto on Fort George Island north of the mouth of the St. Johns River until about 1625 (Milanich and Sturtevant, 1973). As was common for missionary religious orders in the Americas, Father Pareja translated religious treatises and other writings into the local native language—in this case, Timucua—even turning it into a written language using Spanish phonemes.

In his writings Pareja mentions nine Timucuan dialects: Agua Dulce (Fresh Water), Icafui, Mocama (Salt Water), Oconi, Potano, Acuera (or Santa Lucía de Acuera), Timucua, Tucururu, and Yufera. Were there other Timucuan dialects? We simply do not know for certain. It is obvious from Pareja's delineation of dialects that the greatest number were from the area nearest his home mission, probably the region he knew best. If the density of dialects were the same elsewhere in Timucuan territory, we would expect there was more linguistic variation in the region than is presently recorded.

For delineating the region of the Timucua speakers, the writings of the Spanish colonists, friars, and soldiers who traveled and lived among the native people are as important as those of

Pareja. In their day-to-day dealings with the native groups, the Spaniards knew who were and who were not Timucua. Thanks especially to the recent documentary researches of John Worth, Florida Museum of Natural History, and John Hann, Florida Division of Historical Resources, we can now list nearly all the native chiefdoms present in the 1590s, place them on the landscape, and trace their histories through the seventeenth century, a time in which their numbers grew fewer and fewer.

Another help in mapping the geography of the Timucua was to determine who were not Timucua speakers. We know, for example, that the Guale Indians north of the Altamaha River on the Georgia coast spoke a language very different from Timucua. A Muskogean language was spoken by the Apalachee Indians who lived west of the Aucilla River in northwest Florida, west of the Timucua Indians. North of the Timucua, north of the Altamaha River in interior Georgia, other Muskogean languages were spoken by the ancestors of the Creek Indians.

The southerly boundaries of the Timucua similarly can be delineated. The Mayaca Indians in the St. Johns River drainage south of Lake George spoke a non-Timucuan language. To the Timucua's southeast Ais was spoken, still another language, and to the southwest, toward Tampa Bay, several languages were spoken, all apparently non-Timucuan. A few place names in the greater Tampa Bay region as well as among the Mayaca Indians do bear similarities to Timucuan words, suggesting that the languages in those areas may have been related in some fashion to Timucua. When all of this information is correlated we can be reasonably confident that the territory of the Timucua Indians is the region shown in Figure 1.

Data at hand suggest that the various Timucuan chiefdoms living within this territory in the early sixteenth century can be correlated with six archaeological cultures. Figure 2 shows the locations of those chiefdoms; Table 1 lists their correlations with dialects and archaeological cultures.

Timucua Chiefdoms, European Explorers, and Early Spanish Missions

The first Timucua to come face to face with people from Europe probably were Mocama-dialect speakers, perhaps the Guadalquini, who lived on St. Simons Island, Georgia. In 1525 and again in early 1526, scout ships that preceded the Lucas Vázquez de Ayllón expedition landed on the northern end of St. Simons Island (Hoffman, 1994a).

Another incident of early contact took place in 1528, when the army of Pánfilo de Narváez marched northward from Tampa Bay (Hoffman, 1994b) and entered the territory of the Yustaga (also called Uzachile), a Timucuan chiefdom, or, more likely, an alliance of chiefdoms occupying a large region from what now is Madison County, Florida, into south-central Georgia up the Withlacoochee River drainage (east of the Aucilla River and west and northwest of the Suwannee). The Yustaga were the largest and most densely settled of the Timucuan groups. During the first half of the seventeenth century the

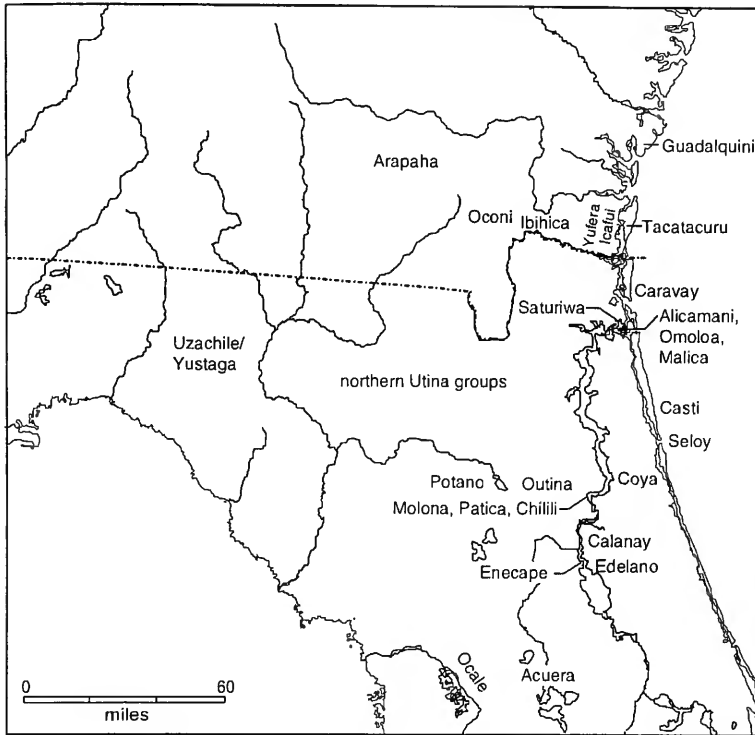


FIGURE 2.—Timucuan chiefdoms in the sixteenth century prior to the Franciscan missions. The Astina, Omittagua, and Onachaguara were probably near the St. Johns River somewhere between the Calanay and the Outina.

greatest number of Timucuan missions were found in their territory. Eight missions were established shortly after 1623, reflecting the size of the Yustaga population (San Pedro y San Pablo de Potohiriba, Santa Elena de Machaba, Santa Cruz de Cachipile, San Ildefonso de Chamile, San Matheo de Tolapatafi, San Miguel de Asile, San Francisco de Chuaquin, and San Augustin de Urihica) (Hann, 1990:470–476; Worth, 1992:63–71).

Following the Narváez entrada, Yustaga territory again was invaded by a Spanish army when the Hernando de Soto expedition marched northward from lower Tampa Bay in 1539. De Soto's army first entered Timucuan territory among the Ocale Indians, a group living near the Withlacoochee River in west-central Florida. In the early seventeenth century, a single mission (San Luís de Eloquale) served the Ocale. Founded by 1630 (Hann, 1996:189), the mission was located by 1655 among the Acuera Indians, another Timucuan group (Worth, 1998a:69–71, 1998b:189–190). At that time a second mission also served the Acuera (Hann, 1996:178).

North of the Ocale and the Acuera in northern interior Florida were other Timucua Indians also encountered by de Soto's army. One of these groups, the people we today call the Potano, may actually have been several small chiefdoms. De Soto expedition accounts record the names of five separate villages a day's march apart, arranged roughly from south to north along a trail that led from southwest of modern Ocala to the Santa Fe River (Itaraholata, Potano, Utinamocharra, Malapaz, and Cholupaha) (Milanich and Hudson, 1993:134–147). But nowhere

TABLE 1.—Timucuan dialects, archaeological cultures, and groups in the sixteenth century.¹

Dialect	Culture	Groups
Acuera	St. Johns	Acuera, Tucuru
Acuera (?)	Northern Safety Harbor	Ocale
Agua Dulce	St. Johns	Astina, Calanay, Chilili, Coya, Edelano, Enescape, Molona, Onachaguara, Omittagua, Patuca, Selay
Icaful	St. Marys (?)	Cascangue/Icaful, Ibihica (?)
Mocama	St. Marys	Alicamani, Caravay, Casti, Guadalquini, Malica, Napa, Omoloa, Saturiwa
Oconi	St. Marys (?)	Oconi
Potano	Alachua	Potano
Tucuru	St. Marys (?)	Mocama-area chiefdoms, Tacatacuru
Timucua	Suwannee Valley	Aguacaleyquen, Cholupaha, Napituca, Uriutina, Uzachile/Yustaga
Timucua (?)	Lamar	Arapaha
Yufera	St. Marys (?)	Yufera

¹The Outina, not included here, were probably no longer identifiable as a separate group when Pareja recorded information on dialects. The Northern Utina groups are those of the Suwannee Valley culture; the Mocama groups are those of the Mocama dialect.

in the accounts is any hint that these towns were unified or allied in any way. Possibly an alliance of small chiefdoms emerged after 1539 as a defensive measure in response to the passage of the Spanish army.

Such an alliance, headed by Chief Potano, did exist in 1564 and 1565 when soldiers from Fort Caroline, a short-lived

French settlement near the mouth of the St. Johns River, joined native war parties in an attack on the main town of the Potano alliance, killing many villagers (Milanich and Hudson, 1993: 173–174). In 1585 Spanish soldiers also raided the Potano, an incident said to be in retaliation for a 1567 Potano attack.

By the early 1600s, four missions had been established among the Potano (Santa Ana, San Buenaventura de Potano, San Miguel de Potano, and San Francisco de Potano) (Hann, 1990:441, 458–460), all well north of the town called Potano visited by de Soto and later raided by the French and Indian war parties. That town was probably abandoned by the time of the more extensive seventeenth-century mission efforts.

When de Soto marched north out of Potano territory he entered the region of the northern Utina, the name modern researchers have given to a number of allied Timucua chiefdoms living in the area now comprising Columbia, Hamilton, and Suwannee counties. From Cholupaha, near the site of the later Santa Fé de Toloco mission, the army moved westerly to the village of Aguacaleyquen, probably at or near the seventeenth-century mission of San Martín de Ayacuto. The chief of Aguacaleyquen led an alliance of northern Utina chiefdoms (Milanich and Hudson, 1993:154–157).

From Aguacaleyquen de Soto's army turned northward to a small village near modern Lake City, Florida. Then the expedition turned west to the town of Uriutina, probably near the later location of mission Santa Cruz de Tarihica. Farther west, another village, Napituca, near modern Live Oak, Florida, was the scene of a bloody battle between de Soto's army and the northern Utina. At least nine village chiefs and several hundred warriors were killed. Mission San Juan de Guacara, founded by 1612, was in southwest Suwannee County south of the location of the Napituca massacre. The army next crossed the Suwannee River into the territory of the Yustaga Indians, who avoided the Spaniards by abandoning their main village to them (Milanich and Hudson, 1993:158–167).

Northward from the Yustaga and the northern Utina in south-central Georgia lived other Timucuan groups, all poorly known. One of these was the Arapaha, who occupied the upper reaches of the Alapaha River. A mission (Santa María de los Angeles de Arapaje) had been established there by the early 1630s (Hann, 1990:470–471; Worth, 1992:70).

Another interior Georgia mission serving Timucua Indians (Santa Isabel de Utinahica) was north of the Arapaha at the confluence of the Ocmulgee and Oconee Rivers (Worth, 1992:76, 1995:124). Still another mission (Santiago de Oconi) served the Oconi, a Timucuan group who lived within the Okefenokee Swamp in southeastern Georgia (Worth, 1992:183, 1995:50–51, 1998b:190–191).

In southeast Georgia east of the Okefenokee Swamp were other Timucua groups: the Cascangue, Icafui, Yufera, and Ibihica. The Cascangue and Icafui, closest to the coast, may have been two villages within the same chiefdom, a polity consisting of nine or 10 villages (possibly including Lamale, Acahono, Tahupa, Punhuri, Talax, Panara, Utayne, and Huara) (Hann,

1996:155, 173). The Yufera were farther inland (Hann, 1996:11), and the Ibihica chiefdom was still farther west, just east of the Okefenokee Swamp between the St. Marys and Sattilla rivers (Worth, 1998b:191). All of these southeast Georgia Timucua groups were visited by friars in the early seventeenth century, and a mission (San Lorenzo de Ibihica) was founded there after 1612 (Hann, 1996:153–154).

Timucuan groups to the east along the Atlantic coast of Georgia included the Guadalquini on St. Simons Island and the Mocama on Cumberland Island. San Buenaventura de Guadalquini, on the south end of St. Simons, was established by 1609. It existed until the 1680s, when Indian attacks and raids by privateers caused abandonment of all the Georgia coastal missions, both the Timucuan missions and those among the Guale Indians, farther north (Worth, 1995:39–40, 195–196; Hann, 1996:175–176).

Another coastal mission, San Pedro de Mocama, was founded in 1587 in the town of Tacatacuru on Cumberland Island (Hann, 1990:437). It was one of the earliest Franciscan missions in La Florida outside the St. Augustine locality. Father Baltasar López used the mission as a base from which he traveled to minister to Timucuan groups living on the mainland as far inland as the northern Utina (Milanich and Hudson, 1993:175).

Missions were also established among Timucuan groups farther south along the coast. Santa María, on Amelia Island, was founded in 1606 at the town of Napa (Napica). Still another mission, San Juan del Puerto (founded in 1587), was on Fort George Island in the village of Alicamani (also spelled Alimacani) just north of the mouth of the St. Johns River (Hann, 1990:436; Worth, 1995:198–199). In the locality around St. Augustine were the Seloy Indians. Mission Nombre de Dios, the first Franciscan mission in Spanish Florida, originally served the Seloy and the native laborers who lived and camped in and around St. Augustine.

Inland from the northeast Florida coast and up (south) the St. Johns River were many more Timucua. Much of our information about these people comes from documents associated with the 1564–1565 French colony of Fort Caroline (e.g., Lawson, 1992) and from documents generated during the early years of St. Augustine (e.g., Barrientos, 1965; Solís de Merás, 1964). Chief Satoriwa, whose main village was on the south bank of the St. Johns River just inland from its mouth (near Fort Caroline), headed an alliance that, the French were told, included 30 chiefs, all living on the lower portions of the St. Johns River or nearby on the coast.

South of Satoriwa's alliance in the St. Johns River drainage was another large Timucuan alliance headed by Chief Outina.² Outina's main village was west of the river on the trail that would become the mission-period *camino real*, the main route between St. Augustine and the interior missions (Johnson, 1991).³ In 1564 Outina's alliance, which maintained belligerent relations with both the Satoriwa and Potano alliances, was said to include 40 chiefs and villages. One village, Encape, is the

Mount Royal archaeological site just north of Lake George and the location of a mission (San Antonio de Encape) founded near the beginning of Franciscan mission activity in the 1590s (Hann, 1990:439–440, 1996:142).

Timucuan Demography

Between 1587 and 1630 all of the Timucuan chiefdoms still in existence apparently received missions, but some chiefdoms already had disappeared. Comparing Timucuan chiefdoms in the mid-sixteenth century with those present in 1600 leaves no doubt that those chiefdoms in greatest contact with Europeans in the sixteenth century were devastated by epidemics even before missions were established. During the seventeenth and early eighteenth centuries the Timucua living at the missions would continue to suffer epidemic-caused depopulation.

By 1600, as the first missions were being established, the Timucuan populations that had sustained the longest and most intense contact with people from Europe were those around St. Augustine and those from the many villages of the Saturiwa and Utina alliances. Essentially this was all the Timucua from Lake George north along the St. Johns River to its mouth and the Atlantic coast to the east. After 1564 and the building of Fort Caroline, first French and then Spanish colonists interacted frequently with the Timucua of this large region.

Those interactions served to introduce diseases. By 1600 the entire population of the area of most intense contact—the lower St. Johns River drainage and the coast—had been decimated; entire chiefdoms were gone. As a consequence, less than a handful of widely-spaced missions were needed to serve all of east Florida: Nombre de Dios in St. Augustine, San Juan del Puerto on Fort George Island to the north, and San Antonio de Encape far to the south on Lake George.⁴

As missions were established, more devastation of the Timucua took place. The first mission-period epidemic is documented for 1595. Then in 1612–1617 a series of epidemics were said to have killed half the mission villagers in La Florida (in Hann, 1988:175; Worth, 1998b:10–11). According to the governor of La Florida, another epidemic in 1655–1656 resulted in many of the Timucua being “wiped out with the sickness of the plague and smallpox” (in Hann, 1986b:111).

Groups like the Potano suffered mightily. Even before 1612, two of the four original Potano missions were abandoned, and the surviving villages were unable to provide sufficient adult males to fill their conscripted labor quotas demanded by St. Augustine (Worth, 1992:54, 165). By 1659 the Potano were effectively gone, and the mission of San Francisco de Potano was said to need repopulating (Hann, 1986a:375–378). Potano territory was under the control of a northern Utina chief and had been for some time.

Because of the presence of the missions, we have a reasonably good archival record of these seventeenth century epidemics, but no such record exists for the pre-mission period (ca. 1595) of interior northern Florida. Even so, based on the evi-

dence from the St. Johns River drainage, it is clear depopulation occurred prior to the missions. The best estimate for the total Timucua population in 1600, early in the mission period, is between 13,500 and 27,000 (Worth, 1998b:8), reflecting a sixteenth-century demographic decline of at least 82% from the 150,000 figure at initial contact. At that time (1600) only 14 chiefdoms still existed, roughly half the number present at contact. But even these statistics are not as horrible as those from the seventeenth century: by century's end, the Timucuan population had dropped to about 1,000 people.

Although depopulation would eventually have doomed the missions and the La Florida colony to failure, that end was hastened by the establishment of British colonies in Virginia and the Carolinas. Beginning in the 1660s and increasing in the 1680s, raids on the Timucua and Guale Indian missions by native slavers, actively abetted by British-backed interests, pressured the missions (Milanich, 1996:204–208; Worth, 1995:15–46, 1998b:140–146). Some Indians fled, more were captured; whole villages were abandoned. The raids would also lead to the resettlement of non-local Indians at old and new missions in northern and northeastern Florida as well as at the Guale Indian missions on the Georgia coast.

The end of the Timucua missions in the interior of La Florida came in 1702–1705, when several large Carolinian raids destroyed them (as well as those of Apalachee). Survivors moved to new towns close to St. Augustine, where they continued to live at missions served by Franciscan friars.

A 1717 census lists 10 of these refugee villages, three of which housed the approximately 250 Timucua Indians who survived. By 1726 that number had dropped to 157 Timucua, and two years later it was down to 70. In 1752 there were only 29 Timucua, all living in a single town (Hann, 1996:322–323; Worth, 1998b:150–156). A decade later, in 1763, when Spain relinquished La Florida and St. Augustine to the British, only a single Timucua Indian was listed on the roster of native people shipped to the town of Guanabacoa in Cuba (Milanich, 1996: xv–xvi, 216).

The Timucua and Colonialism

Given the colony's need for labor, the realities of Timucua geography and political organization, and the ongoing demographic catastrophe, how did Spanish officials deal with the situation? What was the impact on the Timucua? And for what purposes were Indians used as laborers?

La Florida was a royal colony that depended on a yearly subsidy to fund its military and administrative needs. In effect it was a money loser, supported for geopolitical purposes. One way it could help sustain itself and even turn profits for some residents was to find a source of cheap labor. That source was the mission Indians.

The missions harnessed the Timucuan villagers for Spanish use. The system of forced servitude known as *repartimiento*, with its labor quotas for each chiefdom, was organized through

the mission villages and village chiefs. An order would be issued by Spanish officials listing the number of adult males each mission would provide. Because missions were located in the main village of each chiefdom, the Spaniards presented the quotas to the respective village chiefs, who then ordered their subjects to comply and organized the sending of the laborers to St. Augustine.

Technically, the laborers were to be paid for their work, but most often they were not paid, or they were paid in trinkets collected by their chiefs. The Spaniards actively worked through the existing native political systems, supporting and paying chiefs and helping them to maintain their status. The Republic of Indians (Bushnell, 1989), though ultimately ruled by the Spaniards, continued to exist throughout the mission period.

The major export of the colony was corn. Corn also provided a significant amount of the diet for the residents of St. Augustine. As a consequence, a great deal of Indian labor went into the production, processing, and transportation of corn. Corn was produced at Spanish-owned haciendas, at missions in Franciscan-controlled fields, by order of entrepreneurial chiefs, and in fields around St. Augustine. Native people prepared fields, planted, hoed, harvested, shucked, and ground meal. The meal was then transported to St. Augustine or to the Gulf coast on the backs of conscripted laborers. Some was used in St. Augustine; larger amounts were exported. In both cases profits went to friars, hacienda owners, Spanish officials, and, sometimes, chiefs. Corn was a money maker.

Native men were also conscripted to go to St. Augustine to provide labor for projects there, or they remained there after transporting corn or other supplies. Laborers, some of whom spent as many as six months of the year in town, tended fields there, and they helped to build forts and other buildings, worked as servants, cut timber, and mined coquina on Anastasia Island near St. Augustine. During the 1670s several hundred native people were involved in the construction of the stone fort, the Castillo of San Marcos.

Working as beasts of burden and living in and around St. Augustine added to health problems and increased the native populations' susceptibility to the epidemic diseases that periodically swept through mission towns and St. Augustine itself. Travel along the camino real or other trails back to the missions quickly introduced these diseases to mission villages.

In the mission provinces, the Timucua and other mission Indians maintained trails by clearing brush, repairing creek crossings, and even building bridges. Where roads crossed rivers too deep to ford, villagers operated ferries. At the missions, men, women, and children performed various tasks in support of the friars and the mission system. The missions and St. Augustine could not survive without the mission Indians.

The very existence of La Florida depended on native labor. When mission village populations fell because of epidemics, the colony's survival was threatened. To assure bodies for labor, Spanish officials employed several strategies, including moving populations, consolidating remnant groups or even en-

tire villages, and capturing Indians who had opted to flee mission life. All the Timucua were brought to missions; non-Christian Indians were not tolerated in the mission districts.

In the earlier decades of the seventeenth century, the worst depopulation may have been in those missions closest to St. Augustine or those where sustained contact had been longest. The Potano, the first interior Timucuan chiefdom to receive full-time friars, are one example, and there are others. For instance, by mid-century the people from Santa Isabel de Utinahica, at the confluence of the Ocmulgee and Oconee Rivers, had been relocated to St. Simons Island on the coast, which needed repopulation (Worth, 1992:76, 1995:124). Timucua from southern Georgia were also moved to the Oconi mission to supplement the dwindling population there. Sometime between 1630 and 1655 the Ibihica were combined with the Oconi, most likely because of on-going depopulation.

Several years later, the combined remnants of the Ibihica and Oconi were ordered to relocate elsewhere. When they refused, a Spanish raid destroyed them. Republic of Indians or not, failure to obey could be interpreted as a capital offense. Why would the Spaniards destroy the very labor force they were working to sustain? They probably wished to make a point. But they already may have written off southeast Georgia. After the destruction of the Oconi-Ibihica village, the names of the Timucua from that region no longer appear in documents (Hann, 1990:464; Worth, 1998b:44–45, 117). Presumably they were gone, although remnants may have been moved to Timucuan missions elsewhere.

Other examples of chiefdom consolidation are also found in the documents. The Ocale and their entire mission were moved among the Acuera Indians by 1655. Later, as their numbers continued to drop, the Ocale and Acuera were resettled on the St. Johns River. Still later they were moved north to a town on the St. Johns River, where they were to operate a ferry (Worth, 1998b:189–190)).

The number of movements that took place in the early seventeenth century as a result of depopulation may greatly exceed the instances we can glean from an incomplete documentary record. For many decades, archaeologists have noted the presence of quantities of non-local types of aboriginal pottery at the Timucua missions in the territories of the Potano and northern Utina. Most of these ceramics are Lamar culture types from south-central Georgia, including the Altamaha-Oconee-Ocmulgee River region, now thought to have been inhabited by Timucua Indians. One hypothesis is that Timucua Indians from this Georgia region were moved south in large numbers to repopulate Timucua missions in northern Florida (Milanich, 1996: 104–107). If so, the depopulation of northern Florida in the early seventeenth century may have been even worse than thought.

By 1656 the efforts to maintain adequate population levels in the Potano and northern Utina missions to satisfy labor needs had failed. A small-scale rebellion in that year provided the governor of La Florida with an excuse to redistribute the

interior northern Florida missions and their Timucuan villagers to maximize their use by the Spaniards (Worth, 1992, 1998b: 38–116). The rebellion, an attempt by Timucua chiefs to free themselves from the demands of the Spanish military government, was soon put down with little loss of Spanish life.

Faced with inadequate labor to maintain trails, operate ferries across rivers, and provide other services, the governor ordered the abandonment of some missions and the relocation of others. A few new missions were founded, and populations were moved, all in an attempt to distribute the remnant Timucuan population along the camino real leading to Apalachee.

As a result, the post-rebellion Timucuan mission system west of the St. Johns River was very different from the one that had existed before the rebellion. Nearly all the remaining Timucua now lived in mission villages along the camino real, which they maintained. Their villages, spaced a day's travel apart, became way stations for travelers and bearers going back and forth to St. Augustine. Chiefdoms that once consisted of five to 10 villages and several thousand people each were reduced to a single village with a population of only a few hundred at best. Within some missions, villagers might actually have been Timucua Indians moved south from Georgia.

The camino real was a pipeline into northwest Florida, whose missions and agricultural fields became the colony's breadbasket. Productive haciendas and a growing Spanish population living in Apalachee at the San Luis mission were connected to St. Augustine by the road and its Timucuan way stations.

This post-rebellion reorganization of the Timucuan missions west of the St. Johns River has been documented through both archival research and archaeology. We now know, for instance, that Santa Cruz de Tarihica, located at the Indian Pond site in modern western Columbia County, Florida, was moved southwest to near Little River Spring just north of the Suwannee River and adjacent to the camino real (Worth, 1992:59, 311–312, 1998b:173–174). And we can trace the shifting of San Juan de Guacara from the Charles Spring site west to the Suwannee River at Charles Spring, where the villagers ferried travelers on the camino real across the river (Worth, 1992:312).

Relocations and resettlement continued in the years following the rebellion—attempts to keep the Timucua missions along the camino real populated. Arapaha Indians from Georgia were slated to be moved south to repopulate Santa Fé in modern Alachua County, Florida, although it is uncertain if that movement took place. Two years later, Yustaga Indians were moved to the same mission (Hann, 1990:460), and others were sent to repopulate San Martín de Ayacuto (Hann, 1990:461; Worth, 1998b:169), the northern Utina mission near the Ichetucknee River (Weisman, 1992). By 1670 four villages of

Arapaha Indians, perhaps all of those people who remained, had been moved to Yustaga.

All these efforts, however, were in vain, and the Spaniards were forced to begin to move non-Timucua Indians into what had been Timucuan territory in order to have laborers. Abandoned due to Timucua depopulation around 1665, mission Santa María on Amelia Island was resettled by 1673 by Yamasee Indians, only to be abandoned again in 1683 and then reoccupied the next year by Guale Indians moved from the Georgia coast (Worth, 1995:197–198). San Antonio de Encabe on the St. Johns River at the southern end of Timucua territory likewise was abandoned after 1655 because of depopulation and then reoccupied between 1679 and 1680 by Yamasee Indians (Hann, 1990:504–506).

Even this strategy did not work. Yamasee Indians succumbed to diseases just as the Timucua had before them. By the 1690s the northern peninsular Florida missions could not supply laborers, and that portion of the mission system was in danger of collapsing. Spanish officials then looked to central and southwest Florida, well south of Timucuan territory, for new conversions. Missions would turn the Jororo and Calusa Indians into Christians who later could be moved to northern Florida. But the missions established among those groups were not successful.

In the end, the attempts to deal with the realities of Timucuan depopulation all failed. The Carolinian raids of the first few years of the eighteenth century were the final straw. By 1710 all of northern Florida was barren. For the Timucua, colonization had brought “a very great harvest of souls.”

Notes

I am grateful to John Worth, who critiqued a draft of this chapter and made information available from his forthcoming books, now published, on the Timucuan missions. Bill Merrill and Ives Goddard deserve gold stars for their editorial labors in my behalf. Many years ago, while I held a National Endowment for the Humanities post-doctoral fellowship at the Smithsonian Institution, Bill Sturtevant kindled my interest in the Timucua Indians. Always the consummate scholar, Bill has been a mentor and friend since.

1 A 1617 comment by Franciscan friars regarding one result of the epidemics of 1612–1617 (in Hann, 1996:174).

2. Chief Outina and the Outina Indians were well east of the northern Utina Timucuan groups.

3. The camino real was not a single trail. In places it forked, providing alternative routes—a high road and a low road—that later merged. Early in the mission period it may not have even been the main mission trail. At that time the missions and the associated native villages were connected by a complex network of trails.

4. There are two exceptions, both of which were short-lived. For a year or two in the late sixteenth century, a friar was at the village of Moloa (Omoloa) on the St. Johns River north of modern Jacksonville (Worth, 1998a:59), probably ministering to remnants of the Saturiwa alliance. Similarly, mission San Sebastian on the opposite side of St. Augustine from Nombre de Dios briefly served a remnant population relocated from the St. Johns River (Hann, 1990:435–436; Worth, 1998a:45).

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The Interstices of Literacy: Books and Writings and Their Use in Native American Southern New England

Kathleen J. Bragdon

Introduction

As scholarship on vernacular literacy (i.e., native-language literacy that is adopted by previously nonliterate peoples, usually as a result of the establishment of colonial rule) continues to expand, the emphasis remains on the content, style (e.g., metaphorical structure and imagery), and political significance of newly adopted native-language literacy in colonial or postcolonial settings (e.g., Fabian, 1986; Burkhart, 1989; Besnier, 1991; Irvine, 1993). This important body of research emphasizes the various ways in which vernacular literacy functions within specific cultural and historical settings, and it highlights the complex relationship between vernacular literacy, the processes of colonialism, and the ways in which cultures are said to transform and reinvent themselves. Related investigations into the “ethnography of reading” have also directed attention to the cognitive and cultural significance of visual signs (Boyarin, 1992; Harris, 1995). An aspect of vernacular literacy that has received less attention is what Grey Gundaker (1993) has called the “peripheral” uses of literacy: the way in which peoples use writing in art and ritual and as a means of symbolic expression aside from, or in addition to, its function as a graphic representation of speech (see also Schousboe and Larsen, 1989). This essay examines these uses of literacy and the social importance of books and writings in native southern New England in the colonial period between the years 1660 and 1775.

The Origins of Native American Vernacular Literacy in Colonial Massachusetts

At the time of contact with Europeans, the indigenous people of southern New England, including the Massachusetts-speakers (also called Pokanoket, and later, Wampanoag) in southeastern Massachusetts and their neighbors the Narragansett, Pequot, and Mohegans, in Rhode Island and Connecticut, were coastal horticulturists whose still-heavy reliance on game and marine resources encouraged a “conditional sedentism” (Dunford, 1992) with an estuarine focus. According to all contemporary

observers, established hereditary leadership and elaborate social hierarchy, made possible by such an adaptation, were characteristic of the Massachusetts, with social status being marked by, among other things, an elaborate protocol, etiquette, and ritualized speech (Bragdon, 1987). Coastal societies were also linked to one another through networks of kinship and trade (Salwen, 1978). These linkages did not, however, preclude significant dialectal and language variation within the region or a strong likelihood of multilingualism, at least among the elite (Bragdon, 1993).

The vast literature on the contact period in the northeastern United States has by and large ignored the subject of linguistic exchange, Ives Goddard’s (1977, 1978, 2000) work on pidgins being the notable exception. Many explorers record without comment evidence of a widespread knowledge of European languages exhibited by their Indian trading partners and guides. William Wood, who lived near Naumkeag (now Salem), Massachusetts, remarked,

They love any man that can utter his mind in their words, yet are they not a little proud that they can speake the English tongue, using it as much as their own when they meet with such as can understand it, puzzling stranger Indians, which sometimes visit them from more remote places, with an unheard language. (Wood, 1977:110)

Yet native people were fully alive to linguistic distinctions. Rhode Island founder Roger Williams’s Narragansett friends remarked on their own and other’s linguistic identity with phrases such as *nippenowântawem* ‘I am of another language,’ and *penowantowawhettûock* ‘they are of a divers language’ (Williams, 1936:9). In an instructional dialogue illustrating a putative greeting are the phrases *Mat nowawtau hettê mina* ‘we understand not each other’ (Williams, 1936:9), *eenântowash* ‘speak Indian’ (Williams, 1936:8), and *npenowauntawâumen* ‘I cannot speak your language’ (Williams, 1936:55).

Skillful speech was highly valued, as Wood recorded:

In serious discourse our southern Indians use seldom any short colloquiums but speak their minds at large without any interjected interruptions from any, the rest giving diligent audience to his utterance. (Wood, 1977:110)

Williams (1936:56), too, recognized an emphasis on speech skills among the related Narragansetts, noting, “if they be eloquent, they esteeme them as Gods.” The use of mnemonics in the powwow’s (shaman’s) curing rituals and prophecies, in

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greeting rituals, and in the use of practiced insult between enemies, as well as "great action" in the sachems' "harangues" (Winslow, 1910:274), all point to a strong cultural emphasis on speech and its delivery, manipulation, and use as an art form (Bragdon, 1987). Because much of the elaborated speech and speech behavior recorded for the Massachusett and Narragansett was that of powerful and/or high-status members of their communities, it appears that skilled speech and status were interrelated. Social hierarchy was thus marked and reinforced by the ritual, formal, and public nature of speech.

Although we can be fully confident of the important role of the spoken word in the formation and expression of group and individual identity among the American Indian peoples of southern New England, we know less about the visual arts in the precontact and early contact period. Descriptions by Europeans affirm that native clothing, basketry, pottery, and other objects, as well as human bodies, were decorated with "antic" figures (Wood, 1977:108), drawings or tattoos depicting abstract designs, and with representations of animals, birds, and other images of significance lost to non-Indians (e.g., Williams, 1936:165, 191; Wood, 1977:103). These descriptions conform to petroglyphs and pictographic images found on navigable stretches of the Taunton River and in the Narragansett Bay region, including the famous Dighton Rock (Delabarre, 1928:204) (Figure 1). Regionally, these signs and images show many similarities and appear to represent or remark upon vision quests and other shamanic activities (Bragdon, 1995). Images similar to these petroglyphs are also found on seventeenth century documents as the signs or marks of sachems and others participating in land transactions or treaties. Williams (1936:42) noted that when writing was introduced to the Narragansetts, they called it "painting, for that comes the nearest." While cer-

tainly not representations of the spoken word, these images constituted a visual code that evidently was widely understood.

Tragic losses in epidemics of 1617–1619 and 1633 and the defeat of the powerful Pequot in 1636 caused widespread disruption in all regions of native southern New England, and some native people, particularly those of the interior regions, fled west or northward out of the reach of colonial rule. Many of those who remained adopted, at least nominally, a Protestant Christianity and settled or were confined to a number of communities, some of which were called Praying Towns (Salisbury, 1974). Although many of these were destroyed during King Philip's War, in 1675–1676, a surprising number survived, particularly on Cape Cod, Martha's Vineyard, and Nantucket Island, where native Christians and nonbelievers lived together in communities organized around traditional principles of hereditary leadership, communal land ownership, and shared community resources (Conkey et al., 1978:177; Bragdon, 1981).

The "print" culture of Puritanism in the seventeenth century led John Eliot and other missionaries, funded by the Society for the Propagation of the Bible, to develop the justly famous Indian Library (Kellaway, 1962). The entire Bible, translated by Eliot with significant contributions from a skilled Massachusett-speaking consultant, appeared in 1663 and was preceded and followed by translations of at least a dozen significant works by theologians, such as Richard Baxter, Lewis Bayley, and John Cotton, and, later, a grammar, a set of dialogues, and a primer (Eliot, 1666, 1671, 1672; Pilling, 1891). Eliot also embarked on a vigorous campaign of education in native literacy, developing a Roman-based orthography that was suited to the sound system of Massachusett and was amenable to rapid learning, as well as being easily printed. His efforts soon exceeded his own expectations, as indicated in a letter written in 1651:

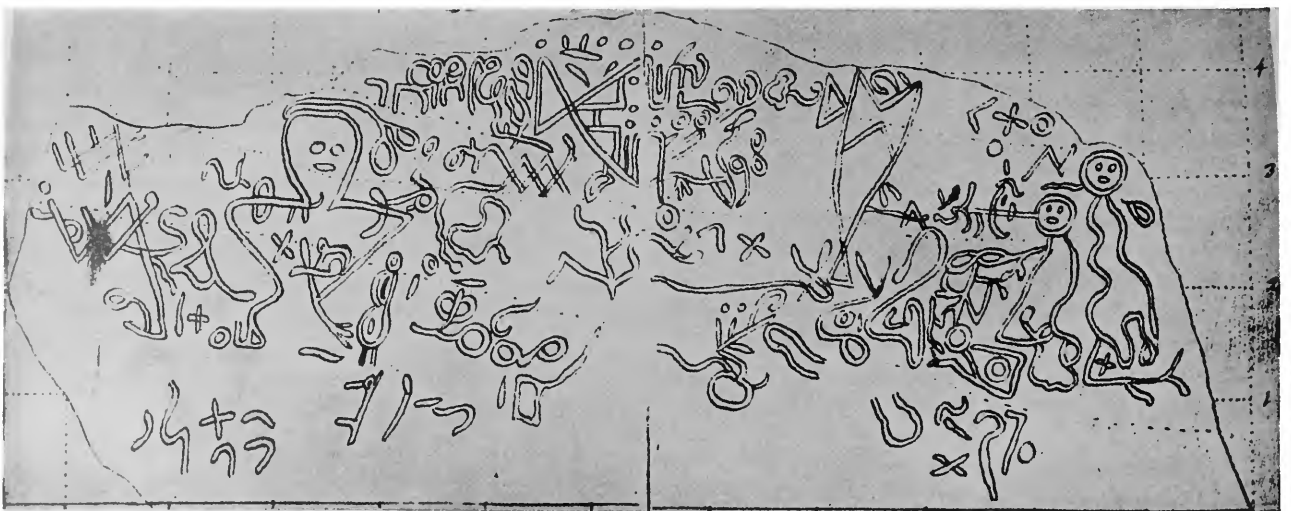


FIGURE 1.—An artist's rendering of pictographs on Dighton Rock. (After Delabarre, 1928).

It hath pleased God to stir up the hearts of many of them this winter to learn to read and write, wherein they doe very much profit with a very little help, especially some of them, for they are very ingenious. And whereas I had thought that we must have an Englishman to be their Schoole-Master, I now hope that the Lord will raise up some of themselves, and enable them unto that work.¹

The small cadre of English missionaries dedicated to introducing vernacular literacy into native communities was soon joined by a larger number of native teachers who taught reading and writing skills throughout the region, such that by the end of the seventeenth century nearly 30% of the native population—men, women, and children—could read and write (Bragdon, 1981:55).

The native emphasis on public speaking as an art form and the possible influence of a long tradition of pictographic representation, coincident with the Puritan text-dominated missionary effort, created the conditions for an “oral literacy” within these native Christian communities.

Literacy and Society among Massachusetts-Speaking Communities

Massachusetts vernacular literacy embraced several activities, cross-cutting a number of traditional cultural domains. In formal schools, and at home, students worked with primers and other teaching aids and studied translated texts. Literate individuals, usually men, kept a variety of records, including marriage records, deeds, and town proprietors’ records. Native officials published marriage banns and issued warrants. One diary or account book in Massachusetts is known, and letters were also exchanged (Goddard and Bragdon, 1988:13–23). The structure and content of the surviving writings in Massachusetts suggest that rhetorical style and formal protocol, particularly that demanded when peoples of differing social statuses interacted, was preserved in written form. The Massachusetts case appears to support Chafe and Tannen’s (1987:398) argument that writing was inherently social, and that reading and writing were “inextricably” tied to speech, even in those modes of discourse that seem most exclusively a matter of writing and reading.

The most prominent use of literacy in daily life among the Massachusetts speakers was in the practice of native Christianity. Much of the reading and writing that was taught was taught within the context of religious instruction, or in preparation for it: the Bible, particularly the Psalms and other books of the Bible, was used in religious services. Those literate, bilingual native students who were educated by private tutors and at Harvard’s Indian College were destined for the ministry.

Strong evidence suggests such religiously oriented literacy was highly public and social in nature. Native ministers incorporated Biblical texts into their sermons (e.g., Hossueit, n.d.). The service consisted of reading, response, and textual exegesis (Bragdon, 1991), but it may have been conducted in an informal setting, often the home of one of the members of the congregation. Annotations in some of the surviving native-owned Bibles suggest that copies were sometimes carried to

the services, where their owners followed along in the texts and made their own notes. For example, the Bible owned by the Pilgrim Society of Plymouth, Massachusetts (Eames no. 46),² contains the following passage in Massachusetts:

At this time...on February 7, 1715,...have already come give great snows. ...At this time, in the house of Pammohkauwut who lives in Duxbury, it looks like...its a storm....But we Indians still survive well on this morning. (Goddard and Bragdon, 1988:447)

Not all church members were literate, but all participated in literacy to the extent that they interacted with a reader, and were connected to the sermon through references to the written word. This participation was amplified by the frequent use of psalmody and the “lining” of hymns, whereby a singer would sing a line from the text, and the congregation would repeat it, alternating line by line for the length of the hymn (Becker, 1982). Native converts on Martha’s Vineyard also read to one another as part of daily religious practice, often conducted at home, with family members and neighbors (Mayhew, 1727; Monaghan, 1990).

The public nature of literacy is also reflected in the vernacular writings in Massachusetts. A brief survey of original documents reveals a number of characteristics that suggest the sociability of the context in which they were created. Many include lists of numerous witnesses, whose verbatim statements are often attached. Petitions, of which four are known, appear to have been read aloud at group meetings, and in three cases they are accompanied by a complete listing of those present.

The inextricability of speech and writing for the Massachusetts is further underlined by the immediacy of expression characteristic of many of the vernacular writings. Elizabeth Little (1980b:63) has pointed out that most of the Nantucket native-language deeds are in reality “recorded oral land transfers.” Direct quotation is the standard form of reported speech in Massachusetts, and these writings preserve direct quotations from principal participants in land transfers or at public confirmations of title.

Petitions also appear to reflect a similar immediacy. Standard expressions of humility, like “we beseech you,” and “we are poor,” characteristic of the speech of petitioners in the early contact period, also appear in written petitions. In addition, certain rhetorical expressions characteristic of speech were sometimes employed in written petitions, such as the exclamatory *Woj* ‘Oh!’ (Goddard, 1993:404). It is important to point out that such expressions are not merely remnants of the oral mode that survived into writing; rather, they are expressions of the ongoing sociability and orality of literacy among the Massachusetts-speaking people.

The physical condition of these documents and manuscripts, and the marginal annotations found in several copies of the Massachusetts Bibles and other published books, also suggest that both reading and writing for the Massachusetts was a contextualized, community event. Some documents show physical evidence of reuse, scrutiny by more than one writer, or revi-

sions, as though the documents were carefully preserved as relics or aids to memory. A multipart document now located in the Huntington Library, San Marino, California, and transcribed and translated line-for-line by Goddard and Bragdon (1988:60–65), is an example (Figure 2). It details a two-step conveyance of land on Martha's Vineyard at East Chop (Ogkashkuppeh). It has three separate records in Massachusetts, two on one sheet of paper, and a third on a separate piece attached to the base of the first. The first record, located at the top of the sheet, is dated 22 September 1706 and records the transfer of land from Thomas Dila (or Tilla) to Nathaniel Cuper. The second record, on the same sheet, conveys the same piece of land from Cuper to John Talman and is also dated 22 September 1706. The third record, on the attached sheet dated 14 November 1706, is a confirmation of Talman's tenure by Samuëll Makkinnit and Qunnagkoo, who name themselves "defenders of the sachemship" (Goddard and Bragdon, 1988:63, 65). The writer of the third record has added lines to both of the previous ones. In the first, the writer added the words "I Thomas Tilla, this land that I convey cost three pounds, (that) which I let Nathaneill Cuper have. I Nathaneill Cuper say it was done; I bought it for three pounds." The same writer added similar statements to the second record. These conveyances, to which were appended the evidently important verbatim verbal exchanges of the buyers and sellers, document the continuing importance of oral agreement, the presence of numerous witnesses, the archiving of written records, and the participation of several writers and readers in the creation of the manuscripts (Goddard and Bragdon, 1988:20–22).

Particularly interesting examples of the way in which native people used writing and books to reinforce or create a sense of community are the annotations in several copies of Massachusetts Bibles owned by native speakers of that language. The largest percentage of surviving copies of the Bible are presentation copies, which were specially bound or had distinctive title pages in English, being designed as gifts to benefactors of the missionary work in New England or for clerics and other interested English scholars. Very few Indian-owned Bibles are known to have survived, but of these, at least eight have annotations and other marginalia in Massachusetts (Goddard and Bragdon, 1988:374–465). Two other books, a copy of *Massachuset Psalter* (Mayhew, 1709) and a copy of *Manitowompae Pomantamoonk* (Bayley, 1685), were also annotated in Massachusetts (Goddard and Bragdon, 1988:466–471).

Surviving Bibles and other books with annotations in Massachusetts show evidence of multiple owners and/or users. The annotated *Psalter*, for example, contains the names of at least two native Massachusetts writers, Solomon Omppan and Benjamin Job, of Martha's Vineyard, and it contains marginal notes dating to 1720 and 1732 (Goddard and Bragdon, 1988: 466–468). The Bible owned by the Library Company of Philadelphia, in Pennsylvania (Eames no. 45), contains the passage "This is Papenau's Book. I am looking after it" (Goddard and Bragdon,

1988:417). The Bible owned by the Pilgrim Society contains annotations in the handwriting of several individuals, including Benjamin Kusseniyett, Francis Ned, and Josiah Ned (Josiah Attaunit), presumably of Plymouth. (Goddard and Bragdon, 1988:447). Annotations are often messages, as if to other readers, such as the admonition in the Bible owned by the native minister Joseph Papenau, of Falmouth and Bourne (later owned by the minister Zachary Hossueit of Gay Head): "This is your book, you Papenau. Read it with concentration. Your God will bless you." (Goddard and Bragdon, 1988:423). Other native-owned Bibles contain end pages and other blank pages with annotations in many hands, creating the impression of a cacophony of voices, or a continuous "conversation" between writers in and readers of these books (Figure 3).

Before the publication of Eliot's earliest translations, there is evidence that handwritten extracts from the Bible were passed from hand to hand in Massachusetts communities (Pilling, 1891:127). The book and other printed or written matter functioned as a focus for discussion and for shared religious practice rather than as a source of private instruction. In those Massachusetts-speaking communities where vernacular literacy was common, those at the interstices of literacy participated in many activities in which the use of documents and books both signified and reinforced the importance of community action and understanding. Reading and writing functioned as ritual actions in which both the literate and the nonliterate could share.

Books, many of which were in the Massachusetts language, also appear to have been prized objects in otherwise sparsely furnished native homes. Late seventeenth and early eighteenth century inventories of the possessions of native decedents from Natick and Nantucket frequently mention books (Bragdon, 1979:140), and storekeepers on Nantucket and Martha's Vineyard were still selling copies of works in the native language in the 1740s (Little, 1980a:90).

The importance of books and writings to the Christian Indians was not lost on others, and their acquisition of literacy may not necessarily have improved their standing among non-Indians. American Indian students at Harvard were subjected to the ridicule of their English classmates, as mocking annotations in Indian-owned exercise books makes clear (Morison, 1936: 356–357). During King Philip's War, nearly all copies of the Bible, most of which were owned by noncombatants, were destroyed by English soldiers (Danckaerts, 1867:383). In the hands of American Indians, the symbolic authority that books had in the seventeenth century (Joyce et al., 1983; Vincent, 1993) was perhaps threatening to those English settlers who were determined to draw sharp boundaries between themselves and the native Other (Murray, 1996). Some Indians, within Christian territories and outside them, also appear to have regarded books, reading, and writing, as well as objects and activities associated with them, as endowed with manitou (supernatural force) and thus sacred, inexplicable, and potentially dangerous (Williams, 1936:118, 126). It is possible that some

Sept 22 1706

niequanutlammuk asuk wachtoyuk kuh manuk noma die unna yanai wame wosketem-
 paog yunuk wawshiyuonk ehtoyu alhuttammuk Adak ne maketomuk ye ahjuampi

Neenuk thomas dila nummak ahkuk wapu nullunnuummauon nathaneil Cuper
 ne weniupag unnamuk sukkehkuu ukaussit asu i taioquagsek ye unneopie alope aneguthon-
 aiyu uoyukshik ta matashin ne thomas naton wutuk at mallashik ye unneopie unneopie unneopie
 wechkehtokong wutuk ne kakechakong ye unneopie aneguthonayun ne hironi kang akoyu
 warayauuk me anok kishok q ta muk hamuoyayen wam ne unnamuk keshkoi ut auctechum-
 moiyu wana ne fatcag wutuk nukhamuoyayen ye quepexummaiyu weenikon asuk pacie hiron
 tokonogge ahket neen thomas dila kuh unno nummakun ye kashke ahkuk nullunnuummauon
 kathanel cuper nashpe wutuk ne unneochekone are ahluowuk nagum asuk ehtoyu ehtoyonk
 matta ne nuppencowetowun asuk nupponetowonk asuk i pummetowonk kunnocam-
 manishkhtayenug

neen thomas dila neen waenthamun ye ninnukhek

Experient Job warayenun ninnukhek — 11
 Quaquagum warayenun ninnukhek — 15

Luh unno renuk kathanel Cuper ye ahkuk ahtau wunmoo kinas dila
 ne ninnukhekarehtu nummakun nullunnuummauon John telman wutuk ne
 nukshewonshik neen John telman kuh unno ye ahkuk wunmoo nathaneil
 ninnukhekarehtu nullunnuummauon nashpe nibkeshewonshikne ne ninnukhek
 ne nathaneil renuk asu aluk nupponetowonk neen nathaneil ye ninnukhek
 ananunummauog John telman pummetowonkane ahtawutuk nashpe wunneochekone
 ahtawochkittich mattape nuppapenuwetowun neen wutuk asuk pummetowonk
 kane kunnocammamunshikwutuk ye ahkuk nummakun wana wame unnamuk
 meammetowonk neen nathaneil Cuper

Sept 22 1706

neen nathaneil John telman ye ninnukhek

Experient Job warayenun ye ninnukhek — 11
 Quaquagum warayenun ye ninnukhek — 15

neen ninnukhekarehtu kuh gunnagoo neen unnamunimun, alhuttammuk
 John telman wutuk ne ninnukhekarehtu ne ninnukhekarehtu
 nupponetowonk asuk kuh nupponetowonk asuk kuh nupponetowonk asuk
 meammetowonk alhuttammuk ne ohk kuh nupponetowonk asuk kuh
 neen neen ninnukhekarehtu unnamunimun nashpe u sech
 neen gunnagoo unnamunimun nashpe u sech
 neen wunmoo John telman u ninnukhek — 11
 neen wunmoo unnamunimun u ninnukhek — 11
 neen wunmoo unnamunimun u ninnukhek — 11

1706 Sept 22
 neen ninnukhekarehtu
 14 - 1706

FIGURE 2.—Conveyance, Martha's Vineyard, 1706. Courtesy of the Huntington Library, San Marino, California (HM 3993 [1]).

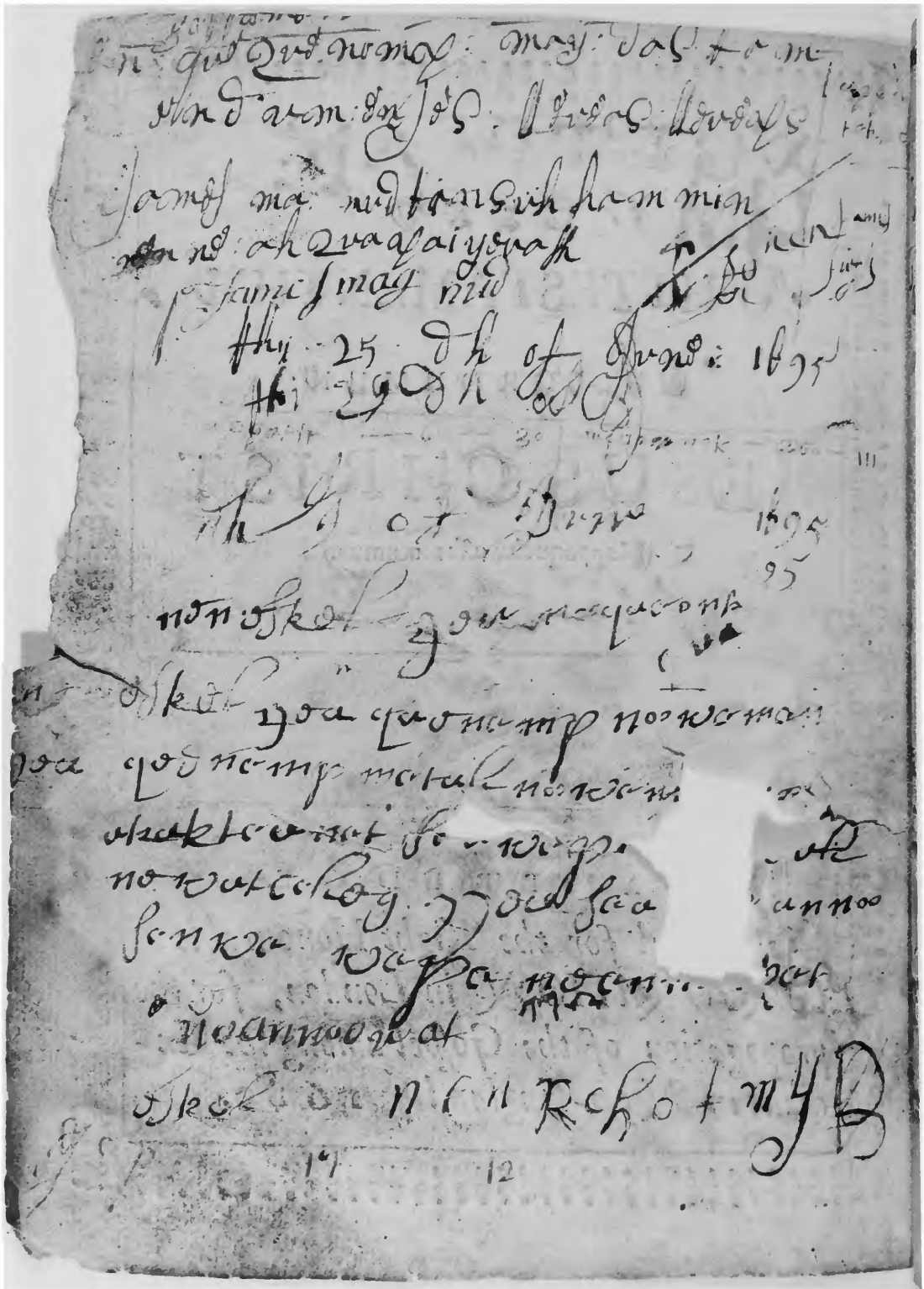


FIGURE 3.—Verso of New Testament title page, Massachusetts Bible (Eames no. 16), Nantucket. Courtesy of the Congregational Society Library, Boston, Massachusetts.

native people linked books and the ability to read to the perceived power of Europeans and to their seeming immunity to disease (Axtell, 1987:305). Others may have rejected these objects as symbolic of a way of life and of a people so threatening to their own.

Native American Books and Writings in Colonial Rhode Island and Connecticut

The remarkable spread of vernacular literacy among Massachusetts speakers was not duplicated among other peoples closely linked to them by ties of language and culture, due to differences in colonial administration, the continuing power of native communities, and the lack of a single-minded and tireless missionary, such as Eliot. During the seventeenth century, missionary activity was limited in what is now Rhode Island and Connecticut. Much of the country was too remote from English settlement, or the native peoples successfully resisted Christianity. Among the Narragansetts, their allies the Niantics, and the indigenous societies of coastal Connecticut, early missionary efforts were less dramatically successful than were those among the Massachusetts, although short-lived missions were established at Branford and Norwich (Conkey et al., 1978:177). Other communities adopted Christianity during the Great Awakening religious movement of the 1740s (Simmons and Simmons, 1982).

When the Reverend Experience Mayhew, a fluent speaker of the Martha's Vineyard dialect of Massachusetts, toured southern Rhode Island and eastern Connecticut in 1717, he found little if any evidence of literacy skills among the native peoples he encountered (Mayhew, 1896). Joseph Fish, missionary to the Narragansett in the mid-eighteenth century, reported that the native minister Samuel Niles preached with an open Bible in front of him, quoting passages he evidently knew from memory, for it appeared that he could not read (Simmons and Simmons, 1982). Yet books, especially the Bible, occupied places of importance even in this region. For example, throughout the seventeenth century, several sources document the theft of English, Greek, and Latin textbooks from English school houses and churches (Bragdon, 1981:49; Hall, 1994; Robert Gross, pers. comm., 1998). Such thefts most frequently were said to have been committed by non-Christian, or "strange," Indians (e.g., MacFarlane, 1933:564). Several seventeenth century authors recall remarks made by Indians about the importance of books. One contemporary observer recounted with approval the explanation given by the Narragansett sachem Miantonimo to an Indian of Connecticut regarding English knowledge of the afterlife:

He hath books and writings, and one which god himselfe made, concerning mens soules, and therefore may well know more than wee that have none, but take all upon trust from our forefathers. (Williams, 1936:137)

Historical and archaeological evidence suggest that books and writings took on a number of social and religious functions

even among nonliterate native people who were on the peripheries of literacy, both English and vernacular. James Axtell (1987) has suggested that in New France, writing and its mysteries may have been part of the perceived powers of the Europeans, especially missionaries, who, it was thought, could influence supernatural powers through writing, a skill they jealously guarded. Archaeological finds of Bible-page fragments in two historic-period graves dating to the mid-seventeenth century, recently excavated in southern New England, suggest that among the nonliterate, non-Christian people of the Pequot and Narragansett, writing, print, and books were indeed believed to be powerful. These printed pages were evidently included as grave goods along with items of native and European manufacture (Amory, 1996; Kevin McBride, pers. comm., 1996; Paul Robinson, pers. comm., 1996).

In the mid-eighteenth century, a school for Indian people of southern New England was established by Rev. Samuel Whitman at Farmington, on the middle Connecticut River, where students were evidently instructed in Latin and English. A document in Latin, composed by the Tunxis Indian John Metauan, in 1736, was sent by the prominent Connecticut minister Eleazer Wheelock, who trained Whitman, to the commissioners of the New England Company in the hopes they would fund Metauan's efforts to enter the ministry (Szasz, 1988:188). In 1755 Wheelock founded Moor's Indian Charity School in Lebanon, New Hampshire. Moor's school helped to foster the careers of several prominent Indian men and women, including such native ministers as the Mohegans Samson Occom and Joseph Johnson, who were both prolific writers, although they apparently left no manuscripts in their own language (Love, 1899; Murray, 1996).

The Later History of Native Literacy in Southern New England

The history of native literacy becomes more difficult to trace in the last decades of the eighteenth century. Although no Massachusetts texts have been located dating to after this period, it seems likely that native-language literacy, or at least the archiving of vernacular texts and books in Massachusetts, did not die out in Massachusetts-speaking communities until the middle to late nineteenth century. When Rev. D.W. Stevens of Martha's Vineyard visited Gay Head in the 1870s, he collected more than 50 documents in the native language there (Pilling, 1891:341). There is little evidence of interference from English in Massachusetts texts dating to the third quarter of the eighteenth century, although many native people were then no longer using that language exclusively, and others had given it up altogether in favor of English.³ In the latest of the documents written in Massachusetts, writers were still capable of a highly elaborated rhetorical style (Goddard, 1993). These data suggest that manuscripts and books in the native language came to reflect its increasingly limited and symbolic function, a "latinization" of the language that almost always precedes lan-

guage obsolescence (Dorian, 1981; Woolard, 1992). Although, aside from the publication of *Some Helps for the Indians* (Piereson, 1658) in the Quiripi language, there is no concrete evidence delineating the history of native-language literacy in Connecticut. Mrs. Fidelia Fielding, of Mohegan ancestry, kept a diary in that language in the first decade of the twentieth century (Speck, 1928:228–251). How this attenuated, but persistent, vernacular literacy coexisted with increasing bilingualism in English and, by the end of the nineteenth century, with the loss of most native languages in the region is a subject yet to be examined.

Conclusions

Books and manuscripts were important items of material culture in native southern New England, objects significant not only for their content and origins, but also for their functions as a focus for social interaction, avenues to spiritual power, and markers of native identity. Books and pamphlets in the Massachusetts language have long occupied a singular place in bibliographic and antiquarian histories of colonial American printing (e.g., Morison, 1936; Hall, 1994). Yet, because their publication in the now extinct Massachusetts language appears so quixotic to most modern scholars, their significance and widespread use among native converts to Christianity in the seventeenth and eighteenth centuries has not been well recognized. John Eliot, who was at the time urging the New England Company to fund a second printing of the Bible, wrote in a letter that even though most of the copies of the Massachusetts Bible and other translations had been destroyed during King Philip's War "they have still fragments of their old Bibles, which they make constant use of."⁴ While commitment to Christianity was surely one motive for the desire for and use of religious translations in Massachusetts, it also seems likely that these objects, like native languages themselves, were prized as symbols of Indian iden-

tity and persistence. The act of writing and its long associations (through painting and pictographic images) with shamanic practice and access to manitou are also clearly referenced in early ethnographic accounts and are implied in later descriptions of Christian and non-Christian native practices. Finally, the importance of books and writings in English to native peoples of southern New England who remained steadfastly non-literate appears to have been widely recognized in the seventeenth and eighteenth centuries (Axtell, 1985).

Much work remains to be done in understanding the ways in which the worldview of the Massachusetts-speaking people and their neighbors was transformed through the adoption of vernacular literacy and how the content and structure of translations into Massachusetts undermined or appropriated the conceptions and beliefs of native peoples. The political context of the unequal power relations in which Massachusetts-speaking people acquired and used literacy in their own language must also be considered in understanding the meaning of literacy within native communities. The various uses to which books and writings were put, which extend beyond their function as a means of acculturation and control, and their significance to native communities both literate and nonliterate, lasting two hundred years after contact with Europeans, suggest that our understanding of literacy needs to be broadened further still, to encompass the social and ideological functions of reading and script at the interstices of literate cultures.

Notes

1. Eliot to Robert Boyle, 28 Apr 1651, cited in Pilling, 1891:127.
2. A numbering system for identifying all known copies of Eliot's Bibles was developed by Wilberforce Eames (Pilling, 1891).
3. Phineas Fish to Andrew Stewart, 3 Jun 1826, concerning the Mashpee Indians. Gallatin Papers no. 64-12, 3, 65, 66, 67, New England Miscellaneous. New York Historical Society, New York, New York. See also Badger, 1835.
4. Eliot to Robert Boyle, Nov 1683, cited in Pilling, 1891:155.

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From Manifest Destiny to the Melting Pot: The Life and Times of Charlotte Mitchell, Wampanoag

William S. Simmons

People who matured in the last quarter of the nineteenth century knew whence they came and also to what destination they moved. . . . From Edward Johnson's Wonder-Working Providence to George Bancroft's History, the message bore endless repetition in the New World. A known beginning and a foreseeable future framed all particular events. Moreover, Americans were not alone in nursing such millennial dreams for their New Jerusalem, for their promised land, dreams that infused their nationalism and sustained faith in their manifest destiny. (Handlin, 1996:335)

Manifest Destiny

New England colonists and their descendants defined their presence in North America in terms of a large and evolving myth that began with the Divine Providence of seventeenth-century Puritans and merged with the Manifest Destiny of nineteenth-century westward migration. Given the priority of their claim to the continent, Native Americans are invoked in this myth in ways that explain their dispossession. One persistent motif was that of the "good" and the "bad" Indian: that Indian character was split between the capacity for loyal and unselfish help toward pioneers and the capacity for uncontrollable deceit and destruction. "Good" Indians, such as Massasoit and Squanto, assisted colonization, and "bad" Indians, such as Sassacus and Metacomet (King Philip), opposed it. That Indians succumbed to advancing white populations was proof that God and universal laws of human progress favored the winners and that Indians were spiritually lost and culturally lacking. Another tributary to this myth was that of the disappearing Indian, the last of their tribe, who vanished or at least lost power in the course of progress, but whose memory or descendants live as a local legend or *genius loci* among European Americans (Lubbers, 1994:44).

Lucy Lillie, a traveller through southeastern Massachusetts in 1885, revealed such thoughts—split Indian character, the disappearing Indian, and the Indian imprint on the landscape—in her account of a journey from East Bridgewater to Fairhaven:

We had no intention, I am sure, of making any aboriginal investigations, and yet we found that everywhere suggestions of the Indian in his most picturesque

as well as warlike moments confronted us. . . . the gaunt, bold figure that confronted Captain Standish and his men, the brave, pathetic chieftain who pledged and kept his faith with the white man, and as well the Indian who destroyed villages and tortured captives, yet who left in that fair and fertile region names that are like music in the ears and rhyme upon the tongue, whose haunts are yet to be seen with the glamour of his best hours upon them—silent lakes and dim forest lands, hill-tops and plains that are called by his names, and still have the pensive charm and grace of his sovereignty about them. (Lillie, 1885:813)

Near the end of her journey, Lillie passed by Assawompsett Pond, in the town of Lakeville in Plymouth County, where Zerviah, Charlotte, and Melinda Mitchell, all descendants of the seventeenth-century Pokanoket, or Wampanoag, sachem, Massasoit, were then living. The Pokanoket, or Wampanoag, were a political subdivision of the Massachusetts-speaking language area of what is now southeastern Massachusetts.

Thence from Fairhaven to Lakeville the country is rich and impressive. The road, when Long Plain is passed, leads to the lakes—Quitticus great and little, Long Pond, and Assawamsett. They inclose all the most famous country of old Indian times in that region, and with their belts of forest land, lie so silent, so sombre, and so grandly, impressively alone that one almost feels that the spell of the red man rests upon them never to be lifted. (Lillie, 1885:826)

Lillie and her travelling companions found Charlotte Mitchell unsettling:

While she talked she looked at us from under her half-veiled eyelids with a curious kind of contempt, as though she felt our race entirely inferior to her own, and I am not sure but that as we drove away a sense of her superiority did not impress us more than anything else. We talked of it afterward as a curious and fitting ending to our journey. (Lillie, 1885:828)

Charlotte (or Wootonekanuske) Mitchell, her mother Zerviah, and her sister, Melinda (or Teweleeema) lived in the heart of this myth-filled world where old frontier incidents, Indian memories, and stories of Indian ghosts were still in the thoughts of living people. A marker erected in 1930 on South Main Street in nearby Middleboro, for example, carries a memory of King Philip's War (1675–1676):

Fifty rods east is the site of the old fort. Built about 1670 as a place of defence and refuge in time of need. During King Philip's War, an Indian making insulting gestures on Indian Rock, across the Nemasket River, was shot from the fort.

Charlotte and Melinda Mitchell heard a lesson in Manifest Destiny in 1898 at their mother's funeral in the North Abington, Massachusetts, Baptist Church:

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Mr. Cunningham [the white minister] had written an address, which he read and in which he told of the advance in civilization that had been witnessed by the deceased. He told of "the supplanting energy of the white man" and its effectiveness in exterminating the race of Indians, and considered this an illustration of the fact that modern civilization changes the character of the people of any nation. He told the congregation that Mrs. Mitchell had been proud of the fact that she was descended from Massasoit, and concluded a long, dry talk with the announcement that pride of birth counted for very little after all, and that death came alike to prince and pauper. Meantime the children of the princess sat before him weeping for the mother who had gone from them; they knew of her long life of suffering. (Kalog, 1898)

From a Native American Perspective

Despite their participation in a world of Anglo-American domination and myth, the Mitchells held an independent perspective on the economic and moral justification of this world and its idealizations. Zerviah, for example, resented the wrongs done by the English of Plymouth Colony to her ancestor, Massasoit:

When their scanty provisions were gone, and they were left in a state of starvation, had they not received timely aid from the noble red man they would have perished then and there. But what has been the reward to Massasoit and his descendants, from the time of the landing of the Puritans down to the present hour? Nothing but deception and neglect. (Mitchell, 1972:iii)

Similarly, Charlotte is reported to have admired both the "good" Indian, Massasoit, of Old Colony myth, and the "bad" King Philip, for his tragic effort to resist the injustices done to her people. Named Wootonekanuske for Philip's wife, Charlotte Mitchell was in a sense wedded to the historic symbol of her people's resistance:

The high triumph of Wootonekanuske's life came [in 1921] when she was chosen to unveil the statue of Chief Massasoit, which is on Cole's Hill in Plymouth. Charlotte Mitchell, by patient review of history recorded in her memory, would often correct the townspeople who had the impression she was a descendant of King Philip. That warrior was her uncle, seven generations removed, she would explain. She traced her ancestry to Massasoit through her mother, who came in direct line from Amie, the daughter of Massasoit. (Vigers, 1983:27)

King Philip was her uncle seven times removed, and for him she had the greatest reverence. She used to say that Massasoit made a great mistake when he signed a treaty of peace with the white men. "He signed the doom of my people right there," said the Princess. "Although King Philip tried to right our wrongs, by that time the white men were too strong for us." (Thompson, 1944)

Zerviah Gould Mitchell (1807–1898) attended public schools in Abington and a private school in Boston; she then taught for a while at a private Boston school. Zerviah's genealogy included (in addition to Massasoit, his daughter Amie, and Metacomet) such well-known figures from New England frontier history as the Christian convert John Sassamon, whose death ignited King Philip's War, and the Black Sachem, Tuspauquin, who was an ally to Philip and adversary to the English in that war. Zerviah Mitchell appears to have raised her children in Abington and Cambridge, apart from known Native American communities of her day, but apparently she taught them the techniques of straw and wood splint basketry, some

historical legends, medicinal and divining knowledge, and a firm attachment to their family land and Wampanoag lineage. Charlotte Mitchell was born 2 November 1848 in North Abington and was educated in Abington public schools as well as in the Harvard Street Grammar School in Cambridge. Her father, Thomas C. Mitchell (d. 1859), a merchant seaman, was part Cherokee and part English. Charlotte was their tenth of eleven children (Peirce, 1884:290–297, 1972:210–219; Vigers, 1983:9–28).

By unbroken transmission from seventeenth-century ancestors, Zerviah Mitchell inherited lands at Betty's Neck on Assawompsett Pond in Lakeville. Betty's Neck, a 27-acre tract known earlier as Nahteawanet, was named for Assowetough, the daughter of John Sassamon, who accepted the English name of Betty:

Witneseth these presents, Pamantaquash, the pond Sachem, being weak in body, but of perfect disposing memory, declared it to be his last will and Testament, concerning all his lands at Assawamsett, or elsewhere, that he is now possessed of, that he would after his disease leave them unto...Tuspaquin, *alias* the black Sachem, for his life, and after the sd Tuspaquin his disease unto Soquontamouk, *alias* William, his sone, and to his heires forever. (ca. 1668, in Peirce, 1884:290)

I, the above-named Assowetough, *alias* Betty, do freely will, give, and bequeath the above said tract of land unto My daughter Mercy, to her heires forever. Witness My hand this 14th day of May, 1696. (Peirce, 1884:293)

Zerviah, with her unmarried daughters, Charlotte (1848–1930) and Melinda (1836–1919), moved from North Abington to the 15 remaining acres of their ancestral property at Betty's Neck in May of 1879. In addition to knowing their genealogy forward from the early seventeenth century, Charlotte Mitchell and her siblings also were heirs to what may have been the oldest private property in Plymouth County that remained uninterruptedly in Native American hands. By virtue of their historically significant family and their unique claim to ancient family property, Zerviah, Charlotte, and Melinda Mitchell cultivated a very distinct Indian identity that was strongly reinforced by Yankee and other inhabitants of the region who similarly valued genealogical ties to seventeenth-century forebears and priority of connection to place in defining themselves.

Speaking for herself and her children, Zerviah Mitchell identified with the white motif of the disappearing Indian: "When we are gone the race of Massasoit will have disappeared from the face of the earth. There are but a few years left to us" (Anonymous, 1894). Charlotte Mitchell's obituary described her as "the last of the once powerful tribe of Wampanoags, which acknowledged Massasoit as its chief, of the direct blood line of leadership in the tribe, and entitled to the rank of princess" and also included what may have been her own explanation for why she was the "last" of her line: "It is said she had never been attracted by any full blooded Indian and that she was averse to a marriage with less than full Indian blood, so that to her last days she had been a spinster by choice for 81 years" (Anonymous, 1930).

Pilgrimages

Whites often were sympathetic to the Mitchell's perspective on Indian-white history. The Mitchell's home became something of a shrine for white reporters, writers, children, and many others who identified with or at least were attracted to the heroic, injured, and indigenous story that the Mitchells presented to turn-of-the-century white Americans. Many were drawn to visiting, hearing about, and personally communicating with this unique family, in whose custody then rested the only Native American voice in the Plymouth Colony encounter story. One local writer, Hezekiah Butterworth, a well-known author of children's and travel literature, boarded in one of the Mitchell's lakeside cottages on occasion and recorded a fascinating Massasoit family legend of a silver pipe:

One of the oldest legends was related to me last summer by Mrs....Mitchell, now eighty-five years of age, and the oldest member of the only surviving family of Massasoit, who lives on...a little principality, if I may so term it, at Lakeville, Mass....

King James of England, on hearing of the goodness and virtues of Massasoit, once sent him a present of a silver pipe. The chieftain prized it highly as a gift from his "white brother over the sea." But one of his warriors did a deed of valor that so won his heart that he resolved to make him a present of the pipe as his choice treasure. The warrior, finding himself about to die, charged his squaw to put the silver pipe into his grave at the burial, but she, out of regard to the value of the treasure, hid it, and covered the grave without it. One evening she went to the place where she had hidden the royal present, resolving to smoke from the pipe alone, and to hide it again. She put out her hand to take the pipe, but it moved away from her. Again, but it moved away, and again and again, but a dead hand was moving it. Then she bitterly repented of her disobedience, and promised to bury the pipe if she were able. At this resolution, the pipe lay still, and she opened the grave, fulfilled the warrior's command, and was enabled to smoke in peace of mind and conscience, we may hope, the rest of her days. (Butterworth, 1893:16, see also 1895:235–236)

Rudolf Haffenreffer, a notable collector of American Indian artifacts, owner of the Narragansett Brewery in the Arlington section of Cranston, Rhode Island, and owner of what had been King Philip's property at Mount Hope, in Bristol, Rhode Island, also made the pilgrimage to Lakeville:

I listened [in November, 1913] to the touching story told me by Queen Tee-weelema and her two sisters, direct lineal descendants of King Philip, two of whom are now living at Lakeville, Mass., in a little hut surrounded by a few acres of land—all that the whites have left them of their glorious heritage from Massasoit. In their hearts, with the memories of those long bygone days, lives the acute sense of irresistible wrong done them by the alien in the land of their forefathers. (Haffenreffer, 1929:34)

Haffenreffer also once invited Charlotte Mitchell's younger sister, Emma Safford, of Ipswich, Massachusetts, to visit his shrine at Mount Hope: "I am the owner of Mount Hope, where King Philip's chair is hewn out of the rock, and also the place where King Philip was killed; and any time when either you or any of your descendants would like to visit Mount Hope, I would like you or yours to communicate with me, so as to visit the beautiful old spot."¹

Although the Mohegan scholar Gladys Tantaquidgeon seems not to have visited the Mitchell family at Lakeville, she interviewed Emma Safford at her Ipswich home in the summer of

1929. Tantaquidgeon was interested in the Mitchell family dyed-straw basketry, which she considered to be a distinctive last expression of an indigenous tradition: "Data pertaining to the straw-grass articles...indicate that the production of this particular type of basket receptacle persisted among certain of the more conservative mainland Wampanoag until a much later date than had been supposed" (Tantaquidgeon, 1930:476–478).

Charlotte Mitchell's Diary, 1896

Charlotte Mitchell wrote in longhand a very legible diary that covers the period from Thursday, 2 January through Saturday, 15 March 1896. It is one of two diaries known to have been written by a Native American woman of southern New England (her contemporary the Mohegan Fidelia Fielding being the other) and is one of three diaries known to have been written by a person of Wampanoag or Pokanoket ancestry, Paul Cuffe and Paul Cuffe, Jr., being the others (Cuffe, 1839; Speck, 1928; Harris, 1972:77–262). In contrast to the isolated, dream-like quality of Fielding's 1902–1905 narrative, which she wrote in Mohegan, Mitchell documents in English the matter-of-fact details of farm activities and relationships with family, hired help, friends, and neighbors. With the exception of a few personal names and place-names, the Mitchells do not appear to have known the Massachusetts language.

In this brief diary, which is only a snapshot of her world and of her life, Mitchell records the details of farm and household activities that would seem to be typical of rural Massachusetts in the late nineteenth century. The daily and seasonal routines that she describes (food preparation, caring for chickens, horses, and cows, cutting and storing ice, marketing eggs, and cutting firewood) characterized the small New England family farm of her day. The relationships that she depicts with hired men, neighboring families, storekeepers, and delivery men indicate that her recurrent interactions and practical interdependencies were with people of European-American and not Native American backgrounds.

The only clearly Indian relationships that she mentions were with her mother and siblings. Although numerous other persons of Wampanoag descent lived in nearby Fall River, Plymouth, Mashpee, and New Bedford, Mitchell appears not to have interacted with them during these three months. By comparison with Fielding, Mitchell focused on immediate practicalities and not on inner states and spiritual concerns. The following passage from the 30 May 1904 entry in Fielding's diary suggests the difference in consciousness between the two:

Birds. I love to see the birds, because [they are] pretty. They do not say anything evil. They eat these things *Mandu* [manitou, god] gives, then they sing, because they do not want for anything. All things *Mandu* gives [them], that is so. All things! Yesterday I saw in the river a snake; he had a fish in his mouth. I hit him, then he gave up the fish. The fish is handsome. The snake is horrid, he bites you, too.... I am afraid of the snake, snake is a spirit. (Speck, 1928:247)

Mitchell never invoked indigenous or Christian belief in her daily reflections. Her voice in most respects is that of a person

who was actively engaged with the non-Indian family farming community in which she, her mother, and her sister had chosen to live. She wrote of her Indian heritage almost inadvertently in references to basketry manufacture, medicinal preparation, fortune-telling, and a property dispute with a neighbor. The following excerpts from the diary give a sense of her day-to-day activities and what was on her mind.²

Thursday January 2nd

Fair and high winds—been thawing—winds very raw. After breakfast Henry [a hired man, apparently white] drove over to the Rock [a nearby village] after Lin [Melinda, her sister]. He wore his new canvas coat and Lin didn't know him at first sight. He stopped at Rat Sherman's and got 3 bags of hay. Paid 50 cts for them. After dinner Henry drove over to the Wayside and brought home the goods the grocery man left. Before Henry got home, Old man Moranville [Josiah DeMaranville] and his wife drove down in here and seemed to be looking round to see if anything had been cut on what he claimed. He had quite a talk with Lin; he seemed to be friendly. Henry got home in good season. Brought the bag-oats and sugar, cream tar tar & box cocoa. Mother was sick about all night last night with wind colic. Was better this morning. Hens laid 5 eggs today. Warmer this evening. I put all of the bedding out on the line today, it was so fine. The Grocery man told Henry that there was something down at the station for him. Lin let the cows up tonight.

Friday Jan 10th

It snowed all night last night and is snowing hard this morning, but it is ever so much warmer; it thaws on top the house. It has been snowing all day. After dinner Henry went to see the men about coming to morrow to work on the ice. He came home just before dark. Lin went down and fed the hens. Lin roasted Jason [Gordon] a chime piece of pork, and they took it home for their suppers. They carried a half bush. of coal down at the same time. Hens laid 5 eggs to day. Mother has been real quiet to day; staid in her room all day.

Saturday Jan 11

Colder. Brown and Bill Cudworth helped them on ice to day. Silly Billy brought the team at noon, and Brown [and] Cudworths dinner. Jason and Salon eat dinner with us. Russel didnt come with the ice plough so they have got to cut it by hand. They had to shovel a lot of snow off the ice before they could commence to cut ice. Jason and Salon took tea with us. I guess they were pretty tired to night. Henry watered the animals at noon as usual. Will Moranville drove down to get Henry to help him fix up his building. Hens laid 2 eggs.

Monday January 27

Sun shone out quite a lot today but cloudy most of the day been thawing. Lin & I have been working on baskets. We put the cows out and Lin put them up. ...Mother has been very good today.

Tuesday February 4

Cloudy & stormy. Mother kept Lin awake night before last, so I staid with her last night & precious little sleep I had of it. Henry worked over to Will Moranville's. Got home in time for supper. He brought a postal for Lin from Robie Riley saying that Mr. Lee wanted 2 bottles more of the same kind of medicine he had before. I cleaned out the hen house, put the hen dressing into bags, what I didn't put into a barrel. I sent Mrs. P's [Parkhurst] magazine home by Henry. Hens laid 9 eggs. Henry handed me \$2.00 on board.

Saturday February 8th

Fair and pleasant. Been thawing. Henry went to Will Moranville's and finished up what he had to do up there. After dinner he drove down to Middleboro with me & I borrowed 2.00 of White but didn't have to use it. I got 25 cts worth of iron for Mrs. Parkhurst for her horse; we went to Lovell & got the hams & bacon that was done & there is 3 more hams, they came to 2.98 cts I paid him. I got some wormwood for Lin to go in her medicine & a box of headache tablets. We got home just [at] lamplight. I got a [Boston] Globe at Drake's & [Boston] Record & Globe was sent Henry. I got at White's. Hens laid 8 eggs.

Wednesday February 12

Fair and cold. Not so high winds as yesterday. Smith was around on the grocery wagon himself today. Lin sent 2 bottles of medicine by him to go to New Bedford by Ex. Henry went across as he calls it about 12 o'clock. Hens laid 16 eggs. I sent White the 2.00 I borrowed by Smith.

Thursday February 13

Snow & rain which made it very slushy. A woman by the name of Jones drove over here from the Rock with her daughter & grandchild. She had both of there fortunes told & bought a bottle of medicine. All came to 1.50. Henry drove over & carried 3½ Doz eggs. They are 24 cts a doz. Henry got home in time for dinner. We had salt fish for dinner. Hens laid 8 eggs.

Sunday February 23

Sunny & cloudy. Henry cut me some holes through the ice & I trap[p]ed for fish but got none. It has been thawing all day. Henry went over to the wayside and got home in good season. He let the little horse out after he got back and he did have a good run. Hens laid 9 eggs.

Monday March 2nd

Stormy all day. Towards night a regular snow storm blizzard. Colder. It snowed so fast that it covered the ground in a very short time. Henry drove over to the wayside right after breakfast. Got home in time to water the cows. After dinner he planed some strips of board. Mother has done quite nicely for her. Lin got ready to color straw but didn't have enough cut up to color. Got 11 eggs today.

Wednesday March 12

Cold and clear. Jason went home today. Henry and I drove him down to Middleboro... I got some herbs for Lin's medicine and some Alcohol and a pair of shoes for myself. I paid the Apothecary man 20 cts that I owed him. I got 2 doz tin boxes for Salve. We got home shortly after 2 o'clock.

Basketry, Medicin , Fortune-Telling, and Land

The Mitchell family's Native American predecessors at Betty's Neck made baskets and brooms in the winter months, which they sold for income to purchase supplies once their stores of corn and grain were depleted (Bennet, 1810:1; Mandell, 1996:199). Charlotte and Melinda Mitchell continued to make and sell baskets, with their distinctive family style of plaited and dyed rye-straw miniature baskets as one source of income:

From this home they went out to earn their livelihood—by selling the baskets, brooms, and beaded work which they had made and the vegetables they had raised. With their wares they were frequent visitors at Sampson's Tavern, here in Lakeville, and at the summer resort of Onset, where Teweelasma also told fortunes. (Vigers, 1983:24–25)

In her diary entry for 27 January, Mitchell noted without elaboration that she and her sister worked on baskets. On 2 March she added that Melinda “got ready to color straw but didn't have enough cut up to color.” Most probably this passage refers to the green or purple commercial dyes that both used to color the miniature rye-straw baskets for which they were best known (Tantaquidgeon, 1930:475–484; McMullen, 1987:175; Turnbaugh and Turnbaugh, 1987:92). A brief reference on 20 February to gathering tag alder could pertain to basket manufacture, for alder was used elsewhere in the northeast as a natural dye (Speck, 1947:28). Around 1902, when Southern New England Indians were abandoning their basketry manufacturing, Melinda told a newspaper reporter that she still made baskets but fewer than before: “It is not easy to get the

material. We used to send 400 or 500 dozen every year to firms in Boston, and seldom were many returned to us. Farming is our principal work now" (Pease, 1902).

The Mitchells, as well as a number of other Native American men and women in the region, had reputations as healers among both the Indian and the white populations. Rebecca Davis, of the nineteenth-century Ponkapoag community in Canton, Massachusetts, for example, "gained some money by the sale of a salve, which she prepared from herbs according to the prescription of some ancient medicine-man" (Huntoon, 1893:39; see also Mandell, 1996:200; McBride and Prins, 1996:321–347). Mitchell's great grandmother, Lydia Tuspaquin, who earlier lived at Betty's Neck, "claimed great skill in the healing art, and was in the act of gathering herbs for medical purposes, when she fell from a high bank into Assawomset Pond and was drowned" in 1812 (Vigers, 1983:19). Gladys Tantaquidgeon interviewed one such herbalist, Rachel Ryan of Gay Head, who prepared "roots and herbs to be used for medicinal purposes" as recently as 1928 (Simmons, 1986:101). One of the most noted local practitioners, William Perry, who had a considerable reputation among country people and was frequently called upon to minister to white families, lived on the Fall River Reservation, a few miles from Lakeville (Simmons, 1986:102–104). Although Perry died during the period covered in her diary, Mitchell does not seem to have known about his death, or at least did not mention it.

On 4 February Mitchell entered in her diary that a Mr. Lee of New Bedford had written her sister, Melinda, to request that she send him two bottles of the same medicine that she had made for him before. Although Melinda sent Lee the two bottles (one ingredient was wormwood, see 8 February entry) on 12 February, Mitchell mentions neither the purpose nor the cost of the medicine. One day later a woman named Jones drove over to Betty's Neck with her daughter and grandchildren from nearby Rock Village to have their fortunes told and to buy one bottle of an unspecified medicine, all for \$1.50. Tom Tate, a white child who boarded with the Mitchell sisters for a few years at Betty's Neck, recalled how they "used the different herbs and things for medication," and that Charlotte had once healed his ulcerated foot with "a poultice of plantain leaves" (French, 1989:238). This limited information suggests that Melinda and Charlotte Mitchell's reputations as curers extended beyond the countryside to at least one major urban area.

The one reference in the diary to fortune telling (on 13 February) reveals that Melinda charged for the service and performed it for non-Indians, but it is silent regarding the concepts and procedures that may have been involved. William Perry, of Fall River, and a number of Mitchell's contemporaries at Gay Head were known to have practiced a range of divinatory techniques.

In their efforts to reclaim or hold on to what they believed to be their territorial heritage in both Fall River and Lakeville, Zerviah Mitchell and her children were involved in legal proceedings with the state and with their immediate neighbors (see

Earle, 1862:118; Lillie, 1885:828; Peirce, 1972:iii–v). According to a newspaper reporter who interviewed Charlotte and Melinda around 1902, they were then in the midst of a property dispute with their neighbor, Josiah DeMoranville:

In the present land trouble, the Mitchells are respondents, Josiah DeMoranville of Lakeville being the petitioner. He has brought a bill in equity to restrain Alonzo H. Mitchell of North Abington and Melinda and Charlotte Mitchell of Lakeville from entering on five acres of land on Cranberry pond in Lakeville, and cutting and removing timber therefrom.

The Indians claim that the land is theirs by royal descent, while the petitioner says it was deeded to him...and that he has been in possession of it more than 40 years. It adjoins land occupied by the Mitchells. A short time ago Mr. DeMoranville started wood-cutting, and the Mitchells, after a protest, began themselves to cut. Court proceedings followed and now there is a truce, as both parties have agreed to await a decision. (Pease, 1902)

This disagreement over wood-cutting rights and property lines helps illuminate Charlotte's comments on her neighbor in her diary entry for 2 January. Charlotte Mitchell held legal title to a 15-acre tract until her death on 29 April 1930, whereupon the estate went to her sisters, Lydia Mitchell (residence unknown) and Emma J. Safford, of Ipswich.³ Massasoit's lineage surrendered this land finally and completely in October, 1943, for nonpayment of back taxes:

Case# 23538. Land Court. This is to certify that the Petition of the Town of Lakeville vs. Emma Safford to foreclose its tax lien under a certain deed for non-payment of taxes...was filed in this Court Dec. 16, 1942. Thereafter due proceedings under said petition were instituted according to law and finally on Oct. 15, 1943, a decree forever foreclosing and barring all rights of redemption under said deed was entered and this notice of disposition of said petition is directed to be recorded in the Reg. of Deeds for District of Plymouth County.

Description in Book 1791, page 424 land with the buildings thereon assessed to Emma J. Safford and is known as the Indian's Land: bounded N[orth-er]ly by Assawampsett Pond; W[ester]ly by the Spooner land; E[aster]ly by land formerly of Josiah DeMoranville; Southerly by the 68 lot sixteen shilling Purchase.⁴

In the Backwaters of Myth

The Mitchell family experience and Charlotte's diary speak from a little-known time and place in Native America. Distant in time by some 250 years from the Pilgrim and Puritan frontiers, and living still on ancestral Pokanoket soil, the three women stood uniquely apart within the rural and industrialized postcolonial and recent immigrant populations. Whereas the myths of Divine Providence and Manifest Destiny took deadly aim at those Indian groups at the frontier's edge, they invested surviving enclaves in long-domesticated areas with a certain amount of nostalgia, identification, and idealization. Native American survivors in these backwaters had opportunities to direct their lives within the nostalgic version of the myth. By virtue of their esteemed lineage and their presence on ancient deeded land, the Mitchells occupied an important symbolic place in the consciousness of local whites, who saw them as a living connection with their own sacred history. Knowing that these Indians still lived somewhere in the nearby woods was very important to them.

Zerviah Mitchell and her daughters accepted and even cultivated their enhanced Indian status among whites as public figures and by marketing their traditional familial skills. With their non-Indian neighbors they lived near the end of the time when Manifest Destiny continued to give shape and historical meaning to the overwhelming majority of American lives. Yet they spoke from a uniquely Indian position in the local enactment of this myth. They upheld it as symbols of long-ago sacred events and also articulated its negative meaning for Indians. Interestingly, many of their white friends and acquaintances identified with the Indian stance. In some ways they were becoming like Indians.

American Indians, Old-Line Americans, and the Melting Pot

In the years following her sister's death, Charlotte Mitchell worked on the family farm as best she could and depended on white friends as well as on a small state pension for her support. She was one of the humble New England country women "who prefer to starve rather than say they are hungry" (Brooks, 1940:472). A reporter who interviewed her in 1926 on the occasion of her seventy-eighth birthday emphasized the interest local women had taken in her welfare and in her opposition to changing women's styles:

For the present mode of living and its fantastic dress, Princess Wootonekanuske has nothing but adverse comment. The bobbed hair of the women and girls is deplorable, she says.... There is a feeling of sorrow with her as the great white winter approaches, and if she had enough money coming to her to have some woman companion with her during the dreary months she would be in better spirits, but it looks to her as if she would have to pass the time alone, when for days she will not see a living person.

Many women have tried to obtain money for her in her just rights, but as for any gift in a sense of charity, it is a delicate matter, for her indignant royal blood will not allow it. Other relatives are living in and about Abington now, but they make but [in]frequent visits to the hunting grounds of their fathers, for they have been married and have homes of their own. (Anonymous, 1926)

In 1927 Mitchell applied without success to the state legislature for an increase, from \$300 to \$600, in her annual annuity. A white fraternal organization, the Nunkatest Tribe, 65, of the Improved Order of Red Men (IORM) raised funds throughout Massachusetts for her relief:

With the effort to increase her annuity a failure, the legislature refusing the request for special aid, it remained for Nunkatest tribe of Whitman to sponsor a fund in aid of Princess Wootoneknuski. A committee was appointed and an appeal was made last week to all Red Men throughout Massachusetts to contribute to the fund. The committee consists of Fred W. Glasier, C.A. Vinton, A.W. Harriman, Samuel Bradshaw, O.A. Smith and A.F. Blanchard.

Already money has started coming in, and with a substantial sum received in the first few days, it is hoped that a fund will be raised ample to make the last days of Princess Wootoneknuski's life comfortable. (Anonymous, 1927)

This was not Mitchell's first contact with the IORM. The organization raised funds for the tercentenary dedication of the Massasoit Memorial on Cole's Hill, overlooking Plymouth Rock and Harbor, and invited Mitchell to unveil the statue (Lemke, 1964:503, 524). The IORM was (and is) a distinc-

tively American organization, dedicated to preserving the moral qualities of the "good" Indian. Their character and purpose are clearly portrayed in their official history, written in 1909, at about the time that Manifest Destiny was yielding to the triumphal mythology of the Melting Pot:

We are the acknowledged conservators of the history, the customs, and the virtues of the original American people,—a people conceded by the early travelers and writers to have been intelligent, brave, and free, loyal in its friendships, generous in its hospitalities, and with many traits of character worthy of emulation by the civilized race. The Improved Order of Red Men is proud to perpetuate the memory of this, the noblest type of man in his natural state that has ever been discovered. (Paton, 1909:11)

Could a higher ambition inspire its members, than to emulate the virtues, preserve the customs, and transmit to posterity the history of an extinct race? Such is our destiny. (Litchman, 1909:608)

Why was the Princess Wootonekanuske then so important to white people, and who were the white people to whom she mattered, some of whom organized themselves into Indian tribes? One thought is that she and her family symbolized permanence in the midst of industrialization, abandonment of rural livelihoods, demographic change, ethnic as well as class re-stratification, and a sense of loss that swept through the late nineteenth-century northeastern United States. Women, particularly country women, were the ones who stayed at home, "where something lurked that was still sublime" (Brooks, 1940:472). In his story about a street not far from Lakeville, H.P. Lovecraft articulated nativist fears of displacement that appear to have been commonly shared by old-line Americans in the late nineteenth and early twentieth centuries in this region:

Men of strength and honour fashioned that Street: good valiant men of our blood who had come from the Blessed Isles across the sea. (Lovecraft, 1970:164)

New kinds of faces appeared in the Street, swarthy, sinister faces with furtive eyes and odd features, whose owners spoke unfamiliar words and placed signs in known and unknown characters upon most of the musty houses. Push-carts crowded the gutters. A sordid undefinable stench settled over the place, and the ancient spirit slept. (Lovecraft, 1970:166-167)

Old-line Americans threatened by immigrants felt in a way like "good" Indians, for in their own minds they were a virtuous indigenous people. Henry Adams, for example, wrote in his autobiography that as a consequence of immigration his "world was dead," and despite his revered lineage of "Puritans and Patriots," he was "no worse off than the Indians or the buffalo who had been ejected from their heritage by his own people" (Adams, 1995:229). In his nativist story "The Street," Lovecraft (1970:164) observed, "There be those who say that things and places have souls, and there be those who say they have not." The Mitchells knew that where they lived, on Massasoit's family land in Lakeville, places and things had souls, or ancestral meanings, as Lovecraft seemed to mean by his use of this word. The local descendants of earlier white generations had their own sacred landscapes, monuments, and histories. Their places, the earliest ones named for moral ideas, English villages, original grantees, and Indian place names, were the ones Lovecraft had in mind. Having become in their own

minds like Indians being colonized, many identified with the “good” Indian in the intensity of feeling and nostalgia with which they embraced their own objects, history, and places. Many also became sympathetic listeners to the Indian side of the story.

Some of these listeners were anthropologists who, in the early years of the twentieth century, were very attentive to what has become known as the “memory culture” of Native Americans. One example is Roland B. Dixon, professor of anthropology at Harvard University, an old-line American who did extensive field research on the ancestral memories of living Indians in the Sacramento valley and Sierra Nevada of northern California. Dixon’s interest in memory culture may have been suggested at least in part by a sense that his indigenous culture, like that of Henry Adams, and like that of the California Indians, was being lost as a consequence of ethnic displacement: “For two centuries they [British Americans] built according to the pattern that was their heritage, then came the great wave of immigration of the last century, which wrought a fateful change” (Dixon, 1928:294). American anthropologists of recent immigrant backgrounds, Franz Boas, Alfred Kroeber, and Robert Lowie being examples, also pursued the study of memory culture but perhaps for different motivations related to their separation from ancestral attachments.

From the moments of earliest contact, Native American and immigrant peoples in North America revised their self understandings in the light of borrowings from and projections upon one another. In this account of Charlotte Mitchell and her immediate family I have tried to show how they drew upon ancestral as well as European-American knowledge in ordering their practical activities, and beyond that, how they asserted their independent and critical perspective as players on the stage of old-line American sacred history. As indigenous whites felt themselves being displaced by nineteenth and early-twentieth century immigrant populations, and absorbed into this increasingly pluralist world, many identified with Native Americans,

whose displacements they had not only witnessed but caused. Although many indigenous and immigrant whites eventually reconciled themselves to the idea of the Melting Pot by construing it in terms of what Lawrence Levine described as the “principle” of Anglo-conformity, many of them felt that the most recent immigrant cultures were of potentially threatening moral value (Levine, 1996:109). In this context, they looked back to their own origins for moral direction and idealized not only their past but also that of Native Americans, in relationship to whom their American identity at least in part originated. That Indians continued to live in the woods near Plymouth reassured those who identified with the old-line American past as they looked beyond the older mythscapes of Divine Providence and Manifest Destiny to the Melting Pot and what Oscar Handlin (1996:335) described as “the unmarked way” of the twentieth century. At this point, the disappearing Indian faded in their imaginations to be replaced by a new enchantment with the persisting Indian, whose identity and culture had survived what they knew to be even greater ordeals.

Notes

First thanks go to Bill Sturtevant, whose visionary *Handbook of North American Indians* project brought me back to my most cherished interest in Native Americana. I also would like to express my appreciation to Kathleen Bragdon, Mitchell Breitwieser, Ann McMullen, Peter Nabokov, and Cheryl Simmons for ideas along the way as I thought through this essay. Finally, I am grateful to Marion Delaney and Evelyn Caughlan of the Dyer Memorial Library in Abington, Massachusetts, for their hospitality while I worked in their friendly and unique institution. They brought the Charlotte Mitchell diary to my attention and graciously arranged permission for me to publish it for scholarly purposes.

1. Haffenreffer to Emma Safford, 24 Nov, Brown University, Haffenreffer Museum of Anthropology.
2. Mitchell diary, 2 Jan–15 Mar 1896, Dyer Memorial Library, Abington, Massachusetts.
3. Probate Records, Plymouth County Massachusetts, volume 34, no. 39743, page 81 (1930).
4. *Lakeville v. Safford*, Lakeville, Massachusetts, book 1855, page 126 (1943).

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Indian Imagery and the Development of Tourism in the Southwest

JoAllyn Archambault

William Sturtevant's first publication about the imagery of American Indians as produced by non-Indians examined an 1838 drawing of a Seminole dance (Sturtevant, 1962). Subsequent publications analyzed the contributions that early artists, such as John White and Jacques Le Moyne, made to the understanding of native peoples in encounters between early European explorers and Native Americans (Sturtevant, 1964, 1965, 1967, 1976, 1977, 1980a). These and other publications discuss the value of early drawings and paintings by artist-explorers as a source of ethnographic data, to be considered within the context provided by associated, contemporary written documents, such as diaries, letters, and official reports (Sturtevant, 1980b). These early images must be considered critically since the influence of preexisting images from European traditions is often apparent and, unless articulated by the researcher, will confuse the naive viewer (Sturtevant, 1968, 1978, 1992). The perseverance of such literary and visual preconceptions can lead to such stereotypes as the Patagonian giants (Sturtevant, 1982).

Sturtevant's interest in imagery led to his research into early American Indian visitors to Europe, who provided Old World citizens with their first look at New World peoples. Some of these visitors died in Europe or otherwise disappeared from the historical record, but their portraits, which ranged in quality from fine paintings to crude engravings intended for advertising posters, were drawn by a variety of local artists. The published portraits allowed many more Europeans to see some approximation of the appearance of these earliest American Indian visitors (Sturtevant, 1993). Some of them were members of traveling entertainment groups, forerunners of the late nineteenth century wild west shows organized by Buffalo Bill and others.¹ These groups traveled widely and performed before hundreds, if not thousands of Europeans eager to catch some glimpse of the people of the Americas. The performances contributed to the creation of icons associated with American Indians, such as the Plains Indian warbonnet, an object adopted by many non-Plains Indians as a pan-Indian ethnic symbol (Sturtevant, 1990).

Sturtevant's writing on imagery, icons, and traveling Indian shows has contributed to my own work on the Gallup Inter-

Tribal Indian Ceremonial (Archambault, 1984) (see "The Gallup Ceremonial," below), the development of tourism, and the commodification of American Indian culture. The Ceremonial's activities are an excellent example of the tandem maturation of the tourism industry and the portrayal of the American Indian as an icon and commercial product, a process in which imagery plays a central role. The Ceremonial is also an excellent example of cultural continuity of a theatrical spectacle.

Sturtevant's work on imagery is founded on a deep understanding of the social and historical context in which the images were created and initially interpreted. His analyses are informed by a thorough knowledge of the textual and visual lineages of the images of interest, and his interpretations have always been founded on data, not on simple opinion.

The Santa Fe Railway and the Fred Harvey Company

This history properly starts with the entrance of the railroad into New Mexico, in 1879. Track was laid to Albuquerque by 1880 and to what is now Gallup by 1881 (Telling, 1952). The Atchison, Topeka, and the Santa Fe Railway, known locally as the Santa Fe Railway, gained control of the rail from Deming, New Mexico, to the west coast terminal in San Francisco, California. This gave the Santa Fe Railway an outlet to the west that had great potential for pleasure travel, an opportunity the railroad quickly seized and exploited (McLuhan, 1985).

Gallup started as a rough and ready section camp for railroad workers. There were a few Spanish-American families living in the area who farmed and ranched, in addition to the Navajo and Zuni living in portions of their original territory. Named after a railroad paymaster, David Gallup, Telling (1952) describes the town in 1882 as being a typical construction camp with saloons and hastily built shacks. The advent of the railroad benefitted all of the nascent towns along its path by providing both steady employment and commerce during the construction phase and access to markets once the line was completed. Those towns that became division points for the railway, as Gallup did in 1889, were assured of a steady income from railway workers, freight, and the sale of locally produced coal, timber, and food. Gallup became the trading center of central western New Mexico and adjacent Arizona and shared in the development of pleasure traveling.

In 1895, with the Santa Fe Railway recovering from bankruptcy, the company president, E.P. Ripley, decided to promote

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southwestern tourism more aggressively (Dutton, 1983:93). Freight revenues generated from southwestern towns were small, and increased passenger travel would bring in needed income. But although the Santa Fe track ran through beautiful, dramatic countryside and was near impressive archaeological sites to which trips could be arranged, the Southwest was still perceived as rough, untamed, and, perhaps, even dangerous. Most of the hostile encounters between Indians and whites had ceased by 1890, but the easterner was likely to need some assurance before traveling in what most thought to be wild country. This public perception would be skillfully assuaged by a publicity campaign, using exotic, sanitized, romantic images and describing a land that existed outside of time, full of picturesque, nonthreatening, primitive peoples (McLuhan, 1985).

The themes that emerged in Santa Fe's subsequent publicity campaign

transformed the Indians into symbolic reductions of its American heritage. The success of that enterprise sprang from the American public's longing for belonging, its quest for roots, and its unconscious desire for liberation from a violent past. (McLuhan, 1985:19)

Central to the advertising campaign were images of a heroic Indian archetype, at one with nature and secure in a culture that was exotic, picturesque, simple, and peaceful. Artists were commissioned to create images for the Santa Fe calendar that "were expected to be 'thematically pleasing and colorfully decorative' and neither 'pictorially perplexing' nor 'intellectually challenging'... a magnification of the railway's sense of popular taste" (Coke, 1963:59, in McLuhan, 1985:29).

The Indian calendar figures were frozen in time. They were portrayed in an idealized past, serene and sometimes meditative, set within a landscape or against a flat background. What developed was a highly selective presentation of Indian society that stressed some decorative aspects of nineteenth century material culture (beads, metal jewelry, hand woven textiles, leather clothing) set within a visual context that evoked an un sullied, aboriginal culture. This was the "real, old time Indian" whose authenticity was unchallenged. The fact was ignored that some aspects of the material culture, for example, beads, manufactured cloth, and metal, existed only because of contact with non-Indians. Other elements of material culture that would have been common in a late nineteenth century Indian home, such as metal tools, buckets, paper, and mirrors, were never pictured. Just as Edward S. Curtis deleted from his photographs some manufactured items that he considered intrusive (Lyman, 1982), so the Santa Fe calendar paintings ignored significant aspects of nineteenth century Indian life considered unauthentic or tacky. There was no attempt to present native peoples as they really lived at the turn of the century, only to show the Indian icon as created by the Santa Fe advertising department. The railway was both creating and preserving visual stereotypes of Indians. In the same process that Sturtevant described for the Patagonian giants (Sturtevant, 1982), the visual images generated by the railway publicists supported generic stereotypes of the Indian as a simple, primitive child of nature;

stereotypes that have circulated in western thinking since the sixteenth century (Berkhofer, 1978).

The calendars were first distributed gratis in 1907 in what may have been the largest general mailing at that time. They found their way into hundreds of thousands of homes, schools, and businesses (McLuhan, 1985:19) and are now considered collectors' items. Indian images dominated the calendars, with dramatic landscapes being a secondary theme. In addition, the railway printed brochures, time tables, folders, and maps and took out ads in many popular magazines of the time, all of which featured the same idealized Indian and landscape imagery (McLuhan, 1985:20). The publicity campaign was a huge success. In less than a decade the Santa Fe Railroad had established a new corporate image for itself, had attracted new passengers for its trains, had helped to establish tourism as a major industry in the Southwest, and had created a new version of an old stereotype, the Noble Redman.

The Santa Fe Railroad was not alone in its production of Indian imagery. The exotic appeal of Indian cultures for potential travelers had been recognized early on by Fred Harvey, who developed the famous Harvey House hotels and food service on the Santa Fe track from Chicago to California. The railroad had a symbiotic relationship with Fred Harvey. The former provided

transportation and infrastructure, and Harvey provided standardized, high-quality services. The Santa Fe built and owned the hotels, and the Harvey Company furnished and operated them as well as dining cars, newsstands, and other shops along the railroad's route. (Bryant, 1974:118)

The first Harvey hotel, the Montezuma, opened in 1882 in Las Vegas, New Mexico (Grattan, 1980:125-126). A Harvey House was built in Gallup in 1895, and El Tovar debuted at the Grand Canyon in 1905.

All of the hotels were given colorful names that emphasized the southwest region, and the architecture combined Spanish and Pueblo elements in what is now called the Mission style (Howard and Pardue, 1996). Native crafts were used to decorate both public spaces and the guest rooms. Navajo rugs, Pueblo ceramics, baskets and textiles from many tribes, and paintings featuring the landscape and Indian scenes were everywhere (Thomas, 1978). Even the tableware in the dining rooms featured Indian motifs. The decor provided a visual context for the emergent Indian-land nexus that is at the heart of southwestern tourism.

The two companies played major roles in the commodification of Indian material culture. The train stopped at every Harvey hotel, allowing sufficient time for travelers to eat and buy souvenirs. Attached to every hotel was a shop where tourists could purchase Indian-made items like the objects they saw decorating the Harvey House. The Alvarado Hotel complex in Albuquerque, built in 1902, was an excellent example of the union of form and function in a Harvey complex. "It was not possible to get to the main hotel facilities without first passing the Indian Building," with its sales rooms, museum, and artist demonstration area (Howard and Pardue, 1996:21). Inside, the

passenger experienced a visual cornucopia of Indian artifacts “arranged in the form of exhibits, cozy corners, etc. to illustrate to people how these things can be utilized to best advantage” (Howard and Pardue, 1996:15). There is no doubt that the museum, the artist demonstrations, and the sales shops were intended to facilitate sales and increase profits for both companies. Herman Schweizer, head of the Harvey Company’s Indian Department, wrote in 1930:

Our place here was established thirty years ago on such a large scale, primarily as an advertising feature of the Santa Fe Railway, with a view of interesting the public in the Indians of the Southwest and their products, which purpose has admittedly been well served, and it has not only been of great benefit to the Santa Fe Railway, but to the Indians and all dealers in these products. (Howard and Pardue, 1996:15)

Entrepreneurs dealing in Indian artifacts benefitted from an increased demand, created in part from the structured marketing organized by the railway and the Harvey Company.

Some Indians quickly took advantage of the opportunity for selling handicrafts to tourists and sold their goods directly to railroad travelers at the train depots and local hotels (Howard and Pardue, 1996). But the bulk of the sales took place in local gift shops, such as the Hopi House, near the El Tovar, both maintained by the Harvey Company. Some Indians sold directly to the gift shops, thereby avoiding middlemen, but the great majority had no option other than to sell to local traders, who then resold the goods to retailers. The Indian artisans were the primary producers of goods, but all evidence points to the fact that they earned very little in return. The middlemen and the retail operators realized far more profit for their efforts, in a classical example of capitalist economies (Adams, 1963). The Bureau of Indian Affairs supported craft sales as a means of generating income for reservation residents and incorporating them in a cash economy (United States Department of the Interior, Office of Indian Affairs, 1939:23).

Indians were hired as artist-demonstrators and entertainers for the Harvey Houses throughout the Southwest (Howard and Pardue, 1966; Thomas, 1978). Sometimes trains were met by groups of dancers who performed for the arriving travelers, and some sites featured nightly dances illuminated by bonfires (Thomas, 1978).

Both companies used photographs of Indians in their promotional materials, but, with rare exceptions, their personal names were never given. In Harvey Company brochures only the Navajo weaver, Elle of Ganado, and the Hopi potter, Nampeyo, were routinely identified in print (Howard and Pardue, 1996:64). Virtually everyone else was an anonymous artist or dancer presented in typical dress or pose and identified by tribe. They had become archetypes of the exotic, friendly native (Dilworth, 1996:141).

The Harvey Company interpretation of the American Indian included slide-illustrated lectures at some of the hotels. The La Fonda Hotel in Santa Fe featured such lectures, given by non-Indians about local history, geography, and Pueblo Indians, for their guests in the Lecture Lounge (McLuhan, 1985:37).

This burgeoning commercialization of Indian material culture, begun earlier by reservation traders and bolstered by the corporate interests of the railway and the Harvey Company, would culminate in the fully mature Indian art market of today. The economic benefits to Native American artists in this process have been substantial and have allowed many families on the reservation to make a living over several generations.

By 1905, if not earlier, the basic elements of southwestern tourism as created by the Santa Fe Railway and the Harvey Company were in place. The physical infrastructure consisted of railway transportation, hotels, dining rooms, and entertainment venues (museum, lecture lounge, gift shops, and performance areas). The Southwest, formerly a harsh, dangerous frontier, was repackaged as an exotic, safe, alluring “oasis” filled with colorful, friendly natives living in a place removed from the normal passage of time. An Indian image had become an icon of American identity, and carefully managed aspects of Indian culture became pastimes or souvenirs for the harried, urban traveler. Imagery in the form of paintings, photographs, and prints promoted this new vision of the railway’s Southwest through promotional materials, slide-illustrated lectures, and their physical presence in hotels, homes, art galleries, and gift shops. All of these elements became part of the Gallup Ceremonial’s program.

The Gallup Ceremonial

The Ceremonial started with entertainment entrepreneur and trader Mike Kirk, who provided a Navajo dance team as part of the local attractions during the summers at the Grand Canyon. Kirk was typical of the entrepreneurs who booked American Indian performers for public events, which were very popular at the turn of the century (Moses, 1996). He owned a trading post at Manuelito, New Mexico, and by 1922 had for some time been taking groups of Indian dancers, runners, and craftsmen to national and regional conventions or festivities. Encouraged by his experiences at the Grand Canyon, he returned to Gallup with plans for a traveling Indian show for the vaudeville circuit. He hoped to secure partial funding for this project from local businessmen, the return to the investors being the attendant publicity for Gallup. But he was persuaded that a local production would stand a better chance of support from local investors (Carroll, 1971).

Having decided to stage the show in Gallup, Kirk and attorney John Chapman sought financial backing. Kirk’s previous employment with the Santa Fe Railway was an asset. Railroad executives, impressed with the dance performances at the Grand Canyon, agreed to provide partial financial backing and publicity support as its contribution (Carroll, 1971). The Harvey Company also provided assistance (Inter-Tribal Indian Ceremonial Association (IICA), 1922). It is probable that the corporate support was critical to the continued success of the fledgling Ceremonial. Given the railroad’s heavy investment in regional tourism, its support of a new celebration in Gallup was

a prudent investment and evidence of the industry's importance in southwestern tourism.

The Inter-Tribal Indian Ceremonial Association was incorporated as a nonprofit organization in August, 1922, and its purpose was "to preserve the purity and integrity of native Indian customs and culture...through staging of an annual Ceremonial."² The original incorporators included some of the most influential traders of the era and two Bureau of Indian Affairs agents attached to the Navajo reservation (Carroll, 1971; Archambault, 1984:30).

Kirk was in charge of securing the Indian performers, and he recruited groups from the Hopi, Zuni, Isleta, Santo Domingo, and Navajo that year. The mayor declared a half-day holiday for everyone, and local citizens were requested to help with repairs to the county fairground where the Ceremonial was to be staged that night. It was an informal affair, with the headlights from cars providing most of the illumination for the evening dances. It was a rousing success by all accounts, and so was born the Gallup Inter-Tribal Indian Ceremonial Association (Carroll, 1971). In addition to the dances there were a number of contests (foot and horse races, tug of war, chicken pull), an exhibit of Indian crafts available for purchase, and products of the industrial training programs at the Bureau of Indian Affairs schools (IICA, 1922).

A new and larger Gallup Harvey House, named El Navajo, was built in 1923, and the town was designated by the Santa Fe Railway as the hub for tours to Zuni, El Morro, and Chaco Canyon. Intrepid motorists were exploring the Southwest in automobiles (20 a day in Gallup), and local businessmen were sure they had a hit in the making (Carroll, 1971). Kirk recruited dancers from 12 New Mexican tribes to appear, and the format was the same as the year previous with afternoon dances, "Indian games and sports," evening dance program, and exhibits (IICA, 1923). In 1924, 5000 fliers were sent to auto clubs in the Midwest and the Pacific coast as part of a promotional campaign, and they yielded results. There were so many visitors that, because of a shortage of hotel rooms, Gallup citizens were asked to house them. The event was beginning to attract the attention of such people as Edward S. Curtis, who came out from Los Angeles to photograph the dances (Carroll, 1971).

By 1925 the Ceremonial format was complete and would alter little over the next 60 years. There was a parade, afternoon sports, an exhibit hall with Indian handicrafts for sale, a concert by an all-Indian band, an evening dance performance, and lots of photo opportunities. All of the performers and artists were Indian. Later, some of the businessmen in the exhibit hall would be Indian themselves, but then all were non-Indian. It was staged for two to three days in the late summer and attracted both Indian and white audiences, all of whom spent money locally. Small cash prizes were given for the best artwork, for agricultural and industrial products, and to winners of the athletic contests. A printed program gave short explanations about the meanings of dances and gave brief sketches of tribes. In some years there were articles about various aspects

of Indian life, usually the expressive arts (for example, dance, art, music, oral traditions), but sometimes current social conditions were described, for example, education, ranching, or Indian veterans. The programs served as the official voice of the Ceremonial Association, and as such they are primary documents in my analysis.

All of the features of the Ceremonial—native dance performances by Indians in traditional dress, colorful athletic contests, artist-demonstrators, sales of arts and crafts, and educational public lectures by non-Indian experts—were familiar features of southwestern tourism and were prefigured in the attractions created by the Santa Fe Railway and the Harvey House hotels. The Indian-land nexus was visible in the landscape photos that appeared in later printed programs. The celebration did not break new ground in the structure and presentation of its events but fit into a comfortable format that had been in place for two decades.

The one element in the early Ceremonials that did not derive from railway-linked tourism was displays of the Bureau of Indian Affairs training programs in the Indian schools. One could see native-produced examples of the domestic arts (sewing, embroidery, canned goods), "industrial" products (carpentry, metalwork), and agricultural produce. The Bureau had been sponsoring similar exhibits since the Louisiana Purchase Exposition in Saint Louis in 1904 and saw them as an efficient means of advertising its success in assimilating young Indians to American culture. At the reservation level, the Bureau established agricultural fairs and encouraged friendly competition by awarding small prizes for the best examples of produce, domestic arts, and so forth. The annual Navajo tribal fair started in 1909, and prizes were given for the best blankets and silverwork, in addition to farm products (Moses, 1996), so local Indians were accustomed to the idea of competition for awards.

Much of the recent interpretation of southwestern Indians, tourism, and popular imagery has focused on the orientalism that is so often apparent in literature and in fine and popular arts (McLuhan, 1985; Babcock, 1990, 1994; Dilworth, 1996). Certainly the promotional materials generated by the Santa Fe Railway and affiliated companies are excellent examples of such presentation.

The Indian Detours, established by the Harvey Company and the Santa Fe Railway in 1925, designated Gallup as the hub for local tours. They were combinations of rail and bus travel that took passengers on extended trips to Indian villages and archaeological sites in northern New Mexico and Arizona far from the rail lines, complete with the food and housing service for which the Harvey company was so well known. Trips to some Indian pueblos were orchestrated so that the groups arrived in time to see local dances (Thomas, 1978). Many of the travelers who booked these trips may well have attended the Ceremonial.

The language used in brochures distributed by the Indian Detours stressed unbridled exoticism. "Motorists crossing the Southwestern States are nearer to the primitive than anywhere

on the continent. They are crossing a land in which a foreign people, with foreign speech and foreign ways, offer them spectacles which can be equaled in a very few Oriental lands” (Thomas, 1978:196). Potential customers were encouraged to discover a “last frontier that has taken 350 years to subdue...find out buried cities...and string together age-old Pueblos where one may ‘catch archaeology alive’” (Thomas, 1978:201–202). Such language, combined with imagery, supported the interpretation of the American Indian as a romantic, simple primitive who was one with a strange, magnificent landscape, living in a place outside time and the pressures of an urban lifestyle.

Imagery of the Ceremonial

Given the institutional history of the Gallup Ceremonial, its close ties to the Santa Fe Railway and Harvey Company, and its centrality within state tourism, one would expect to see a repetition of similar themes, language, and imagery in its publications. But examination of the Ceremonial’s annual printed programs reveals conflicting images and text in a display of multivocality and complexity. All of the images (40 photographs and 5 drawings) used in the 1922–1928 programs portray unidentified Indians in traditional clothes and in activities such as dancing, weaving, baking, or standing in picturesque poses. None of the images shows Indians engaged in modern activities that would have been common experiences for many, such as visiting an office or a medical clinic or attending school. The lack of images showing engagement with the modern world supports an orientalist interpretation of the Indian as “exotic others, good and bad, tamed and wild” (Dilworth, 1996:58), “ethnic others who were happy to remain outside modernity” (Dilworth, 1996:6). But upon reading the accompanying text one finds a more complicated reality that is at odds with the imagery. The text may describe a native person who is assimilating to American culture, learning new skills, and adjusting to the challenges of new lifeways.

There were three major themes in the earliest programs of the Ceremonial. The first was that Indians were no longer dangerous: “They are not the menace of fifty years ago” (IICA, 1922:4). The second was that Indians were supervised, productive, and learning new skills—they were gainfully employed under the supervision of the federal government:

[T]he Indians of today are a producing race....Under the tutelage of the government the Indians have been encouraged to continue their native craftsmanship and art and have been taught other lines of industry and trade.... This Association has encouraged the various tribes...to place upon exhibition specimens of their blankets and rugs, silverware, pottery, basketry, bead work and leather work.... The domestic science exhibits...embroidery, canned fruit and vegetables, pastry and other baked goods, demonstrates what is being done by the Indian women. The school exhibits and exhibits of manual training show the result of the education of Indian children. The produce from the farm, garden and orchard illustrates the productiveness of a primitive race when encouraged and protected by a benevolent government. (IICA, 1922:4)

Some of the exhibits mentioned above were sponsored by the Bureau of Indian Affairs and displayed the success of the forced assimilation programs in Indian schools. The third theme articulated the Ceremonial Association’s intent to inform the American public about the value of Indian culture through its annual spectacle and associated publications and educational activities:

[W]e promise that in future Ceremonials the exhibits will be more varied and...will demonstrate the advancement of these misunderstood people.... To the uninitiated the customs and life of the Indians seem strange and meaningless and the Indians themselves are deemed lazy and indolent. Such is not the case. Each custom has its significance and the traditions of the origin of these customs are just as poetic and interesting as those of any race of people. This program has been prepared to explain briefly some of the dances and sports which will be produced on the different days of the Ceremonial. (IICA, 1922:5)

This last reads like a classical statement of cultural relativism and social tolerance written at least 40 years before it became the hallmark of the turbulent 1960s. In an era when cultural chauvinism and conformity were the rule and the primacy of Anglo-American customs was unquestioned, this statement was politically progressive. Nor was it an attitude that was shared by all Gallup citizens. On at least one occasion the Ceremonial Association was taken to task by local ministers for having the parade on Sunday, which conflicted with the Christian day of worship (Archambault, 1984). Even national pan-Indian organizations of the times, like the Society of American Indians, experienced conflict over the issue of traditional religion and cultural practices, with some supporting total and immediate assimilation and others arguing for tolerance of the same (Hertzburg, 1971).

The progressive perspective may have been inspired by similar statements written by the writers and protoanthropologists Hamlin Garland (1896), George Wharton James (1900, 1902), and Charles Lummis (1891), all of whom supported the right of the Hopi to practice their own religion, specifically the Snake Dance. Certainly it was not a position representative of most elected officials or government officials, although by the 1920s there were some supporters of the Indian right to freedom of religion even within the ranks of the government service (Moses, 1996).

An interpretation based on orientalism cannot account for the complex and multivalent themes so readily apparent in the Ceremonial’s programs. It also displays the danger of selective use of evidence when using historical documents. It would have been easy enough to focus on the imagery to the exclusion of the text and make an argument for the construction of the Indian as icon and primitive. But consideration of all of the evidence available in the programs undermines this approach.

Even while advocating cultural acceptance and understanding, the Ceremonial’s annual programs illustrate the power of the federal government over American Indian communities. Whatever may have been the political aspirations of Indian leaders of the time, the government’s program of cultural and economic assimilation was not to be denied. Not only had the United States tamed the wild Indian, but it was now teaching

the Indian the arts of civilization. These elements reflect the participation of the Bureau of Indian Affairs officials who were members of the Ceremonial's organizing committee. Their presence was essential because in 1922 it would have been difficult for Indian performers to have been hired without tacit acceptance by the local superintendent, even though the government no longer required and enforced the writing of contracts between native peoples and employers (Moses, 1996:142).

While orientalism fails as an explanatory device for all of the complicated and conflicting streams of interest and influence that can be extracted from a fuller reading of the historical documents and context, the persistence of the Indian as icon in the Ceremonial's annual programs is undeniable.

Moving forward to 1955, the annual program advertises an essentially unchanged calendar with the exception of displays of agricultural and industrial products, which had disappeared by 1935. In the exhibit hall were dozens of dealers, both Indian and white, selling native arts guaranteed to be authentic and good quality. Working Indian craftsmen were available for questions, and a display of a Navajo man creating a sandpainting was very popular. A seminar presented "leading [white] experts on Indian affairs speaking on topics bearing on the Indian" before a large audience (IICA, 1955:2). The grand entry of the dance groups into the performance area was well staged and dramatic, and the closing dance sequence was a Plains war dance, a proven crowd pleaser. Special accommodations were made for photographers. After the morning parade the dance groups had to be available for photographic setups, often staged against scenic backdrops. Photo clubs attended in large numbers, and some of the pictures were published in journals and magazines across the land (IICA, 1948, 1951). The majority of the images in the program portray the Indian as exotic, heroic, noble, anonymous, in harmony with the land. The orientalism so obvious in the tourist brochures of the early century was still visible in 1955.

There were, however, some changes: the 1955 program was handsomely produced, with a full-color cover and eight color pages inside, all dedicated to images of Indian individuals and art except for four photos of picturesque landscapes. There are 88 black-and-white photos and 30 color photos. On the full-color panels, all of the 10 Indians featured in one- or two-person portraits, and the 11 Indians in two group photos, are in traditional dress or dance regalia; none of them is identified by personal name. The captions make reference either to their tribe, their social status ("old mother, grandmother"), or their activity ("Apache Crown dancers") (IICA, 1955:9-10). In the 32 black-and-white photos that feature one- or two-person portraits of Indians, three of them are named and 29 are anonymous. All of them are dressed in some variant of traditional dress or dance regalia. The three identified Indians are well-known Indian artists. In an article about the presentation of the *Palme Académique* award by the French Republic to 12 prominent Indian artists, all of the artists in two photos are named, as are all of the non-Indians. In the eight photos of non-

Indians, all of them are identified by name and position within the Association. The only non-Indians who are not identified by name appear in photos of visitors to the exhibition hall activities.

The implications are obvious. Indians are individuals less than they are members of a tribe. Despite the fact that many of the Indian dancers or artists were well known and had participated in the Ceremonial for years, they were reduced to ethnic symbols in the Ceremonial's program. This depersonalization was not a new development for this publication but was of long standing, and it continued to be the rule until quite recently. Some of the photographs, however, featured Indians dressed in modern clothes and living in mid-century America. A photo essay on the Indian encampment featured native visitors to the Ceremonial engaged in various activities: eating, visiting, getting water, and singing at an evening social dance. While there was an ethnic quality inherent in all of the photos, it was very clear that these were contemporary native people, not icons trapped in a timeless past.

The text inside the 1955 program is mixed in the same fashion as the imagery, combining both an orientalist and a journalistic approach. Language evoked the allure of the magical and mystical. "This is true beauty, pageantry, mystery and enchantment in a natural setting and only Gallup affords it (IICA, 1955:7)" As the "final beat of the tom-tom fades on the night air, the rumbling of wagon wheels and clatter of hoofs will vanish into the plains from whence they came, and another Ceremonial will be history" (IICA, 1955:7). Never mind that the Indian participants returned to conventional jobs, schools, and often substandard housing, for the moment they were the drivers of "quaint wagons...pulled by typical Indian ponies...[with the] eyes of bashful Indian children peeking out from under the flaps: they're seeing 'city life' for the first time...It is a charming and amusing scene to climax a morning parade" (IICA, 1955:6). The Land of Enchantment populated by a native people who possessed an authentic culture and lived outside time was the stuff of dreams. But it still managed to attract tourists from across the country to the Ceremonial, the self-styled "Queen" of the Indian shows. In contrast, although unstated, were the harried lives of the urban visitors, alienated from their own lives, in search of a community lacking at home.

The text went on, however, to describe Indian artists working as professionals within the milieu of modern American art. The first article described the award of the distinguished *Palme Académique*, "a French civilian decoration given for meritorious services rendered the arts in fitting tribute to the international status achieved by American Indian artists and artisans of the Southwest" (IICA, 1955:25).

The recipients were 12 distinguished artists known throughout the area for the quality of their work. Paul Coze, a French artist and Consul to Arizona, was responsible for its conception and for obtaining the cooperation of the French government in this unusual award to American citizens. The ceremony took place in front of the Ceremonial's grandstands and was re-

ported widely in the newspapers of the region. Its lasting importance lay in the recognition of American Indian art as having a place at the table of international art.

The second article, by Dorothy Dunn, a long time observer and promoter of American Indian art, is a journalistic account of Indian art history, starting with the precontact period and ending with some of the artists who had just received the *Palmes Academiques*. Dunn argued that Indian arts could and should be accepted as American arts and incorporated into the national body of pure and applied design. Like Hamlin Garland (1894) before her, she believed that the future of American arts lay in the recognition of regional culture and its roots in the land, especially as understood by its native people. She described the artists as struggling with the same dilemmas of artistic growth and vision as any other artist, thereby placing them in a modern context of aesthetic concept, product, consumer, and marketplace.

Gallup provided a critical source of potential sales and public exposure to all of the American Indian fine artists working in the mid-century period. Most, if not all, of the first generation of twentieth century American Indian artists exhibited or sold their work at the Ceremonial. The Studio style in which they worked has been discussed broadly by others, and it is gener-

ally agreed that it evoked a sense of times past, not present. The subject matter was the traditional culture of the nineteenth century presented in sentimental, nostalgic, and sanitized images (Brody, 1971). Serious collectors and dealers attended the Ceremonial, and many deals were made while standing by the exhibitor's booth or at the numerous parties in town. Indian exhibitors were immersed in the "tangled relationships between artist, consumer, and art object in the marketplace" (Dilworth, 1996:215).

In conclusion, a close reading of the Ceremonial's programs provides a complicated dialogue with the past and present, notwithstanding a superficial gloss of stereotypic imagery and text. Contextualizing the Ceremonial within the larger frame of regional tourism and federal history vis-à-vis Indian peoples, the annual programs illustrate the multiple, intricate, and often contradictory elements that are part of its history.

Notes

1. Research notes on Captain Hadlock, in the possession of William C. Sturtevant.
2. Articles of incorporation, 1922, in the records of the Inter-Tribal Indian Ceremonial Association, Gallup, New Mexico.

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Hawaiian Art: From Sacred Symbol to Tourist Icon to Ethnic Identity Marker

Adrienne L. Kaeppler

The arts of Hawai'i have evolved from sacred objects and structured sound and movement systems embedded in religious rituals to objects and performances that mark Hawaiian identity.¹ Some of these cultural forms have taken on the additional dimension of tourist icons—promoted by the Hawai'i Visitors Bureau and abhorred by activists. During the two centuries since the European discovery of Hawai'i, the arts have waxed and waned, especially since the coming of the Christian missionaries, in 1820. Along the way there have been revivals, outsider's constructions, and the recent renaissance—each of which has been accompanied by a series of revitalized meanings of the verbal and visual surface manifestations of artistic forms. Three main cultural revivals can be delineated: one led by King Kalākaua in the 1880s, one that occurred in the 1930s and was at least partially tied to tourism, and one that began in the 1970s and focused on Hawaiian identity.

This essay focuses on featherwork and dance. It explores how the underlying meanings of these cultural forms were expanded to make them acceptable in the nineteenth-century Christian world and how these forms are used as elements of Hawaiian identity. In effect, it addresses how sacred Hawaiian feathered objects and a ritual movement system were transformed into works of art and how these works of art have become markers of ethnic identity.

The terms "art" and "the arts" have been used in so many ways, in both specialized and general contexts, that they mean very little. Here I regard the arts as cultural forms that result from creative processes that use or manipulate (i.e., handle with skill) words, sounds, movements, materials, scents, or spaces in such a way that they formalize the nonformal. Art intensifies the ordinary in much the same manner as poetry intensifies language. The cultural forms produced have structured content that conveys meaning, are visual or aural manifestations of social relations, and may be the subject of an elaborate aesthetic system. Aesthetics is defined here as evaluative ways of thinking about these cultural forms.

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Art, in the Western sense of the word, was not a conceptual category of traditional Hawaiian culture, but *no 'eau* (skillfulness or cleverness) was a part of all activity and is the first important concept for the study of Hawaiian art. It can be argued that art did not exist, or, that art, as *no 'eau*, was all pervasive. The emphasis of *no 'eau* was on the process. The resulting products were passed as heirlooms from generation to generation, and the occasions on which they were used became part of them. They became chronicles of social relationships objectified in visual and verbal forms and were inherited not only as products made with skill, that is, works of art, but also as information. The skillful process of fabrication of an object or performance and its later repair or refurbishing, in addition to its objectified social relationships and changing symbolism, were aspects of an aesthetic system concerned with ongoing process and use (Kaeppler, 1985:109–110).

The second important concept is *kaona* (veiled or layered meaning), which encapsulates the aesthetic of indirection, a concept important throughout Polynesia. *Kaona* can be thought of as a creative potential that enables understanding the invisible through the visible, thereby gaining a more profound understanding of both what is seen and what is unseen. The unseen is the underlying system of cultural and social philosophy that artists express through the visible.

Hawaiian Featherwork: Sacred Symbols and Expanded Meanings

Hawaiian featherwork was part of a system of sacred symbols and ritual objectifications. The term "symbol" is generally regarded as something that stands for or represents something else. I use the term here to refer to the visible manifestation of invisible concepts or knowledge, and specifically to concepts about embodiment of the divine. In Hawai'i the divine was transmitted genealogically from the gods to chiefs, whose bodies were vessels of divine *mana* or sacredness. The most sacred parts of a chief's body were the head (especially the top of the head) and the back (especially the backbone). It was necessary to protect these body parts during dangerous or sacred situations, and feathered helmets, cloaks, and capes protected and drew attention to these body parts. Important elements were color, design, length, shape, and backing.

FABRICATION

The process of making feathered pieces was related to making an even more sacred object that embodied the divine, an 'aha cord. The Hawaiian concept of 'aha refers not only to cordage made of plant fibers, human hair, or animal intestines, but also to a prayer or service whose efficacy depended on recitation under *kapu* (taboo) without interruption (Pukui and Elbert, 1986:5). 'Aha cords were described in the Hawaiian language newspaper *Ka Nupepa Ku'oko'a* (19 Jul 1884) as follows:

The cords were made by chiefs and kahunas [priests] with the worship of certain gods. They were of sennit braided tight into a rope, some with a depression down the center, some like fish nets, others like the koko carrying net for wooden calabashes and still others with fringes. There were many kinds made by chiefs and priests who placed their faith in the gods they worshipped. The chiefs took the sennit cord as a sign of their high rank, of a lineage from the gods and also to observe the kapu of the priesthood.²

The process of making an 'aha cord consisted of one or more priests chanting a prayer while braiding the cord: "All of the chief's priests concentrated their prayers on it as it was being made under *kapu*. The priests forbade all those outside to enter, nor could those on the inside go out while the 'aha was being put in place, for the penalty was death" (Kamakau, 1991:162–163). The braiding captured the prayer and objectified it, and it became a "tool" of the *kahuna* (Kamakau, 1976:143). It would be useful for chiefs to carry or wear such a prayer during sacred or dangerous situations, and I believe that is what they did.

The base of the feathered cloak was *nae*, a net structure of *olonā* fiber (*Touchardia latifolia*). This backing was often in small pieces, made by several people of varying skill. If the *nae* was fabricated while chanting prayers, it could entangle or capture (*ho'ohēihei*) them to serve as perpetual prayers to protect its wearer. The addition of red feathers gave the *nae* even more sanctity. Red was the sacred color in Hawai'i, as elsewhere in Polynesia, and red feathers were considered among the most sacred natural products.³

The Hawaiian term for feather cloaks and capes is 'ahu'ula, (red shoulder garments). Although some small feathered capes are entirely red, most feathered pieces are a combination of red and yellow.⁴ Sacred red feathers attached to a perpetual-prayer backing would constitute protection for the sacred backbone of a chief. Red feathers activated that to which they were attached. They formed the important outer layer of god images and a feathered "temple" into which an 'aha cord could be placed. A temple ceremony, *kauila huluhulu*, focused on readorning the images with feathers (Pukui and Elbert, 1986:135).

An important taboo called *kua'ā* (flaming back) prohibited approaching a chief from the back; breaking this taboo was punishable by death (Pukui and Elbert, 1986:155). A feathered cloak might suspend this taboo in warfare or in a procession when it was appropriate for individuals of lesser status to walk behind a chief. Feathered helmets offered protection for the sacred top of the head. The base of a helmet was intertwined

'ie'ie vine (*Freycinetia arborea*) activated by the addition of red feathers. Some helmets were entirely covered with feather-covered cords similar to 'aha cords, and feather-covered cords were sometimes attached to the edges of the helmets.

DESIGN

When examining designs on cloaks and capes, it is difficult to determine which is figure and which is ground, what are the designs, and what are the spaces between them (Figure 1). Important design motifs were circles, crescents, and triangles. The designs and colors appear to be related to specific chiefly lines, and the foregrounding and backgrounding of the motifs changed over time. Information about design elements and their combinations was not recorded, nor do we know the "grammar" of the underlying design system. We know who some of the cloaks belonged to and can therefore associate some relationships between designs and people. Circles (*pō'ai*) seem to be related to certain chiefs, especially Kahikili of Maui and Ka'eo of Kaua'i. Triangles seem to be associated with the chiefs Kalani'ōpu'u and Kamehameha from the island of Hawai'i. Many capes incorporate crescent designs.

Circles have the metaphorical meanings "surrounding" and "besieging" (Pukui and Elbert, 1986:307) and are related to pillars that hold up the earth and sky. Kamakau (1976:5) noted that at the edge of the ocean next to the base of the sky that lies around the platform of the earth, there is a circle or band (*pō'ai*) called the pillars of the earth, and at the lower edge of the firmament are the pillars of the sky. Translating this into cloak designs are arrangements such as those on two eighteenth-century cloaks: the so-called Joy cloak from Kaua'i (Figure 2) and a cloak in the British Museum that probably came from chief Kahikili from Maui (Kaepler, 1985:112, 117).

Hoaka (crescent) is a powerful word. Besides naming a design used in the openwork carving on *pahu* drums (Figure 3) and a motif used in tattoo, *hoaka* also means (1) to cast a shadow, to drive away, ward off, frighten; (2) a spirit, apparition, ghost; and (3) brightness, shining, glittering, splendid. *Hoaka* is the term used for helmet crests and also has the figurative meaning "glory" (Pukui and Elbert, 1986:68). During certain rituals, the arms of the human participants were raised skyward, forming crescents like those carved on *pahu* drums. Crescent designs could give additional sacred qualities to sacred red-feather-covered, prayer-enhanced backings (Figure 4).

FIGURE 1 (opposite, top).—Feathered cloak of Kalani'ōpu'u, high chief of Hawai'i Island during the visit of Captain Cook (1779), illustrating the difficulty of separating triangle and crescent designs from background. The upper half was the original cape. National Museum, Wellington, New Zealand (cat. no. FE 327). Photograph by John C. Wright.

FIGURE 2 (opposite, bottom).—Feathered cloak with circle design. Courtesy of the Bishop Museum (neg. no. XC 76891). Photograph by Seth Joel.



ACQUIRING FEATHERED OBJECTS

Feathered cloaks and helmets were made for specific individuals. The cloaks often began as short capes, probably for wear on a specific occasion; they could be ritually renewed by lengthening (Figure 1) or by adding important feathers as an overlay. This is comparable to the ritual renewal of temples (*heiau*) that were rebuilt or refurbished for important or dangerous situations. Because the cloaks and helmets had touched the sacred bodies of the chiefs, they carried the sacred power (*mana*) of that person and were dangerous for others to wear or even touch.

Feathered cloaks could also be acquired by appropriation or inheritance. If a chief were killed in battle, his cloak would be taken as a battle prize. After a chief's natural death, his cloak would be kept by his son, as a symbol of his legitimate acquisition of power. Liholiho (Kamehameha II) had three cloaks—one that he inherited, with the sacred *mana* of his father, Kamehameha the Great; one that embedded the power of Kamehameha's paternal line in that it was the cloak of Kekuaokalani (son of Kamehameha's full brother), taken as a battle prize by Liholiho, thereby consolidating his power; and one that embedded the sacredness of Liholiho's mother's line—it belonged to Kiwala'ō (Liholiho's mother's father), taken as a battle prize by Kamehameha I.

It is unlikely that Liholiho (or any other Hawaiian) ever wore these cloaks because of the important taboo against wearing clothing that had touched the body of someone else, especially the body of a high chief. Clothing embodied personal *mana*, and individuals who did not respect prohibitions associated with clothing were vulnerable to sorcery (Handy and Pukui, 1958:181–182). An important clothing taboo was that a son could not wear the clothing of his father (or a daughter could not wear the clothing of her mother). A father could wear the clothing of his son, but apparently only if the child were not of higher rank through the female line. It was best not to wear clothing that had belonged to someone else if one did not want to make one's body vulnerable.

What did a chief do with extra, potentially harmful, feathered cloaks inherited from ancestors or taken as battle prizes? They could be given to unsuspecting Europeans, who would probably not be harmed because they were obviously subject to a different taboo system. Although Liholiho gave feather cloaks as royal gifts during his trip to England in 1823, he never gave away the cloak of his father or that of Kiwala'ō, or Kekuaokalani—all of which legitimized his right to rule and embodied his genealogy. Which cloak was Liholiho's own is unknown. Presumably, he took it with him to England, where he died; probably it remained there, and this information was not given to the European who received it. It is probably a cloak now in Edinburgh, said to have been worn by Liholiho's "favorite medicine man." It is unlikely that Liholiho actually wore the cloak in England himself—he usually appeared in European dress. It could be carried or worn by one of Liholiho's attendants—who would not be harmed if Liholiho willfully





FIGURE 3.—*Pahu* drum with crescent design. Height 44.1 cm. Pitt Rivers Museum, University of Oxford (cat. no. 1954.4.3). Courtesy of the Pitt Rivers Museum (neg. no. A4f25).

withdrew his *mana* while another person wore it with his permission. After Liholiho's death, the chief Boki—ranking member of Liholiho's entourage—knowing the clothing taboo, probably gave it away, as no one in Hawai'i could wear it, and Liholiho had no son to inherit it.

FEATHERWORK AS STATUS AND ART OBJECTS

These ritual objects, dangerous to others and incompatible with Christianity, were transformed along with Hawaiian society during the late eighteenth and early nineteenth centuries. Material culture not only changed by importing and adapting Western objects, but traditional Hawaiian material culture evolved as part of changing relationships and changing categories to meet the needs of a changed society. Objects were part of the transformation of social relationships among people, the gods, and the universe.

In pre-European times, authority in its ideal Hawaiian form derived from the power of the most genealogically prestigious chiefs, especially before the charismatic chief Kamehameha (ca. 1758–1819) acquired guns and powerful followers. Kame-

hameha operated by what was expedient rather than by what was genealogically correct. He downplayed highest genealogical descent and its concomitant taboos, and he promulgated the change from the notion that genealogical prestige gives power and therefore authority, to the concept that power gives authority and therefore prestige. His son, Liholiho (of higher rank than Kamehameha through his mother) induced his own changes, including the overthrow of the state gods and their restrictive taboos, among them the important taboo prohibiting men and women from eating together. The skepticism about traditional beliefs and practices that followed the influx of foreign ideas, and the unpunished lapses of taboos, induced at least some priests to support and encourage Liholiho. Within a few short years, during which Christian missionaries arrived, the concept "power gives authority and therefore prestige," evolved further to "chiefly status equals authority." Status, rather vaguely defined and without the sanctity of the gods, became the norm. Values and traditions that continue today derive from this concept. The bilaterally extended kin group (*'ohana*) grew in importance as did the tradition of feasting together—without gender or rank proscription.

The primarily peaceful reigns of Kamehameha's successors, and the influx of foreign ideas, expanded values to emphasize the *'ohana*. Along with social changes, objects of sanctity, protection, utility, ritual, and power took on expanded value as works of art in the Western sense. Prestige, power, authority, and status became more interchangeable, and traditional Hawaiian objects became objects of value for the enhancement of status.

In pre-Christian times, shared cultural knowledge was necessary to understand what meanings were attached to designs or motifs, how they could be combined into patterned sets as a visual grammar, and how to decode the messages embodied in them. If chiefs were going to continue to wear feathered cloaks, it could be on the basis of tradition and aesthetics rather than as objectified prayers, which was not a concept compatible with Christianity. Feathered objects retained their importance as status objects suitable for ceremonial occasions.

With the demise of the Kamehameha line of chiefs in 1872, the wearing of featherwork almost ceased except for harmless replicas (Figure 5). A short feathered cape was made for Kalākaua by Mrs. John Ena (Brigham, 1918:52), but long cloaks were not made for King Kalākaua, Queen Kapi'olani, or Queen Lili'uokalani. Other featherwork pieces were acquired by them to become what might be called the state cloaks and capes. Queen Lili'uokalani posed for an 1892 photograph seated on a cloak on her throne, but the primary use of featherwork pieces during the late nineteenth and early twentieth centuries was for funerals (Figure 6). The techniques of fabrication were all but forgotten, and only recently have they been revived. Featherwork pieces made in recent years (often by non-Hawaiians), are considered art objects and are used primarily for display.

Starting with the eighteenth-century voyages of Captain James Cook, feathered objects became part of museum and private collections in Europe and America, where they were considered exotic "artificial curiosities," and later, art objects.



FIGURE 4.—Feathered cape with crescent design. American Museum of Natural History (cat. no. 80.0/784). Courtesy of the American Museum of Natural History (neg. no. 335315). Photograph by A. Singer.

Except for the important legitimizing cloaks of the Kamehameha chiefs mentioned above, by 1840 most cloaks and helmets had been given to ship captains and prominent Europeans. The last descendant of the Kamehameha dynasty was Princess Bernice Pauahi Bishop, and the remaining Kamehameha dynasty featherwork is now in the Bishop Museum, which was founded by her husband, Charles Reed Bishop, to conserve the difficult-to-care-for and sometimes dangerous-to-touch art objects that she had inherited.

Nineteenth-century Hawaiians, like most people, wanted to be up-to-date. In 1818 Kamehameha wanted to be painted in his red vest, whereas the artist, Choris, wanted to depict him in his traditional clothing. Liholiho and his entourage wore European-style clothing during their visit to London in 1823, but the English artist John Hayter represented chief Boki wearing a feather

cloak and helmet—and the cloak and helmet were left in England. Hawaiian self-presentation enlarged nineteenth-century traditions. The *'ohana* and values associated with it—especially feasts (*lū'au*)—became more prominent than the eighteenth-century values associated with warring chiefs (before 1819 it was forbidden for men and women to eat together). During the eighteenth century, feathered cloaks were protective devices worn during sacred and dangerous situations; during the nineteenth century, Hawaiian chiefs wore feathered cloaks as visual expressions of status and prestige on ceremonial occasions.

Many Hawaiian objects (such as stone food pounders) became obsolete technologically or taboo to their original owner's descendants because of clothing restrictions. It became useful to have treasure houses in which to keep these important artifacts, and museum collections have become important for forging cul-



FIGURE 5 (opposite, top).—Replicas of feathered cloaks worn by the Royal Court during Aloha Week, Hawaiian Village, Ala Moana Park, ca. 1947. Courtesy of the Bishop Museum (neg. no. CP 115,269). Photograph by Tai Sing Loo.

FIGURE 6 (opposite, bottom).—Funeral of Prince Albert Kuniakea in front of 'Iolani Palace, 1903. Courtesy of the Bishop Museum (neg. no. CA 96494). Photograph by F. Davey.

tural, ethnic, or national identity. Objects serve as ethnic identity markers in a future that recognizes its roots in the past, and even if they are not worn or used, it is appropriate to keep them safe.

Hawaiian objects, especially feathered cloaks and capes, were part of categories that embedded social distancing. The motifs and structure of specific objects were part of an aesthetic system based on *kaona* and *no'eau*, embodying artistic memory and philosophy. With the coming of Christian missionaries in 1820, the life of objects became less sacred and more social and political. Since the mid-nineteenth century, these objects have become art in the Western sense of the term, and featherwork now plays a significant role in Hawaiian concepts of the past. Identification with these objects reveals how individuals and groups perceive themselves and want to be perceived by others. Today, mutual support, environmental conservation, and sharing are values associated with the extended family, while feather-cloaked “chiefs” appear in replica during Aloha Week events (Figure 5) and indicate points of interest to tourists for the Hawai'i Visitors Bureau (Figure 7).



FIGURE 7.—Hawaii Visitors Bureau sign featuring a chief dressed in a feathered cloak and helmet, directing tourists to the statue of King Kamehameha, Honolulu, 1990. Photograph by Adrienne L. Kaepler.

Ritual Movements: Sacred System to Broad Participation

Movement systems were equally important cultural forms in old Hawai'i. Two movement systems existed in pre-Christian times: *ha'a*, a ritual movement system performed as a sacrament to the gods on the outdoor temples, and *hula*, formal or informal entertainment performed for a human audience. *Ha'a*, performed as rituals for Lono—god of peace, agriculture, and fertility—and other gods, combined chanting, drumming, and movements at sacred ritual junctures. Movements objectified the sacred, sung texts while the performers carried out “ritual work”—tying, braiding, and placement of sacred objects, such as *'aha* cords, into sacred receptacles—that was concerned with the conservation and proliferation of human, plant, and animal life. We have little first-hand knowledge of *ha'a* movement sequences because the temple rituals of which they were a part were overthrown in 1819, but it is likely that movement sequences, like the texts they accompanied, would have had a standardized form that ideally was performed without deviation.

After the overthrow of the state religious system, the sacred sung texts and movements of *ha'a* were removed from the temples and went underground for nearly 60 years. During the Kalākaua revival in the 1880s, it was permitted to perform them openly again; *ha'a* were transformed into *hula pahu*, and their interpretation was expanded in ways that would make them appropriate to new contexts in a Christian world. Religious metaphors, myths, and rituals were recontextualized, expanding cultural traditions with understandings of a universe that also included Christian ideologies.

Hula, in contrast to *ha'a*, were usually composed in honor of people and places and conveyed this information in an indirect way through veiled or layered meaning. This *kaona*, especially in relation to words and their combinations, had a power of its own that could harm as well as honor. During the reign of King Kalākaua, some *ha'a* were reconstituted as *hula* and were performed in his honor; these eventually became associated with him as name songs. One of these was the most important of the remaining *ha'a/hula pahu* repertoire, “Kaulilua,” derived from a ritual for Lono (Kaepler, 1993). As embodied today, its expanded interpretation and *kaona* refer to a passionate, yet disdainful woman—an ancestor of Kalākaua. The movements did not change, but its new interpretation helped to legitimize Kalākaua's rise to power. In 1836, when Kalākaua was born, the state religion had been overthrown only 17 years. Of chiefly lineage, son of Kapa'akea and Keohokalohe, Kalākaua was no doubt filled with stories of the “olden days.” He would have understood how Kamehameha I had used aspects of tradition to advance his own cause and legitimize his line and how lineage manipulation could be used for status verification. Although Kalākaua was an elected king, he was also interested in demonstrating his high rank and status according to Hawaiian tradition. He encouraged performances of old dances and continued the practice of prohibiting the attendance of non-Hawaiians at certain dance events, such as wakes for high chiefs.

The *hula* tradition that emerged from Kalākaua's court was basically a secular form suitable as court entertainment, combining traditions of a number of *hula* masters into what can be considered the style of the Kalākaua era. This was essentially *hula ku'i*, dance that combined old and new, usually performed in conjunction with chant and a gourd idiophone (*ipu*). In *hula ku'i* movements interpret the text, whereas in older *hula*, movements allude to the text. Although sometimes performed in his honor, *hula pahu* was not a significant part of the Kalākaua *hula* revival, but traditions associated with this form reappeared in the 1920s and spread in the 1930s.

DANCE AND CLOTHING AS TOURIST ICONS

A fortuitous combination of the right people at the right place and time stimulated a revival on the island of O'ahu in the late 1920s. This conjunction and the characteristic Hawaiian attitude expressed in the proverb All knowledge is not taught in one school (*'a'ole i pau ka 'ike i ka hālau ho'okahi*), resulted in many older *hula* reappearing with renewed vigor. This time they moved to Waikīkī. The Royal Hawaiian Girl's Glee Club was formed in 1927 by Louise Akeo, and this group was among the first to perform at the new and elegant Royal Hawaiian Hotel. The Royal Hawaiian Girl's Glee Club danced for the entertainment of tourists with a varied *hula* program. This program included *hula pahu* taught to the dancers by the well-known traditional *hula* master Keakaokala Kanahale. On the same program, the dancers performed "half foreign *hula*" (*hapa haole hula*) accompanied by Johnny Noble, Hawaiian composer and bandleader at the Hotel. In 1937 the Kodak Hula

Show was founded, featuring Louise Akeo and the Royal Hawaiian Girl's Glee Club. They, and their performing descendants, have performed ever since in what has become the most enduring show for tourists in Hawai'i (Figure 8)—still performed essentially in the style of the 1930s.

Sam Pua Ha'aheo, another well-known traditional *hula* master of the 1930s, moved his talents to the secular arena in night clubs, such as Don the Beachcomber, Niumalu Night Club, Hawaiian Village, and the Queen's Surf. Sam Pua Ha'aheo was the musician/chanter for the entertainer Lei Conn, and later his premier student, Kau'i Zuttermeister, replaced him.

In the romantic mural paintings of Eugene Savage, the tourist orientation of the time is recorded. His depictions include the revived *hula* performances of the 1930s and feature the sacred *hula pahu*, descended from the temples of old, but now in the service of a feather cloaked and helmeted King (Figure 9). These murals were painted originally for the S.S. *Lurline*, the premier tourist ship, and were reproduced as menu covers. The contemporary paintings of Frank MacIntosh (Figure 10) were also reproduced as menu covers and illustrated Hawaiians in what became "aloha clothing." Such illustrations were at least partially responsible for what tourists believed Hawai'i was like and helped to build expectations of exotic dance, food, and clothing. As barkcloth and featherwork were no longer made or worn, however, new exotic clothing, such as the aloha shirt, sarong, and *mu'umu'u*, was created to fill the void and became the "must have" tourist attire. This clothing was worn to a *lū'au*, the Kodak Hula Show, and evening dance performances at Waikīkī Hotels—which eventually featured such luminaries as Hilo Hattie and 'Iolani Luahine. Indeed, dance, food, and



FIGURE 8.—Kodak Hula Show, ca. 1961. Courtesy of the Bishop Museum (neg. no. CP 112,963).



FIGURE 9.—Menu cover based on a mural painting by Eugene Savage, illustrating dancers accompanied by a *pahu* drum. These menus were used on the Matson ship *Lurline* in the 1940s and 1950s. National Anthropological Archives, Smithsonian Institution. Courtesy of the National Anthropological Archives, Smithsonian Institution.



FIGURE 10.—Menu cover by Frank McIntosh used on ships of the Matson Navigation Company between 1937 and 1947, showing exotic aloha clothing and flowers. DeSoto Brown Collection. Courtesy of the DeSoto Brown Collection.

unusual clothing became icons for tourists who had to experience them.

During the 1940s, performances centered on the Armed Forces, and movie stars, such as Dorothy Lamour, got into the act. *Hula* continued to change into Waikīkī tourist art and reached wide audiences through Arthur Godfrey, the Radio show “Hawaii Calls,” and Hollywood films, such as *Bird of Paradise*. Traditional *hula* went into decline and by the 1960s was considered by many to be an endangered tradition. Concerned Hawaiian residents—both Hawaiian and non-Hawaiian—decided something must be done to preserve the old dances and their histories, and they enlisted the services of knowledgeable Hawaiians who would be recognized as Hawaiian cultural treasures. Under the aegis of the State Foundation on Culture and the Arts, an organization called the State Conference on Hawaiian Dance (now the State Council on Hawaiian Heritage) was established in 1969. The purpose of the group was the preservation and the perpetuation of the dances—and with them, the cultural heritage—of old Hawai‘i. The group was composed of representatives of cultural institutions and was backed by important living repositories of traditional dance. The renaissance of traditional dance went into full swing.

In the meantime, the Merrie Monarch Festival was begun in Hilo, Hawai‘i, in 1963. “Aloha Week” tourist festivities brought tourists to Honolulu, and the Merrie Monarch Festival was calculated to bring people to Hilo. Although the Merrie Monarch Festival includes craft demonstrations, parades, and other festivities, it is best known for its *hula* competitions,

which draw some 20 group-entrants each year, and a special exhibition evening featuring a *hula* studio that has not entered the competition (Figure 11). Much controversy has been engendered regarding whether prizes should be given for performances that preserve traditions of old Hawai‘i or for creative new choreographies based on old Hawaiian forms. The controversy has not been resolved, and in some ways the Merrie Monarch Festival tends to be divisive among competing groups. Competition between unrelated kin groups (or dance-groups), like the wars of old, can be harsh and bitter. For some, not winning means you have lost—and is, like losing a battle in old Hawai‘i, degrading—a modern version of traditional jealousies among chiefs of warring lines. But this, too, is an element of ethnic identity, an identity that separates Hawaiians (and would-be Hawaiians) from the larger society, whose values are primarily Western. Hawaiian identity perpetuates the values of respect and support for one’s own ingroup, sometimes at the expense of other groups, even if they, too, are Hawaiian.

Unlike the revivals of the Kalākaua era and the 1930s, which were limited to a relatively small, select number of people, the renaissance of the 1970s has involved hundreds, perhaps thousands, of individuals—men and women, young and old—making *hula* an important outward manifestation of the Hawaiian renaissance. All important ethnic identity occasions, for example, the important Ho‘olōkahi day (“to bring about unity”), that ended the “Year of the Hawaiian” on 23 January 1988, featured traditional *hula* and the playing of *pahu* drums.



FIGURE 11.—Performance by the Zuttemeister Hula Studio at the exhibition evening of the Merrie Monarch Festival, 1990. Photograph by Adrienne L. Kaepler.

CONTEMPORARY HULA

Today, *hula* is presented in three ways: as art, as tourist entertainment, and as ethnic identity marker.

Hula as art has considerable time depth, having been part of entertainment for chiefs and visiting dignitaries from as far back as oral traditions and written sources testify. The importance of *kaona* and evaluative criteria for judging *hula* suggest that *hula* has always been considered art. Hawaiian Pageants held in the first decades of the twentieth century presented *hula* as a staged or dramatic form (Figure 12). These pageants took place at a beach or at Kilauea volcano, honoring chiefs or Pele the volcano goddess in an audience-oriented form. *Hula* as art continued into the 1930s, when Keakaokala Kanahele and her protege Eleanor Hiram were hired to perform in concert, at parties, and at important events, such as the visit of President Roosevelt in 1934. Dance programs at the Honolulu Academy of Arts, at least as early as the 1930s, presented Hawaiian dance as art, and since the establishment of the ethnomusicology program at the University of Hawai'i, Hawaiian dance has been presented in concert. The "Dances We Dance" concert dance series presents Hawaiian dance alongside other great dance traditions of the world as an art form. An important event in the recognition of *hula* as an art outside of Hawai'i took place in July 1987, when two full evenings of the American Dance Festival in Durham, North Carolina, were devoted to Hawaiian *hula* and featured four traditional *hula* schools.

Hula as tourist entertainment was a logical extension of *hula* as art and was presented to entertain early explorers, officers of

visiting ships, and the passengers of luxury liners. Traditional *hula* was brought into the mainstream of tourist entertainment when Louise Akeo and the Royal Hawaiian Girl's Glee Club began to perform at the Royal Hawaiian Hotel in the late 1920s. Since then, traditional and nontraditional versions of *hula* have been part of the tourist scene, from the large hotels in Waikiki, with a wide range of visitors, to luxury hotels on the neighboring islands.

The most important significant use of *hula* in recent years has been its emergence as an ethnic identity marker. Since the beginning of the Hawaiian renaissance in the late 1960s, *hula* has played an important role. Previous to this renaissance, *hula* was considered part of cultural identity and was presented as one of many interrelated facets of Hawaiian culture. The many lecture demonstrations by Kawena Pukui with Keahi Luahine, and later with Patience Namaka Bacon, during the 1930s and 1940s for historical societies, museums, and schools, focused on dance as a vehicle for understanding Hawaiian culture. This has been retained in such places as the Bishop Museum, which presents dance performances as part of an overall cultural experience. Dance is also presented as part of cultural identity when Hawai'i is represented in folk festivals at Pacific Festivals of Arts and other world venues. *Hula* was presented as a folk tradition, as an aspect of culture, and as an ethnic identity marker at the Festival of American Folklife at the Smithsonian Institution in 1984 as part of the Grand Generation program (Figure 13) and in 1989, when Hawai'i was the featured state.

Cultural identity has taken on a political dimension as it has become the visual manifestation of ethnic identity. Dance, as



FIGURE 12.—Postcard of a pageant presented at the Mid-Pacific Carnival, ca. 1910. Courtesy of the Bishop Museum (neg. no. CP 115,267).



FIGURE 13.—Three generations of Hawaiian dancers: (left to right) Noenoelani Lewis, Kau'i Zuttermeister, and Hau'olionalani Lewis. From the Grand Generation Program at the Festival of American Folklife, Smithsonian Institution, Washington, D.C., 1984. Photograph by Adrienne L. Kaepler.

part of politics, can be an aural and visual statement of distinctiveness. A *hula pahu* that developed from a temple ritual is “Au‘a ‘ia”—often performed today in identity-promoting contexts because it embodies the prophecy of great changes predicted for the Hawaiian Islands and is an encouragement for Hawaiians to hold fast to their heritage. Originally part of important temple rituals that dealt with the war god Kūkā‘ilimoku, in the 1880s it took on expanded meaning when the prophecy it embodied was at least partially fulfilled. Another important song used today in Hawaiian identity situations is “Kaulana nā Pua” (“Famous are the Flowers”), which expresses the feelings of the Hawaiians at the overthrow of the Monarchy and annexation by the United States. It notes that they would rather eat stones than sign demeaning papers.

Art and Identity

More Hawaiians are appreciating and championing the importance of their heritage and how it can be used to promote identity in the modern world. Identity arises from the desire of Hawaiians to perceive and present themselves as different from the surrounding cultural and social environment in which they find themselves after more than 200 years of foreign contact and immigration. Identity is promoted politically for the redemption of land and reparation for the cultural alienation that occurred since annexation. There are movements toward sovereign recognition and self-determination. Visual manifestations of identity are based on traditional Hawaiian artifacts and dance.

In contrast to featherwork, primarily a historic art form, much of which is now in museums for safekeeping, more and more Hawaiians—male and female—are studying *hula* as part of ethnic identity. They are engaged in understanding the movement conventions of *hula* and how they communicate. Dance communicates, but only to those who have the competence⁵ to understand the structure of the movement system as well as knowledge of its sociocultural background and history. Traditionally, *hula* functioned to promote prestige, power, status, and social distancing, but in its three revivals, it has fostered the renaissance of traditional Hawaiian culture. Unlike featherwork, Hawaiian dance has become politicized, and it is now widely understood that knowledge and competence in this cultural form is valued as the most important visual ingredient of ethnic identity.

Notes

1. Any exploration of the social life of ritual objects that fell into disuse in 1819, and especially the meaning of designs embedded in these objects, can be said to be speculative. The presentation here is based on visual and literary metaphors in social, cultural, and historical contexts derived from observations of Hawaiians published in Hawaiian newspapers. English translations are in the Hawaiian Ethnological Notes (HEN) in the Bishop Museum Archives, Honolulu, Hawai‘i. Also important is the dictionary of Mary Kawena Pukui and Samuel Elbert (1986). Some of the research on which this essay is based has been used in my earlier publications (Kaepler, 1985, 1988, 1992, 1993; Kaepler et al., 1993), which can be consulted for more ethnographic and historical details not relevant to the thrust of this paper.

2. Translation in the HEN.

3. Similar concepts of sacred processes for making sacred fibrous products by prayer and entanglement were found in Tahiti, as was the importance of the addition of red feathers (see the Orsmond Manuscript in the Bishop Museum Archives for 20 pages of 'aha entries).
4. Yellow feathers were rarer and more difficult to procure; they came from birds that were primarily black. In the nineteenth century, yellow feathers ac-

quired a political significance in that only powerful chiefs could obtain them (Kaepler, 1985:121).

5. I use "competence" in Dell Hymes's sense of "communicative competence," which enables viewers to understand a grammatical movement sequence that they have never seen before because they know the structure of the system that the movement sequence expresses.

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The Neets'aii Gwich'in in the Twentieth Century

Jack Campisi

The Neets'aii Gwich'in (also known as the Chandalar Kutchin) own and use 1.8 million acres (729,000 ha) of land within the Arctic Circle north of the confluence of the Yukon and Porcupine rivers of Alaska (Figure 1). The property rises from 440 ft (134.1 m) above sea level (ASL) along the Yukon River to 2000 ft (609.6 m) ASL at its northern boundary. The tree line in the region is at 1200 ft (365.75 m); the land below the tree line is covered by a boreal forest consisting mainly of white and black spruce, white birch, aspen, poplar, and willow. The Christian River marks the eastern boundary of the tribal land, and the east fork of the Chandalar River marks its western and part of its northern boundary. The entire region is subject to permafrost, which may reach depths of several hundred feet or more. The climate is subarctic, with a normal winter range of -49°F to $+10^{\circ}\text{F}$ (-45°C to -12°C) and a summer range of $+32^{\circ}\text{F}$ to $+67^{\circ}\text{F}$ (0°C to 19°C). It is not uncommon, however, for winter temperatures to drop to -65°F (-48°C) and summer temperatures to reach $+80^{\circ}\text{F}$ (27°C) (Caulfield, 1983:17–18).

In addition to extensive river systems, the area has innumerable streams and lakes, which contain whitefish, northern pike, arctic char, lake trout, burbot, and suckers. Several species of salmon, including king and dog salmon, occur in the Yukon River. While there are many species of small game animals, including snowshoe hare, Arctic ground squirrel, and porcupine, and several fur-bearing species, such as black and grizzly bear, lynx, wolverine, mink, weasel, beaver, muskrat, martin, and wolf, the most important food sources for the Neets'aii Gwich'in are moose, Dall sheep, and caribou (Caulfield, 1983:20–22). Although all of these animals play a part in the tribe's subsistence, the last mentioned, caribou—*Rangifer tarandus*—has special importance to their subsistence and identity.

This paper is concerned with the Neets'aii Gwich'in in the twentieth century. It focuses on three aspects of their history: the impact of technological changes, the influence of Christianity and government policies, and the role of particular tribal leaders in the adjustment to change and the preservation of a way of life. Within the past 150 years, the Neets'aii Gwich'in have adopted the gun, steel trap, airplane, snowmobile, radio, television, processed foods, tobacco, and alcohol, to mention

but a few of the more important items. Even before technology could influence them, however, Christianity arrived. By the 1870s Anglican, and later, Episcopal, missionaries had converted most tribal members and had developed a contingent of native lay ministers to continue the proselytizing. To add to the factors impacting the Neets'aii Gwich'in, the United States purchased Alaska in 1867, and although some decades passed without federal government intrusion, the mineral wealth of the territory alone meant supervision from afar by interests ignorant of or hostile to native rights, needs, and desires. The granting of statehood in 1959 only increased the pressures.

In spite of the intrusions on their culture, the Neets'aii Gwich'in have not given up the trap line or the hunt, and they continue to depend on more traditional food resources, such as caribou, salmon, and moose. They have adopted Christianity by fitting it to their belief systems, and they have been successful in their struggles with and against federal and state officials to maintain their way of life. In large part this has been made possible by the combination of geographical remoteness, strong cultural values, and extraordinary leadership at propitious moments.

These factors have brought significant changes in technology and lifestyle to these people, while serving to intensify their view of themselves as a unique, separate people. To understand this evolution, an exploration of the nineteenth century Neets'aii Gwich'in is in order.

The Neets'aii Gwich'in in the Nineteenth Century

In 1847 Alexander Murray traveled to the confluence of the Yukon and Porcupine rivers and established Fort Yukon, then in Russian America, as a trading post. During his one-year stay, Murry met a number of people from different Gwich'in tribes, including the Neets'aii Gwich'in, whom he located northwest of the "Vanta Kootchin" (Crow River), "being right in the midst of the Carribeux lands, I suppose no better place could be found for provisions" (Murray, 1910:36). He estimated their number at 40 men, referring to them as the "Ney-et-se Kootchin' (Gens du large)" (Murray, 1910:35–36), which he translated as "People of the wide country" (Murray, 1910:83). His contact with the tribe was minimal; he reported only one visit by four individuals in the fall of 1847 (Murray, 1910:62).

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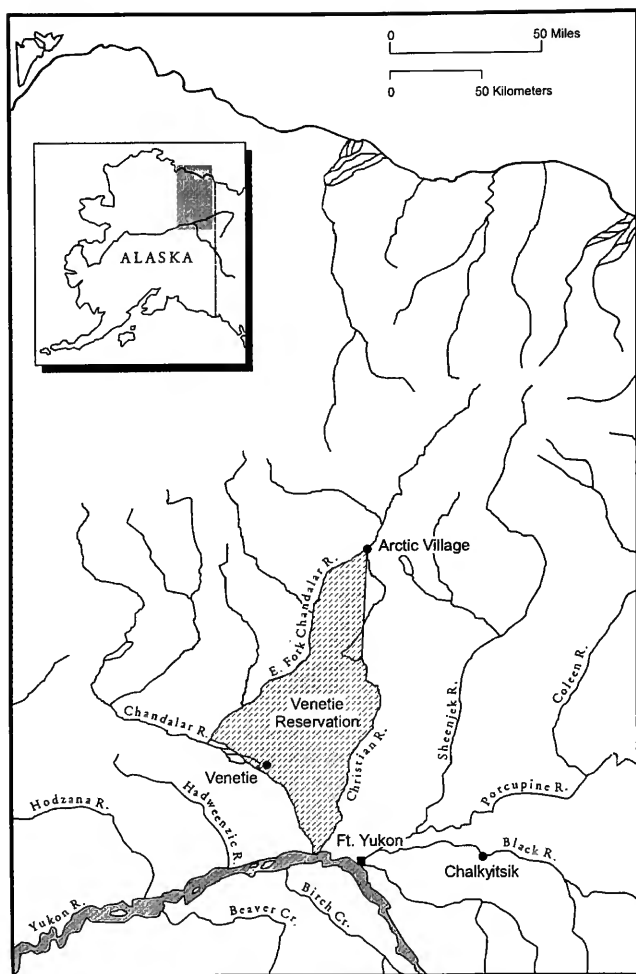


FIGURE 1.—Neets'aii Gwich'in territory.

The Neets'aii Gwich'in that Murray met so briefly at Fort Yukon were but one of nine (possibly 10) Athapascan-speaking Gwich'in tribes that occupied interior Alaska and western Canada (Slobodin, 1981:514–515). These tribes ranged from the headwaters of the Colville River in the west to the Mackenzie River in the east, and north and south of the Yukon River. Osgood (1934:172) translated Neets'aii Gwich'in as “those who dwell off the flats” (i.e., Yukon River). He called them the Chandalar River Kutchin, a name derived from the corruption of the French *gens du large*. Slobodin (1981:515) located them north northwest of Fort Yukon, between the Chandalar River on the west and the Sheenjek River on the east.

It took less than two decades for the Gwich'in to feel the impact of the Euro-Americans. Around 1860, weakened by a series of defeats at the hands of the Inupiat and Koyukon, members of the Di'haii Gwich'in moved eastward from their territory at the headwaters of the Koyukon River and began joining the Neets'aii Gwich'in (Burch and Mishler, 1995).

This infusion of Di'haii Gwich'in was fortuitous. The Neets'aii Gwich'in, along with other Gwich'in, had suffered grievously from a scarlet fever epidemic that struck the area in 1863 (Burch and Mishler, 1995:158; Raboff, 1999), so the gradual influx of Di'haii Gwich'in survivors may well have been welcomed.

The 1860s also saw the rise of an intense competition between Protestants and Catholics for the minds or, more particularly, the souls of the Gwich'in. The early skirmishes in this battle for converts took place primarily in the westernmost parts of the Canadian Subarctic, at Fort Simpson, Fort McPherson, and La Pierre's House, but by 1861, the Anglican priest William West Kirkby had made his way down to Fort Yukon (Mishler, 1990:121). The following year, the Reverend Robert McDonald replaced Kirkby at Fort Yukon, after learning that the Oblate priest Jean Séguin was headed there with conversion on his mind. The competition between the two lasted more than a decade, with the Anglican winning out over the Oblate, partly because Father McDonald spoke Ojibway, Cree, and French fluently (he was one-quarter Ojibway). There were, however, other factors that made for success. The Anglican missionaries prevailed in part because they

arrived first and stayed the longest but also because they were supported by the Hudson's Bay Company Protestant infrastructure, which included sympathetic bilingual interpreters and (apparently) a large supply of tobacco. . . . However, the Anglicans also prevailed because they and their American counterparts, the Episcopalians, encouraged a Native ministry and Native lay readers. (Mishler, 1990:125)

Some of the lay readers were reportedly former shamans who were able to integrate aspects of the new religion with traditional beliefs (Mishler, 1990:125).

With the purchase of Alaska in 1867, the Hudson's Bay Company was forced to move its operation to Canada, and with it, in 1869, went Father McDonald. He settled at Fort McPherson, where he developed an Gwich'in orthography he named Tukudh. Subsequently, he translated the Bible, the Book of Common Prayers and some hymns in Tukudh (Krauss and Golla, 1981:78). Of perhaps more importance, MacDonald taught a number of Gwich'in to read and write his orthography.

One of those who learned to write in Tukudh was Albert Edward Tritt. He was born on 17 May 1880, near Arctic Village, the son of Edward Tritt and Sarah Andrew. He grew up in the shadow of the Brooks Range, on the upper reaches of the east branch of the Chandalar River. Tritt informs us through his journal that his first contact with Tukudh came from his father's daily reading of the Bible. His later conversion to Christianity was a result of a vision, but he was heavily influenced by his father's devotion, as well as by a number of religious experiences that occurred while he was still a child. In one, a dying member of the tribe told of her visit to heaven and the message she received. She had entered a house of many rooms, where she was provided food and given a message. She was told to “make one trip on earth” and to tell the people that each was being watched from heaven for “all kinds of doing right and wrong.” She told

those present that the next day they would find ample caribou, and the next day they did indeed locate a small herd. Tritt, barely seven years old, went with his mother while "men, women, children got round the caribou & kill all of them. ... When they were shooting the caribo the noise & echoe of the guns sure scare me it was like a thunder. ... We were all happy everybody was" (Tritt, n.d., box 2, folder 1:4-7).

Robert McKennan, who conducted research among the Neets'aii Gwich'in during the summer of 1933, met and interviewed Tritt, who by that time was a well-respected preacher. Tritt described how he had struggled "to understand the meaning of the Bible," how he had retreated to the mountains for 40 days, how he agonized over the mysteries of the Bible, and how, in the end, he had received a revelation. "During this quest for true understanding," wrote McKennan, "in true apocalyptic fashion he was struck by a blinding flash of light and fell into a faint. When he recovered consciousness, he was a new man and was sure that his vocation lay in bringing the gospel to his people together with reading and writing" (McKennan, 1965:87). The following winter Tritt went back to Fort Yukon, where he studied Tukulud and the Bible.

Around 1910 Tritt led what McKennan characterized as a revitalization movement among the Neets'aii Gwich'in around Arctic Village. He returned to Arctic Village and began his ministry, preaching a conservative form of Christianity and advocating the return to old hunting methods. He stressed the use of caribou fences and, in fact, convinced his followers to build a caribou fence several miles in length. Completed around 1914, the fence was poorly located, "with the result that the Indians never succeeded in taking caribou in it, and it came to serve only as a symbol of the old hunting life" (McKennan, 1965:87).

Why did Tritt decide to build a caribou fence near Arctic Village? McKennan argued that it was a part of, indeed the major proof for, the nativistic content of Tritt's revitalization movement. According to McKennan, Tritt had been a shaman but had abandoned this role after his revelation (McKennan, 1965:86-88). It seems odd that he would give up shamanism and yet try to reinstate a communal hunting method as part of a belief in the need to return to old ways. Would not a return to shamanistic practices and beliefs have served better for a return to old ways?

There is another explanation for the building of a caribou fence near Arctic Village. Before 1900 the Neets'aii Gwich'in relied heavily on caribou fences. Rifles were unavailable and the few muskets the tribal members had, although used in conjunction with the fences, were ineffective except at close range. Ammunition was scarce. Tritt spoke of two or three men going to Old Rampart in the summer to get supplies, including ammunition. "Each one got 10 or 20 bullets, a little powder, and 100 brass caps. This was a winter's grubstake. They always had to look for bullet in meat to make it over and use again. This was the only way the supply lasted all winter" (Dalziell, 1922:2).

In the fall, families tended their fences and waited for the caribou to come. If they did, all members of the family would surround the herd, pushing it along the fence, where some were caught in snares and others were trapped and killed in the surround at the end of the fence. After a successful hunt the women and children would haul the meat and hides back to camp. The head was roasted, and the meat was boiled in a pot, and the family members ate and told the story of the hunt.

After the feast the Chief made a speech to the people, telling them to divide what they have killed honestly among themselves. Don't forget old men and women, poor and helpless. They are the ones that got the meat for you, "in their minds." God wants you to take care of them. Keep all bones of meat and give assent with one accord. When speech is made some go out doors. Others listen from within tents for all can hear. No one questions the Chief's authority, even if he wants them all to move to another place. (Dalziell, 1922:3)

Families whose hunt failed moved to the camps of those who had been more successful and who shared their larder until all was consumed. Then it was time to scatter again in search of caribou. In Tritt's words: "Whole tribe kept together and 'not each man for himself'" (Dalziell, 1922:3).

During the last decade of the nineteenth century, the Neets'aii Gwich'in underwent a major change in their hunting method. Around 1890 a few tribal members crossed the Brooks Range and purchased three rifles from the Eskimo. A year later the Eskimo returned the visit, bringing with them more rifles (Tritt, n.d., box 1:11, 13, box 2:4-5). The use of rifles lessened the need for cooperative efforts and thus placed less importance on maintaining the fence and corral. Because of the change in hunting technique, the tribal membership scattered. Now, instead of having a relatively stable population around Arctic Village for at least part of the year, to whom Tritt could preach, he had to travel to bring his message. For the next half century, Tritt traveled continuously, preaching anywhere he met Gwich'in (Tritt, n.d., box 3). From this perspective, Tritt's interest in constructing the caribou fence related more to a desire for a fixed group to whom he could preach than to a plan to return to past ways.

The Development of Neets'aii Gwich'in Camps

With the changes in hunting method and the distribution of the population came a change in the patterns of leadership. About 1890 the tribe's long-time leader, Chief Peter, died (Tritt, n.d., box 1:12). No single successor was accepted by the tribal members. In his place, several men, heads of extended families, were recognized as leaders. Among these was Chief Christian, born in 1866, who founded the settlement at Arctic Village. Tritt and Chief Christian differed over Tritt's views on religion, and when their differences could not be resolved, Chief Christian moved his followers to his camp on Christian River. This schism occurred around 1924. It appears, however, that Chief Christian spent time in both Arctic Village and Christian Village, and the disagreement between the men did not prevent their association. During the first decade of the

twentieth century, a third leader, Old Robert, settled with his family at Venetie. Thus, by 1920, there were three groupings of Neets' aii Gwich'in. A United States Bureau of Education report in 1915 identified Christian Village, which it described as consisting of two villages "and some scattered families strung out for about twenty five miles" and Chandalar Village¹ (Venetie). No mention was made of Arctic Village, but it may have been the second village referred to in the report in connection with Christian Village.

When McKennan visited the Neets' aii Gwich'in in the summer of 1933, he found them located around three camps, which he referred to as bands. Each camp consisted of a few extended families, each related to the other and also related to families in the others' camps (McKennan, 1965:19). Arctic Village had a population of 36. The principal families were Tritt, Frank, Peter, and John. Christian Village, to the south of Arctic Village, had a population of 25, principally members of the Christian and Simon families, and on the Chandalar River, about 50 miles (80.45 km) from its confluence with the Yukon River, was the village of Chandalar (also known as Old Robert's Village or Venetie). Its population, including two nearby fishing camps, was 63, and the principal families were Robert and Leviti (McKennan, 1965:19-20).

Except for the impact of guns on the Neets' aii Gwich'in social organization previously described, life styles changed little during the first three decades of the twentieth century. Although the camps were permanent, the tribal members were far from sedentary. They were still almost entirely dependent on game and fish for subsistence. Families moved frequently in search of food. In the summer, small groups visited each others' hunting camps or returned to the permanent camps, exchanging information on game availability, holding potlaches and parties, and celebrating weddings, births, and funerals. Their diet consisted mostly of meat and fish, supplemented infrequently by small quantities of tea, sugar, flour, and dried apples. When the camp was moved, the men left first to scout the new location, leaving the women to take down the caribou-skin tents, load the dogs, and then reassemble the camp. Each family unit made its own decisions as to where it would next go, reflecting the great autonomy each head of household exercised (Campisi, 1989; Peter, 1992).

In this context, it is important to understand what the Neets' aii Gwich'in meant by leadership. What were the qualities that made an individual a leader, and what was the extent of that leadership? In 1962 the Neets' aii Gwich'in tribal government produced a remarkable document entitled, "A short history of the first people who gave leadership to the Native people in the Chandalar Country, the type of work they did and the future plans for the people,"² which summarized the qualities of a leader: service, generosity, and luck. People followed to the extent that an individual demonstrated these qualities. Strength and wealth were admired, but only to the extent that they supported service and generosity. The tribal council named seven individuals and in each case identified the quali-

ties that made them leaders. Three were respected because they were lay readers, individuals who "made service for the people." Another was considered a leader because of his skill and good luck as a hunter and his willingness to share. "He was not the only one [who] had a muzzleloader but he always killed some animals. He was a good luck man for meat and split it with the people."

Two of the remaining three leaders were also given recognition because of their accomplishments. Albert Tritt was, of course, recognized for his religious commitment and activities. He built and served churches in Arctic Village, Chalkyitsik, and Venetie. He also established a native store at Arctic Village, constructed a trail from Arctic Village to Fort Yukon, and built a school at Arctic Village.

By contrast, Chief Christian's leadership related more to his business acumen. He owned a store at Arctic Village and was known for his skill as a trader. He was the first to establish a permanent dwelling at Arctic Village, and, later, he founded the settlement of Christian Village. He was also a noted hunter: "1886 His community started to use him for animals and hunting."

Finally, there is mention of John Fredson, who was respected for his education and for establishing and teaching in the school at Venetie. Fredson was born in 1895 along the Sheenjek River. While still quite young his mother died, and his father, unable to provide for his nine children, left him in the care of the schoolteacher in the Gwich'in village of Circle. She raised him and several other children for a few years and then took him to St. Stephen's Episcopal Mission at Fort Yukon, where he impressed Archdeacon Hudson Stuck. He spent the next 10 years alternating between the mission school and living with his father and siblings. Upon completing his schooling in Alaska, Stuck arranged, in 1916, for him to continue his education at Mount Hermon Academy in Gill, Massachusetts. There he stayed until he graduated, in 1921, except for a few months of service in the United States Army in the fall of 1918. After graduation, he returned to Fort Yukon, where he worked in the hospital for a year (Mackenzie, 1985).

In 1922 he received a scholarship to the University of the South at Sewanee, Tennessee. It was while he was a student at the college that he was sought out by Edward Sapir, for whom he recorded stories in Gwich'in. He stayed at Sewanee for two years and then returned to Fort Yukon, where again he worked in the hospital. He completed his college education at the University of the South in 1930 and returned home. He lived with Dr. Grafton Burke and worked for a few years for the Northern Commercial Company. While at Fort Yukon he met and married Jean Ribaloff. The depression had hit the area hard, and with a family on the way he sought more permanent and certain employment. Given his education and training, government service seemed the best avenue to follow (Mackenzie, 1985).

Fredson moved to Venetie in 1937 to be a teacher, taking his family with him. He soon recognized the threat that over-exploitation of game posed to the Neets' aii Gwich'in. Less than

six months into his service, he began organizing a fur cooperative to secure better prices for the Neets'aii Gwich'in trappers and pushing for a reservation to protect game and thus the way of life. In January 1938 he wrote to the Alaska Indian Service raising the subject of a reservation for his people:

If the Chandalar school is to be permanent, in our opinion, the first big step is to put that section of the country under reservation. There is no trader there now. The people want the reservation. And they realize that the time has come when such action is necessary to protect their fur and game.

Please note the petition attached. This is the story. The planes are rapidly changing methods of trapping. Two of the residents took plane this fall and in a short time came back with one hundred martens. How long can this keep up? We suggest that a section of the Chandalar country be made into a reservation for the Natives. Attached is a map. Also the possible boundaries.³

The letter was accompanied by a map showing the proposed boundaries, as well as a petition from "We, the undersigned, being a group of adult Indians having a common bond of residence in Venetie, Arctic Village, Christian Village and Ka-chick Village, Alaska do hereby respectfully petition the Honorable Secretary of the Interior to grant a reserve for the use of the Indians in this neighborhood."⁴

The Department of the Interior received Fredson's letter and initiated steps to implement the request; however, there were problems. First, the department had to develop a constitution and by-laws as the Indian Reorganization Act (IRA), as amended in 1936, required. Because of the remoteness of many of the Alaskan villages, it was impossible for the Bureau of Indian Affairs's Alaskan field staff to visit the villages more than once a year, let alone conduct elections.⁵ The solution was to direct Fredson to carry out the necessary steps.

On 17 June 1939 Fredson received a letter outlining the information needed to justify the establishment of "a reservation for Chandalar natives."⁶ The information required included the proposed boundaries, the location of villages and fish camps, systems of communication and transportation, and the use of land.

Fredson provided the required data and informed the Bureau of Indian Affairs that Johnny Frank, the chief at Venetie, had traveled to Arctic Village to obtain signatures on a petition seeking the establishment of a reservation: although he "had hard trail and he often had to camp out in the open," he succeeded in signing up most of the residents, although "a few were so far away that he could not reach them. The people of Arctic Village and Christian Village move back and forth so much that it is difficult to place them. At present they are camping half way between the two places."⁷ Fredson informed the bureau that they had collected three petitions signed by a total of 67 tribal members.

In January 1940 Claude M. Hirst, general superintendent in Alaska, forwarded the request for a reservation to Assistant Commissioner of Indian Affairs William Zimmerman, Jr. He explained that the request came from the residents of the four villages—Arctic Village, Christian Village, Venetie, and Robert's Fish Camp (Ka-chick)—adding that "although these villages are some distance apart the people are from closely inter-

related families and use in common the game, fur and fish which are the chief resources of the area." He then described the boundaries of the proposed reservation, adding the following justification for its creation:

The native Indians of this region have always been self-sustaining but their income is being threatened by the intrusion of white trappers who enter the region by airplane and reach trapping grounds which are inaccessible to the natives by dogteam. These white trappers are interested in securing all the furs possible and over trap the country leaving insufficient breeding stock to assure a continuing fur crop sufficient to maintain the native population. A reservation is necessary to protect this well-established and to date self-maintaining economy of the Native people.⁸

Before the reservation could be established, however, the Neets'aii Gwich'in had to approve a constitution. Again, Fredson informed the tribal members and arranged for the vote. On 25 January 1940 a constitution was ratified at Venetie "by a vote of 30 for and 0 against in an election in which over thirty percent (30%) of the legal voters cast their ballots."⁹ Another three years passed before the Department of the Interior issued a proclamation setting aside 1,408,000 acres (570,240 ha) for the Neets'aii. (After a survey in the 1970s, it was determined that the area was actually 1.8 million acres (729,000 ha).) Included in this reservation was all the land from the middle and east forks of the Chandalar River on the west, north to Arctic Village, then south along the Christian River to the Yukon River, and west along the Yukon River to its confluence with the Chandalar River.¹⁰

Before the reservation could be officially designated as such, one step remained: a majority of tribal members had to accept it. Accordingly, Fredson sent out notices that an election would be held on whether or not to accept the reservation. The election took place on 1 March 1944. Fredson reported that of the 72 eligible to vote, 49 were present, and of this number 47 voted affirmatively. (Fredson and the chairman of the meeting abstained "since we directed the proceedings.") Among those voting was the Reverend Albert Tritt, about whom Fredson wrote is "counted in the Arctic Village or Sheenjek group." The meeting was followed by dog races, a potlach, and dancing.¹¹ On 28 March the department informed the commissioner of Indian affairs that the election had been held and that the tribe had approved the reservation.¹²

The establishment of so large a reservation caused considerable distress among territorial officials. Anthony Dimond, delegate from Alaska, asked the department for an explanation of the grant. In its reply, the department stressed several central points. There were 47 Neets'aii Gwich'in families dependent for their livelihood on hunting, trapping, and fishing in a region of severely limited resources, thus requiring the setting aside of an extensive area. The department closed its argument with the following explanation:

The people of the four main villages of Venetie, Arctic Village, Christian Village and Robert's Fish Camp, as well as of the semi-permanent camps are all more or less inter-related. Many of the trails and camps are used jointly by the people of the different villages. Such camps and trails are indispensable to the utilization of the resources of the region. Since the entire area lies north of the

Arctic Circle, the resources are sparse and large areas are required for the support of a few people. Travel over these areas in search of game and fur would be impossible without well developed trails and a sufficient number of shelter cabins or camps.¹³

The land reserved for the Neets'aii Gwich'in by the federal government corresponded generally to that requested by Fredson, but it differed in two essential ways. The Fredson map drew the boundaries some distance west of the Chandalar River and east of the Christian River, giving the tribe control of the valley of each river. His map also included all of the land between the 145th and 147th meridians north of the middle fork of the Chandalar River, well into the Brooks Range. Fredson had tried to secure for the Neets'aii Gwich'in their favorite moose, sheep, muskrat, and caribou hunting areas. Instead, the department ran the boundaries along the middle courses of the rivers and just north of Arctic Village. Nonetheless, Fredson had preserved for the tribe most of their traditional territory. On 23 August 1945 Fredson died of pneumonia (Mackenzie, 1985:184).

Post-Reservation Period

For the first 10 years after its institution, the IRA-style government had little impact on tribal life. The rules of behavior were still based on traditional practice, and political and social control flowed from the network of intratribal marriages and the harsh realities of life in the Subarctic. It was not until the 1950s that Arctic Village could claim a more or less year-round population similar to Venetie. According to Caulfield (1983:92):

Until the middle of the twentieth century, the Neets'aii Gwich'in continued a highly mobile way of life, utilizing semi-permanent settlements such as Arctic, Christian, Venetie and Sheenjeck villages as well as seasonal camps at places such as Old John Lake, Wind River T'sukoo, Caribou House, T'ee't'ree, and the Kones River. Occasionally families would move to Fort Yukon or Venetie for a period of time and then return to their homeland.

From 1950 on, the tendency has been for the population to concentrate in Venetie and Arctic Village (Table 1). A number of factors have influenced the gradual development of permanent communities. First, the establishment of a federal government school at Venetie in the late 1930s encouraged families to settle in the area. The same process was repeated in Arctic Village two decades later. The desire to have their children educated in public schools was (and is) strong among tribal members, as is the desire that the children learn the Gwich'in language, culture, and ways of subsistence. Leaders of the status of Rev. Albert Tritt and John Fredson had strongly advocated education.

Second, the development of regular air service to the villages furthered the impetus for residential consolidation. It also spurred the move toward a cash economy and ameliorated the danger of famine. Scheduled flights brought in supplies, food that could be stored, fuel for machinery, and clothing—the necessities of modern life. Fairbanks was less than two hours away, and a trip to Fort Yukon that once took a week or more was now within an hour's reach.

TABLE 1.—United States Bureau of the Census data* for three settlements of Neets'aii Gwich'in. The data do not include tribal members living in other Gwich'in villages, in Fairbanks, or in Anchorage.

Settlement	1920	1930	1940	1950	1960	1970	1980	1990
Arctic Village	40	24	53	110	85	111	96	
Venetie	32	62	86	81	107	112	132	182
Christian Village		36	34					

*Population summaries, 1920–1990, Washington, D.C., U.S. Government Printing Office.

Third, the availability of fuel and parts made the snowmobile an indispensable part of village life. Trappers could now service trap routes in excess of 100 miles (160.9 km) in length. Their stays on the trap line could be shortened, and their efforts could be made more productive. The same was true with hunting and fishing: the snowmobile expanded the range and decreased the reaction time for hunters. Since all of the houses depend on wood for heat, and since trees do not grow in abundance in the subarctic climate, the wood supply close to the villages was quickly exhausted. The snowmobile provided a means of hauling firewood from some distance, removing the need for villages to move in search of fuel.

Fourth, with the expansion of the villages and the development of schools and airfields, came a concomitant expansion of public services that resulted in more or less permanent jobs. These services included a post office, water systems, schools, electric generation plants, health care, and airfield maintenance. Additionally, there is seasonal work in home construction and repair, road maintenance, and forest-fire-fighting jobs in the summer. These produce income, some of which stays in the villages. Added to these tasks are services rendered by the more able-bodied to the elderly and infirm, who generally receive small incomes from social security and social welfare programs.

The establishment of a second permanent village, the increased contact with non-Gwich'in society, and the growing population on the reservation necessitated changes in the system of governance. As originally written, the IRA charter centered power at Venetie. To share representation with those living in Arctic Village, the tribe in the 1960s repeatedly requested that the constitution be amended, but the Department of the Interior took no action. As early as 1962, Arctic Village and Venetie attempted to form a joint council but were thwarted by bureaucratic inaction.¹⁴ The department continued to reject efforts of the Neets'aii Gwich'in to reorganize their government through the 1960s and 1970s, apparently because the reservation-wide powers of the common council conflicted with the same powers vested in the IRA council by the latter's constitution. Finally, in 1976 the tribe went ahead with its restructuring and formed a common council with staggered terms of three years each for the nine council members. The odd number of council members was managed by having two elected alternately from each village. In addition, the positions

of first and second chiefs are rotated every three years between the two villages.

The tribal council deals with any issue that affects the reservation as a whole. It manages controversies that arise with the state and federal governments, makes general rules for the use of the reservation, and deals with issues of tribal sovereignty. Matters affecting tribal enrollment and membership, children and the state, welfare, and taxation fall within its purview. Its interests include leases of land, fees for use of the landing strips, land development and use, and protection of the reservation's resources, particularly the Porcupine caribou herd, which is vital to tribal survival.

In addition to tribal government, each village has its own council. They are similar in organization and carry out essentially the same functions. Each operates an airfield and an electric generating plant, collecting and keeping the fees. Both sell fuel to the residents, run laundromats and showers for public use, and operate health clinics. Each village maintains its own roads and runway with a small fleet of bulldozers, trucks, and graders. Both run water systems, drawing water from the Chandalar River. Venetie sells water to the school; in Arctic Village, the school has its own supply.

The village governments have as primary functions the settling of disputes among its residents and enforcing tribal rules. For example, the possession and use of alcohol and drugs are prohibited; violators are punished by fines and, in persistent cases, banishment from the reservation for specified lengths of time. In addition to banishment and fines, the village councils have a more subtle, but equally potent form of persuasion. The village has a limited number of paid jobs available in the summer and always has more applicants than work. The work is generally divided among members so that all share, but if an individual breaks the rules, he or she falls to the bottom of the list, or if the violation is particularly egregious, is taken off the list completely.

Land, Subsistence, and Identity

The strength of the Neets'ait Gwich'in's attachment to their land is illustrated by their reaction to the Alaska Native Claims Settlement Act of 1971. Under the act, the Neets'ait had the option of either taking a cash settlement and turning over the subsoil rights to the Doyon Native Corporation, or rejecting the cash and keeping all rights and ownership of the reservation. They chose the latter. The importance of that decision was perhaps expressed best by Larry Williams of Venetie when he testified before the Alaska Native Review Commission.

We'd rather have the land, and that's the way it stands today. And it's up to the people in the tribal council to keep up that tradition of keeping the land as it is, and we call ourselves a sovereign people. And that's the way it should be, because we don't have to ask anybody, we going to hunt on our land or to get timber to build our cabins. We go out and do it without any waste, and we have our own laws that [we] follow, that's been in existence before the White man law came into the village, came into the country. And we still follow that. That's a traditional law. (Berger, 1985:142)

Much of the life in the two villages revolves around subsistence activities. From the Neets'ait Gwich'in point of view, these connote a rich and varied food supply and a key dimension of tribal self-sufficiency and sovereignty. It includes the seasonal exploitation of the resources available on the reservation, supplemented by day labor.

During the summer months many tribal members take temporary employment with the villages or go to Fairbanks and Anchorage for jobs; others fish for salmon on the Yukon River and for burbot, graylings, pike, and whitefish in the streams and lakes. With the onset of autumn, individuals hunt moose and caribou, shoot ducks and geese, and bring in a supply of wood. Dall sheep are hunted in the Brooks Range, and moose are available in the lowlands that border the river systems.

Trapping begins in the late fall and continues through the winter. Trap routes are sometime quite long and distant from the villages, many exceed 100 miles (160.9 km) in length, and men generally work the trap lines in pairs. Commonly, these pairs consist of brothers or other close kin. There is considerable preparatory work that goes into trapping. Trails must be cleared so that the snowmobiles can move easily; caches of food, fuel, traps, and tools must be distributed along the trap line, and cabins must be constructed and repaired.

In 1991 there were upwards of 40 families that made their living trapping. So many individuals ran trap lines that there was not enough land available within the reservation, forcing trappers to extend their trap lines beyond reservation boundaries. The trappers are divided into two types: those who have short lines close to their village, that is, individuals who can tend the traps in a day or with an overnight stay, and those whose lines require them to stay out for weeks at a time. These trappers often put out 150 to 200 traps and an equal number of snares, and their trap lines often run 100 miles (160.9 km) or more in length. They concentrate on trapping lynx and martens and, to a lesser extent, wolves.

Over the years there has evolved a general agreement among the Neets'ait Gwich'in regarding which areas of the reservation and adjoining lands are recognized as the principal hunting and trapping areas of each village. It is common for pairs of men from the two villages to hunt together, increasing the subsistence opportunities when game such as caribou and moose are scarce in a particular area.

There are strong cultural prohibitions against killing more game than is needed. Caulfield (1983:205-210), who has done extensive research with the Neets'ait Gwich'in, has identified five customary laws related to hunting and land use: (1) each village has "a prescribed area of use which, though not totally exclusive in nature, places limits upon use of the land by non-community residents;" (2) there are prescribed subareas in which individual families hold a usufruct right; (3) each village has the authority to set limits on the game taken within the area recognized as its prescribed area; (4) each village may determine when there is sufficient caribou or other game to permit

harvesting; and (5) exploitation and industrial utilization of the land are secondary to the protection of the habitat.

The rules of behavior that govern the tribe and the villages work, to a large degree, because of the close kin networks that exist. These ties continue to be a decisive factor in Neets' aii society, despite the increased frequency of contact with other native people. In 1991 there were 41 households in Arctic Village, all descended from six patrilineal lines: Albert Tritt, Sr., Paul Williams, Sr., Birchcreek James, Peter Peter, Joseph Gilbert, and Isias Sam. All but one of the married couples living in the village had Indian spouses. In Venetie, the 58 households are descended from seven patrilineal lines: Old Man Robert, Old John, Paul Erick, Albert Tritt, Sr., Chief Christian, Johnny Frank, and Elijah Henry. In Venetie, the Franks are married to members of the John, Roberts, Tritt, Henry, and Fredson families. The Christians are married to the Robertses and Ericks. In Arctic Village the Tritt family is married to members of the Roberts, Christian, James, Frank, Sam, and Williams families. The James family is married to the Christian and John families; the Williams to the Ericks; the Peters to the Gilbert, Tritt, and Sam families (Campisi, 1989, 1991).

Added to the importance of kinship is the Neets' aii sense of themselves as a tribe, one that is inextricably linked to the land, and more particularly, to the Porcupine caribou herd. This herd, numbering in excess of 150,000 (Davis, 1997:41), winters in western Alaska and eastern Yukon Territory, Canada, and in the spring migrates to its calving grounds along the Arctic coast. Following the spring calving, the herd moves southerly to return to the boreal forests, where it disperses for the winter. The caribou are hunted from mid-August to their departure for the calving grounds in late April. Even though famine is no longer a danger, year-round survival would be an economic impossibility for the Neets' aii Gwich'in without the Porcupine caribou herd; the cost of transporting replacement food would be prohibitive.

The importance of caribou to Neets' aii Gwich'in life cannot be overstated. It is a defining feature of their worldview. Individuals repeatedly say that they do not feel well unless they have caribou regularly. According to Slobodin (1981:526), the Gwich'in have a special relationship with caribou:

Every caribou has a bit of the human heart in him, and every human has a bit of the caribou heart. Hence humans will always have partial knowledge of what caribou are thinking and feeling, but equally, caribou will have the same knowledge of humans. This is why caribou hunting is at times very easy, at other times very difficult. All hunted creatures are to be respected, but none, except the bear, more so than the caribou.

Despite the changes inflicted upon them by state and federal authorities, and by virtue of their adaptation of introduced technologies, the Neets' aii Gwich'in have managed to maintain a viable tribal society in a politically hostile world. This, in part, has been the result of their relative isolation, but in large measure it has flowed from their ownership of the 1.8 million acres that make up their property and which permits the exercise of political and social autonomy. Thus land and subsistence have become synonymous with sovereignty, and borrowings from the dominant culture—whether religious, technological, economic, or governmental—have been adapted to support a fiercely independent way of life.

Notes

1. Report, "Surrounding Villages," by the Bureau of Education, Alaska School Service, 1915, General Correspondence 1908–35; Records of the Alaska Division, 1877–1940; Records of the Bureau of Indian Affairs, RG 75; National Archives Building, Washington, D.C.
2. Minutes of Dec 1962, Neets' aii Gwich'in Tribal Archives (NGTA), Neets' aii Gwich'in Tribal Office, Venetie, Alaska.
3. John Fredson to Claude M. Hirst, 1 Jan 1938, NGTA.
4. Petition for Reserve, Jan 1938, NGTA.
5. William Zimmerman, Jr., to the Secretary of the Interior, 12 May 1939, NGTA.
6. George A. Dale to John Fredson, 17 Jun 1939, NGTA.
7. John Fredson to Clyde G. Sherman, 30 Dec 1939, NGTA.
8. Claude M. Hirst to William Zimmerman, Jr., 25 Jan 1940, NGTA.
9. Donald W. Hagerty to John Collier, 17 Feb 1940, NGTA.
10. Venetie, Arctic and Christian Robert's Fish Camp, Alaska; Proclamation Designating Indian Reservation, 20 May 1943, signed by Assistant Secretary of the Interior Oscar L. Chapman, NGTA.
11. John Fredson to Claude M. Hirst, 1 Mar 1944, NGTA.
12. T.W. Wheat to John Collier, 28 Mar 1944, NGTA.
13. Oscar W. Chapman to Anthony Dimond, 13 Dec 1944, NGTA.
14. Minutes of 9 Dec 1962, NGTA.

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IV. Anthropology Evolving

Classifying North American Indian Languages before 1850

Elisabeth Tooker

Anthropology, it might seem, has always been with us, and histories of the discipline often so presume. The beginnings of anthropology, however, are more fairly dated to the last quarter of the eighteenth century and the first quarter of the nineteenth century. In these decades, the words that characterize the discipline—ethnography, ethnology, and anthropology (in the now most commonly used sense of that word)—were introduced and gained acceptance in various European languages, and the great project of nineteenth century anthropology was undertaken: the systematic mapping of the peoples of the world.

Fueled by the intense interest in exploration at the time, the premise of this great mapping project was that each “nation,” “people,” or “race”—the terms were virtually interchangeable—could be distinguished on the basis of physical appearance, customs, and language. These were the same kinds of criteria—physical appearance and behavior, including vocalizations—on which “species” of other animals were identified, and this led, at least in the English-speaking world, to the inclusion of anthropology in natural history.

In one important respect, however, the identification of human species, as they were then often termed, differed from that of other animals. The latter might be best classified on the basis of physical characteristics; the former on the basis of language. This was so because each of the other animal species was generally (if imprecisely) defined as being able to breed with members of its own but not with other species. The definition of human species involved no such criterion. The various human species could interbreed with each other with the result that physical characteristics—skin, hair, and eye color, hair and head form, height, and the like—shaded from one species to another. So also did customs, a consequence of diffusion of ideas across species lines. Language was an easier and more convenient way of identifying what were then called nations:

In judging of the relations between savage and civilized life, something may be learnt by glancing over the divisions of the human race. For this end the classification by families of languages may be conveniently used, if checked by the evidence of bodily characteristics. No doubt speech by itself is an insufficient guide in tracing national descent, as witness the extreme cases of Jews in England, and three-parts negro races in the West Indies, nevertheless

speaking English as their mother-tongue. Still, under ordinary circumstances, connexion of speech does indicate more or less connexion of ancestral race. (Tylor, 1871:43–44)

Tylor was expressing an idea long in place and one applied not only to European languages in the seventeenth and eighteenth centuries, but also to non-European ones. For example, writing in 1641, the Jesuits noted of the Iroquoian languages:

We have every reason to believe that not long ago they all made but one People,—both Hurons and Iroquois, and those of the Neutral Nation; and that they came from one and the same family, or from a few old stocks which formerly landed on the coasts of these regions. (Thwaites, 1898:193–195)

Similarity of words as well as custom and physical appearance was used in the seventeenth century, notably by Hugo Grotius and Johannes de Laet in the 1640s (Charlevoix, 1761:16–31), to answer the question of the origin of American Indians. But as they employed it, the method had obvious faults. As Pierre de Charlevoix noted in 1744 of this work, in what was probably the most influential discussion of the matter in the eighteenth century,

The simple resemblance of names, and some slight appearances, seemed, in their eyes, so many proofs, and on such ruinous foundations they have erected systems of which they have become enamoured, the weakness of which the most ignorant are able to perceive, and which are often overturned by one single fact which is incontestable.

But what is most singular in this, is, that they should have neglected the only means that remained to come at the truth of what they were in search of; I mean, the comparing the languages. In effect, in the research in question, it appears to me, that the knowledge of the principal languages of America, and the comparing them with those of our Hemisphere, that are looked upon as primitive, might possibly set us upon some happy discovery; and that way of ascending to the original of nations, which is the least equivocal, is far from being so difficult as might be imagined. (Charlevoix, 1761:49–50)

Establishing relationships between languages on the basis of their grammar requires considerable knowledge of the languages themselves, making it an unwieldy method when dealing with large numbers of languages. The easier method is to compare vocabularies, and undoubtedly for this reason word lists that could be used in collecting data for comparative purposes were developed by various individuals. The most notable was the philosopher Gottfried Wilhelm Leibniz.

From at least the time of Leibniz’s famous appeal (Gulya, 1974:258–259), these lists have comprised basic vocabulary, the kind of words taught to children and hence known to all and easy to collect. They are also apt to consist of words that

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change relatively slowly. Although the lists compiled by various individuals are not identical, they typically include such words as those for the following: family relationships (e.g., father, mother, son, daughter, brother, sister, uncle, aunt); parts of the body (e.g., head, face, nose, eye, hair, mouth, hand, fingers, foot); natural phenomena (e.g., God, sky, sun, moon, star, air, wind, rain, lightning, snow, ice); animals (e.g., dog, wolf, bear, fox, bird, snake, fish); things ingested (e.g., food, drink, water, bread, meat); actions (e.g., eat, drink, speak, see, be, stand, laugh, sleep); seasons (e.g., spring, summer, autumn, winter); colors (e.g., white, black, red, green); senses (e.g., hearing, sight, taste, smell); and numbers (e.g., one, two, ten, twenty, one hundred, one thousand).

The invention of movable type and hence the publication of printed books facilitated the comparative study of languages, and some such studies were made, but these now seem to be tentative steps. In the mid-1780s three individuals undertook research that more directly influenced modern comparative historical linguistics.

Perhaps the most famous is Sir William Jones. In an address before the Asiatick Society of Bengal in 1786, Jones observed that the similarities in both roots of verbs and forms of grammar between Sanskrit, Greek, Latin, Germanic, Celtic, and Old Persian (similarities not shared with the Semitic languages) could not have originated by accident and thus were evidence of a common source. This idea was picked up by others whose work firmly established Indo-European as a language family.

The second most famous is Catherine the Great of Russia. In contrast to the more western countries of Europe, Russia had a great diversity of languages, a fact that partly accounts for Catherine's interest in the languages of the world and the study of them that she undertook two years before Sir William Jones's famous pronouncement. Distraught over the death of her lover of four years, Alexander Lanskoj, in June 1784 (Key, 1980:55), she turned her attention to the comparison of vocabularies. On 9 September 1784 she wrote to a friend:

I've got hold of as many dictionaries as I could find, including a Finnish, a Mari and a Votic [Finno-Ugric languages]; from these I compile my word-lists. Also, I have collected a lot of information about the ancient Slavs, and I shall soon be able to prove that the Slavs named the majority of rivers, mountains, valleys and regions of France, Spain, Scotland and elsewhere. (Cronin, 1978:232)

It was later, apparently, that Catherine compiled the word list that she used as a basis for comparison. In a letter dated 9 May 1785 she wrote:

I made a list of between two and three hundred radical words of the Russian language, and had them translated into every tongue and jargon that I could hear of; the number of which already exceeds two hundred. Every day I took one of these words and wrote it down in all the languages I had been able to collect. (Pickering, 1818:322)

But she then wearied of the task and turned over her materials to Peter Simon Pallas, who prepared them for publication. The results were printed in Russian in two volumes with a prefixed Latin title, *Linguarum totius orbis vocabularia compara-*

tiva augustissimae cura collecta (Pallas, 1787–1789). In it, Pallas compared 285 words, 130 in the first volume and 155 in the second.

About the same time, Catherine decided to expand the study to include data from America and Africa and, for the purpose of obtaining this information, in 1786 had the 285-word list printed. Copies were sent to various individuals who might be able to provide vocabularies or might know someone who could. One recipient was the Marquis de Lafayette,¹ who on 10 February 1786 sent copies to Benjamin Franklin, George Washington, and perhaps others he knew in the United States (Key, 1980:61–63). Whether Franklin, then 80, sent copies on to others is not known, but Washington did, sending Lafayette's request to Thomas Hutchins, then surveying western lands for the Continental Congress.² After he had written Hutchins, Washington learned of Richard Butler's appointment as superintendent of Indian affairs in the Ohio region and on 27 November wrote to him also (Fitzpatrick, 1938, 28:525; 1939, 29:88–90).

Washington had more than a casual interest in Catherine's project. Writing to the Reverend Jonathan Edwards, Jr., on 28 August 1788 to acknowledge receipt of Edwards's description of Mahican (Edwards, 1788), Washington echoed the ideas of Charlevoix, among others:

You have been rightly informed relative to the application, which had been made to me from Europe, for Documents concerning the Indian Language. It seems that a Society of *Literati* are endeavouring to make discoveries respecting the origin and derivation of different Languages. In the prosecution of this curious study, all Judicious philological communications must be important, yours, I conceive, will not be deficient in that quality. I have long regretted that so many Tribes of the American Aborigines should have become almost or entirely, extinct, without leaving such vestiges, as that the genius and idiom of their Language might be traced. Perhaps, from such sources, the descent or kindred of nations, whose origins are lost in remote antiquity or illiterate darkness, might be more rationally investigated, than in any other mode. The task you have imposed upon yourself, of preserving some materials for this purpose, is certainly to be commended. (Fitzpatrick, 1939, 30:64)

The year before, on 25 March 1787, Washington had written Lafayette that both Butler and Hutchins had assured him that they would obtain vocabularies for Catherine the Great (Fitzpatrick, 1939, 29:183–184). A week before, on 18 March, James Madison had written Washington enclosing a Cherokee and Choctaw comparative vocabulary he had received from Benjamin Hawkins.³ In this letter, which Washington did not receive until 31 March (Fitzpatrick, 1939, 29:191), Madison wrote:

Recollecting to have heard you mention a plan formed by the Empress of Russia for a comparative view of the aborigines of the New Continent, and of the N. E. parts of the old, through the medium of their respective tongues, and that her wishes had been conveyed to you for your aid in obtaining the American vocabularies, I have availed myself of an opportunity offered by the kindness of Mr. Hawkins, of taking a copy of such a sample of the Cherokee & Choctaw dialects as his late commission to treat with them enabled him to obtain, and do myself the honor now of inclosing it. I do not know how far the list of words made use of by Mr. Hawkins may correspond with the standard of the Empress, nor how far nations so remote as the Cherokees & Choctaws from the N.W. shores of America, may fall within the scheme of comparison. I presume how-

ever that a great proportion at least of the words will answer, and that the laudable curiosity which suggests investigations of this sort will be pleased with every enlargement of the field for indulging it. (Hunt, 1901:320–321)

Later that same year, on 30 November, Butler sent the Shawnee and Delaware vocabularies he had collected to Washington, and on 10 January 1788 Washington sent a copy of them along with a copy of Hawkins's Cherokee and Choctaw vocabularies to Lafayette⁴ (Fitzpatrick, 1939, 29:373–377; Key, 1980:67). Hutchins, however, never sent Washington the promised material. He died in April 1789, apparently before he had collected the information Washington had asked him to furnish.

A second section to Pallas's *Linguarum*, which was to have contained in one volume data on the languages of America and Africa, was never published. In its stead was published a four-volume edition of Pallas's materials by F.I. Yankovitch de Mirievo (1790–1791), having a different arrangement and covering the entire world. Pallas's volumes were, however, the more influential.

In the spring of 1796, Benjamin Smith Barton received a copy of the *Linguarum* from the noted chemist Joseph Priestly, who had come to the United States two years before. A native of Philadelphia, Barton (1787) had published a volume titled *Observations on Some Parts of Natural History* while in Edinburgh studying medicine. Reflecting the increased interest in archaeology after the founding of the Republic—part of the attempt on the part of Americans to create a history of the continent separate from the history of England—Barton's study contained some archaeological data. While in Edinburgh, Barton also attempted to find some resemblance between American and Asiatic languages, but he had little success. On his return to the United States in 1789, he took up the study again and was somewhat encouraged by his results (Barton, 1797:xxiii–xxiv). After receiving the book from Priestly, Barton redoubled his efforts, collecting more data from his reading, through correspondence, and on his travels. In 1797 he published his results in *New Views of the Origin of the Tribes and Nations of America*, which was republished the following year with a vocabulary list extended to 70 words from the original 54 words (Barton, 1798). Although his reasoning has much to commend it, later students regarded his data as being too inadequate to support his conclusions, and today his study is regarded more as an interesting curiosity than as a scientific contribution.

Modern comparative studies of North American Indian languages, however, owe more to the third important figure active in the 1780s, Thomas Jefferson, than they do to Barton. In his *Notes on the State of Virginia*, first published in 1785, Jefferson made the same observations as Charlevoix had 40 years before in a work with which Jefferson was familiar:

Great question has arisen from whence came those aboriginals of America? ...A knowledge of their several languages would be the most certain evidence of their derivation which could be produced. In fact, it is the best proof of the affinity of nations which ever can be referred to. How many ages have elapsed since the English, the Dutch, the Germans, the Swiss, the Norwegians, Danes

and Swedes have separated from their common stock? It is to be lamented then, very much to be lamented, that we have suffered so many of the Indian tribes already to extinguish, without our having previously collected and deposited in the records of literature, the general rudiments at least of the languages they spoke. Were vocabularies formed of all the languages spoken in North and South America, ...and these deposited in all the public libraries, it would furnish opportunities to those skilled in the languages of the old world to compare them with these, now, or at any future time, and hence to construct the best evidence of the derivation of this part of the human race. (Jefferson, 1955:100–101)

Late in December 1783, less than two years before *Notes on the State of Virginia* was published and a full six months before Catherine the Great began her linguistic project, Jefferson initiated his own comparative study of the American Indian languages—sending letters requesting vocabulary lists to various individuals, including Benjamin Hawkins⁵ and Thomas Hutchins (Boyd, 1952:427, 431). Only Hawkins replied, in 1787 sending Jefferson, then in Paris, a copy of the Cherokee and Choctaw vocabulary that he had also sent to Madison.

In July 1784 Jefferson sailed for England, and in 1785 he was appointed to succeed Benjamin Franklin as minister to France. He left in 1789 and the following year accepted Washington's offer to serve as his secretary of state. Back in the United States, Jefferson's interest in American Indian languages was renewed. On 13 June 1791, in the presence of James Madison and General William Floyd, Jefferson obtained a vocabulary list from some Unquachog (Poosepatuck) living in a village in Brookhaven Township, Long Island.⁶ By the next year, if not before, he had a 282-word list printed, comparable to Catherine the Great's but not copied from hers.⁷ (Jefferson had not then seen a copy of Pallas's *Linguarum*, nor did he see one for some years.) For 35 years, Jefferson's list was extensively used in the United States; it was supplanted only by the list composed by Albert Gallatin, who had been Jefferson's secretary of the treasury.

At the end of 1793, Jefferson resigned as secretary of state and returned to his home, Monticello, in Virginia. On 3 March 1797 he became president of the American Philosophical Society in Philadelphia, what its members saw as the American equivalent of the Royal Society of London. The following day he was inaugurated vice-president of the United States. Four years later he became president of the United States.

During these years, Jefferson collected from others more vocabulary lists as he could, perhaps totaling 40 (Bergh, 1907:4–5). Jefferson also made up at least one long comparative list of such data that he had on 22 languages.⁸ A substantial portion of these manuscripts were lost, however, when robbers stole the box that contained these papers while they were being shipped back to Monticello at the end of Jefferson's term as president. Only some were recovered, and Jefferson lost interest in completing the project.

In 1814 Jefferson resigned as president of the American Philosophical Society. The following year Barton died. An era had ended. Linguistic studies in the United States passed from Jefferson and Barton, who had both encouraged and criticized

each other's work, to Peter Duponceau, John Pickering, and Albert Gallatin, who also exchanged information and ideas.

Before coming to the United States from France in 1777, Duponceau had served as secretary to Antoine Court de Gébelin, then a noted philologist and author of *Monde primitif* (1773–1782), an important study in universal grammar. Not until 1815, however, when the American Philosophical Society established the Committee of History, Moral Science, and General Literature, did Duponceau show much interest in linguistics. He became the committee's corresponding secretary, seeking out information as he could. One of his correspondents was the Reverend John Heckewelder, a Moravian missionary whose correspondence and memoirs on the Delaware Indians Duponceau published (Heckewelder, 1819). Extending Barton's comparative study, Duponceau collected more vocabulary lists, applying to and receiving from Jefferson his surviving lists. And reflective of the work of European philologists, Duponceau was interested in grammar, proposing that American Indian languages were characterized by what he termed "polysynthesis"—an idea that subsequently was widely discussed.

The publication of Heckewelder's data at the instigation of Duponceau spurred John Pickering's interest in American Indian linguistics. Son of Timothy Pickering, who among other things had negotiated a treaty with the Iroquois and who was John Adams's secretary of state, John Pickering had early been attracted to the study of languages, becoming an authority on Greek. Pickering published some manuscript materials and republished some important early works on American Indian languages, but he is perhaps most renowned for his "On the Adoption of a Uniform Orthography for the Indian Languages of North America" (Pickering, 1818). In it he proposed a standard orthography that was widely used by missionary societies, which in the nineteenth century were perhaps the greatest sources of data on American Indian languages.

Both Duponceau and Pickering made some comparative studies. It was, however, Gallatin who completed Jefferson's project. In 1823, at the request of Alexander von Humboldt, Gallatin wrote an essay on the languages of North American Indians, which subsequently received favorable notice in Balbi's (1826) *Atlas ethnographique du globe*. In the next several years Gallatin undertook a more extensive study of Indian languages, collecting data, some of which he obtained from Duponceau, including that collected by Jefferson. In 1826 he published a some 600-word vocabulary list and a preliminary classification of 71 languages of Indians of the United States into 15 families.⁹

Also in 1826 Gallatin went to London on a government mission, and not until 1834 did he return to the subject. The results of his research were published in the transactions of the society in 1836 under the title "A Synopsis of the Indian Tribes within the United States East of the Rocky Mountains, and in the British and Russian Possessions in North America" (Gallatin, 1836). This volume contained vocabulary lists on 82 languages

of North American Indians, languages Gallatin classified as belonging to 28 families.

At the time, the subject of language was deemed important enough for a philologist to be included in the scientific personnel of the Wilkes Expedition, in 1838–1842, America's great exploring expedition to the Pacific. Horatio Hale, then just graduated from Harvard College, was appointed to the position. The son of the noted editor and poet, Sarah Josepha Hale, he probably through her met Pickering, with whom he studied on an informal basis. And it may have been Pickering who encouraged Hale to collect and publish as a college freshman the data in his pamphlet *Remarks on the Language of the St. John's or Wlastukweek Indians, with a Penobscot Vocabulary* (Hale, 1834). John Pickering's nephew, Charles Pickering, was also on the Wilkes expedition and after his return published *The Races of Man and their Geographical Distribution* (Pickering, 1848).

Gallatin, in part because of the lack of available data, had reduced the number of words on his comparative list to 120. To extend Gallatin's classification, Hale used the same list with a few changes. He published the data he had collected on the languages of the Northwest Coast and the Plateau in his final report, *Ethnography and Philology* (Hale, 1846), along with a nearly complete classification of the languages of the region, except the northernmost (Goddard, 1996:293).

Only 250 copies of Hale's final report were printed. This circumstance and the fact that Gallatin earlier had obtained virtually no data on the languages of North American Indians west of the Rocky Mountains led Gallatin to excerpt from Hale's volume materials on Northwest Coast languages. He reprinted them along with a revision of his own classification in *Hale's Indians of North-west America* (Gallatin, 1848).

Gallatin's death a year later, in August of 1849, again marked the end of an era. Duponceau had died in 1844, and Pickering in 1846. Hale, who had gone to Europe after he finished writing his report, did not return to the study of Indian languages for almost a quarter century. The task of classifying North American Indian languages passed first to William W. Turner. After Turner's death, in 1859, it passed to George Gibbs, and then after Gibbs's death, in 1873, to John Wesley Powell, whose "Indian Linguistic Families of America North of Mexico" (Powell, 1891) expanded Gallatin's classification to all parts of the continent—a classification that became the departure point for subsequent discussion of deeper linguistic relationships. It remains an important one to this day.

Notes

1. Marquis de Lafayette to Benjamin Franklin, 10 Feb 1786, American Philosophical Society Library (APSL), Philadelphia, Pennsylvania, cataloged in Freeman, 1966, Freeman no. 1171.

2. The text of George Washington's letter of 20 Aug 1786 addressed to Thomas Hutchins, published in Fitzpatrick (1938, 28:52) and Abbott (1995:222), is from Washington's letter book. An autograph letter also dated 20 Aug 1786 and having an identical text—a letter stated in 1907 to be in the possession of A.S. Morgan (Fitzpatrick, 1938, 28:525 n. 63) and sold at auction in

1999 (Sotheby's, 1999:157)—bears at the end the notation "(Addressed to Col. George Morgan of Prospect near Princeton)" in an unknown hand, and for this reason is generally said to be a letter Washington sent to Morgan. Other evidence, however, suggests this may not have been the case. More likely, this autograph letter is the one Washington sent to Hutchins, the notation at the end of the letter having been added at some later date by someone unfamiliar with its history.

When going through Hutchins's papers after his death, in 1789, George Morgan found a copy of Lafayette's letter to Washington and Washington's request to Hutchins. In September of that year, he wrote Washington, sending a copy of the Lord's Prayer in Delaware that he had obtained from David Zeisberger, a Moravian missionary, and offering to send Zeisberger's Delaware vocabulary and grammar then in the possession of Morgan's son. Morgan also offered to send Washington a Shawnee vocabulary and grammar as well as a translation of the Lord's Prayer he had obtained from Alexander McKee (Twohig, 1989:591–592). If Washington had written Morgan on the same day he wrote Hutchins, it seems likely that Morgan would have sent Washington these materials then, or at least remembered the letter three years later when he did write Washington. It also seems likely that Morgan took Washington's letter to Hutchins with him. Only later, probably after Morgan's death, did this letter come to be regarded as one written by Washington to Morgan, and a notation to this effect was added.

3. About the same time Hawkins sent this vocabulary to Madison, he also sent a copy to Jefferson (see below). The copy Madison sent Washington, now in the Washington Papers (see note 4, below), is identical to the copy sent to Jefferson, now in the APSL (Freeman no. 663), except for the omission of a few words and phrases. An endorsement in Jefferson's hand (Boyd, 1955:203)

at the end of his copy, stating that "This vocabulary was from Benjamin Hawkins, probably before 1784," is probably in error.

4. Washington to Lafayette, 10 Jan 1788, Papers of George Washington, 1592–1943 (bulk 1748–1799), Library of Congress, Washington, D.C., microfilm 12,935-124N-124P-124P (series 1-8D).

5. Just why Jefferson selected Hawkins for this task can only be conjectured. Both were members of the Continental Congress in 1783, and Hawkins, a delegate from North Carolina, served on a number of committees dealing with Indian matters, including the committee on Indian affairs (Pound, 1951:35). In 1885–1886 he was a member of the commission that negotiated treaties with the Cherokee, Choctaws, and Chickasaws (Pound, 1951:45–51), and it was perhaps at this time that Hawkins obtained or made arrangements to obtain the Cherokee and Choctaw vocabularies he sent to Jefferson and Madison. From 1790 to 1795 Hawkins served in the United States Senate. Defeated in his bid for a second term, he became agent to the Creeks and other southern Indians, a position he held until his death, in 1816.

6. Jefferson manuscript, 1791, Unquachog Vocabulary, APSL, Freeman no. 2335.

7. Jefferson, Vocabulary, broadside printed ca. 1790–1792, APSL, Freeman no. 2051.

8. Manuscript, 1802–1808, Comparative Vocabularies of Several Indian Languages, APSL, Freeman no. 1289.

9. Printed circular letter of 8 pages consisting of a cover letter dated 16 May 1826 by James Barbour; No. I: Vocabulary; No. II, Verbal Forms and Select Sentences; No. III: A Table of Indian Tribes of the United States East of the Stony Mountains, Arranged According to Languages and Dialects, Furnished by Albert Gallatin, United States Department of War, Washington, D.C.: microfilm no. 234, roll 429, National Archives, Washington, D.C.

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Origins of Museum Anthropology at the Smithsonian Institution and Beyond

William W. Fitzhugh

Anthropological subjects were among the first fields of inquiry identified by the Smithsonian founding fathers in 1846. Resolution number 3 of the Smithsonian Board of Regents' meeting of 4 December 1846 called on the secretaries of state, the treasury, war, and the Navy "to furnish... suggestions... in regard to the procurement... of additions to the museum... especially to its ethnological departments."¹ Resolution number 4 requested the secretary of war to obtain from the commissioner of Indian affairs "suggestions as he may deem proper regarding the procurement from the Indian country of collections for the museum of the Smithsonian Institution illustrating the natural history of the country, and more especially the physical history, manners and customs of the various tribes of aborigines on the North American continent" (Board of Regents, 1847:11).

These instructions originated the oldest institutional anthropological program in the Americas and one of the earliest systematic approaches to the study of non-European cultures in the history of Western science. Although the discipline of anthropology did not become formally professionalized until Franz Boas established the first Department of Anthropology at Columbia University, in 1901, the practice of anthropology and its subfields—linguistics, ethnology, archaeology, and physical anthropology—had been pursued vigorously at the Smithsonian for more than 50 years and had been taught in universities since 1879 (Tooker, 1990). Because most anthropologists in North America trace their origins to the beginnings of academic anthropology, the importance of this instance of early museum collecting, research, and exhibition is not widely known. In fact, one of anthropology's deepest roots lies in natural history as it was practiced in European and American museums from the 1830s to the 1850s.

The museum role was especially important in imparting a scientific method for data gathering, analysis, and classification, not only of material culture, but of social life, behavior, and language. The models for these developments came originally from the early biological systematics of Linnaeus (1764), as applied in America by Spencer Fullerton Baird to zoological

field studies before and during his early years as assistant secretary of the Smithsonian Institution. The biological ancestry of early anthropological field method is not widely recognized, probably because of the corrosive effect that evolutionary and racial theory (e.g., Morton, 1839) had on early anthropological thought (Stanton, 1960; Stocking, 1966). Aspects of these anthropological developments have been discussed (e.g., Hinsley, 1981) largely without recognition of the debt owed to Baird, Robert Kennicott, and other early Smithsonian naturalists who worked under his supervision. While none of these collectors and researchers were trained ethnologists, their methods, adapted from biology, produced the Smithsonian's first important ethnological collections and research programs.

The Baird-Kennicott collecting program in northwestern British America (Lindsay, 1993) became a model for later Smithsonian field programs that almost always included ethnological, archaeological, and linguistic components. While Kennicott's ethnological collections and field observations were never published, his project pioneered field methods that were later applied by Smithsonian collectors in the Yukon-Mackenzie region of northwestern Canada and Alaska between 1870 and 1890. Despite advances in field method and analysis, it was not until John Wesley Powell arrived at the Smithsonian, in 1879, that analysis and publication of ethnological collections became standard practice, in the newly founded Bureau of American Ethnology (BAE), which Powell had succeeded in getting Congress to establish as a permanent anthropological survey, with himself appointed as director.

Origins

The earliest and most visible product of the Smithsonian's charter was the publication of "Ancient Monuments of the Mississippi Valley" (Squier and Davis, 1848) in volume 1 of *Smithsonian Contributions to Knowledge*. As the Smithsonian's premier scientific publication, this series broadcast a commitment to serious scholarly endeavor, and the selection of an archaeological contribution by the Board of Regents signalled their intent that cultural scholarship was one of the institution's core interests. This publication investigated the biggest historical mystery of the young republic, the identity of the "mound-builders" and their relation to historic American Indian tribes

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(Silverberg, 1968; Willey and Sabloff, 1980; Trigger, 1989). The work laid out the archaeological evidence pertaining to the mounds and took a skeptical view of the popular European supremacist theories of Indian origins (Thomas, 1894; Stanton, 1960:11). But it was not until the publication of *Report on the Mound Explorations* (Thomas, 1894) that their Indian origin was established conclusively.

The *Contributions* series, presided over by Joseph Henry, the first secretary of the institution, established the Smithsonian as a scholarly “mouthpiece” for a nation whose private publication resources were extremely limited and were not sufficient for the type of broad scientific discourse he and the Smithsonian founders envisioned. One thousand copies were printed and were sent to libraries across the country and throughout the world. In this way the Smithsonian’s publication outreach program and the scientific credentials of the nascent institution were launched in one fell swoop. The fact that this volume and most of the published contributions that followed were written by scientists throughout the country—from universities, natural history societies, amateur science societies, and others—established Smithsonian science as a broad national endeavor. Only later was authorization given for the institution to do original research and field expeditions on its own, an innovation that followed the appointment of Baird as assistant secretary, in 1850.

This vision of the Smithsonian as a central node in a broad national and international network of scholarly activity and exchanges was at the very core of the Smithsonian’s original charter. In those days, the institution’s “public” was the dedicated naturalist, the historian, and the antiquarian, for without a museum or any means to communicate broadly other than through its circulars and publications, its only real constituency was the educated elite. This was an age, literally, of rapidly expanding horizons, of national territorial growth, and of intellectual achievement in the wake of revolutionary progress in biology and geology. The opportunities for scientific advance were enormous, and scholars and naturalists throughout the country, as well as in Europe, saw the Smithsonian as both a leader and a political lobbyist for the emergence of a uniquely American science.

It took more than James Smithson’s mandate, “the increase and diffusion of knowledge,” to ensure that his bequest of \$508,318.40 in 1838 dollars (about \$6 to \$7 million today; Pamela Henson, pers. comm., Apr 1997) would not be squandered by Congress in other directions. There was also an urgent need to provide curatorial care and scientific analysis for the large government collection of artifacts and art held in the United States Patent Office in Washington, D.C. Among this collection were about 5000 ethnographic objects collected by the United States Exploring Expedition of 1838–1842, commanded by Lt. Charles Wilkes, from South America, the South Pacific, the Oregon Territory, and the British Northwest Coast (Viola and Margolis, 1985). Having experienced difficulties in completing the scientific analyses of the Wilkes collection and

having noted the damage incurred by curatorial neglect, including deterioration and loss of specimens and documentation, the need for a national museum had become clear, not only for natural history, ethnology, and linguistic collections, but for the American Indian paintings of C.B. King and George Catlin, and for other treasures stored by various groups in Washington, D.C., that became consolidated in 1840 as the National Institute (Hinsley, 1981:17).

Baird’s Science

With these problems in mind, Henry decided to hire an assistant to manage the collections and to organize a scientific program. Recommendations by Harvard’s Louis Agassiz (a Smithsonian Regent) and other leading scholars led Henry to select Baird (Figure 1), who had assisted James Dwight Dana in his studies of the Wilkes collection. Baird arrived in 1850 with two railroad boxcars full of specimens collected during his natural history fieldwork in eastern North America. If the Smithsonian authorities ever meant to limit the institution’s function to library and publication exchanges, as Henry and others had originally argued, they made a huge mistake in hiring Baird (Goode, 1897; Dall, 1915; Washburn, 1965, 1967; Rivinus and Youssef, 1992).

Baird was an accomplished biologist whose research had already won him a respected place in the leadership of American science. He was an organizer whose efforts over more than a decade had established a vast array of scientific collaboration with scholars and amateur field collectors throughout North America. Ultimately, the purpose of his acquisitiveness was not to build a museum but to conduct scientific research.² Today we cannot imagine the need for such broad-scale collecting, both of so much material and by so many collectors, but for a biologist seeking to systematize the newly discovered fauna and flora of the New World, it was necessary to begin by building large reference collections to which new specimens could be compared for purposes of scientific classification. In many cases the North American flora and fauna differed markedly from Europe’s more familiar and better-studied biota. In addition, beyond the arcane methods of taxonomy and classification, geography, climate, and environment were beginning to be recognized as having an impact on organism morphology, evolution of species, formation of ecological complexes, and delineation of life zones. Even species identity itself was still a novel and malleable biological concept. For these reasons, Baird believed that one had to build large collections from contiguous geographical zones to understand regional variation and recognize diagnostic features of species and subspecies.

To cite one example of his organizational and collecting techniques, Baird convinced his wife’s father, General Sylvester Churchill, to issue a circular to military officers at bases in remote locations, asking them to make natural history collections in their spare time. Baird’s circular provided details as to what species should be collected, what observations were



FIGURE 1.—Spencer F. Baird, assistant secretary of the Smithsonian Institution, established and implemented the institution's field programs and documentation systems. Courtesy of the Smithsonian Institution (neg. no. 46853-A).

required for each species, and what information should be recorded about geography and environment. Other government agencies conducting land and boundary commission surveys, naval explorations, or gathering tidal and climatic observations were also solicited and were provided with written instructions. Given the rapidly expanding United States territorial boundaries of the day, the result was a deluge of information and specimens. When Baird landed his job at the Smithsonian, his organizational abilities resulted in an explosion of government and private collaborations, and he used his position, with Henry's authority and concurrence, to establish the Smithsonian as the premier collecting and exchange center for biological materials in North America. Two boxcars was "peanuts" to the flow of collections that soon began to arrive. Yet even with meager assistance from Henry's budget, Baird parlayed the Smithsonian's unofficial "National Museum" into a national repository and center of scientific research.

During these early years, Baird's goals were strictly scientific and could not be considered educational or display oriented. His purpose in building collections was not for public edification in the general sense. Rather, he wanted to compile large, synoptic, documented reference collections to assess bio-

logical variation and to revise earlier Linnaean classifications of American fauna that had been based on inadequate numbers of specimens from restricted geographic regions. One of Baird's first projects at the Smithsonian was to use the large collection to prepare his *Catalogue of North American Reptiles in the Museum of the Smithsonian Institution* (Baird and Girard, 1853). Scientifically, this work was an important revision of an earlier outdated study. It is best known today, however, for having infuriated his former mentor, Louis Agassiz, the doyen of biology at Harvard, who felt upstaged and outraged by the prominent role played in the project by Charles Girard, his estranged former student (Rivinus and Youssef, 1992: 98–105; Lindsay, 1993:16–17).

Baird's acquisitiveness, however, did not lead immediately to castle-building. In Baird's view, once basic classification was accomplished with the aid of large suites of specimens, only a few individual specimens identified as "types" needed to be retained in a permanent collection. Upon completing his studies, type specimens would be marked with small green tags and saved for the museum's reference collection, and the masses of similar specimens could then be discarded. In practice this meant large numbers of duplicate collections could be distributed to natural history societies and universities throughout the country and abroad, and (with loss to the legacy of Smithsonian collections; see Walsh, 2002) this was done assiduously, accompanied by the completed publications and collection records. Thus were born the two pillars of the Bairdian Smithsonian legacy that have continued until the present day: a concern for the importance of empirical (especially natural) science and the importance of sharing scientific information and specimens.

Baird displayed a genius for the organization of field collecting, data compilation, and mapping but later was criticized for his lack of contributions to biological theory (Rivinus and Youssef, 1992:100; Lindsay, 1993:16). His method, however, was not without forethought. He believed that advances in this pioneering stage of American natural science would come from assiduous collecting followed by classification and comparative assessment of large documented collections and subsequent taxonomic revisions of increasingly wider geographic scope. By 1854 Baird's array of collectors, often referred to as "Baird's missionaries" (Rivinus and Youssef, 1992:83), were supplying the Smithsonian from 26 different expeditions and numerous other government surveys. In return, Baird provided instructions, collecting equipment, small sums of money for field expenses, and return freight. All contributors received copies of the institution's annual reports in which full credit was given to each organization and collector, and many of the latter found their names attached to new species.

This was the system Baird established for natural history. The result was exponential growth of collections, rapid publication, broad dissemination of results through the institution's *Miscellaneous Contributions* and *Contributions to Knowledge* series and annual reports, and establishment of a network of

collectors and scientists receiving Smithsonian exchange collections and publications. Operating largely outside the universities (which had few capabilities for field research at this early date), he sometimes ruffled the feathers of the university elite. But on the other hand, he trained a new, independent breed of field naturalist whose careers lay largely outside the cluttered, elitist hierarchies of academy life, and whose contributions brought the newly explored and largely undisturbed regions of North America within the sphere of the scientific establishment (including talented amateurs) for the first time. In most cases it was not until after 1900 that university-based scientists arrived in America's "outback." In short, Baird had created a populist scientific revolution.

Six years into his term as Secretary, Henry summed up what he saw as the role of the Smithsonian:

The prominent idea embraced in the Smithsonian organization is that of cooperation and concerted action with all institutions and individuals engaged in the promotion of knowledge. Its design is not to monopolize any part of the wide fields of nature or of art, but to invite all to partake in the pleasure and honor of their cultivation. It seeks not to encroach upon ground occupied by other institutions, but to expend the funds in doing that which cannot be as well done by other means. It gives to the words of Smithson their most liberal interpretation, and "increases and diffuses knowledge among men" by promoting the discovery of new truths, and by disseminating these in every part of the civilized world. (Board of Regents, 1853:31)

This, I think, outlines the mission and politics of the Smithsonian in a fashion that is as relevant today as it was 150 years ago. There are, indeed, many things that can be done by the Smithsonian that are completely unique and national in scope. Synoptic programs like the Smithsonian's National Museum of Natural History exhibitions, the *Handbook of North American Indians* series, the Smithsonian Folklife Festival on the Mall, the National Science Information Service, the Smithsonian Traveling Exhibition Service, Folkways Recordings, and the recently established Institute for Conservation Biology are examples of the Smithsonian's response to a broad national mandate that reaches beyond individual research to the understanding and stewardship of the world at large.

Early Ethnological Collecting

Despite the fact that it was Baird, a biologist, who was responsible for hiring and directing the naturalists who made the Smithsonian's first ethnographic collections, it was primarily Henry's interest in ethnology and archeology, before Baird was hired, that established a place for human studies under the Smithsonian's "big tent."

Soon after arriving at the Smithsonian, in 1846, Henry received a letter from the eminent New York ethnologist, Henry Rowe Schoolcraft, urging him to adopt his "Plan for the Investigation of American Ethnology," which incorporated a library of philology, archaeological investigations of the earthworks of the Mississippi Valley, and a "Museum of Mankind" for collections of America's native peoples (Schoolcraft, 1846; Hinsley, 1981:20). Henry, already aware of the contributions made dur-

ing previous decades by philological and archaeological studies of the ancient world, became convinced that the answer to Thomas Jefferson's question, "from whence came those originals of America" (Jefferson, 1955:100), might also be settled in a similar manner, by dedicated cultural, archaeological, and linguistic field studies.

It has often been noted that, compared to the hard sciences and many social sciences, anthropology has been slow to develop scientific methods, and that its theory is largely borrowed from other disciplines. This appears to have been true in the emergence of Smithsonian anthropology, in which biology and linguistics played a formative role. The collecting instructions of George Gibbs, a philologist who had been an active member of the Northwest Boundary Survey Commission of 1857–1861 (see "Baird, Kennicott, and Systematic Museum Anthropology," below), although formulated outside the context of explicit cultural theory, recognized the importance of combining evidence from archaeology, ethnology, philology, and other fields and laid the foundation for Boas's later formation of an integrated science of culture. By establishing systematic data-collecting techniques, by building an empirical database, and by systematizing the collections and organizing exhibitions that explored cultural theory and cultural classifications, the Smithsonian made important contributions several decades before the establishment of anthropology in other museums and universities (Fitzhugh, 1996).

In searching for the origins of museum anthropology as it developed at the Smithsonian, I have been impressed by the contributions made by the Bairdian system of science described above. In this respect, the earliest phase of American museum anthropology had a different history from academic anthropology as the latter began to be practiced in the 1890s. If we may judge from modern tendencies reviewed below, museum anthropology may also have a different future as well.

Anthropological collecting, of course, was not a Smithsonian invention, even as a concerted museum-based activity. The European "Cabinet of Curiosities" was clearly the forerunner in this field (Urry, 1984). The European cabinets were devoted to the preservation and display of natural and "artificial" curiosities. For most of these cabinets, scientific inquiry was not a guiding force. Indian materials that arrived in Europe in the sixteenth and seventeenth centuries were collected in a haphazard manner as examples of the arts and industries of Native Americans and usually lacked scientific documentation.

The European cabinets, called "Kunstkammern" after about 1600, began in the mid-1500s in Florence, Prague, Dresden, Vienna, and other European locations. Copenhagen was one of the first cities to begin to collect, store, and display curiosities, which included ethnographic materials from Europe and many other areas of the world. As early as the early seventeenth century these collections were displayed in the Wormianum Museum maintained by Ole Worm, a widely-traveled physician and naturalist. Although his museum and its later incarnations had a staff, a catalog (maintained systematically beginning in

1825), and a cultural and typological basis of object organization, it was not until 1841, when a new curator named Christian Thomsen separated the non-European materials within the *Kunstammer* and created the Ethnographical Department, that a scientific plan emerged (Dam-Mikkelsen and Lundbaek, 1980). Although Thomsen is best known for developing “three-age” systematics (stone, bronze, iron) for archaeology, in 1839 he also began supporting ethnographic collecting, and organized these collections in a systematic manner.

In an earlier era, the Cook voyages of the 1770s represented a step forward from previously undocumented collecting, thanks to the scientific and geographical observations of Joseph Banks and a dedicated natural history program conducted by the father and son team, Reinhold and George Forster (Kaeppler, 1978:2). The result was a more systematic approach to collecting, but one that still emphasized artificial curiosities. The fact that some specimens were described in print and others have survived in museum collections permits some understanding of the cultures encountered. Even so, the charge to the scientists by the British Admiralty was vague and unstructured. Cook was to collect materials illustrating characteristic features of native peoples that would serve to identify them to voyagers rather than for purposes of scientific description or comparative study (Kaeppler, 1978:37–48).

Sir Hans Sloane described his artifact collections, which were purchased by the British Crown in 1753 and founded the British Museum (MacGregor, 1994), as “miscellanies.” They lacked any systematic basis, either in their collection or cataloging, other than their chronology of accession (King, 1994:231), and reflected Sloane’s primarily antiquarian interest. His collecting began during his service as a physician and amateur naturalist in Jamaica in the late seventeenth century. Only his botanical collections showed scientific organization, due to the more advanced state of this field. Like Ashton Lever, founder of the Leverian Museum, which displayed Cook collections in the 1780s (Kaeppler, 1972, 1978:12–15), Sloane rarely traveled, seems not to have grasped the importance of cultural context, and his collection was assembled secondhand from others.

During this early period, Russia developed a strong awareness of the value of specimens as scientific documents (Kinzhilov, 1983; Dzeniskevich and Pavlinskaya, 1988). The *Kunstammer* established by Peter the Great in St. Petersburg in 1714 is an early museum collection that exhibited anthropological materials. Most Russian expeditions to America began collecting ethnological and biological materials in the 1780s, when Governor Boehm of Kamchatka transmitted objects purchased from the James Cook expeditions to the Russian Academy of Science’s *Kunstammer* (today the Museum of Anthropology and Ethnology (MAE)) in St. Petersburg. Later, Joseph Billings and Gavril Andreevich Sarychev donated ethnological materials to the MAE from their North Pacific voyages of 1785–1794. The Russian Admiralty Museum acquired materials from the Krusenstern and Lisianski voyages to Alaska

(1803–1806), and 100 of these specimens were transmitted to the MAE after 1930. Other Russian expeditions, including those commanded by Golovnin (1817–1818), Kashevarov (1830–1860s), Arkimandritov (1840s), and Zagoskin (1842–1844) all provided Alaskan specimens to the MAE.

As early as 1741, German naturalist Georg Wilhelm Steller, on Vitus Bering’s voyage to Alaska, speculated on relationships between the native peoples of Alaska and Kamchatka on the basis of linguistic similarities (Steller, 1988). In Russia, however, Ilyia Voznesenskii’s work in Russian America (south-east Alaska to central California) between 1839 and 1849 most closely marks the transition from informed observation to scientific purpose. Voznesenskii, who was then a preparator in the *Kunstammer*, was sent to Alaska specifically to make documented collections for the Russian Academy of Science museums. The Academy gave him explicit instructions for collecting ethnological specimens. His collection of about 1000 objects was documented to tribe, place, name, function, and material. Many specimens were noted in his diaries and were drawn. According to Dzeniskevich and Pavlinskaya (1988:85), his collecting methods “were unusual for ethnography at the time: Voznesenskii collected objects systematically across functional categories, so that the MAE acquired synoptic series of clothing, canoes, masks, and other artifact types” (see also Liapunova, 1967). Some of Voznesenskii’s objects and other specimens from earlier Russian expeditions appeared in the joint American-Soviet traveling exhibition *Crossroads of Continents* (Fitzhugh and Crowell, 1988; Liapunova, 1994).

One of the little-known contributions to early ethnology and material culture studies involved Philipp Franz von Siebold’s collections from Japan, made between 1823 and 1829 at the Dutch trading entrepôt at Dejima in Nagasaki (Kreiner, 1993:27, 1996). A medical doctor and amateur naturalist employed by the Dutch to gather information about Japan, Siebold purchased books, manuscripts, and maps; made large natural history collections of botanical, zoological, mineral specimens; and compiled an extensive ethnographic collection of Japanese objects and some Ainu materials (Forrer, 1996; Brown, 1996). Although purchased from friends and shops, Siebold pursued what documentation was available on these materials. Japan was still officially closed to foreigners at this time, and Siebold almost lost his life when Japanese officials discovered prohibited items (maps, images of shoguns, drawings of ship-building equipment, etc.) in his collections (Brown, 1996:121). Accused of spying and expelled, Siebold returned to Holland where he organized his collection and opened a private museum in the 1830s. He was a prodigious writer and published a five-volume *Flora Japonica* (Siebold, 1835–1870) and two volumes (crustaceans and reptiles) in *Fauna Japonica* (Siebold, 1833–1850). Although he failed to publish his ethnographic collections directly, discussions of these materials appeared in his monumental work, *Japan* (Siebold, 1930), and his museum arranged them in systematic cultural categories. Here, the organizing principle was cultural region, that is, grouping together Japa-

nese materials from Honshu, Ainu, Yezo (Hokkaido), and so on. And although he considered his organization inferior to the cross-cultural comparative organization that grouped similar objects from different regions and cultures together as done by Edmé-François Jomard (Forrer, 1996:26) in his museum in Paris, perhaps Siebold was the wiser, anticipating Otis Mason's culture area principle of ethnographic organization nearly 50 years later. Siebold's was certainly an ethnographic museum in the making, as one eye-witness account from 1835 confirms:

There is probably nothing comparable to what is exhibited in the three rooms containing the systematically exhibited treasures which Mr. Von Siebold brought from Japan. One is transferred among all inventions, customs, habits, the art, science, and industry of a population which was, until recently, as unknown as man on the moon. From the toilettries of ladies, one enters into the studio of the artisan, from the golden pagoda and schools into an armoury, and, as to leave out nothing, whole streets in miniature with their wares, temples for their gods, and houses of pleasures are exhibited. They have been made over there by the Japanese themselves, which is a great safeguard for their authenticity." (Major-General Ludwig Freiherr von Welden, in Forrer, 1996:30)

Later, the fine arts portion of Siebold's collections, and similar items gathered after him by his son, Heinrich, found their way to the Rijksmuseum, in Amsterdam, and the ethnographic materials went to Leiden. While Siebold's collections were not gathered directly from their makers and lack direct documentation and precise geographic placement, they represent an impressive inventory of cultural materials, and his museum's carefully classified organization of objects had an inherently scientific purpose (Forrer, 1996:24–25).

Another collecting program of the early 1800s that contributed (though more marginally) to the development of systematic museum studies is that of Jean Louis Berlandier (Ewers, 1969; Berlandier, 1980) in northern Mexico and Texas. Berlandier's surveys between 1825 and 1834 were originally commissioned by the eminent botanist Augustin Pyramus de Cantoletto through the Academy of Natural Sciences in Geneva (Muller, 1980:xi–xxxvi). Although Berlandier's focus was on natural history, which was his specialty, he also acquired anthropological materials, but the project was poorly organized, and Berlandier, who continued to live in Mexico, died there in 1851 before completing his major publication. His collections, consisting primarily of zoological and botanical specimens with some archaeological and ethnological materials, were sold after his death with the assistance of Baird, who acquired some of the zoological and ethnological specimens for the Smithsonian (Rivinus and Youssef, 1992:90). The records on these materials were spotty, consisting of journal notes and unfinished manuscripts, and while his information on American Indian culture is useful, his ethnological collecting was not informed by detailed description or scientific method.

It appears likely that American collecting prior to 1848 did not meet the contemporary Russian Academy standards, which in turn may have been inspired by German methods (William C. Sturtevant, pers comm., 1985). Other than Meriwether Lewis and William Clark, whose collections for Jefferson displayed differences among Indian groups but lacked scientific

method, one of the earliest major American collecting enterprises was the United States Exploring Expedition of 1838–1842 (also known as the Wilkes Expedition), the first United States expedition to explore the Pacific Ocean and which reached as far north as the Columbia River. The Wilkes Expedition had a designated scientific team (Viola and Margolis, 1985), and its members were instructed to gather and document natural history, ethnology, and native language. Horatio Hale (1846) wrote a monograph on ethnography and philology which provides extensive linguistic detail but has little useful ethnographic description, and no systematic catalog was prepared. More serious intent was shown by Titian R. Peale, son of the artist Charles W. Peale, who collected ethnological material for his father's "American Museum"—the Peale Museum—in Philadelphia, but these specimens were later dispersed by sale. There seems to have been little scientific purpose to the ethnological collecting other than to secure artifacts typifying different cultures and regions or to ascertain whether some practical value might be discovered in them. Kaepler (1978:20) notes that "most of the objects were collected primarily as curios and as evidence for the prevailing evolutionary view of culture. Detailed information about where the objects were collected or how they were made or used was often not recorded and kept with the objects." The end result was an amalgam of objects of various cultural provenance. Following analysis of the Wilkes collection at the Patent Office in Washington, 25 sets of duplicates (about half of the original collection of 2500 catalogue numbers, which totaled in all about 5000 objects) were given away or exchanged with other museums. Slightly fewer than 2000 Wilkes specimens remain in the Smithsonian collection today.³

Despite the movement toward more extensive field documentation and classification of objects, and the fact that these early collectors to varying degrees followed an informed method of systematic collecting, the purpose of classification was to organize and present objects in museums rather than for scientific comparison and accumulation of systematic knowledge. Ethnological and archaeological objects still were considered as curiosities of unique interest rather than as objects expressing an underlying system of human knowledge set in geographical, chronological, and cultural context. Finally, none of these collections was gathered with the express purpose of building large collections for scientific description and comparison.

Lewis Henry Morgan

The closest example of "systematic" ethnology collecting of an American Indian group dating to this era is Lewis Henry Morgan's work with the Seneca and other Iroquois groups conducted in the late 1840s for the regents of the University of the State of New York (Sturtevant, 1987; Tooker, 1994). Briefly, the history of this collection is as follows. In 1847, the regents decided to add a "historical and antiquarian" collection to the

state's Cabinet of Natural History, in Albany, founded in 1843 to house New York State's geological and natural history survey collections. To further this end, the regents sent a general request to the citizens of New York for objects. Morgan responded by sending from his personal collection about 50 objects in 1848, most of them archaeological, and about 35 more in 1848, over half of them ethnological. At the same time he suggested to the regents that a larger collection of ethnological objects be made. As he wrote the regents, "If a scholar of after years should ask of our age an account of our predecessors, such a collection would be the most acceptable answer it could render. It would enable the Red Race to speak for itself through these silent memorials" (Morgan in Tooker, 1994:45). The regents concurred, and with their financial support, Morgan made a collection of Seneca ethnological objects in 1849 and another of Seneca and other Iroquois objects in 1850. He transmitted with both collections catalogs and descriptive notes that included native names, raw materials and other documentation, which were published in the regents' reports. In making these collections, Morgan had the assistance of his Iroquois friend Ely S. Parker, who accompanied him to the Six Nations Reserve in Canada in 1850 and who may have helped to identify materials and to supply names and other information. In all, Morgan sent about 500 archaeological, ethnological, and ethnobotanical specimens to the New York Cabinet. A large number of these materials were destroyed in a fire that gutted the capitol building in 1911. Fortunately, Morgan had duplicated much of this collection with materials acquired on the Tonawanda Seneca Reservation, most of them from members of Parker's family who lived there, and this collection survives in the Rochester Museum and Science Center, Rochester, New York.

Sturtevant (1987) noted that a possible stimulus for the creation of the Albany collection may have been a visit made in 1847 by New York Governor John Young to the Hartford Historical and Antiquarian Museum, which displayed an American Indian collection. But in Sturtevant's view, a more direct motivation probably was Morgan's visit in 1846 to the United States Patent Office exhibitions:

He [Morgan] wrote in his 1848 report that the proposed New York State Cabinet, by adding an Indian collection to the existing natural history collections, might soon "not be unworthy of comparison with the more universal collections of the National Government at the Patent Office." That is where the collections of the so-called National Institute were displayed from 1841 until they were transferred to the Smithsonian in 1857. A floor plan published in 1844, two years before Morgan's visit, shows that there were many natural history exhibit cases, and eleven cases displaying ethnographic items. Nearly all of these were from the United States Exploring Expedition in the Pacific under Lt. Charles Wilkes in 1838–1842. Evidently the entire Wilkes expedition ethnographic collection was exhibited—amounting to over 2500 pieces, of which about 300 were North American (from California and Oregon), including 85 bows and arrows, and the largest number from one region was 1202 objects from Fiji, exhibited in four cases—something like 300 objects per case—which allows very little space for labels and no opportunity for any rational arrangement. (Sturtevant, 1987:5–6)

Morgan employed a type concept (probably adapted from his familiarity with the New York State natural history surveys) and systematically classified, described, and published the material culture and ethnobotanical samples of a single ethnic group. As Sturtevant has noted, Morgan, beginning in 1850, organized his collection in types rather than as separate objects.

[H]e had a notion of artifact types, which he must have gotten by paying close attention to what the Senecas told him, and probably especially to what Ely Parker explained. These were *ethnographic* collections, in a narrow, technical sense.... So here and elsewhere the types are Seneca ones. Morgan's essay is a description of *types*.... He does say that the specimens on his list "are classified under their aboriginal names into 83 distinct classes, and number in all about 300." (Sturtevant, 1987:9–10; emphasis in original)

Morgan's collection demonstrates a significant advance in American thought about scientific anthropology. Like Baird's work, which was being conducted at the same time, it seems likely that Morgan began with a biological model—indeed, he sometimes refers to his types as "species." But here the Bairdian analogy ends, for Morgan did not employ the biological concept of morphological variation to investigate cultural systems and culture change over space. He was not concerned with collecting large numbers of similar objects to ascertain variation within classes. He did not distinguish cultural differences between artifacts collected from the Seneca from those he collected among the Canadian Iroquois. Rather, his types were culturally defined by what conveyed a particular sense of a general Iroquoian "genius." These value judgments were not part of the Bairdian system and may represent Morgan's awareness of an important difference between biological and cultural systems, the idea that types may not exist in cultural systems in the same way they do in biology. He did not seek to exclude nonindigenous materials or elements or styles that indicated European influence. Rather than collecting ethnological data for the study and analysis of Seneca culture in time or space, Morgan was stimulated by the desire to record and thus to preserve in a museum setting selected features of a vanishing culture for posterity. As noted by Sturtevant (pers. comm., Dec 1995), his work predates the development of "salvage" ethnology that motivated later nineteenth century collecting, especially as espoused by the BAE, and his Iroquois materials were not so heavily "traditional" as the idealized systematic BAE collections of 1880–1920.

In sum, it appears that Morgan worked within a classic natural history paradigm (not surprising for a scholar who had also authored a monograph on the American beaver) and employed a more anthropological concept of culture than did Baird's collectors, but his collection tells us little of cultural variation in space. Lacking large series of objects from a broad region, Morgan's Iroquois collections could not have been used for studies of geographic cultural variation even if the Albany collection had survived. Nevertheless, the collection presented an unvarnished view of Seneca material culture at an important period in time. Morgan's work was a more faithful representation of the humanistic dimension linking objects, ethnography,

and culture—i.e., what was to become the essence of Boasian anthropology—than what was being studied in the Smithsonian's early collecting program. One could only have wished that Baird and Morgan had collaborated on their visions, both for the enhancement of Iroquoian studies and for improvement of the Smithsonian's field collection and publication program.

Finally, Morgan's writings are full of surprises. Those familiar with recent Smithsonian history may find his musings to Ely Parker apocalyptic: "What would be a pretty name for a 'Collection of Indian Antiques', or 'Indian Relics', or 'Aboriginal Curiosities', or 'Cabinet of Indian History', or 'Indian Museum'?... One word would be preferred. It must all be in one word." (Morgan to Parker, in Tooker, 1994:56). This, from 150 years ago, sums up the conundrum in the 1970s when Smithsonian Institution Secretary S. Dillon Ripley searched for a suitable name and concept for a Museum of Man. It is by this circuitous route we emerge at the doorstep of the Smithsonian Castle in 1850 once again, with the ascension of Baird as director of the Smithsonian's informal national museum.

Baird, Kennicott, and Systematic Museum Anthropology

During the 1850s Baird consolidated his position at the Smithsonian and developed a strong relationship with Henry, whom he greatly admired but fought constantly with over the need for official recognition for the museum. Henry refused to accept the Patent Office collections into the museum without specific congressional authorization and funding. But Baird's rapid collection-building program finally paid off in 1858, when Congress officially authorized expenditures for the United States National Museum. In the meantime, the biological and ethnological collections had been pouring in and were classified and organized by a staff of volunteers and part-time employees. Henry's interests in cultural studies continued to remain primarily in historical, archaeological, and linguistic research. In addition to "Ancient Monuments of the Mississippi Valley" (Squier and Davis, 1848), he also published "Aboriginal Monuments of the State of New York" (Squier, 1849), "Archaeology in the United States" (Haven, 1856), and several treatises on American Indian languages prepared with the assistance of William W. Turner.

With Turner's death in 1859, his place as the Smithsonian's (unpaid) Indian linguistic collaborator was taken up by George Gibbs. Gibbs immediately set to work preparing circulars for distribution to Baird's network of collectors and in 1862 published "Instructions for Archaeological Investigations in the United States" (Gibbs, 1862). This remarkable document presented a rationale for detailed observation of archaeological stratigraphy and time sequencing. It was obviously influenced by geological and archaeological developments in Europe, both in terms of theory and field method. Jefferson had conducted stratigraphic excavations in a Virginia Indian burial mound almost 100 years earlier, and published his results (Trigger, 1989:69), but his work had since been forgotten.

The following year Gibbs (1863) published a second pamphlet of instructions that specified the types of collections and information desired, including crania and specimens representing native artifacts and arts, "hints for ethnological inquiry" intended "to place before us a moving panorama of America in the olden time" (Gibbs, 1863:7), names of tribes, geographical location, population numbers and trends, physical features, language and writing, dress, food, dwellings, arts, trade, religion, government, social life, war, medicine, literature, astronomy, history, and antiquities. Concerning the latter, he noted that "if the work [excavation] cannot be thoroughly done, it is better to leave the mound unopened for a more favorable opportunity" (Gibbs, 1863:12). After Gibbs's death the task of collecting and classifying linguistic data from American Indian tribes was taken up with great fervor by Powell, who spent two decades mapping the distribution of Native American languages in North America (Goddard, 1996). These developments in American anthropological collecting occurred 14 years before the British Museum extended its instructions for systematic biological collecting to the field of ethnology in its *Notes and Queries* series (British Association for the Advancement of Science, 1874; Lindsay, 1993:36)⁴ and effectively modernized its ethnography program (King, 1994:238).

Kennicott in Rupert's Land

At the same time the Smithsonian expanded its contacts in a new direction. Baird had expressed interest in developing a collecting program in the Mackenzie region of northwestern Canada as early as 1850 (Rivinus and Youssef, 1992:85–87). With the United States Army and other government agencies fully engaged with the Civil War, Baird turned his attention to Central and South America and to the newly accessible northwestern frontier, known to the Hudson's Bay Company traders as "Rupert's Land" (Lindsay, 1993:7). In 1857 he had Henry send a letter of introduction to Sir George Simpson, governor of the Hudson's Bay Company's operations in Canada, asking for permission and collecting assistance from its "servants" (post directors, known as "factors") in the Mackenzie region. The idea of collecting was not new to the Hudson's Bay Company (HBC), which had since the mid-eighteenth century encouraged its field managers to collect natural history specimens for the Royal Society in London, the members of which included many HBC governors (Rivinus and Youssef, 1992:83; Lindsay, 1993:42).⁵ Soon after receiving official permission from Simpson in 1859, Baird sent Robert Kennicott (Figure 2), a young, gifted (but somewhat mercenary) Chicago naturalist, north to organize a collecting effort with the assistance of the HBC factors.⁶ The project was sponsored jointly by the Smithsonian Institution, the University of Michigan, the Audubon Club of Chicago, and the Chicago Academy of Science. Baird's intent was to gather from this unknown land a complete record of natural history and ethnology. In the process, he also wanted to establish a systematic



FIGURE 2.—Robert Kennicott, Baird's eminent field naturalist, opened British America and Alaska to American science.

collection that extended west into Russian America, so that one could begin to assess the relationship of northwestern North American biota and cultures with Siberia and the Old

World. Here was a grand scheme indeed! One that excited Kennicott and could provide the Smithsonian with specimens and information from a part of North America totally new to American science.

Although he probably was Baird's most gifted student, because of his premature death, at age 31, Kennicott is a little-known figure in American science. He has been described by biographer Donald Culross Peattie as a budding John James Audubon or Alexander von Humboldt (Peattie, 1936:94). Born in New Orleans in 1835, Kennicott later moved to Illinois where he was trained in natural history by the physician Jared P. Kirtland, who was recognized as the most eminent naturalist in the west and who was well known to Baird. In 1853, at Kirtland's urging, Kennicott began corresponding with Baird, to whom he had been sending rattlesnakes and other specimens for several years. Baird had a huge effect on Kennicott's development (Vasile, 1994), and in 1857 Kennicott came to Washington, D.C., to work on Baird's reptile classification project. He spent the winters of 1857–1858 and 1858–1859 in Washington, and while home during the intervening summer, he helped found the Chicago Academy of Sciences and a natural history museum at Northwestern University. It was during this period that he made his first trips into Canada, to the Red River, in 1857. In spring of 1859, utilizing the trading post infrastructure of the Hudson's Bay Company, Kennicott began his major collecting program in northern British America with \$2000 of private cash provided by Baird.

Between 1859 and 1862 Kennicott organized a collecting network that supplied the Smithsonian with nearly 12,000 specimens from more than 23 collectors (Lindsay, 1993:131). Five hundred of these were ethnological collections from the Mackenzie Inuit (Inuvialuit) and Dene Indians; the remainder were animal and bird pelts, bird eggs (by the thousands), fish, plants, and minerals (Figure 3). About half of these materials were collected by HBC traders Roderick MacFarlane and Bernard Ross. Ross had previously participated with the Smithsonian, through his association with George Gibbs, on the Northwest Boundary Survey Commission in 1857. MacFarlane and Ross, in turn, acquired many of their specimens from native people by purchase or exchange. During this period Kennicott spent much of his time training HBC men and native Dene and Inuvialuit to document and prepare specimens. Writing to Baird, he noted, "you know that there is very little chance of my ever killing such things as musk oxen, barren ground bear, and reindeer. . . . I can only hope to get them by hiring the Indians to bring them in from a great distance" (Rivinus and Youssef, 1992:86).

The Kennicott project produced the largest and most systematically gathered collection of natural history and ethnological materials acquired by the Smithsonian up to that time. It established a method of operating in remote regions using the local native population and the existing infrastructure, in this case, that of the HBC, which was offered to the Smithsonian nearly free of charge. Company men were more than eager to provide

357. Shell (unio) from Clear Water Riv, Tributary of Athabasca Riv. !!
 358. Unio from North end of Lake Winnipeg (overlooked at the postage)
 359. Three pipe heads made by Slave Indians of Mackenzie Riv
 360. Esquimaux pipe manufactured by Esquimaux near the mouth of
 Mackenzie Riv - In smoking a little hair is placed in the bottom
 then a little tobacco on top, and the smoke is ^{drawn} ~~inhalated~~ into the lungs
 instead of merely into the mouth.
 361. Cinchet of grizzly Bears claws worn on the head or around
 the cap - Slave Indians of Ft. Liard.

FIGURE 3.—Extract from Kennicott's field ledger. Smithsonian Institution, National Museum of Natural History, Department of Vertebrate Zoology.

assistance (Coates, 1984; Lindsay, 1993). Given the drab fare of company instructions received by the factors, "there is no doubt that this was one circular they took seriously" (Thomas, 1985:291). Native peoples received financial credit at the posts for contributing objects and documentation, and the HBC men were listed as Smithsonian collaborators and donors. In some instances they found their names credited in subspecies designations. Bernard Ross became so interested in natural history that he began to write original contributions for the Smithsonian *Annual Report* and other biological publications.

The Kennicott enterprise rivals Morgan's Seneca collection as the first intensive American effort to develop a systematic collection of ethnological materials gathered according to a prescribed scientific plan. The collection was undertaken using Baird and Gibbs's system of documentation and was the Smithsonian's first attempt to develop a comprehensive cultural collection from a circumscribed geographic region. Because Kennicott's ethnological collections were never fully published they are relatively unknown, but they represent an important development in anthropological field collecting, incorporating detailed descriptions and observations of place, tribal affiliation, and native terminology; they also utilized native people as collectors and informants to a greater degree than was the case for collections made in previous decades, such as those by Lewis and Clark, Berlandier, Catlin, and Voznesenskii, or those of Prince Maximilian, who collected on the upper Missouri in 1832–1834 (Maximilian, 1906; Ewers et al., 1984).

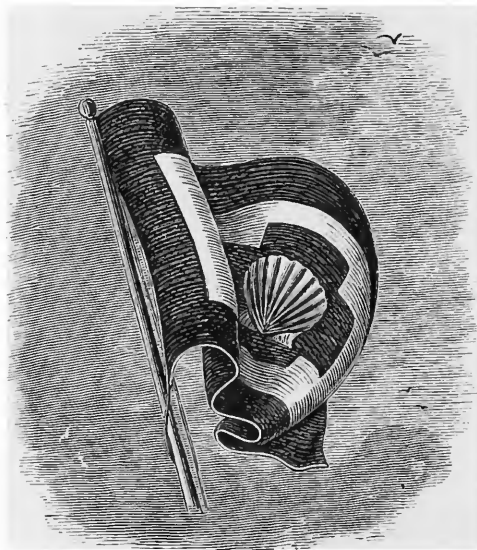
Unlike Morgan's collection, which, though published, had few intellectual offspring, the Kennicott-Baird program in the Mackenzie District began a long and productive tradition of

scientific collecting at the Smithsonian. In 1865–1866, after Kennicott's return from the Mackenzie, Baird sent him to Russian America to lead a survey team charting a route across Alaska to Siberia for the Western Union Telegraph Company (Collins, 1946; Fitzhugh and Selig, 1981) (Figures 4, 5). The project was complex and had strategic importance for emergent United States interests in the Northwest. But with Western Union in charge, the Smithsonian science program remained a remote second priority. After an auspicious beginning, the scientific program fell prey to conflicted goals. Bound by contract to complete a rapid survey, Kennicott became overwhelmed by the problems of running a huge exploration party in unknown country where logistic support like that provided by the HBC in the Mackenzie was absent. Fits of depression set in, and on 13 May 1866 he was found dead of unknown causes on the banks of the Yukon River.⁷ Later that year a rival company completed the trans-Atlantic cable. Western Union cancelled the survey, and the scientific team, now led by William Healy Dall, returned to Washington to report its findings. Despite problems, the Kennicott expedition produced the first significant scientific information on Russian America made by American observers, and during the next year Baird publicized the findings in Congress and circulated scientific reports to his network of state natural history societies, urging them to support the purchase of what some had dubbed "Seward's Folly." Although the political significance of Baird's campaign to convince Congress to purchase Alaska has been disputed (Sherwood, 1965), Kennicott's Mackenzie and Alaska surveys laid the groundwork for an explosion of Smithsonian survey and field collecting activities following the annexation of Alaska, in 1867.



FIGURE 4.—Caribou hide tunic collected for Robert Kennicott by Bernard R. Ross of the Hudson's Bay Company from Peel River Loucheux, British America, in 1866. Smithsonian Institution, National Museum of Natural History, Department of Anthropology (catalog no. 1855-6). Courtesy of the Smithsonian Institution (neg. no. 85-1379).

APPENDIX.



Flag of the Scientific Corps.

MEMBERS.

ROBERT KENNICOTT,

W. H. DALL,

H. W. ELLIOTT,

H. M. BANNISTER,

J. T. ROTHROCK,

FERDINAND BISCHOFF,

CHARLES PEASE.

FIGURE 5.—Flag of the Western Union “Scientific Corps” (from *Alaska and Its Resources* (Dall, 1870)).**Baird’s Alaska Program**

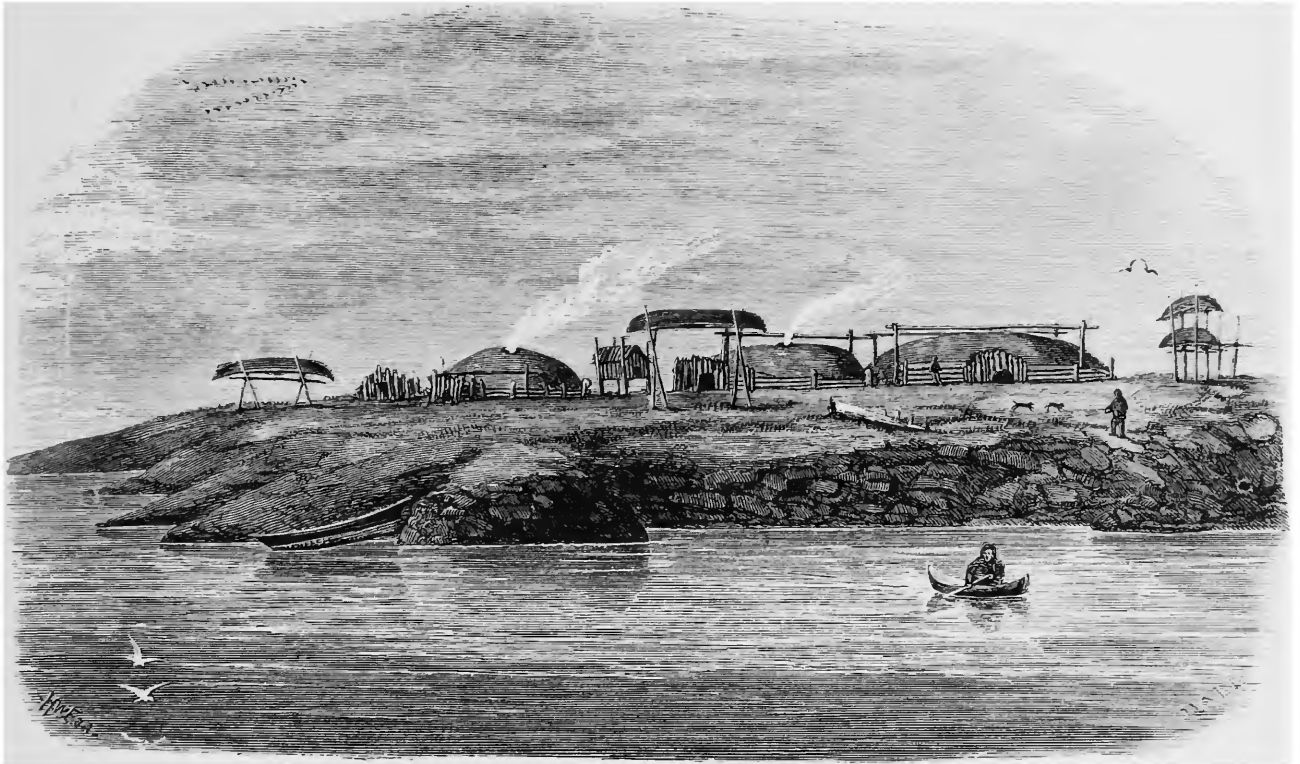
Kennicott’s opening of Alaska began a heyday for Smithsonian field collecting and natural history. Over the next 10 years Baird set up collecting programs that covered the entire Alaska territory and adjacent parts of Canada and Russia (Fitzhugh, 1988). His techniques were similar to those used by Kennicott in the Mackenzie District. Baird was assisted by the Alaska Commercial Company (the successor to the Russian America Company), other private trading companies, and a network of United States Government installations established by the Army, Coast Guard, and Signal Corps, to gather meteorological and natural resource data and administer government services. Baird convinced these agencies to hire his naturalists to conduct government studies and in their spare

time had them gather collections and data for the Smithsonian. Baird provided instruments, collecting materials, credit in trade goods at posts, and freight for shipping specimens to Washington.

Dall, a charter member of Kennicott’s Telegraph Survey team, became honorary curator of mollusks at the Smithsonian and wrote *Alaska and its Resources* (Dall, 1870), the first English language book on Alaska (Figure 6). His surveys of western and southern Alaska netted large natural history collections, and he published important papers and monographs on anthropological materials. Dall was emphatic in recommending to Baird the opportunities for biology and ethnology in the lower Yukon region, with the result that Lucien Turner, another Smithsonian naturalist, was posted at St. Michael, near the entrance of the Yukon River, to begin a collecting program in 1871. In 1877 Turner was shifted to Unalaska to make room for a more accomplished young Smithsonian naturalist, Edward William Nelson, who took up residence at St. Michael from 1877 to 1881 (Figure 7). During this period Nelson collected more than 12,000 ethnological items, all fully documented, and thousands of biological specimens. Most important, Nelson was the first of Baird’s northern collectors to publish his collections completely, reporting on geography (1882), natural history (1887), and ethnology (1899) (Figures 8–10).⁸

Nelson represents the epitome of Baird’s naturalists. Like Kennicott, his collecting program was conducted with Native American assistants whose work, combined with Nelson’s own extensive collecting trips, created a huge inventory of cultural and biological materials from thousands of square miles of western Alaska, the Bering Strait, and adjacent coastal Chukotka. Nelson’s ethnology collections are important today because he collected with the impartial eye of a naturalist. His scientific writings on people and cultures are relatively free of the evolutionary paradigms and western superiority that tainted much early ethnological field observation until well after the Boasian revolution that followed 1900. Nelson’s diaries (though sometimes less objective) are rich in anthropological detail (Nelson, 1877–1881), and his 1899 monograph “Eskimos About Bering Strait” describes Alaskan Eskimo cultures with the same descriptive clarity found in his reporting of natural history and animal behavior.

Baird’s impact is best seen in the following (partial) list of naturalists whose biological and ethnological collecting he promoted in Alaska and other regions of Arctic and Subarctic North America. These include Kennicott in British America (1859–1862) and in interior Alaska (1865–1866); Dall in Alaska (1865–1885); Turner in St. Michael (1871–1877), the Aleutians (1877–1878), and northern Quebec (1882–1884); Nelson in the Yukon-Kuskokwim, Seward Peninsula, and Bering Straits region (1877–1881); John Murdoch at Barrow (1881–1883); Charles MacKay in Bristol Bay (1881–1883); William J. Fisher in Kodiak Island (1880–1885); and James G. Swan (1850–1880s), John J. McLean (1883–1884), Robert Ni-



KEGIKTOWRUK IN THE FALL.

FIGURE 6.—“Kegiktoiwruk in the Fall.” Yupik Eskimo village drawn by Dall and published in *Alaska and Its Resources* (Dall, 1870).

black (1885–1887), and Lt. George T. Emmons (1882–1900) in southeast Alaska. These men provided the Smithsonian with magnificent collections of Alaskan natural history and anthropological materials. The fact that these collections and their documentation were gathered and have been preserved and protected down through the years is one of the most important contributions the Smithsonian has made to northern science and cultural studies.

But there was also a downside. Except for Dall, Nelson, Turner, Niblack, and Murdoch, few of these naturalists published their ethnology collections. None were anthropologists (nearly an unknown breed until the 1890s in any case), and only Dall became a Smithsonian employee. By the 1880s Baird had succeeded in recruiting the services of volunteers and curators in various fields, and it was they who took on the task of publishing segments of the Alaska collections. As a result, Alaskan ethnographic collections were partly published by Dall (1870), Otis T. Mason, Walter Hough, Charles Rau, Walter Hoffman (1897), and others recruited to the effort by Baird and his successor to the directorship of the National Museum, George Brown Goode. For the most part these works were typological and comparative and failed to capture the “living”

ethnology and first-person veracity seen in the Nelson and Murdoch monographs. For these reasons the publications and the museum exhibits that followed (Gibbs, 1882; Holmes, 1903; Ewers, 1959; Fitzhugh, 1996) were criticized by Boas and others for their flawed evolutionary underpinnings and lack of cultural context (Hinsley, 1981:98).

Nelson’s approach to ethnography is evident in his writings (1899). He knew that cultures were not monolithic, and he recognized that geographic variation operated in culture as well as in biology and understood that evidence of history and cultural influence could be elicited from field data collected with spatial precision. Nelson’s huge systematic collection from throughout western Alaska and Beringia crossed linguistic, geographic, and cultural borders and thus presented an ideal data set for analysis following principles of cultural and biological variation. Nelson commented at one point, “In the evening I secured a small vocabulary from a Nunivak Native who is here [in Tununak, a Yupik village in Nelson Island between the mouths of the Yukon and Kuskokwim Rivers]. The language is almost identical with that spoken here, and the people have no trouble in communicating with each other” (Nelson, 1877–1881, entry for 22 Dec 1879). But his profes-

FIGURE 7.—Edward W. Nelson on one of his trips up the Yukon River, ca. 1880. Courtesy of the Smithsonian Institution (neg. no. SI-6342).



sion was ornithology, and when he finally found time to prepare his ethnological monograph, more than 10 years after returning from Alaska, it is not surprising that he did not pursue its full theoretical potential. Nevertheless, even today his collection of more than 8000 specimens, housed in the Smithsonian's National Museum of Natural History (which contains the collections of the former United States National Museum), can be used for detailed spatial studies like those Baird conducted on eastern North American reptiles, or like those Boas (1903) planned for the Jesup North Pacific Expedition and which Leroi-Gourhan (1946) accomplished partly in "Archéologie du Pacifique-Nord."

Following the close of the first chapter of anthropological collecting in the north, the Smithsonian's United States National Museum began to focus on research and collecting efforts in the Plains, Southwest, and California, and these were paralleled by vigorous programs launched by the BAE (Hinsley, 1981:83–125). Arctic collecting continued to provide the Smithsonian with specimens, but by this time exhibition and description of existing collections occupied the museum staff, and field work began to come under the purview of newly hired cultural specialists whose research interests lay further south. It therefore fell to others, especially to the National Museum's anthropology curator, Otis Mason, to translate Baird's collect-

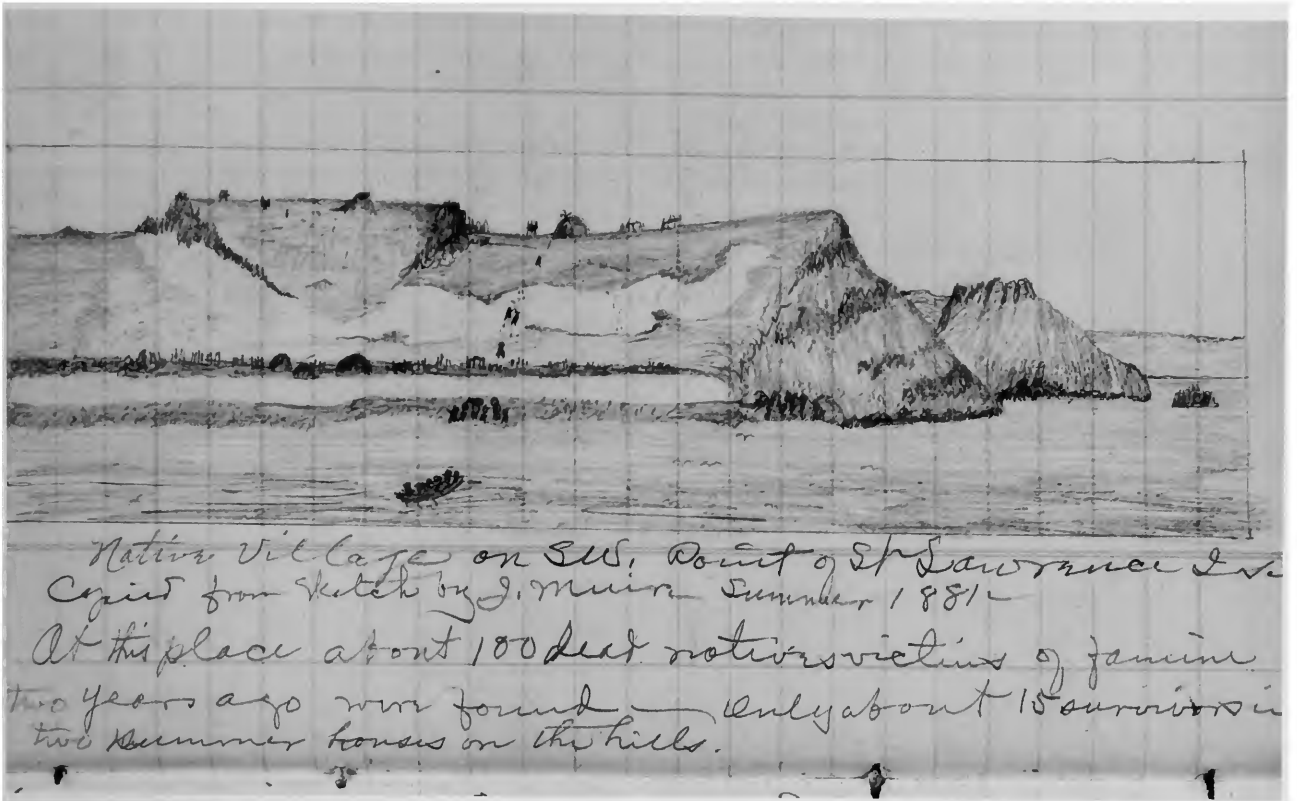


FIGURE 8.—“Native village [Gambell] on SW. Point of St Lawrence Is. Copied from sketch by J[ohn] Muir, Summer 1881. At this place about 100 dead natives victims of famine two years ago were found—Only about 15 survivors in two summer houses on the hills” (Nelson, 1877–1881, entry for 3 Jul 1881).



FIGURE 9.—Ingaliks from Lower Yukon, photographed by Edward W. Nelson in 1880. Courtesy of the Smithsonian Institution (neg. no. SI-6367½).



FIGURE 10.—Norton Sound kayak hunting photographed by Edward W. Nelson and hunting visor (right) collected from this hunter. Smithsonian Institution, National Museum of Natural History, Department of Anthropology (catalog no. E176207). Courtesy of the Smithsonian Institution (neg. no. 3846) and the National Anthropological Archives, Smithsonian Institution (neg. no. 83-10730).

ing and research method into “museum anthropology.” As noted by Hinsley:

In 1883 he [Mason] referred to anthropology as “the application of the instrumentalities and methods of natural history to the study of man”; Franz Boas recognized a few years later that biological analogy was the “leading idea” in Mason’s work.... “Culture history” he [Mason] once proposed, “takes up the thread of human social groupings where biology drops it and traces its further weavings.” Mason’s first step with a specimen was to identify its geographic and ethnographic provenance, shape, structure, purpose, and unique properties, [in Mason’s words] “just as a naturalist would with a plant or an animal.” (Hinsley, 1981:91)

This was fine method for a Bairdian naturalist, but for the emerging field of academic ethnology, more would be required.

Baird’s Legacy

Despite their different views about the future of the Smithsonian, both Henry and Baird shared a fundamental belief in the importance of empirical evidence in science and a skepticism of unsubstantiated theory. Powell was of similar mind. All the publications and projects conducted by the Smithsonian during



its first 50 years emphasized the contributions of basic data and incremental knowledge, and these goals continue to inspire the institution’s programs today. Baird’s approach, involving the integration of increasingly larger sets of data, was as suitable a

method for anthropology as it was for biology, and the field methods employed by early naturalists following the Bairdian system produced excellent building blocks for broader cultural analysis and integration.

Baird's record was less substantial when it came to analysis and publication of cultural materials. Kennicott never got to publish his collections, but his followers—Dall, Nelson, Turner, and some others—urged on by Powell, had more success. None, however, carried the plan to completion as anticipated by Baird, that is, detailed analysis of ethnological collections with respect to geographic distribution and regional differences. This task was taken up at the level of artifact types by Hough and Mason in the 1890s, and in the early 1900s ethnologists Gudmund Hatt, Eric Holtved, and Kaj Birket-Smith, using arctic collections in Denmark, extended the method to whole ethnological culture complexes, in essence conducting an "archaeology" of the present. But by this time, questions were being asked of ethnology other than trait-list comparison and distribution, and new studies required richer contextual data, history, linguistic information, and other evidence. It was in these directions, away from formal material culture studies, that Smithsonian ethnologists turned during the development of the BAE.

Other than Mason's culture-area hypothesis, these Smithsonian programs had little impact on later developments in anthropology, which veered strongly away from its earlier biological and evolutionary underpinnings. One could argue that Baird had little impact on the development of anthropological theory because he was working with a biological paradigm and had only a general sense of anthropological research problems. Boundaries of cultural types and forms rather than interactions in time or space were the features motivating Mason's and Hough's studies of the arctic collections. In later years, under Powell and Holmes, history became the paradigm of choice, and this framework has dominated Smithsonian work to the present day. Baird's naturalists made their greatest contributions in securing large, comprehensive documented field collections that would not have survived otherwise, and they provided a firm material culture baseline and cultural descriptions useful for later archaeological and ethnological studies.

Powell and Mason documented language and culture areas but did not seek understandings about how those boundaries came to be. Perhaps it was enough that they recognized that boundaries did not conform easily to geographic or ecological zones and were not strictly environmentally determined. It remained for Boas to approach the problem of cultural process from a different direction, from the inner workings of culture itself, and of unique histories. This was an approach that the arctic naturalists could not have accomplished, since few (other than Nelson) ever became proficient in the native language of their research areas. In short, when advances began to be made in Smithsonian anthropology, the Bairdian paradigm was a necessary but insufficient means for anthropological success. Powell, James Dorsey, James Mooney, Frank Cushing, and others turned primarily to non-material culture

studies and saw material culture collections as of secondary importance. While the tradition of field documentation continued, the questions changed. In short, the legacy of Baird for arctic research was in the collection, field documentation, and preservation of large systematic collections, and occasional extremely important descriptive publications (assisted by Powell and the BAE), rather than in advancing the frontier of anthropological theory, which Baird's naturalists were not equipped to do.

Museum Anthropology and the Native Constituency

Ironically, 150 years later and after many decades of eclipse, material culture studies of the type promoted by Baird are again emerging at the cutting edge of anthropological research, revitalized by new theory and by the emergence of Native American interest and scholarship. As a result, today the arctic collections of the National Museum of Natural History (NMNH) seem poised for a somewhat different future than was envisioned by the salvage and research paradigm under which they were collected. At that time it was assumed that the cultures of arctic peoples, though better "preserved" than those of native peoples further south, would eventually disappear in the wave of westernization sweeping the country. Preserved, documented, and published in part, the northern collections offer great potential for future researchers and Native American artists and cultural specialists. Because the Smithsonian never hired a curator of northern ethnology, these collections lay dormant and were used only occasionally for exhibit renovations or loans to other institutions.⁹ Without adequate funds and display space for developing exhibits that take advantage of these rich inventories, more than 99.9% of the Smithsonian's northern materials have been resting in quiet splendor on shelves in the Museum Support Center in Suitland, Maryland, awaiting future publication and exhibition opportunities.

Recently, media reports about Smithsonian anthropology have emphasized the collections of the newly acquired National Museum of the American Indian (NMAI) without reference to the older collections of the NMNH. Although less-well documented and less published, the NMAI materials contribute importantly to the Smithsonian's overall holdings of arctic peoples, adding much twentieth-century Alaskan and Canadian materials where the older NMNH collections are weak. Generally, the NMNH collections are strongest for the earliest periods, from the 1840s to the 1920s, while the NMAI northern collections are strongest for the period between 1910 and the 1950s and have important eighteenth century materials that George Heye purchased from early but largely undocumented European and American collections. The NMAI also has strong collections of Latin American antiquities, mostly purchased by Heye's collectors without documentation. Together, the NMNH and NMAI collections provide one of the largest col-

lections of American Indian research and exhibition materials in the world.

Today, in a time of new populism and ethnic awareness, these collections do not need to remain under one roof in Washington, D.C., and would be better used if a portion could be placed closer to people with specific interest in them. Even if we someday find a way to create a Smithsonian "Museum of Cultures," I believe we nevertheless need to explore new curatorial arrangements that bring portions of our northern collections closer to the native peoples who created them and who continue to see them as unique, "living" cultural treasures. Keeping these treasures in vaults in Washington or New York has preserved them effectively for the past 150 years. But interest in the repatriation movement shows that collections are not only of interest to scholars but have a wider constituency that includes Alaska Natives and state residents.

In the future our mission as a national museum of cultures has to be broader than it has been in the past. While scholarly collecting, archiving, and research must continue, in the twenty-first century, Smithsonian research in anthropology will be conducted as museum-community partnerships, not by scientists with notepads and collecting bags combing the "hinterlands" as in days of yore. The repatriation movement is the harbinger of a new phase of community interest that will invigorate research conducted in collaboration with local knowledge and initiative. Native peoples and museum specialists need to think more about how this will be done. While I am certain that the museum anthropology practiced by our predecessors largely as an isolated academic research enterprise is dead, today's community-focused museum anthropology is showing vigorous signs of life.

The Smithsonian's Arctic Studies Center has been experimenting with new approaches to bring our northern collections into closer contact with their cultural roots in the North (Krupnik, 2000; Loring, 2001). Since 1988, when congressional support for a continuing program of northern research and education began, we have sought to present Smithsonian collections to the scholarly community and public through a variety of exhibitions. The most ambitious was the joint Soviet-American exhibition *Crossroads of Continents: Cultures of Siberia and Alaska*. Versions of this exhibit toured nationally in 1988–1991, visited rural settlements in Alaska in 1993–1996, and were seen in the Russian Far East in 1996–1997. Scholarly publications and educational materials have been created for a variety of audiences, and internet and film programming now bring collections from the Smithsonian and other institutions with North Pacific holdings before even wider audiences.

In 1993 an agreement with the Anchorage Museum of History and Art permitted the Arctic Studies Center to open a regional office in Anchorage, and this program has steadily expanded its offerings and impact on the cultural life of Alaska. Workshops and training programs, plans for small regional exhibitions, and collaboration with Alaskan institutions have cre-

ated a niche that offers great potential for preserving Alaska's cultural heritage and for training a new generation of Native cultural specialists in museum practice.¹⁰ The value of this approach will not only enhance access to museum collections and documentation; it will also lead to new collecting and documentation projects both in traditional arts and in novel types of cultural media now emerging.

The crucial link yet to be made is to begin a transfer of collections and research materials from Washington, D.C., to our new facility in Anchorage. We plan to shift a portion of the Museum's Alaskan ethnographic collections from each of the major tribal groups (Inupiat, Yupik, Aleut (Unangan), Kodiak (Alutiiq), Athapaskan, Tlingit, Haida) to a supervised storage facility at our facilities in the Anchorage Museum of Art and History where they can be used for traveling exhibits prepared with Native trainees and curators, and for museum training programs, publications, Native arts, and general educational activity. The NMAI is participating in this project and will also loan parts of its collections to Alaska. The rationale for this collection sharing or "affiliation program" (the official Smithsonian designation for this practice) is based on the need to provide representative materials from early collections not available in other Alaskan museums for direct use in Alaska. Alaskan museums have significant ethnological collections, but early collections are few and often lack documentation. By making Smithsonian collections available first-hand in Alaska, unique cultural resources can help perpetuate and invigorate living cultural traditions. Their presence will also contribute to economic development, professional training of Alaska Natives, and local museum growth. Having these materials available on a rotating basis, under professional care, we can also ensure that these materials remain in the public domain, where they can be appreciated by all. This is not the case today, where the great collections from early Alaska are geographically remote and largely inaccessible to a growing, newly recognized native and northern constituency.

In the long run, to pursue the past curatorial policy of collection growth without use and diffusion is to court disaster, for the Smithsonian's cultural collections are only as secure as the national will for stewardship. After 150 years of "increase" it is time to share cultural treasures and expertise that the institution has with great care and diligence acquired and maintained. The efforts of Baird and Kennicott, and those of many who followed, have given us extraordinary resources to work with. History has shown that collections must circulate and "breathe," or they will eventually be lost through neglect or political contrivance. The Arctic Studies Center program is designed to enhance the use and availability of these invaluable collections to all sectors of society. In this goal we are in close agreement with Smithson's original mandate and the pioneering legacy of Henry and Baird, who established documentation standards and institutional goals that became indispensable for the foundation of museum anthropology.

Notes

This paper was originally prepared for the Smithsonian symposium "What About Increase" and was presented on 13 Mar 1995. A revised version was given at the Smithsonian Archives Forum on 13 Dec 1995. I gratefully acknowledge contributions made by William C. Sturtevant to these drafts and for the generous use of his unpublished manuscript material (Sturtevant, 1987). Elisabeth Tooker provided important information, especially on the Morgan connection. John C. Ewers, Adrienne Kaeppler, Edmund Carpenter, and Jane Walsh also provided helpful advice, and I thank Ron Vasile of the Chicago Academy of Science for information on Kennicott's biography. Previous research by Hinsley (1981), Rivinus and Youssef (1992), and Lindsay (1993) have been crucial to this effort.

During the preparation of this paper it became evident to me that the early history of anthropological collecting at the Smithsonian and elsewhere is less well known than I had imagined. In fact, research on this subject has only begun. The notes offered herein are intended primarily as a stimulus in this direction. I had initially intended to focus primarily on Kennicott, but the broader aspects of the roles of Kennicott and Baird took this project farther afield, leaving much research still remaining to be done on the history of Kennicott's field activities. Perhaps this paper will challenge others to assess the Baird-Kennicott legacy in establishing a scientific framework for systematic ethnological field collecting in the Americas. If so, it will not only have honored the career of one of museum anthropology's strongest proponents, William C. Sturtevant, but it will stimulate interest in understanding the origins and future of museum anthropology.

1. As Sturtevant has noted (pers. comm., 1996), the broadest mid-nineteenth century meaning of "ethnology" approximated the "four-field" meaning of anthropology today, while in its narrower context it stood related to but apart from archaeology, physical anthropology, and linguistics. Franz Boas welded these fields into a unified science of anthropology in the 1890s.

2. See Lindsay (1993:13–37) for an extensive discussion of Baird's science program in general and in northwestern North America.

3. The Smithsonian's early policy of distributing its collections, carried out most vigorously during the 1880s and 1890s, continued well into the twentieth century. According to Smithsonian anthropologist Jane Walsh (pers. comm., 1997), determining the inventory history of the Wilkes Collection is problematic because objects were frequently cataloged in lots, with numerous specimens assigned to a single catalog number.

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4. *Notes and Queries* was issued in five editions, through 1929, by the British Association for the Advancement of Science, with a sixth edition (1951) published in cooperation with the Royal Anthropological Institute of Great Britain.

5. Baird was the first American scientist to explore the possibility of working with the HBC, an idea that may have resulted from his awareness of collections made by HBC factors for the Royal Society in London, for Edinburgh (through Daniel Wilson's impetus), and for Canadian institutions (Lindsay, 1993: 41–43).

6. For works on Kennicott's biography, northern expeditions, and the HBC's Smithsonian connection, see the Smithsonian Institution's annual reports for 1859–1866; Anonymous, 1867–1869; Preble, 1908; James, 1942; Nute, 1943; Collins, 1946; Deignan, 1947; Fitzhugh and Selig, 1981; Coates, 1984; Thomas, 1985; and Vasile, 1994.

7. Stroke, heart failure, and suicide have been cited as possible causes of Kennicott's death, but the truth will probably never be known. In fact, Kennicott had been unhealthy as a child but had overcome his frailty by drive and will power. On the other hand he was emotional and temperamental, and in the days before his death he had been depressed and under great stress.

8. Nelson later went on to conduct the first natural history survey of western Mexico; founded and led the United States Department of Agriculture's Biological Survey from 1916 to 1927, negotiated and wrote the first international legislation protecting migratory bird species (The Migratory Bird Treaty Act) and the Alaska Game Law of 1925—all the products of a bachelor who in later life resided in the Cosmos Club in Washington, D.C., until his death, in 1934 (Goldman, 1935; Lantis, 1954; Collins, 1982).

9. Recent research and exhibition use of the Alaska collections began with the exhibition *The Far North* (Collins et al., 1973). This was followed by intensive study of the E.W. Nelson collection for the exhibition *Inua: Spirit World of the Bering Sea Eskimo* (Fitzhugh and Kaplan, 1982). In the 1980s Jean-Loup Rousset studied the Kennicott, MacFarlane, and Ross materials, but his work remains unpublished, and these founding collections of Smithsonian ethnography have never been exhibited. A fourth project, the analysis and exhibition of the William J. Fisher ethnographic collection from Kodiak Island and the Alaska Peninsula, has been published by Aron Crowell and others (Crowell, 1992; Crowell et al., 2001).

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Collections as Currency

Jane MacLaren Walsh

The Smithsonian Institution was created by an act of the United States Congress in 1846 with the \$500,000 bequest of a British scientist, James Smithson, who mandated that the institution accomplish two things—the increase and the diffusion of knowledge. The first two secretaries of the Smithsonian, Joseph Henry and Spencer Fullerton Baird, interpreted this mandate in accordance with their own particular scientific perspectives. Henry believed the institution's principal function should be research and publication; Baird viewed the formation of collections and their description as paramount. This paper considers Baird's point of view more closely, particularly with regard to the collections he amassed and dispersed, but it is abundantly clear that both secretaries' activities contributed to the increase and diffusion of knowledge.

Henry was a physicist who, among his myriad accomplishments, invented and operated the first electromagnetic telegraph. One of the most highly respected physical scientists of his day, he had no serious competition for the post of founding secretary of the institution. As George Brown Goode (1897:116) wrote, "For two decades he lived in the laboratory and the lecture-room, and at the end of that period he was accepted as one of the world's great investigators, distinguished alike for skill and originality in experiment and for breadth and philosophic comprehensiveness in deduction." Henry had a clear vision of what he wished the Smithsonian Institution to become: a research center that would establish and maintain close connections with a worldwide network of scientists and scientific reporters. As an initial step toward this end, in 1847 Henry established a program for the exchange of Smithsonian publications (Board of Regents, 1848:183).

In 1849 and 1850 the institution exchanged its first publication in the series *Smithsonian Contributions to Knowledge*, Squier and Davis's "Ancient Monuments of the Mississippi Valley," with 173 institutions throughout the world (Gwinn, 1996:221). This early initiative developed into the International Exchange Service, which acted as a clearing house for scientific publications from universities and learned institutions throughout the United States and Europe. European monographs were sent to the Smithsonian and sorted and sent

out to American recipients; American publications, requested via circular, were sent free of charge to European addresses (Winlock, 1897:399–400). The importance of this service cannot be overstated. Ten years after its inception, Secretary Henry wrote that "few, if any, American institutions of note, publishing transactions or reports, have any other medium of exchanging them with foreign correspondents" (Board of Regents, 1859:45). The creation of the exchange service was an affirmation of Henry's conviction that "the worth and importance of the institution are not to be estimated by what it accumulates within the walls of its building, but by what it sends forth to the world" (Board of Regents, 1853:20).

In late 1849 Henry chose Baird, a 26-year-old naturalist, as assistant secretary. Upon his arrival in 1850, Baird more than quadrupled the number of extant natural history specimens simply by the addition of his own collection, which he brought with him in two railroad boxcars (Goode, 1897:167; Rivinus and Youssef, 1992:27–28). The institution had begun to collect natural history specimens sometime before the appointment of Baird, but it is to the young naturalist that credit must be given for bringing the Smithsonian Institution into the museum business. That was his intention from the beginning of his tenure with the institution, and indeed one he had discussed in correspondence with James Dwight Dana prior to his appointment. Dana, who had been a member of the "Scientific Corps" of the first government-sponsored international scientific exploration, the United States Exploring Expedition of 1838–1842 (or Exp. Exp. in the shorthand of the day), was in 1849 a professor at Yale. He cautioned the young Baird, "As to your application to Prof. Henry—The fact is that Henry has no idea of requiring, yet a while, a Curator. He intends to have nothing to do with the Exp. Exp. collections, or any other government property."¹ Not easily deterred, Baird obtained the position, with the assistance of Dana and others.

Henry "was not a museum man—most of the time" (Hinsley, 1981:54). His feelings about the Smithsonian's becoming the caretaker of the government's massive collections housed in the United States Patent Office, in Washington, D.C., underscored his general lack of enthusiasm for collections—he viewed the material as a large jumble of curiosities without any real scientific value. "The formation of a museum of objects of nature and art requires much caution," he wrote. Referring specifically to the Patent Office material (principally the Explor-

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ing Expedition collections), he pointed out that “this museum was collected at the expense of the government, and should be preserved as a memento of the science and energy of our navy, and as a means of illustrating and verifying the magnificent volumes which comprise the history of that expedition” (Board of Regents, 1851:20).² Having said that, the secretary cautioned that the collection was so large it would “immediately fill the space allotted for collections [in the castle],” and that “in a short time another appropriation would be required for the erection of another building” (Board of Regents, 1851:21). In a final effort to avoid the burden of the Exploring Expedition collection, the Secretary appealed to the Senate’s xenophobia. “It could not be the intention of the Congress that an institution founded by the liberality of a foreigner, and to which he has affixed his own name, should be charged with the keeping of a separate museum, the property of the United States” (Board of Regents, 1851:21).

Baird, by contrast, subscribed fully to Dana’s notion that collections were “better than books to the naturalist,” containing “the whole that was ever put in words on the subjects they illustrate and a thousand times more.”³ He hoped to build the largest and most comprehensive natural history collection in the world, and he quickly saw that Henry’s International Exchange Service and the steadily enlarging network of scientists and reporters could be put toward that end. After all, Baird had been recruiting collectors to exchange specimens since he was 17. As a boy in Carlisle, Pennsylvania, he had initiated a correspondence with the legendary John James Audubon and had exchanged specimens with Audubon he had himself gathered (Rivinus and Youssef, 1992:30–31). Although naturalists have always traded duplicate specimens to gain others not already represented in their own collection, the new assistant secretary would eventually develop this strategy into a museum industry.

Early in 1850 Baird prepared a letter to be circulated to “friends of science generally, and especially...officers of the Army and Navy.” In it he requested assistance in the collection of specimens of animals, plants, minerals, and fossil remains to “lay the foundations of a collection of American Natural History” (Rivinus and Youssef, 1992:82–83). Within five short years of this circular even Secretary Henry was extolling Baird’s collecting abilities. In the annual report for 1855 he wrote, “No collection of animals in the United States, nor indeed in the world, can even now pretend to rival the richness of the museum of the Smithsonian Institution in specimens which tend to illustrate the natural history of the continent of North America” (Board of Regents, 1856:31).

The institution, now firmly on the path to forming a national museum, was compelled finally in 1858 to accept the curation of the government’s collections, which were transferred to the Smithsonian by congressional order. As Henry had predicted in his objections of 1850, the institution was simultaneously forced to request an appropriation for the care and disposition of the material.

The transfer of the government’s collections enlarged the institution’s general holdings by approximately 20% but increased its anthropological collections by more than 300%. The size and scope of the Patent Office material was impressive on all counts, comprising

about 1,000 books and pamphlets, 50 maps and charts, 500 castings in plaster (medals and seals), 186 paintings, about 1,600 bird-skins, 160 skins of quadrupeds, 50 skins of fishes; 200 jars, 2 barrels, and 10 kegs of fishes, reptiles, etc., in spirits; 50,000 botanical specimens, 3,000 insects, several hundred thousand shells, 500 corallines, more than 2,000 crustaceans, 300 starfishes, etc., 100 sponges, 7,000 separate specimens of minerals, and 50 boxes of the minerals and geological specimens. (Goode, 1897:307–308)

Not mentioned in this list are some 5000 ethnographic and archaeological objects collected by the United States Exploring Expedition, or those objects gathered by later expeditions, such as those to Africa (1843) and Japan (1853–1854) commanded by American naval officer Matthew Perry, and perhaps another 1500 to 2000 objects of Native American manufacture collected by soldiers and Indian agents.

In 1856, perhaps in anticipation of the transfer of the government’s collections, Baird traveled the four and one-half blocks from the Smithsonian to the great hall of the Patent Office to take a closer look at the objects. Writing his initial impressions on a single page, he proposed that “no country has better collections of so large an area or as good.” The specimens, once stored in better surroundings in the Smithsonian, and “subject to scientific supervision—can be arranged in proper order—duly classed and cataloged; carefully labeled—so as to be intelligible to all,” he added (Baird, 1856). The notes seem intended as part of an argument, presumably to Henry, for the acquisition of this material. The assistant secretary obviously saw these specimens as an important resource, and he was determined to make them available to everyone once they were combined with the extant Smithsonian collections. He even envisioned “handbooks or manuals of the different collections prepared for more critical study and understanding of specimens.” The end result of having a central repository of natural history and ethnographic collections would be that “any association or individual can send collections,” and using the government’s collections and the Smithsonian’s own considerable holdings for comparison, they could then have “labeled series returned” (Baird, 1856).

Despite, or perhaps because of, his admiration of and eagerness to obtain the government’s collections, at three separate points on the page Baird noted the numerous duplicate specimens, for which he saw obvious and immediate use: They “should be weeded out and distributed among other American museums,” and the “collections will thereby occupy much less space” (Baird, 1856), a potential selling point to Secretary Henry. The other anticipated result of distributing these collections to other museums, although one not written down by Baird, would be that exchanging these famous artifacts would bring a return of more, and perhaps rarer, specimens.

In 1859 the “burden” of the government collections brought a yearly endowment of \$3650 to cover incidental expenses (Goode, 1897:322). Listed as 12 separate collections, the Patent Office material joined another 23 government collections already housed in what came to be called the United States National Museum. Throughout the 1850s Baird had been amassing large numbers of “mammals, birds, reptiles, fish, shells, and minerals from the exploring and surveying expeditions of federal departments, as well as from state governments, local scientific societies, and individuals” (Hinsley, 1981:66–67). While fewer in number, specimens of anthropological and archaeological interest had also been gathered by the Pacific Railroad Survey, the Mexican Boundary Survey, surveys of the Amazon River basin, and explorations of Nicaragua, among other government expeditions exploring the “national domain” (Goode, 1897:317). By the end of 1858, the institution had managed to catalog some 25,506 specimens, including mammals, birds, reptiles, fishes, skeletons and skulls, crustacea, and bird eggs.⁴ Almost as soon as the collections were cataloged, duplicates were laid aside “to be distributed to other parties... not only for the purpose of supplying a great want, but also of relieving the shelves and cases of the Institution of a redundancy of material” (Board of Regents, 1859:59). To govern the processes of lending and exchanging specimens of undescribed taxa and effectively distributing duplicates of specimens of described taxa, the Smithsonian developed a series of rules.

The first and foremost rule concerned the institution’s original mandate—the advancement of science. To accomplish this purpose, duplicate specimens would be distributed as widely as possible to scientific institutions in the United States and abroad, to be used in the identification of species already known to science. The second rule dictated that museum personnel make full sets of properly labeled general duplicates, which would in turn be presented to colleges and other institutions of learning to promote education. The third rule specified that all due credit would be given to the Smithsonian Institution in the labeling of the specimens and in all published accounts. The fourth and fifth rules concerned what was anticipated, or expected, in return for the institution’s generous gifts. In the distribution of specimens abroad, type specimens illustrating species described by foreign authors would be required in exchange. Specimens presented to American colleges and other institutions in the United States would be given in exchange for collections from specified localities in their own particular regions. Thus if colleges wished to obtain duplicate collections from the Smithsonian, they would be required to come up with representative collections of birds or fishes or minerals from their own regions (Goode, 1897:318).

Undescribed specimens were covered under a slightly different set of rules. In this case, the first rule was that uncataloged items would never be entrusted to inexperienced persons and that preference would be given to individuals engaged in preparing complete monographs on the subject. Investigators

would be allowed to take specimens to their places of residence and keep them for a reasonable amount of time. Sets of type specimens from these collections, including all duplicates, were to be returned to the institution, and, as always, credit was to be given to the Smithsonian in any publication (Goode, 1897:319).

The distribution of undescribed specimens provided considerable assistance to the young institution, which lacked funds and staff. “Collections which would have remained useless for years were rapidly classified by competent naturalists and separated into series, some to be reserved by the Institution and others to be distributed to kindred scientific establishments and to colleges and schools” (Goode, 1897:319). The list of collaborators includes almost every name prominent in American natural history in the last half of the nineteenth century.

The annual report for 1858, with its list of 25,506 specimens cataloged, makes no mention of ethnological or archaeological material (Board of Regents, 1859:57). This is because the anthropological collection was first systematically described and numbered beginning on 9 March 1859. This material was sorted for duplicates for exchange the same year. By the close of 1867, with nearly two ledger books filled, the institution had cataloged some 5000 objects from ethnographic and archaeological contexts. The Smithsonian distributed 1048 of these carefully cataloged specimens, or about one-fifth of the described material, that same year (Board of Regents, 1868:72).

By the end of the institution’s first quarter century, ledger books indicate that the majority of the anthropological collection originated from the North American continent. It consisted specifically of Arctic and Subarctic specimens collected by Smithsonian naturalist Robert Kennicott with the assistance of various members of the Hudson’s Bay Company, such as Bernard Ross and Roderick McFarlane. The Northwest Coast of North America was represented by objects collected by Dr. J. Evans and the displaced New Englander James Swan, two of the many important collectors listed in the ledger books. The western and central Plains material was collected by the Wheeler and Hayden expeditions of the 1870s, by Lt. G.K. Warren, who explored the Missouri River region, and by a variety of other collectors, such as soldiers, Indian agents, and United States Army doctors. The remaining curiosities came from just about everywhere else: Japan and Africa via Perry; Thailand as royal gifts to American presidents; Mexico from the Swiss naturalist Jean Louis Berlandier, the American diplomat Brantz Mayer, and United States Ambassador to Mexico Joel Poinsett; and from numerous islands in the Pacific, collected by the United States Exploring Expedition.

For the year 1871, the Smithsonian Board of Regents noted that the general collection then numbered 169,360; of this figure the “ethnological specimens” numbered 10,931. To the end of 1871, the number of ethnological specimens distributed was 1342, or about 12%. The final count that year for all the duplicate specimens distributed, however, is astounding: 308,080. This figure, which included all specimens exchanged from all

branches of natural history, is more than twice the total number of specimens cataloged (Board of Regents, 1873:42).

Baird, by all indications, took particular, personal interest in the distribution of all duplicate materials. In 1867 Baird supervised the assembly of numerous sets of duplicate natural history and cultural specimens for distribution. These included sets labeled "mammal skins," "Pacific fishes," "shells," "bird skins," "eggs & nests," "Minerals & Rocks," "Esquimaux curiosities" and "Fejee curiosities."⁵

The 25 "Fejee" sets provide an interesting example of a sort of museum starter kit composed almost exclusively of United States Exploring Expedition duplicates. As a sampler it was meant to illustrate the expedition's around-the-world voyage, but in actuality the institution sent the objects it had in greatest supply, its most exotic specimens. Each set contained about 15 objects, including a bow and arrows from Oregon Territory or from northern California, some halibut or eel hooks from the Northwest Coast, and a variety of items from Pacific islands, principally Fiji. This portion of the selection included samples of Samoan, Hawaiian, and Fijian bark cloth;⁶ a Samoan or a Hawaiian fish hook; a basket; grass skirt; three to five spears; four to six war clubs; and a number of shell ornaments from Fiji. "Esquimaux" sets were assembled using the "Anderson River Esquimaux" collections, including specimens principally collected by Robert McFarlane. These sets mostly also contained about 15 specimens, although occasionally as many as 77 objects were packed up and sent off.⁷ In addition to the Eskimo and Fijian sets, there were duplicate collection boxes labeled simply "Ethnologica," which contained an unspecified number of objects.

The list of distribution recipients is also quite interesting. It details various mid-nineteenth century institutional affiliations and the personal connections that formed a kind of nineteenth-century naturalist network. It also gives us some indication of the number of then-extant university museums and natural history societies. In the case of the Exploring Expedition duplicates, in the space of a single year, 1867, sets were sent to the universities of Michigan, Kentucky, Toronto in Canada, and Cristiania in Norway. They also went to Amherst, Dartmouth, Harvard, Williams, West Point, Yale, and the City College of New York. Fiji sets of similar size and composition were delivered to the cabinets of natural history societies in Albany, New York; Portland, Maine; Montreal, Quebec; Springfield, Worcester, and Salem, Massachusetts; Bloomington and Springfield, Illinois; Montpelier, Vermont; Jefferson, Missouri; and St. Paul, Minnesota. In 1872, the remaining sets were delivered to Wells College in Aurora, New York; Columbia College in New York City; and Brown University in Providence, Rhode Island. The majority of the specimens sent to colleges and universities can still be found in the collections of those institutions. The items distributed to natural history societies found their way into state museums, and in both instances a large proportion of specimens are still on exhibit.

Sets of ethnologica are listed in 1872 as shipped to such English institutions as the Blackmore Museum in Salisbury, the Christy collection in London, and the Peabody Museum in Cambridge as well as to the National Museum in Lisbon, Portugal. These gifts numbered 151 specimens. Larger collections consisting of several hundred objects from cultures of the Arctic to the South Pacific were sent in 1867 to the Royal Ethnological Museum in Copenhagen, Denmark, and to the Academy of Sciences in Chicago, Illinois, the latter entirely destroyed in the great fire that consumed the city.⁸ Once again the material distributed was culled from the largest collections, those from the Arctic and the Pacific, perhaps somewhat skewing the nineteenth-century museum-goer's view of world ethnography.

A number of the sets sent out in 1867 were payment in kind for specimens already received from those institutions.⁹ By the end of 1868 the catalog ledgers already gave a strong indication of the importance and value to the Smithsonian of these exchanges as a means of enhancing its collections. They list as museum acquisitions a collection of archaeological material from J.W.P. Jenks,¹⁰ later of Brown University; prehistoric stone tools from Professor Jilsson of Sweden; and specimens from Herr R.L. Vortisch of Germany, M. Edouard Lartet of France and Spain, and Sr. Sartorius of Mexico. Henri de Saussure sent Swiss archaeological specimens, Minister Crampton sent objects from British Guiana, and G.R. Gliddon exchanged a small collection of Egyptian artifacts, all within a decade of the first ethnographic cataloging.

Although collections from other departments were distributed and exchanged in far greater numbers, ethnographic and archaeological specimens also seem to have been in great demand. Perhaps the most popular items were southwestern ceramics, particularly Zuni pots. Beginning in the late 1850s and lasting well into the early 1960s, the anthropological collections were diffused to museums, colleges and universities, high schools, elementary schools, schools for the blind and deaf, asylums, public libraries, and societies and private collectors in the Americas, Europe, India, Asia, Africa, Australia, New Zealand, and Tasmania.¹¹

In addition to already cataloged items considered to be duplicates, some traded specimens had never been entered into the record. Significant smaller collections were separated out from the larger body of material and sent to foreign institutions by collectors often while still in the field. For example, a handwritten index card in the records of the registrar records a distribution transaction without giving too much detail. Dated 3 June 1885, it reads "Oxford, England, University Museum. Specimens of Pottery, Bureau of American Ethnology. Baird. #4296."¹² The corresponding invoice for distribution #4296 is missing from the record, but the Smithsonian's Bureau of American Ethnology (BAE) correspondence files in the Smithsonian National Anthropological Archives contain a 40-page document and letter signed by John Wesley Powell, founder of the BAE, which fills in the particulars of this single transaction.

Powell sent 10 boxes of material to E.B. Tylor and H.N. Moseley at Museum House in England in June, 1885. Included in this shipment were published reports of the Smithsonian and the BAE, a collection of photographic illustrations showing general views of southwestern pueblos, and a series of native portraits, along with a descriptive catalog of these images apparently written by James Stevenson of the BAE. Powell also sent more than 200 specimens, principally pottery, from Zuni, Hopi, Acoma, Santa Clara, Cochiti, and San Ildefonso pueblos. These artifacts had been culled from a much larger collection made by Stevenson and his wife, Matilda, another BAE collaborator, in 1884. Stevenson, according to Powell's letter, had "been arranging and labeling the collection made by him last summer, [and] he has from time to time set apart the articles which I now ship to you. At the same time he has made a careful catalog of the whole material, and added such notes as he deemed would be of value in connection therewith."¹³ Powell noted that the collection was to be divided between Tyler and Moseley. The objects, now mostly housed in the Pitt Rivers Museum, Oxford, have only Stevenson numbers, 1–213, assigned to the collection, and they bear no other marks connecting them to the institution.

This is one example, among what I believe to be dozens, for which the transactions records are either difficult to locate or do not exist at all. Indeed, further complicating the historical picture, and the possibility of recreating the size and scope of the original collections, is the fact that numerous trades of already cataloged specimens were never entered into the catalog record and only exist as index cards describing "boxes of ethnologica." Too often specimens came into the institution, were accessioned, cataloged, and eventually exchanged or sent as gifts, with no official record kept. Occasionally one finds an index entry but no description of individual objects or catalog numbers.

Despite innumerable omissions, I have been able to document the exchange or gift of a total of more than 12,000 objects from the anthropology collections from 1867 to 1960. This figure encompasses principally ethnographic objects. The number of archaeological specimens dispersed is many times that figure, usually including objects removed in groups of six or eight from large lots. For example, in a lot comprising several hundred arrow heads, described by a single catalog number, as many as 75 are listed as exchange pieces.¹⁴

It is also important to note that many of the exchanged items cannot in any sense be described as duplicates, even by nineteenth-century standards. I have located numerous unique specimens that were traded away, leaving our own collections unrepresented by even the type. Harvard's Peabody Museum, for instance, was given two full-size Chinook wooden cradles in 1888, nearly identical, and both of them early. The two came from the collection of George Catlin, and one of them, according to a notation in the Peabody catalog, was "probably collected by Lewis and Clark, preserved in the Gov. Clark collection and given to George Catlin" (Susan Haskell, pers. comm.,

27 Oct 1997). The cradles the institution retained are neither the size nor the quality of those it gave away. In 1867 the Smithsonian gave the Danish Museum in Copenhagen a rare, even in the nineteenth century, Fijian "oracle," a seed-encrusted, carved coconut. According to the catalog, the institution had two, but the second one is missing from inventory. Haida ship pipes and carved argillite objects, each a unique creation, were traded off before the turn of the century with astounding regularity.

The institution, particularly under Baird's auspices, used its collections, at least the anthropological collections, as a kind of excess currency, currency to be exchanged for specimens and collections it lacked, to repay other museums for objects and natural history specimens already sent, to purchase specific items from dealers, and to purchase material other than collections, such as publications. Collections even became a sort of political currency, used for gifts to foreign ambassadors and heads of state, perhaps in exchange for favors.

It should also be mentioned that many of the trades reflect important collegial relationships cultivated by members of the institution. Harvard's Peabody Museum was by far the most visible, particularly during the tenure of Frederick Ward Putnam; Secretaries Henry and Baird established numerous joint research projects with Harvard scientists. One result of this longstanding relationship is that the Peabody was the recipient of the largest number of distributed anthropological specimens. To be more precise, out of the 12,000 identified objects exchanged with some 500 institutions and individuals, Harvard received more than 1200 specimens, or 10% of the total. The closest competitors to Harvard were Denmark's National Museum in Copenhagen, also an early research partner and the recipient of some 600 objects between 1867 and 1930, and the Trocadéro in Paris, with another 600 objects, nearly all sent in 1885. Two museums in Buenos Aires, Argentina, received a total of 603 archaeological objects and 244 ethnographic specimens around the turn of the century. The institution also helped foster museums and collectors in smaller, less powerful countries. A prime example of this is Louis Guesde, a private collector on the island of Guadeloupe in the West Indies who, in return for an album of 68 watercolor drawings of pre-Columbian objects in his own collection, received more than 300 archaeological specimens, books, and ethnographic objects between 1881 and 1890.

Universities and public museums were not the only beneficiaries of Smithsonian largess. Individual collectors and the proprietors of private museums received substantial collections as well, usually in trade, although occasionally as gifts. George T. Emmons, of Princeton, New Jersey, received more than 100 ethnographic specimens in trade over a period of two decades. Italian collectors Enrico Giglioli, of Florence, and Luigi Pigorini, of Rome, along with American circus magnate Phineas T. Barnum, whose collections went to Tufts University, all received sizable amounts of material. Edward Lovett, of England, made a series of trades netting himself some 300 ethno-

graphic specimens; his collection is now in the Manchester Museum. J.W. Hudson, of Ukiah, California, traded baskets, and E.W. Keyser added to his collection of Arapaho and Cheyenne material in Washington, D.C. Victor J. Evans, also of Washington, and George G. Heye, of New York, traded with the institution over the space of several decades. The Evans collections was returned to the institution as a bequest in the 1940s, and the Heye collection, now the National Museum of the American Indian, came full circle as a result of yet another act of Congress.

Some individuals with whom the institution traded appear to have been dealers, as was evidently the case with Anton Heitmüller, the proprietor of an antique shop on 14th Street in Washington, D.C. The numerous transactions with this gentleman were overseen by William Henry Holmes, head curator in the Department of Anthropology, and do not represent what can in any way be described as good horse trading. These incredibly lopsided transactions appear to be an indication of perceived value at the turn of the century. Between 1900 and 1915 Heitmüller received nearly 300 ethnographic objects, some from the earliest collections of the United States Exploring Expedition. One transaction in 1900 involved two items from Heitmüller's collection—two altars, whose provenance was a Catholic church in Hildesheim, Germany. In exchange for these two items, Holmes sent Heitmüller 12 alabaster carvings from India, 140 pieces of pottery (about half were from Zuni and Acoma pueblos and the rest were pre-Columbian vessels from Chiriquí, Panama), 4 brass plaques, 8 baskets, 1 bible, 1 flag, and 1 priest's robe from Ceylon. As if this were not more than sufficient, in response to a complaint from Heitmüller that the list was four items short, Holmes sent a second batch of 55 pueblo pots "to complete the exchange."¹⁵ In 1915, in exchange for 68 "heating and illumination devices" consisting principally of candles and candle sticks from the same German church and some colonial American pieces, the institution sent Heitmüller 61 specimens of ethnologica, including four United States Exploring Expedition artifacts, a variety of Plains Indian objects, African and Japanese weapons, and yet another dozen Zuni pots.

By contrast, some exchanges resulted in spectacular additions to the collection. One transaction of this type consists of an East African shield and spear that were exchanged with W.W. Rockhill, of Berkeley Springs, West Virginia, for 83 ethnographic specimens from Tibet. This particular collection constitutes one of the earliest and most important from that region of the world in the Smithsonian. This exchange took place in 1891 under the auspices of Otis T. Mason of the National Museum.

When the institution used its collections to purchase political favors, specimens appear to have been given away wholly for diplomatic purposes, such as those to ambassadors and heads of governments. The German ambassador to Washington, for instance, received 43 stone implements as a gift in 1894, Baron Ludwig Ambrozy of Austria was given 17 baskets in 1905,¹⁶

and the Sultan of Turkey was given a fully dressed "Sioux Chief lay figure" or manikin in 1897.¹⁷

Political figures closer to home apparently enjoyed a certain inside track as far as gifts and exchanges were concerned. There are a number of distribution invoices listing individuals with the title "Honorable" before their name. Most of these appear to be members of the United States Congress who, it is likely, had some influence on Smithsonian funding. In several instances Southwestern pottery, particularly Zuni and Acoma specimens, were sent out as gifts or in exchange for publications. The case of Joel P. Heatwole, representative from Northfield, Minnesota, is puzzling. This gentleman was the recipient of a number of separate gifts of "duplicate pottery specimens" (7 Acoma, 3 Hopi, 1 Zia, and 19 Zuni pots and 5 prehistoric Chiriquí pots from Panama) in July 1898, in exchange for "\$100.00 worth of publications, Volumes of Proceedings of the National Museum."¹⁸ In 1900 the Heatwole was shipped 60 pots and 7 baskets in exchange for publications valued at \$30.00. According to William H. Holmes, "The pottery is all of the modern ware of Moki [Hopi], made for trade, and of no Museum value whatever, the commercial value being, as nearly as Professor Mason can determine about twenty-five cents per specimen."¹⁹ The appended list does indicate some 40 Hopi vessels. In addition to these, however, are three pre-Columbian Zapotec pots from the collection of Lewis H. Ayme, from Oaxaca, Mexico, and about 12 Acoma and Zia pots collected by the Stevensons.

In attempting to reconstruct events leading up to these exchanges, I assume that the institution sent its own publications for distribution to members of the House and Senate. Presumably some members had more books than they needed, but why the institution was compelled to exchange its collections for the return of its own publications remains unclear. One would hope that the collections were given to museums in the representatives's districts, although this is also not certain. Whatever was happening, it was not an isolated event, at least at the beginning of the twentieth century. Another member, Ernest W. Roberts, received 20 ethnographic specimens in 1906 in exchange for Smithsonian publications and, in a letter, also noted receiving a set of "coins recently issued by the United States for the Philippine Islands." Continuing, Congressman Roberts wrote, "I have at my home quite a number of Smithsonian publications, which I will also forward as soon as I return to Massachusetts."²⁰ Senator A.F. Barrott, in publications exchanges, received in 1906 six archaeological pots excavated by J.W. Fewkes of the BAE, four pots in 1907, three Chiriquí pots in 1909, one Jeddito pot in 1910, and two decorated dishes in 1913.²¹

Another series of exchanges, or gifts, which took place in 1914, must surely indicate a different sort of intervention on the part of a representative, whose district would have included Brooklyn, New York. In this case Smithsonian largess was at its most expansive: nearly every public and parochial school in Brooklyn received a variety of sets of duplicate specimens, in-

cluding marine invertebrates, mammals, birds, Indian baskets, and casts of archaeological specimens.

Over the past 150 years, the Smithsonian Institution not only has used its treasury in countless barter, trades, and diplomatic pursuits, for the most part very wisely, but it has also been extraordinarily generous in spreading the wealth of its collections throughout the world. The records show that the institution sent collections to nearly every country in Europe, with many nations receiving collections for several major museums. Leningrad, Moscow, and Irkutsk, in the former Soviet Union; India; China; Korea; Japan; and the Philippines are also represented. Egypt and South Africa were trading partners in the early part of the 1900s, as were several museums in Australia and New Zealand. Mexico was an early exchange partner, along with Costa Rica, Guatemala, El Salvador, Cuba, Puerto Rico, Guadeloupe, and nearly every country in South America.

One unfortunate aspect of the distribution history was the uneven nature of selections made for dispersal, which necessarily involved collections the institution had in the greatest supply. This fact may have had the unintended consequence of creating a somewhat narrow view of North American ethnography, particularly in museums outside of the United States. The largest proportion of duplicates distributed to foreign institutions came from the American Southwest and the Arctic. Southwestern Indian artifacts alone account for 34% of the distributed duplicates, with Zuni and Hopi material comprising half of that inventory and Arctic specimens making up another 20% of the total. I have visited a number of museums with which the Smithsonian had an exchange history and where the American collections are almost exclusively from the Smithsonian; inevitably, nearly all are from the Arctic or the Southwest.

Conclusions and Possibilities

Baird used the biological collecting model—type specimens supported by series of duplicates—as his starting point for all museum collections. Clearly there are some drawbacks to this concept when applied to anthropological collections. Despite the fact that, generally speaking, distributed artifacts are well represented within the collections retained by the institution, no single artifact is an exact duplicate of another. Obviously, the Smithsonian lost some of the enormous diversity of specimens originally collected and diminished to some extent what might have been seen in the nineteenth century as an embarrassment of riches. Nevertheless, I believe that such drawbacks to collections dispersal are outweighed by the benefits to the population at large. The case of the United States Exploring Expedition collection, one of the institution's earliest and most extensive, provides a good example of the consequences and beneficial aspects of the distribution policy. The 25 distribution sets, originally selected by Baird and his assistants, were made up of artifact types considered to be the most numerous and representative of the collection. Within these artifact categories, occasionally more than half the objects collected were ex-

changed, although normally the percentage did not exceed one-third. The point should also be made that within the entire collection, the majority of artifact categories remained untouched. A good example of an artifact type chosen for distribution is that of the Fijian *liku*, or woman's skirt (a wide, woven, vegetable fiber belt). The original official collection listed 120 *liku*, of which 35 were exchanged with 28 museums. The exchanged examples, including four now in the Bishop Museum in Hawaii,²² allow for a much broader viewing of this highly perishable item of clothing worn by Fiji islanders in the 1830s, which otherwise would be unavailable to interested observers and students of culture. Even though no two of these belts are exactly alike, the institution's collection of some 85 of them provides sufficient and eloquent testimony to the ingenuity and creativity of Fijian women in the manufacture of their clothing.

Once all instances of distributions are added up, the United States Exploring Expedition collection had been diminished by approximately 23%. A comparison of Exploring Expedition artifacts distributed to museums in the United States and Europe with those retained in the institution's collection indicates that near-duplicates or items of a highly similar nature were, in fact, selected for exchange. One or two unique items were sent out with the duplicates, but those, as with the majority of exchanged Exploring Expedition artifacts, remain in the collections of the recipient museum and can still be studied. In the final analysis, the Smithsonian Institution maintained the majority of artifacts amassed by the United States Exploring Expedition, retaining approximately four-fifths of the collection. The remainder are cared for in the storehouses and exhibition halls of some 40 museums and institutions of higher learning. The reconstruction of the original official collection has required considerable research and extensive travel, but it has been possible to refit nearly all of the pieces into the puzzle.

The era of museum building in this country began with Baird's collecting collectors. Baird understood that by sharing the Smithsonian's great wealth, he could increase it and broaden its scope. Museums worldwide gained access to America's premier ethnographic and archaeological objects and were assisted in building their own American collections. In return, the institution's world view was broadened, and its holdings became truly international. Baird clearly believed James Dana's words, that "collections are better than books." The historical evidence of the world's material culture offers us insights into the universal human condition and our own humanity as well. Each object is a book waiting to be read and is a different book to every reader.

Collections are yet to be formed to illustrate the material culture of the twenty-first century, but those of the last two centuries should not be allowed to gather dust. The institution is now in need of a twenty-first-century Baird. The collections he amassed so diligently and comprehensively are in need of a dusting off and airing out. As the new century opens to us, we need to view the institution's great treasury of nineteenth- and twentieth-century collections not as a burden, or, as a few have

suggested, the evidence of a failed idea, but as Baird saw them—currency to be used in a new marketplace of ideas. After 150 years of exchange and gift-giving, the business to enter into now is that of loans, perhaps even the development of a museum lend-lease act for the year 2001.

The Smithsonian has always lent its specimens for study and for exhibition, and has recently begun to actually contemplate furnishing museums that address the culture and history of specific groups or regions. The proliferation of ethnic-specific museums and study centers creates an opportunity for the Smithsonian to lengthen its educational reach and encourage greater use of stored collections. The Arctic Studies Center of the Department of Anthropology, for example, has directly addressed the concerns and interests of arctic peoples through small traveling exhibitions designed for town and village schools or public buildings, and it is now conducting research to prepare loans to a Smithsonian affiliate, the Anchorage Museum of History and Art, and Alaskan tribal museums as well (see Fitzhugh, 2002). The institution is attempting to carry this idea a few steps further by entertaining proposals for long-term loans of artifacts to illustrate the cultural history of countless Native American and immigrant American communities and in so doing finally move far beyond the National Mall in Washington, D.C., to a series of satellite museums and research institutions throughout the United States. Through the affiliations program, Smithsonian collections can be used to form a core for research and exhibition and could be expanded and broadened by the addition of regional material, much in the way Baird did in the nineteenth century. In this manner the institution will diffuse knowledge to an ever-widening audience and, at the dawn of the twenty-first century, combine the best ideas of its first two secretaries, Henry and Baird. The Smithsonian can carry on the tradition of exchanging specimens started by Baird while reaffirming Henry's belief that the worth of the institution is measured not by what it holds in its storehouses but rather by what it sends forth to the world.

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1856. Miscellaneous notes. Record unit 7002, Spencer F. Baird Papers, 1833–1889, box 63, Smithsonian Institution reports and memoranda, etc., folder 7, Smithsonian Institution Archives, Washington, D.C.

Board of Regents, Smithsonian Institution

1848. *Second Report of the Board of Regents of the Smithsonian Institution, ...during the Year 1847*. 30th Cong., 1st sess., S. Misc. Doc. 23, 208 pages. Washington, D.C.: Government Printing Office.
1851. *Fifth Annual Report of the Board of Regents of the Smithsonian Institution, ...during the Year 1850*. Special sess., March 1851, S.

Notes

1. Dana to Baird, 27 Aug 1849, New Haven, Connecticut, record unit 7002, Spencer F. Baird Papers, 1833–1889, box 19, private incoming correspondence, 1847–1887, folder 4, Smithsonian Institution Archives (SIA), Washington, D.C.
2. The reference is to the 13 volumes published in 1845 by the United States Exploring Expedition.
3. See note 1, above.
4. The statistics for collection-building can be found in Board of Regents, 1858. In 1851, the year after Baird's arrival, there were 911 entries in the Smithsonian catalog ledgers, all "skeletons and skulls." By 1856 there were 11,222 entries, with the categories of mammals, birds, reptiles, and fishes added. Two years later the described collection had more than doubled (Board of Regents, 1858:59).
5. Record unit 120, second series, volume 3:93, SIA.
6. Large pieces of bark cloth, some with intricate painted and stamped decorations, were cut up into six or eight pieces and were distributed to as many institutions.
7. Record unit 120, second series, volume 3:96, SIA.
8. Record unit 120, second series, volume 3:94, SIA.
9. C.C. Rafn, of Copenhagen, had sent Danish prehistoric stone tools in 1852 (Board of Regents, 1853:67).
10. Jenks, the founder of the Natural History Museum at Brown University, and a close personal friend of Baird, was another prodigious collector.
11. Large-scale exchanges of anthropological material effectively ceased during the late 1920s.
12. Record unit 120, card index, SIA.
13. Powell to E.B. Tylor and H.N. Moseley, 2 Jun 1885, Washington, D.C., National Anthropological Archives, Bureau of American Ethnology correspondence, letters sent.
14. The ledger pages on which these transactions were recorded are truly amazing to behold, although they are extraordinarily difficult to read.
15. Record unit 186, distribution invoice nos. 13952 and 14129, and record unit 305, accession file no. 37132, SIA.
16. Record unit 120, card index, SIA.
17. Record unit 186, distribution invoice no. 19490, SIA.
18. Record unit 186, box 18, distribution invoice no. 11970, SIA.
19. Memorandum from W.H. Holmes to F.W. True, 18 Jun 1900, Washington, D.C., record unit 186, distribution invoice no. 13723, SIA.
20. Roberts to Richard Rathbun, 5 May 1906, Washington, D.C., record unit 186, distribution invoice no. 20689, SIA.
21. Record unit 186, distribution invoice no. 20654, Smithsonian Institution, Department of Anthropology, original catalog ledger books, SIA.
22. The *liku* in the Bishop Museum are such exceedingly rare specimens they are still on prominent display. The National Museum in Suva, Fiji, has only two *liku* from this period.

Misc. Doc. 1, 145 pages [extra edition of 326 pages.] Washington, D.C.: U.S. Government Printing Office.

1853. *Seventh Annual Report of the Board of Regents of the Smithsonian Institution, ...during the Year 1852*. 32d Cong., 2d sess., S. Misc. Doc. 53, 96 pages. Washington, D.C.: U.S. Government Printing Office.
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The Creation of Anthropological Archives: A California Case Study

Ira Jacknis

The contemporary interest in ethnographic representation, especially in writing, has led to a new consideration of how fieldnotes are created and used and their relation to “native reality” and to published ethnographies (Sanjek, 1990). Rarely mentioned, however, is what happens to these fieldnotes after they are no longer used by their creator, due to either loss of interest or death. In the past they were regarded as private documents that were made obsolete by their analysis and publication. There was also the all-too-common difficulty of properly deciphering and understanding someone else’s notes. Today, however, after more than 150 years of professional activity, these now substantial archives have become the subject of intense interest among both fieldworkers and native peoples (Silverman and Parezo, 1995; Parezo, 1996). These preserved field materials may, in some cases, be the only records of languages and cultures that have vanished or have changed radically. Cared for by the alternate disciplines of archivists and librarians but long ignored by anthropologists, anthropological archives can be considered as a distinctly “anthropological problem” (Hallowell, 1965). As this case study shows, anthropological archives have been situated in complex socio-cultural fields, subject to differing and, at times, competing claims by changing sets of creators, curators, and users.

Fieldnotes and other ethnographic inscriptions have a double existence. First, they are representations, carriers of meanings, which may be transcribed or reformulated, especially if they are formed of words. With the exception of native-made artifacts, all are mediated by the anthropologist and by the recording device, capturing some cultural elements and losing others. Second, they are physical and enduring objects subject to their inherent physical limitations (Kopytoff, 1986; see also Kenworthy et al., 1985). They can, and typically do, move from hand to hand. They may be destroyed, modified, copied, or finally deposited in an archives. A study of how anthropological archives come to be is analogous to the investigation of formation processes in archaeology, or of taphonomy in paleontology (see Fowler and Fowler, 1996:132–133; Parezo, 1996). All these studies investigate how the objects of study—vanished lifeways or organisms—leave records in material deposits.

An investigation of the formation processes of field records is thus a cognate of the recent burgeoning attention to the history of artifact collections in museums, one of William Sturtevant’s (1973) abiding interests, as well as a reflection of his joint concern for ethnohistory and the history of anthropology. Like the history of collections, the history of anthropological archives must ultimately be conducted comparatively. Nevertheless, we may use this case study to sketch the fundamental processes. The choice of a California example is particularly appropriate to honor Sturtevant, who as an undergraduate majored in anthropology at the University of California (UC) at Berkeley and who himself has contributed to its archives.¹

Historical Perspectives

The anthropological archives at UC Berkeley is one of the oldest and largest collections of anthropological manuscripts in the country.² These holdings were begun very soon after the 1901 appointment of Alfred L. Kroeber (1876–1960) to the newly founded Department and Museum of Anthropology at the university. An even older and larger collection, and a conscious model for Kroeber, was the archives at the Smithsonian Institution’s Bureau of American Ethnology (BAE), Washington, D.C., founded in 1879 (now the National Anthropological Archives). The other principal collection, the library at the American Philosophical Society (APS) in Philadelphia, Pennsylvania, has collected manuscripts on American Indians since the eighteenth century, but its focus on specifically anthropological materials is relatively recent. The donation of the Franz Boas Collection of Materials on American Linguistics to the APS in 1945 stimulated further donations, such as Boas’s own papers (1961–1964) and those of his students and colleagues (van Keuren, 1986).³ The anthropological archives in Berkeley is unusual in size and scope for a university or museum. Instead it serves a regional sphere, largely, as Kroeber argued in a letter to the BAE, because California was more than a state; it was an entire Native American cultural area (Kroeber to Hodge, 20 Jun 1913, cited in Darnell, 1998:203). And since 1901, the University of California was the place to study it (or at least so thought Kroeber).

The 1901 founding date was relatively early in the establishment of academic departments of anthropology. In fact, Berkeley was the first department of anthropology west of Chicago. Boas had started teaching anthropology at Columbia only in

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1896, and Kroeber was awarded Columbia's first anthropology doctorate in 1901. Joining Kroeber at the inception was Pliny E. Goddard (1869–1928), who earned his doctorate in linguistics from Berkeley in 1904.

In the early years, collecting and research tended to predominate over teaching, although Kroeber lived to have at least two generations of graduate students. Samuel Barrett earned Berkeley's first anthropology doctorate in 1908, and although younger colleagues like Thomas T. Waterman and Nels Nelson studied at Berkeley, after J. Alden Mason's degree, in 1911, there was not another anthropology doctorate there until William Duncan Strong in 1926. A larger group of students, including Julian Steward, Ralph Beals, Lila O'Neale, and Anna Gayton, began in the twenties, followed by an even larger group in the thirties, among them Cora Du Bois, Margaret Lantis, Philip Drucker, George Foster, and Robert Heizer.

As was common at the time, Kroeber had a fairly domineering mode of professorial direction; he assigned ethnographic work in California to virtually all his students. Among those were some who were more drawn to other subfields and other regions: Mason, who later became a Peruvian archaeologist, worked with the Salinan in 1910, and Du Bois, who did psychological anthropology in Indonesia, made a field trip to the Wintu in 1929. With Kroeber's retirement, in 1946, there was a shift to more diverse research on California with the hiring of Heizer, the establishment of the UC Archaeological Survey, and the founding of the Department of Linguistics under Murray Emeneau and Mary R. Haas. Instruction in other regions of the world was also added to the curriculum, beginning in 1946 with the hiring of David Mandelbaum, a specialist in India.

The single most important factor in the formation of the anthropological archives at UC Berkeley was Kroeber's theoretical proclivities (Jacknis, 1993, 1996b; Buckley, 1996). Among the most decisive was Kroeber's Boasian approach to ethnography, a set of linked implications that has come to be known as the "salvage paradigm" (Clifford, 1987). Kroeber's initial assumption was that his primary subject—Native American culture—was vanishing, and as these were nonliterate cultures, it was up to the anthropologist to create the objects that would form the basis of study for future generations of scholars. Kroeber shared these tenets with most of his colleagues, but he also confronted a unique situation in California. Because of the great diversity of native cultures in the state and the comparative lack of study, there was a need for survey and mapping. Given the pressures of time and the lack of graduate students, research had to be collaborative. Because of his personal ambitions, Kroeber wanted to be the one to direct and centralize this research (Long, 1998).

Although survey work had been conducted by the university since Kroeber's arrival, it was formalized in 1903 by the establishment of the Ethnological and Archaeological Survey of California, with funding from Phoebe Hearst, the founding patron of the anthropology department and museum. In this effort, Kroeber saw himself as continuing in California the sur-

vey work of the BAE (Darnell, 1998:199–209). Most of his own fieldwork came in his first decade in California; by the second decade, he had turned to summarizing it, along with the research of others, in the "Handbook of the Indians of California," essentially completed by 1918 but not published until 1925. In the preface he acknowledged making use of the work of many colleagues and students: Barrett, Roland Dixon, Edward Gifford, Goddard, John P. Harrington, Philip M. Jones, Llewellyn Loud, Mason, Nelson, Edward Sapir, and Waterman (Kroeber, 1925:viii). In the thirties, Kroeber launched a collaborative survey, the Culture Element Distribution Survey of Native Western North America, carried out in the field between 1934 and 1938 (Kroeber, 1939). Among the many who participated were Gifford, Steward, and Harold Driver. Because the goal from the beginning was to have comparative data that could be analyzed statistically, the research materials had to be shared and be accessible to Kroeber and those drawing the conclusions. At about the same time, Kroeber and the University of California participated in yet another survey—the Committee on Research in Native American Languages (Leeds-Hurwitz, 1985). Between 1927 and 1937, the Committee sponsored the Native Californian research of associated scholars such as Jaime de Angulo, Hans-Jørgen Uldall, and Harrington.

Gradually, these several surveys led to the accumulation of a substantial archives, whose exact beginnings, however, remain obscure in the historical records. At least by 1909, Kroeber was thinking of archival preservation. Writing to his Harvard colleague Dixon, he noted: "As to the Chimariko material, whenever you are entirely through with the notebooks we should like to keep them here as part of permanent record."⁴ Moreover, it is clear that Kroeber himself used manuscript material—originally created by others—from the start, first for compiling his Handbook, wherein he acknowledged "the abundant use made of manuscript data" (Kroeber, 1925:viii), and then in analyzing the data from the Culture Element Distribution Survey.⁵

The Collections and Their Repositories

There is no single anthropological archives at UC Berkeley, but at its core are the relevant materials currently housed in the University Archives in The Bancroft Library.⁶ Perhaps the most important of these are the so-called ethnological documents or manuscripts, 216 numbered items (some containing many separate subgroups). The collection has been known as such at least since 1971, when student assistant Dale Valory (1971) prepared a guide to the papers. The previous year they had been transferred to the University Archives from the Museum of Anthropology. The manuscripts collection is diverse, consisting of manuscript drafts; fieldnotes; linguistic, ethnographic, and ethnobotanical card files; genealogical data; photographs; and maps. According to the University Archives, this material totals over 50,000 pages. Another relevant holding of the archives is the Department and Museum of Anthropology correspondence, 1901–1956. Although not primarily composed of

field materials, the collection contains much that is relevant in contextualizing the field documentation.⁷

Unlike the manuscripts or the departmental correspondence, which in a sense never left the university, a major group of material was kept by the creator and only donated after his or her death. These personal papers (kept in the Bancroft's Manuscripts Division) often included large bodies of fieldnotes, correspondence, manuscripts, and other kinds of records. Perhaps the première collection is Kroeber's own, donated in 1961, with subsequent additions. Kroeber had had a fair amount of his own material on deposit in the department but removed it sometime before 1957. Among the groups of personal papers rich in California material are those of Barrett (1879–1965) and Heizer (1915–1979). Other anthropologists represented are Robert Lowie (1883–1957), Theodore McCown (1908–1969), Mandelbaum (1911–1987), and William Bascom (1912–1981), all university faculty members.

The Bancroft also houses one major collection of Native Californian documentation not produced by a UC anthropologist: the papers of C. Hart Merriam (1855–1942). Originally a naturalist with the United States Biological Survey, Merriam spent much of the last three decades of his life in meticulous ethnological fieldwork. Beginning in 1910, this research was supported by a trust administered by the Smithsonian Institution. Merriam and Kroeber had maintained a distant relationship (Griset, 1993:36–37), but in 1950—four years after Kroeber's retirement—Merriam's journals and field notes were loaned to Berkeley's Department of Anthropology from the Smithsonian, where they had been deposited by Merriam's daughters. The intention was to keep them accessible in California so they could be analyzed and published (Heizer, 1969; see also Merriam, 1955). Heizer supervised the papers, depositing them gradually in the Bancroft between 1954 and 1977. Along with rich textual materials is an important collection of over 4400 photographs. Because of family sentiment, Merriam's artifact collection went to the anthropology department at UC Davis.

In addition to archival material created by anthropologists, The Bancroft Library holds other important bodies of material concerning Indians of western North America, such as explorers' accounts and mission records. Much of this material was included in the collection amassed by historian Hubert Howe Bancroft, which was acquired by the Berkeley library in 1905. Other relevant holdings are the ethnohistorical research records of UC professor Sherburne F. Cook and manuscripts of Alphonse Louis Pinart, a nineteenth century French linguist and ethnologist.

While the Bancroft has the largest body of anthropological archives on the Berkeley campus, there are other, specialized collections. In the early years of the department, research in archaeology and linguistics was conducted, but Kroeber did not stress training in either field. After his retirement, in 1946, both fields began to be actively taught on the campus, and both, in turn, generated fieldnotes.

The UC Archaeological Survey was established in 1948, under the direction of Heizer, and was succeeded by the Archaeological Research Facility (ARF) in 1960, which continues to this day. The Archaeological Survey was the repository for the reports of many of the excavations undertaken by university personnel, especially after Heizer's arrival. Until about 1975, when a series of regional centers was established, it acted as the central repository of archaeological site reports in the state. Apparently from very early on, the Archaeological Survey got control of relevant archaeological manuscripts that had been accumulating on the campus, with an especially large transfer coming in 1965 (Heizer, 1948, 1972).⁸ As Heizer (1972:38) reported, "This archive is varied in its coverage and quality and consists of original fieldnotes, maps, old manuscripts which have been published and which are mainly of historical interest." In 1990 the ARF manuscript collections were transferred to the custody of the Lowie (now Hearst) Museum of Anthropology, Berkeley, California.

When the Department of Linguistics was established at UC Berkeley, in 1953, it included the Survey of California Indian Languages, initially under the direction of Emeneau and Haas (1910–1996). Haas soon became the director, a position she held until her retirement, in 1977 (Hinton, 1996:93–138; Parks, 1997). The Languages Survey was dedicated to preserving records of endangered languages and generated its own collection of fieldnotes and sound recordings. Most were produced by graduate students, but the survey also acquired some linguistic manuscripts, as had the Archaeological Survey, of older material from other units on campus (Golla, 1995:150–151). Since the early 1960s, the sound recordings of the Department of Linguistics have been housed in the language laboratory (now the Berkeley Language Center) (Rodriguez-Nieto, 1982). The survey collection includes not only unique, primary materials but also secondary records, such as photocopies that are not usually preserved in archives run by librarians.

Although the Hearst Museum still preserves some fieldnotes in accession records, along with various sorts of institutional records, most of its anthropological material is in the form of non-literary media: artifacts, still photographs, films and videotapes, and sound recordings. The latter—mostly wax cylinders that have been transferred to tape—include both song and speech (Keeling, 1991; Jacknis, in press). Almost all the extensive document files that the museum once held have been transferred to the Bancroft. Thus, many collections generated by a given anthropologist—Kroeber, or Bascom, for instance—have been split up between the museum and the library, with the former taking the photographs and sound recordings and the latter holding the extensive written notes that accompany them.

As one can see, these various collections reside in various institutional homes, and they have moved around during their existence. It is uncertain where much of this material was housed during the years when the anthropology department was in Berkeley and the museum was in San Francisco

(1903–1931); it may have resided in both venues. When the museum moved to Berkeley, it took up residence in the old civil engineering building (1931–1959), and by 1951, at least, most of the ethnological documents seem to have been stored in the seminar room of the university library, a room kept locked when not in use. Shortly after the museum moved to its new home in Kroeber Hall in late 1959, all of the anthropological archives were likewise transferred to the anthropology library, in the same building (arriving in 1961). While the personal collections of scholars have generally remained under their control during their lifetime, many of them were actually stored in departmental quarters.

Scope, Creators, and Curators

Given the original focus of the department, it is not surprising that most of the ethnological manuscripts at The Bancroft Library deal with California. There is, however, material from neighboring regions, such as the Great Basin and the Southwest, as well as from the more distant regions of Alaska, British Columbia, and Mexico. The non-New World material is primarily from the Philippines (Roy Barton and John Garvan⁹)—a special interest of Kroeber and University President David P. Barrows—with a little from Gifford's work in Oceania. The geographical balance of campus anthropological archives did not shift until the 1980s, when The Bancroft Library acquired the personal papers of Bascom (Africa) and Mandelbaum (India). These geographical emphases match almost exactly the foci of department research, at least until the recent past. (More recent records have yet to be deposited). In terms of date, the material comes from throughout the department's history, although with a concentration from the 1930s in the ethnological manuscripts collection.

As one would expect, these records were created primarily by UC faculty and students, particularly those whose research was funded by the university. But not everyone was represented equally. By happenstance, Goddard, Gifford, Frank Essene, and Waterman are more heavily represented in the ethnological manuscripts.¹⁰ (Although, ironically, not all their material can be presently located.) Almost all of Kroeber's fieldnotes are in his personal papers. In fact, essentially for reasons of control, the fieldnotes of faculty tended to come in as personal papers after the individual's death, whereas student records were more often incorporated into the department collection of ethnological manuscripts soon after they were written. The working assumption seems to have been that students had to deposit their records at the university when their research was funded by or through the university, as it so often was. Several collections came from former students who did not go on to a career in anthropology. For instance, Paul-Louis Faye was a student of Kroeber's in the twenties. When he shifted fields from anthropology to French, Faye turned over his material on Cupeño myths to Kroeber. A related situation was that of Hugh W. Littlejohn, a graduate student in anthro-

pology who died "some years" after his 1928 field work on Nisenan (Kroeber, [1958]:20).

The collections also contain a good bit from those not directly associated with the university, people whose work was of special interest to Kroeber for its value to California Indian studies. A case in point is Philip Stedman Sparkman, who had recorded linguistic data from the Luiseño and other southern Californian groups. After Sparkman's death, in 1907, Kroeber managed to obtain his manuscripts. "Even the slight discrepancies," Kroeber noted, "resting on information independently obtained and representing significant differences of point of view, may have value for future students" (in Sparkman, 1908:188).

The various keepers (curators or archivists) of these collections have been diverse and have included both those holding ultimate authority (such as faculty members) and those responsible for day-to-day maintenance (a clerk or archivist). We know little about the caretaking of the collection in its early years, but it was probably performed by a clerk or secretary, working under Kroeber's supervision. Quite a number of items were kept personally by Kroeber in his office safe. Toward the end of his life, Kroeber seemed to have been especially concerned with the archives.¹¹ During 1957–1958, he spent much time sorting through and annotating the collection. In the course of his dictated commentary, Kroeber suggested some basic principles of archival method, as he saw it. For instance, necessary transcriptions should be performed by people who are familiar with the material, so that errors in reading Native American names, locations, and so on, are not introduced, and any lists, labels, or finding aids should "give the most essential facts conveniently," in order not "to leave essential guiding information to be searched out afresh each time by going to the documents themselves" (Kroeber, [1958]:21, 25). Although sound principles in any case, one notes in them the viewpoint of a practicing scholar.

The sixties was the period of greatest activity for the formation of the archives as an ordered collection. This effort had been initiated by Kroeber's review and annotation of the material in 1957–1958, but the most intense work was begun at the end of 1966 by Valory, then an anthropology graduate student, who did most of the processing (Kroeber and Valory, 1967). Valory's own motivation was to use this material, especially Goddard's Tolowa and Tututni notes, in his dissertation (Valory, 1970). "This project was undertaken voluntarily by me," he reported, "under the supervision of Dr. A. B. Elsasser of the Lowie Museum. The work involved examination and evaluation of the collections themselves, library research to determine whether or not materials had been published, ought to be, could be, etc.; and correspondence with authors or with experts who might be concerned with the best possible use and/or evaluation of the materials."¹² This work continued until Valory revised his guide and the material was transferred to the Bancroft, where university archivists currently care for the collection. The archaeological manuscripts were first curated by Heizer in his Archaeological Research Facility and cur-

rently are curated by the Hearst Museum. The Linguistics Department still supervises the records in the Survey of California and Other Indian Languages (as it was renamed in 1965).

One can discern in this history a process of gradual “institutionalization,” in several senses. First, there is an increasing specialization, with separate collections formed to match the growth in the amount and scope of research. It has also entailed the replacement of personal and ad-hoc practices by regularized procedures, with a dedicated staff and space. But most significant has been the shift from a period in which matters were decided largely on the basis of their relevance to anthropology to one where principles of archivism and librarianship take precedence. Leanne Hinton, the current director of the California Language Survey, glosses this as a distinction between a “working archives” (the survey) and a “preservation archives” (the Bancroft). She added, “As these materials get more valuable, they tend toward the Bancroft” (Hinton, pers. comm., 28 Oct 1996), meaning that over time the feeling grows that they should be cared for not by individuals but in an archives where they can be preserved and made accessible to all.

What Is There and What Is Not There

At various times, inventories were made of the collection. One of the earliest, dated to 1951, lists 39 items, corresponding to what Kroeber annotated in 1958. Yet there was a lot more archival material on campus that was not considered by Kroeber. Generally, as in the case of both the ethnological documents and the archaeological manuscripts, these inventories are simple sequential lists with items in no apparent order. Because of their repeated use and reference by archivists and scholars, these lists and numbering systems have been maintained for both collections.

Some fieldnotes that might be expected to have been deposited in the Berkeley archives seem to have never made it. As linguist William Seaburg (1994:9) comments, “Anthropology has not been particularly solicitous of its intellectual heritage, especially its treatment of fieldnotes and other field-generated documentation.” Dixon, for instance, is reported to have deliberately destroyed his Maidu field notebooks (Bernstein, 1993:21). Du Bois’ notes on the Wintu, which she produced on a Berkeley-sponsored field trip, cannot be located.¹³

The notes of Goddard are a particularly instructive example of the fate of fieldnotes. Goddard taught in the anthropology department from 1901 until 1909, when he accepted a curatorial post at the American Museum of Natural History, where he remained until his death in 1928. In looking at his field materials preserved at Berkeley, one first notices the virtual absence of ethnographic notes. A comment by Kroeber explains this gap: “Ethnographic information Goddard tended, on the whole, to record only in his memory, and that is no doubt why he published so little, except on Hupa where he lived—for other tribes, the bits are likely to be marginal notes to his linguistic records. He did however observe a great deal of culture: I recall

quizzing him with profit for a full hour on his return from his two Tolowa trips” (Kroeber, 1967:273).

As a working scholar, Goddard seems to have taken all his unpublished field materials with him to New York. After his death, these notes traveled through the hands of several colleagues—Elsie Clews Parsons to Gladys Reichard (his literary executor), to Florence Voegelin (at the Indiana University Linguistics Archive)—until arriving back in Berkeley, at which point Kroeber transferred some to the APS Library (Kroeber, 1967; Valory, 1971:50).¹⁴ So, while some of these original field records never made it to the Berkeley archives, other, related, material ended up on the East Coast. This illustrates a common feature of archival research, that a given collection may be split between repositories (Parezo, 1996:145–147). Attempts can be made to avoid this, but to a large extent it is an inevitable result of the multiple paths through which field materials flow as they are created and used.

The Goddard records also illustrate another common mode of transmission. Whether through the action of the creator or his/her literary executors, parts of a scholar’s work may be given to students or colleagues in order that they might “complete” or otherwise use the fieldnotes. A prime Berkeley example is the papers of Heizer, many of whose “works-in-progress” were given to various students (Clark, 1979:266–267). Most of these still have not made their way back into the collection.

Quite a number of manuscripts that were once in the collection are no longer there, and their absence was noted in the inventories. Valory, Elsasser, and others removed things that they deemed “of no interest or value” from an anthropological standpoint before making lists and handing over the material to the University Archives.¹⁵ For example, several documents were removed in 1962, “as they are neither ethnological or original documents.”¹⁶ Another manuscript, James Bennyhoff’s working notes and drawings of fish spears and harpoons, was returned to him on the grounds that they had been “adequately published.”¹⁷ “WPA typescripts of books unavailable to U.C. scholars during the Depression” were removed because they were merely copies,¹⁸ and Kroeber ([1958]:29) questioned the value of a carbon copy of something “probably printed.” Complaining about the retention of apparently useless maps, Kroeber ([1958]:32) noted, “This is a classic illustration of the painfully careful preservation of irrelevant information because no one on the staff who would know intervenes to say what is irrelevant.” He did recommend, however, keeping an early version of the Sparkman Luiseño grammar, which he subsequently published (Kroeber and Grace, 1960), for the “historical record,” but not for “any fuller or new information” (Kroeber, [1958]:30). Key criteria for the retention or removal of records during this period were their uniqueness, ethnological significance, and publication status.

In addition to single or small groups of items, entire collections have left the campus. The best case in point is the Washington Matthews papers. Matthews (1843–1905), the founder of Navajo studies, was supported in his last years by Phoebe

Hearst in order to translate and edit his notes. According to the arrangement, Matthews's papers came to Berkeley following his death. Although Goddard, a fellow Athapaskanist, did some initial work with them, they were largely ignored after 1909, when Goddard went to the American Museum. In 1951 they were given to the Museum of Navajo Ceremonial Art, founded by Mary Cabot Wheelwright in Santa Fe, New Mexico, in the belief that they would be more often consulted there (Wheelwright Museum, 1985). This inverse of the Goddard papers demonstrates that archival collections are deposited for contingent and historical reasons, not for reasons of sheer logic or appropriateness.

This active form of deaccessioning prevailed during the time that the collection was controlled by Kroeber and the anthropology department. With minor adjustments as series are processed, the University Archives has tended to preserve collections as presented. These active changes in the status of archives are an indication of the exercise of control. Before 1971 for the anthropological papers and still today for the Languages Survey, academic rather than narrowly "archival" standards of value have been applied. Papers came into and went out of the collection according to personal connections between the creators and the curators, in addition to whatever disciplinary values they may have held. During such a period, papers may have been removed or destroyed if the creator were living and deemed to have a say in their status.

Absences, such as those noted here, are significant historical markers. To note only what is currently in an archive is to miss a vital part of its history, for it is presentist and retrospective, ignoring much that was once important. An understanding of these formation processes and their effect on the archival sample is necessary to reconstruct the entire picture, both ethnographically, for the culture described, and historically, for the life and work of the anthropologist.

Changing Uses and Users of the Collections

For archival collections, issues of preservation, reproduction, publication, and access are intimately interwoven. The Department of Conservation of the Bancroft and the university library has carried out a series of preservation efforts devoted to the Kroeber, Heizer, and Merriam papers and to the ethnological documents. These projects of ordering and describing, repair and rehousing, and microfilming serve as a means of preservation as well as reproduction.

Archives have always borne a complex relationship to publication. Generally, archives have preserved what has not been published, although there is the understanding that some of the manuscripts are potentially publishable. One of the more important publications of a manuscript in the Berkeley archives is Kroeber's *Yurok Myths* (1976; see also Kroeber and Gifford, 1980). Heizer was one of the most active editors/publishers of original archival material, including much in the Bancroft (e.g., 1970; Merriam, 1955). On occasion, publication has

been problematic, for example, Isabel Kelly's notes on the Coast Miwok. During the 1970s, portions of her notes were copied—for research purposes only—from the Lowie Museum. Kelly was very upset when they were published without her permission and with errors (Kelly, 1978:424). Her full Coast Miwok fieldnotes were published after her death (Collier and Thalman, 1991).¹⁹

Kroeber's ethnographic material has been the subject of two digitization projects. In 1998 his photographs, housed at the Hearst Museum, were digitized and incorporated into the Bancroft's Finding Aid series to complement his collection of papers. (In 2000 the museum and library started a larger project to digitize the museum's entire collection of ethnographic field photos of California Indians.) The Kroeber images will become part of a separate effort: a World Wide Web site, under construction, that will be dedicated to Kroeber's representation of Yurok culture, which may form the basis for a future CD-ROM production. These various forms of photographic and digital reproduction are allowing the reintegration of field records that have been split up for practical and administrative reasons. A student will then be able to trace Kroeber's ethnography, while contextualizing it with alternative documents (letters, newspapers, local histories, etc.), revealing what Kroeber left out and how he accomplished what he did.

Archives, dedicated to the preservation of unique objects, are incomprehensible apart from their interplay with the changing technology and customs of reproduction. Almost all archival material can be copied in some form or another, and this capacity has only accelerated in recent years as magnetic sound recording, photocopying, microfilm, and now digitization allow ever easier and more precise forms of exact duplication. The Berkeley archives once included typed transcriptions and carbon copies, which archivists have generally decided to exclude, acting under the necessary assumption that they have to prioritize the limited storage space at hand.

These contemporary technologies raise postmodern questions of authenticity. Publication, photography, and now the Internet begin to blur the distinction between original and copy. These new forms of reproduction have transformed archives. During the time of Boas and Kroeber, field material in the hands of individuals or archives were of little use to the profession. Publication was the main means of making them accessible to scholars. This explains the feeling against Harrington, who was reluctant to publish, and Kroeber's compulsion to publish.²⁰ Now with photocopying and changes in disciplinary values, publication can actually be less valuable, as it is so often selective, with a bias against description (Hinton, pers. comm., 28 Oct 1996).

These photographic and digital surrogates make possible a new kind of archives, overcoming some of the inherent limitations of collections of unique items. These alternate versions make possible the sharing and duplication of collections between distant repositories—as, for example, with the Goddard collection. They can also allow the simultaneous preservation

(by archives) and analysis (by students) of collections, as seen, for example, with the Heizer papers. In a sense, such technology is a form of publication that moves archives away from the model of museums (unique items) and more to that of libraries (multiple copies).

The greatest change in how the anthropological archives has been used came when it was transferred to the University Archives. Before going to the library, access was controlled by the anthropology department and museum, and thus permission tended to be given based on the personal relationship of the prospective researcher to the curator. While the creators were still alive and possibly interested in using unpublished material, there was a certain reticence to grant access. (Formal permission documents seem to have been lacking.) Part of Valory's job was to contact all living creators or the executors of deceased researchers and ask them about their feelings on donation and access. Now, with these permissions secured, the collection is generally accessible to all who can show a serious interest. The accumulated archival materials at UC Berkeley are used extensively by a wide range of patrons: anthropologists (Jacknis, 1995; Milliken, 1995), historians of anthropology (Darnell, 1998; Golla, 1984), the popular press (Margolin, 1981), and, most importantly, native peoples.

One recent outreach effort was the California Indian Library Collections (CILC) project, administered by the Lowie Museum (1988–1994). The CILC made copies of various forms of archival material on the Berkeley campus, primarily photographs and sound recordings, and distributed them to county libraries, where they could be consulted by local Native American populations (Davis and Koué, 1989). Native peoples have been especially interested in studying and obtaining copies of photographs of family relatives. A particularly exciting program was “Breath of Life/Silent No More,” a workshop on Native Californian language preservation that Hinton has organized (Hinton, 1996). In 1996, 1997, 1998, and 2000, individuals whose native languages were extinct were exposed to the wide range of archival material on the campus, which they could use to help revive their traditions.

Many, especially Native Californians, have criticized Kroeber, both for his omissions as well as for many of his positive statements. Hupa scholar Jack Norton, for instance, took Kroeber to task for his failure to treat the genocide of Native Americans and the importance of the religious dimension of north-west Californian cultures (Norton, 1979:6–7, 18; see also Costo and Costo, 1995:12–13, 44). Boas, Kroeber, and other anthropologists of their generation proved to be mistaken in their belief that Native Americans would become extinct. Yet much of Native American culture has changed, and some has been lost. Boasian anthropologists strove to create an archive for future generations. Ironically, they were thinking of scholars, not native peoples (Jacknis, 1996a:209). What is of value in the ethnographies of Kroeber and his colleagues is the intricate descriptive detail, inevitably flawed though it may be.

Anthropological archives will become only more important with the passage of time. The participants in the Silent No More workshop showed “who the most appreciative audience of our work is,” as Hinton (1996:16) remarked. She continued, “There is no one in the world who has more at stake and is ultimately more concerned with the quality of our work than the members of the speech communities themselves. The work of Harrington, Merriam, Kroeber, Barrett, and others has never been respected as thoroughly by linguists or treated with such passionate gratitude by them as it is today by Native Californians.” An appreciation of how and why these archives have been created will allow us all to better mine the rich cultural documentation they preserve.

Notes

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1. Some of Sturtevant's correspondence is included in the anthropological archives at UC Berkeley, in the papers of Robert F. Heizer, editor of the *California* volume of the *Handbook of North American Indians*, of which Sturtevant is the general editor (Sturtevant, 1981; see also a small Sturtevant accession, 77/60, in The Bancroft Library, University of California, Berkeley, California).

2. This essay cannot address all the many complex issues of anthropological archives, and given the great scope of this collection, it can only be a sketch. This task is made even more difficult by the lack of a comprehensive published review of the history of anthropology at UC Berkeley. Thoresen (1975) and Long (1998) are excellent for its founding and early years; see also Jacknis (1999, 2000). Grace Buzaljko is completing what promises to be an extremely valuable study, “The Berkeley Anthropologists, 1901–1991.”

3. Boas seems to have been much less self-conscious than Kroeber in his formulation of a local ethnographic archives, with the exception of the Committee on Research in Native American Languages, 1927–1937. Even here, his intention was to publish the many manuscripts he accumulated. It was only the lack of available funding during the 1930s and his death in 1942 that led to their eventual donation to the American Philosophical Society (Leeds-Hurwitz, 1985:150).

4. A.L. Kroeber to R.B. Dixon, 5 Jan 1909, folder file 1909 D, carton 4, Department of Anthropology correspondence, Bancroft Library, UC Berkeley. Alex Long kindly shared this letter with me. Dixon did indeed deposit “Chimarriko Culture and Language,” a 166-page typed manuscript based on his 1906 field work (funded by Phoebe Hearst for the university), which formed the basis for a publication (Dixon, 1910).

5. Darnell (1998:205) reported that Kroeber and Roland Dixon consulted manuscripts in the BAE collection in Washington, D.C., while working on their reclassification of Native Californian languages.

6. The distinction between the University Archives and The Bancroft Library has been nicely spelled out by James Kantor, former university archivist: “Until 1970, The Bancroft Library was primarily a depository for materials relating to the history of the American west, with some attempts, since the 1950s, to include California literary materials. The Bancroft did not seek collections of manuscripts relating to other fields; therefore it did not seek most faculty papers. The University Archives, which goes back to the beginning of the university in 1875 [the university itself was founded in 1868], was a department of the university library until it was transferred to The Bancroft Library in 1962; it did collect faculty papers. I assumed the position of university archivist, as a

staff member of The Bancroft Library, in 1964, and soon after transferred all faculty papers (that is, manuscript collections) to the Manuscripts Division of The Bancroft Library, where they sit today. Thus, in 1964 such collections as Kroeber's papers were transferred. Meanwhile I did actively collect materials relating to the university, including its academic projects, so that it made eminent sense to acquire the correspondence files of the Museum and Department of Anthropology, and the Ethnological Documents" (in litt., 20 Dec 1996).

7. Like the general history of UC Berkeley anthropology, there is no comprehensive review of disciplinary archival resources at the university, although each repository has its own finding aids. Much relevant information on the history of the archival collection is contained in two binders in the Hearst Museum Archives, Berkeley, California. Both, however, contain much more than their cover titles indicate. In the notes, I have referred to them by their colors, since they were referred to that way at the time.

Blue binder: "A.L. Kroeber and Dale Valory, A Guide to Ethnological Documents 1-39 of the Department of Anthropology in the R.H. Lowie Museum of Anthropology."

Brown binder: "Master List File Copy of Various Documents Stored in the R.H. Lowie Museum of Anthropology 'Archives.'"

8. See "Manuscripts in the U of C Archaeological Survey Files," 1950, brown binder (see note 7); "California Archaeological Manuscripts," 1951, brown binder, noted as transferred from the museum to the Archaeological Research Facility in 1965 (Shirley R. Gudmundsen to Edna Flood, 8 Sep 1965, brown and blue binders).

9. Barton, an amateur but dedicated ethnologist, worked extensively in the Philippines between 1906 and his death, in 1947. While studying to be a dentist at the UC in the teens, he came into contact with Kroeber and the anthropology department. Garvan, an employee of the Ethnological Survey of the Philippines in Manila between 1900 and 1920, prepared his manuscripts in the two decades before his death, in 1940.

10. Before obtaining his anthropology doctorate from Berkeley—on Eskimo mythology, in 1947—Essene spent much of the previous decade in ethnographic research among California Indians.

11. Kroeber's retrospective activity during the 1950s coincided with his preparation for publication of his California Indian linguistic and ethnologic re-

searches, which was supported by a grant he had obtained in 1951 (Hymes, 1961:19).

12. Dale Valory to Nelson Graburn, 11 Mar 1967, blue binder (see note 7); Memorandum from Dale Valory "To Whom It May Concern," 20 Oct 1968, blue binder.

13. Du Bois's Wintu notes are neither at Berkeley nor in the Tozzer Anthropology Library at Harvard with the rest of her papers (Seaburg, 1994:24). She may have given them to a student or to a colleague.

14. A related case of traveling Goddard fieldnotes is reported by Seaburg (1994:9-10). "A frequent practice, though, has been the dispersal of notes to various colleagues and students or to Native collaborators or their descendants. Sometime after Pliny E. Goddard's death in 1928 his Lassik and Galice Creek Athapaskan notes were given to Gladys Reichard, who gave them to Melville Jacobs in the early 1940s. They are now part of the Melville Jacobs Collection at the University of Washington Libraries. Edward Sapir's Hupa fieldnotes were given to his student, Harry Hoijer, who in turn gave them to his student, Victor Golla, who eventually gave them to the American Philosophical Society Library... Such examples of the physical odyssey of field materials are by no means unusual. As a result of this practice notes have been lost or their whereabouts remain obscure."

15. Valory to W.R. Bascom, 15 Mar 1967, blue binder (see note 7); cf. Valory to N.H.H. Graburn, 11 Mar 1967, blue binder.

16. Memorandum from Valory regarding Museum of Anthropology Archives, 31 Jan 1967, blue binder (see note 7).

17. James A. Bennyhoff to Valory, 28 Jan 1967, blue binder (see note 7).

18. Memorandum from Valory "To Whom It May Concern," 20 Oct 1968, blue binder (see note 7).

19. Kelly's original ethnographic notes are with many of her other papers in the Department of Anthropology, Southern Methodist University, Dallas, Texas, under the care of Robert V. Kemper (Collier and Thalman, 1991: xxiv-xxvii).

20. As Kroeber once remarked: "I would rather publish as fast as I can feel reasonably sure of my material, even at the risk of errors and uncertainties, so that it will be out where others can shoot at it. Nothing is worth much in any profession until it is published" (Krieger, 1961:21).

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Starring the Anthropologists in the *American Men of Science*

David J. Meltzer

Whatever the power of an “official myth of democratic equality” within the scientific community, practitioners know—deeply and viscerally—that not all scientists are created equal; some are more equal than others (Rudwick, 1985:419). They gain (and lose) status relative to their work, the work of their peers, and the changes and developments in their field. A measure of the relative status of individuals within a discipline or community of practitioners thus becomes a barometer for tracking changes—individual and institutional—within a discipline itself.

Gaining a measure of relative scientific status, particularly within a historic scientific community, is not always simple or straightforward. As a relative entity, scientific status is contingency-bound and mutable and is one without fixed, supra-historical standards (Rudwick, 1985:419). Moreover, perceived status resides largely in tacit and informal judgments—estimates of worth that are arrayed along what has felicitously been termed a “gradient of attributed competence” (Rudwick, 1985:419). That gradient runs from the few elite scientists who are regarded as arbiters of fundamental questions of theory, method, and substantive result, to the many non-elites whose roles are more sharply limited. Positions along such a gradient might coincide little with outward trappings of accomplishment or formal hierarchies of status, including election to the National Academy of Sciences (Visher, 1947:4; Rossiter, 1982:287; Rudwick, 1985:419). Instead, the gradient of competence is usually ferreted out from less direct clues

by noting the ways in which [the practitioners] treated and referred to each other and each other’s work. Their correspondence is by far the richest source of evidence, because it was both informal and private; but published work is also revealing, at least if read between the lines. (Rudwick, 1985:419)

James McKeen Cattell’s efforts in compiling directories of American scientists have provided a more explicit and direct (but perhaps not unbiased) source for mapping the field of competence among select historic scientific communities, including anthropology. In the spring of 1903, Cattell sent 10 of the most prominent anthropologists in America a list of some 50 of their peers, with the request that they rank the names on the list according to their contributions to the field of anthro-

pology. All responded to his request, although more than one expressed misgivings about the venture. Cattell tabulated their responses, and the 20 top-ranked anthropologists each received a star next to their name in the 1906 (first) edition of the *American Men of Science* (hereafter, *AMS*). Cattell repeated this process for subsequent editions of the *AMS* (the actual records of each judge’s responses are available only for the first two editions, and the complete rankings are available only for the first three editions).

Those records provide a window into scientific status and rank in early twentieth-century anthropology and how these changed over time. Indeed, gauging such matters on a long-vanished intellectual landscape is often made even more complicated by our own later perceptions of what was or what ought to have been the relative status of particular individuals (views that might well vary by one’s own subdiscipline, theoretical persuasion, when the judgment is made, and so on). These historical records, of course, are not themselves free of bias, but they provide for the historicist (sensu Stocking, 1968) an opportunity to glimpse—warts and all—just how these individuals saw each others’ merit at the time all were still alive, without the benefit (or burden) of a century of hindsight. Such records yield a fine-grained map of the field of competence and an uncommon perspective on the elite and non-elite in early American anthropology.

Through these measures of individuals, we also gain insight into the process of disciplinary change and the development of early twentieth-century American anthropology. When the first of these directories was published (1906), American anthropology stood poised on the threshold of major disciplinary change. Borne of a natural history tradition and having come of age when theories of social evolution dominated the intellectual landscape, the discipline was institutionally—and intellectually—firmly entrenched within the government research bureaus and museums of Washington, D.C. Yet by the second decade of the twentieth century, anthropology was moving fast toward a new center within the burgeoning university system, one dominated by Columbia University’s Franz Boas and his students, who yearly were increasing in number and who possessed the formal training needed to meet the increasingly strict requirements of anthropology’s newly created professional ranks. This group generally shared Boas’s disdain for evolutionary schemes that embraced all of humankind in a single developmental formula. They envisioned anthropology as a disci-

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pline where language, thought, customs, and ideas were paramount and where material objects—the focus of traditional museum anthropology—played a far less significant role (Stocking, 1974; Hinsley, 1981:251).

That sea change in the discipline is marked in many ways and at many levels (from theoretical to institutional), and its consequences are especially evident in the changing status and rank of individuals within the anthropological community itself. All that is played out in the evolution of the stars over the successive editions of the *AMS* over the first several decades of the twentieth century.

James McKeen Cattell and the *American Men of Science*

Just after the turn of the century, James McKeen Cattell (1860–1944), a Columbia University psychologist and entrepreneurial editor of scientific journals (including *Science*, *Popular Science Monthly*, and *American Naturalist*), began to compile a list of American scientists for the Carnegie Institution of Washington, D.C. Intended to be a who's who of the American scientific community, the project quickly escalated beyond its original intent of providing a simple reference list. In Cattell's (editor, 1906:v) words, it became "a contribution to the organization of science in America," a way of making some 4100 scientists—mostly working apart from each other across the nation—acquainted with one another's work.¹

Cattell, however, wanted to do more than just publish a directory. He also wanted to sort these individuals, identifying those whose "work is supposed to be the most important" (Cattell, editor, 1906:vi; see also Cattell, 1903:566). This was, after all, an age in which (for the first time in American society) there was a pronounced emphasis on using numerical measures as a means of sorting, segmenting, and imposing order on the increasing disorder of everyday life (Chudacoff, 1989:5; see also Weibe, 1967:77, 111–132, 156–159).

To accomplish this task, Cattell approached "ten leading students" from each of the 12 principal sciences in America and asked them to arrange the names of the workers in their field in "order of merit" (Cattell, editor, 1906:vi). In *AMS 1* (the first edition of *AMS*), 1000 scientists, representing approximately 25% of all entries, were starred, and 20 of the stars (2%) belonged to anthropologists. Cattell allotted stars to each field based on the percentage of that field's practitioners to the total number ($n=4131$) of scientists in *AMS 1*. For *AMS 2* and *AMS 3*, the selection process was repeated, and stars were added in each field.

All individuals who had received a star in a prior edition of *AMS* retained their star until their death (Cattell and Brimhall, 1921:viii). They did so even if they no longer warranted star status, and such "gratuitous stars"—as they have been called (Rossiter, 1982: 289–290)—had a more insidious result than just causing "considerable confusion" about what a star really meant. In *AMS 3* and subsequent editions, Cattell capped the number of stars at roughly 1000, despite the fact that the over-

all number of individuals included in the *AMS* volumes rose from 4131 in *AMS 1* to about 34,000 by *AMS 7*, which was published in 1944 and was the last edition in which stars were assigned. New stars were added only in numbers that would preserve the percentages assigned that science in *AMS 1* (2% in the case of anthropology), which effectively ignored the differential growth of each field (Cattell and Cattell, 1933:1261; Visser, 1947:5). This also capped the number of new stars that could be added to any discipline, and when there were many gratuitous stars (as there were by *AMS 3*, for example), fewer slots were left open for new ones, however much candidates were judged by their peers to be deserving of a star and however little the gratuitous stars were deserved. Earning a star was only half the battle: one sometimes had to wait for the death of an individual holding a gratuitous star to free up a slot. Under the circumstances "exclusion was not necessarily a dishonor" (Visser, 1947:23; Rossiter, 1982:290).

The process of starring continued through *AMS 7*, but with Cattell's death, in 1944, the task of compiling and editing the *AMS* fell entirely to his son Jacques, who for a variety of reasons and in the face of criticisms discontinued the practice of starring in *AMS 8* (published in 1949). He held out the hope that later editions might include a measure of rank (Cattell, 1949; Rossiter, 1982:290).

While it is, of course, of interest to identify those who were the stars of a field, it is of even greater value to learn the relative ranking of individuals within the star category, how that ranking was determined, and by whom. In that way one could develop a finer-grained map of scientific status and a better understanding of the manner in which competence was graded and status was determined and awarded. One could learn how (and perhaps why) status changed, and even whether one's star was earned or—by virtue of a previous appearance in *AMS*—was gratuitous. The contemporary landscape, not to mention possible biases in the sorting process, is revealed by knowing who the judges were and how they voted.

Cattell obviously possessed this information, but he published almost nothing of it (even though he was keenly interested in measuring biases in judging (see "Finding the Stars," below; see also Cattell, 1906:662–663). In *AMS 1* through *AMS 7* the stars of each science are identified, but no divisions of rank within that category are made. All stars are treated equally, the judges are unnamed, and their votes are unknown. This was deliberate on Cattell's part and was likely done at least in part to ensure cooperation, by assuring the judges that it was

not intended that the lists [the rankings of anthropologists] shall be published, at all events not within ten years. No individual [judge's] list will be published. They will be destroyed when the averages have been calculated, and the arrangements will be regarded as strictly confidential. (Cattell, 1910:539)

Only in *AMS 5* (published in 1933) is there a published ranking of the stars, but that ranking is of the starred scientists in *AMS 1*, published three decades earlier (Cattell and Cattell, 1933:1268).

To see whether any of the raw ranking data for the anthropologists survived in unpublished form, I examined the Cattell papers at the Library of Congress, Washington, D.C. There I discovered Cattell had not wholly erased the voting records as he had promised. Complete rankings of anthropologists exist for *AMS 1* and *AMS 2*. Cattell had mostly kept his word, however, for what survives are not the individual judge's responses but, instead, Cattell's compilation of their rankings. On those lists, Cattell identified the judges by their initials, making them easily identifiable. The Cattell papers also contain fragments of judges' rankings for later editions of *AMS*, but the year or edition for which they were compiled could not be reliably determined. There are, however, listings of the stars for *AMS 3* through *AMS 6*, but only the *AMS 3* listing had the names arranged in rank order (mean scores were beside each name, but the identity and rankings of the judges were not found). Thus, after *AMS 3* it is impossible to spot the gratuitous stars, although the newly elected stars in *AMS 4* to *AMS 6* are easily identified.²

The *AMS* ranking data do more than provide a map of individual status within the community. They can also illuminate the views and opinions of the judges (individually and collectively) who made the rankings, mark changing intellectual trends in the field, and provide a means of examining the potential influence of, say, institutional affiliation, gender, and interdisciplinary specialization on status and rank within a field, among other things (knowing the ranks is one matter; explaining their meaning and significance is quite another). A comprehensive analysis of the *AMS* data along these many lines, however, is well beyond the scope of the present paper. Rather, it examines some general trends in the judging and the rankings.

The list of judges and the actual ranking data are provided (Tables 1–4) so that others may explore the many angles and aspects of this record.

Finding the Stars

In *AMS 1* a total of 113 individuals listed their "department of investigation" (Cattell, editor, 1906:vi) as anthropology or one of its related fields (e.g., ethnology, archeology, folklore). Of that total, 108 were men and five were women, making the title *American Men of Science* somewhat misleading (Rossiter, 1982:25). In *AMS 2* the number of anthropology entries was virtually the same: 112 individuals, 105 men and seven women. The number of women anthropologists would rise considerably in later editions of *AMS*, largely due to such mentors as Boas and Gladys Reichard at Columbia and Barnard, respectively (Rossiter, 1982:145). It is unclear whether the count of women in the first two editions of *AMS* accurately represents their number in turn of the century anthropology (quite possible, given the few opportunities for training, experience, or employment then available to women), or whether there were biases in the process which under-counted their numbers. In either case, they helped constitute the pool of anthropologists who were to be ranked.

Ranking the anthropologists required judges, and the 10 individuals Cattell selected for that task for *AMS 1* and for *AMS 2* (Table 1) well represented the several centers of American anthropology of the time—Cambridge (Harvard University and the Peabody Museum), New York (Columbia University and the American Museum of Natural History (AMNH)), and

TABLE 1.—The judges for the *American Men of Science*, editions 1 and 2. Institutional affiliations of the judges are listed as of the year the rankings were made for each edition.

Judge	Institutional affiliation	Advanced academic training
1903 (<i>AMS 1</i>)		
Franz Boas	Columbia University, AMNH	Ph.D. Kiel, 1881
Alexander Chamberlain	Clark University	Ph.D. Clark, 1892
Stewart Culin	Brooklyn Institute of Arts/Science	—
George Dorsey	Field Museum of Natural History	Ph.D. Harvard, 1894
Livingston Farrand	Columbia University	M.D. Columbia, 1891
Alice Fletcher	Peabody Museum, Harvard University	—
Frederick Hodge	Bureau of American Ethnology	—
William Holmes	Bureau of American Ethnology	—
WJ McGee	Bureau of American Ethnology	—
Otis Mason	United States National Museum	Ph.D. Columbian University, 1875
1909 (<i>AMS 2</i>)		
Franz Boas	Columbia University	Ph.D. Kiel, 1881
Alexander Chamberlain	Clark University	Ph.D. Clark, 1892
Roland Dixon	Harvard University	Ph.D. Harvard, 1900
George Dorsey	Field Museum of Natural History	Ph.D. Harvard, 1894
Livingston Farrand	Columbia University	M.D. Columbia, 1891
Frederick Hodge	Bureau of American Ethnology	—
William Holmes	Bureau of American Ethnology	—
Aleš Hrdlička	United States National Museum	M.D. New York Homeopathic, 1894
Alfred Kroeber	University of California, Berkeley	Ph.D. Columbia, 1901
WJ McGee	Inland Waterways Commission	—

Washington, D.C. (the Bureau of American Ethnology (BAE) and the United States National Museum (USNM), at the Smithsonian Institution) (Stocking, 1976:9). No center dominates either list. The four Washington anthropologists (Frederick W. Hodge, William Henry Holmes, WJ McGee, and Otis Mason) constituted the largest group among the *AMS 1* judges, and given that a higher proportion of anthropologists were then employed or had ties to government research bureaus or museums, this arguably was a representative panel. By *AMS 2* the number of New York judges—those affiliated with Boas or Columbia University—were comparable to the number of Washington judges. The judges for subsequent volumes are unknown.

The only noticeable omission from the panel of judges is Frederic Ward Putnam, whose administrative genius helped establish anthropology programs at Harvard, Columbia, and the University of California at Berkeley and in museums in Cambridge (Peabody), Chicago (Field Museum of Natural History), and New York (AMNH) (Mark, 1980:55). Putnam was highly ranked in both *AMS 1* (fifth) and *AMS 2* (second), and by 1903 he had long a member of the National Academy of Sciences. His absence may have been for insignificant reasons (including being asked to judge but choosing not to do so). In any case, Alice Fletcher (*AMS 1*) and Roland Dixon (*AMS 2*) represented the Boston node among the judges. Fletcher's case gave anthropology the distinction of having a woman serve as a judge for *AMS* (as best can be determined from available evidence, this was unusual if not unprecedented among the sciences). Her serving as a judge is the more remarkable in that Cattell became a strong supporter of women's advancement in science only after the first two editions of *AMS* (and in apparent response to complaints about his lack of understanding of the problems facing women scientists and how that might effect their chances for a star (Rossiter, 1982:108–109)).

In 1903 and again in 1909 Cattell sent each judge a list of all the anthropologists in the United States.³ How Cattell, a psychologist, derived the initial list of names is unspecified, but he likely relied heavily on his Columbia colleagues, Boas and Livingston Farrand. That initial list was considered incomplete by the judges, however, for at least two dozen individuals received write-in votes from several of the judges—including Boas, Holmes, and Mason.⁴

Those additional names helped round out the representativeness of the pool, and indeed it seems most of those active in anthropology appeared on this and the subsequent list (there were 71 individuals on the ballot for *AMS 1*, 55 for *AMS 2*). Those who were listed in the *AMS* volumes as anthropologists but were left off the ballots included, in the case of *AMS 1*, a few who would later become stars but who were then still relatively young and unknown (e.g., Pliny Earle Goddard, Clark Wissler) or who were stars in their primary field (e.g., zoologist C. Hart Merriam). The other approximately 40 anthropologists left off the *AMS 1* ballot were mostly now-forgotten in-

dividuals whose contributions were not then (nor can they be now) accorded particular significance—with a few notable exceptions. Charles Abbott, Henry Haynes, Ernst Volk, and George Frederick Wright were all highly active in archaeology, and all were listed in *AMS 1*. None, however, was considered for star status. It may not be a coincidence that all four were active proponents of an American Paleolithic, a position vigorously opposed by Holmes (who freely wrote in other names on the list).

An additional consequence of the write-in votes (beyond increasing the representativeness of the pool) was that an individual judge might rank anywhere from 39 to 66 names for *AMS 1* and 41 to 48 names for *AMS 2*. This had unintended statistical effects in averaging the collective rankings, notably because Cattell did not set all the judges' rankings to the same numerical scale.⁵

Given that Cattell solicited the rankings for *AMS 1* in 1903, a full three years before that volume's publication (in 1906), the stars of *AMS 1* are not as current as those in later editions, where normally only a year lapsed between ranking and publication (e.g., Visher, 1947:11). That is most clearly evident in the case of BAE ethnologist Washington Matthews, who was highly ranked in 1903 (in the top 10) but died prior to the publication of *AMS 1*. He was nonetheless identified as a star when Cattell published the *AMS 1* rankings in *AMS 5* (Cattell and Cattell, 1933:1277).

Cattell asked each of the judges to determine the "merit" of their peers, a term which he understood to mean their contributions

to the advancement of science, primarily by research, but teaching, administration, editing, the compilation of text-books, etc. should be considered. The different factors that make a man efficient in advancing science must be roughly balanced. (Cattell, 1906:660; see also Cattell, 1910:539)

Not all of his requests for such rankings were favorably received, either in anthropology or in other fields (Cattell, 1906:661, 1910:540). Some thought the idea impractical, unfair, or inherently flawed.⁶ Many who participated had misgivings about the venture. As Frederick Hodge, who provided rank orders for *AMS 1* and *AMS 2*, noted:

I have made the "appraisal" as nearly as I know how, following your suggestions strictly. My classification of course is not based on an individual's ability, but on productiveness and usefulness.... In other words, if all the individuals should die now—at once—in how far has each added to the sum of anthropologic knowledge through fieldwork, lectures, popular or text books, administration, teaching, or editing.... I have weighed each case the best I could, and have, I believe, a good reason for placing each where it is. Personality has of course been as completely eliminated as one who is very human knows how.⁷

Holmes, on the other hand, was confident he could "arrange the list of anthropologists according to my estimate of the work done by each."⁸ The difference between them speaks more clearly to differences in their personalities than to the ease of making such judgments.

In fact, it was precisely because he was concerned about the “personal equation” in judging that Cattell used 10 judges, so as to mitigate individual effects by using mean rankings. He then ran a series of statistical tests—using as data the psychology judges’ rankings—to measure the “accuracy” (more correctly, the reliability) of the ranking process. He did so by determining how far each judge’s rankings for a specific individual differed from the overall mean ranking for that individual.⁹ Cattell found, generally, that those individual rankings varied about the mean following “the normal distribution of the probability curve” and observed that one could improve the precision of the rankings by weighting each judge’s rankings according to their deviations from the mean (Cattell, 1906:662–663). But he chose not to do so because the calculations would be “somewhat tedious” and the results would not “considerably alter the order” (Cattell, 1910:541).

In Cattell’s opinion these calculations were the first-ever measure of “accuracy or reliability of judgment” (Cattell, 1906:663, 1910:541), and he believed that what was true of the judges in psychology would be true of the judges from all the other disciplines—although he did not examine the ranking data from those other disciplines. In order to see whether he was right in regard to the anthropologists, and to probe the more interesting issue of whether (and in what direction) the rankings of individual judges may have varied from the mean rankings, I ran a series of statistical calculations using the *AMS 1* and *AMS 2* ranking data assembled by Cattell (Tables 2, 3, respectively).

Generally speaking, Cattell was right. Spearman’s rank order correlations were calculated between each judge’s rankings for *AMS 1* and *AMS 2* and between a judge’s rankings and the overall mean rankings. In both editions, each judge’s rankings correlated significantly with the overall mean rankings, although the rankings of some judges (notably Holmes) tended to correlate more closely with the mean rankings than did those of other judges (such as McGee).¹⁰ Those whose rankings tended to correlate least with those of their peers were, in the case of *AMS 1*, Boas, Fletcher, and McGee and, in *AMS 2*, Boas, Aleš Hrdlička, and McGee.

In order to get a more detailed picture of the nature of individual differences, *z* scores were also calculated for each judge’s rankings (this is not an entirely inappropriate statistic to use on ordinal level data because the ranks as viewed across the judges meet the distributional requirements of the test). The *z* scores provide a measure of the significance of the difference between the rank that a judge gave an individual and that individual’s mean rank from all the judges. Thus, they also provide a measure of the degree and the direction to which a judge’s ranking of a certain individual differed from that of the other judges and indicate whether particular individuals prompted significant disagreement among the judges as to their relative merit. I shall refer to these unpublished results in the course of examining the several maps of the stars.

The First Star Map: Anthropology in *AMS 1*

There are few surprises among the anthropology stars of *AMS 1* (Table 4). Included are representatives of each of the subfields of anthropology (roughly in proportion to their abundance in the field generally) and virtually all of the major figures of late nineteenth and early twentieth century anthropology. Each judge received a star,¹¹ although Farrand’s star was questionable (see below).

Just as the Washington anthropologists were the largest bloc of judges in *AMS 1*, they also received more stars than any other institutional group within the discipline. Fully seven of the top 10 (and 10 of the top 20) stars were at the Smithsonian’s BAE and USNM (McGee was at the BAE until his unceremonious departure in 1903, soon after the ranking for *AMS 1* was completed (Hinsley, 1981:253–256)). The *z* scores reveal that the Washington-based judges consistently, and often as a group (with the occasional exception of McGee), ranked their colleagues more highly than they ranked anthropologists elsewhere or than the other judges ranked the Washington anthropologists.

Even so, the individual fast atop the list of stars for *AMS 1* was a non-Washington anthropologist: Boas. Having handily won that position in *AMS 1*, he would maintain that ranking at least through *AMS 3* (the last edition for which ranks are known).

Boas was just 48 years old at the time *AMS 1* appeared, which put him among the younger stars. Nearly half of the anthropologists who received stars were over 60 years of age, and their ages ranged upward, in the case of the venerable Cyrus Thomas, to the age of 81 (Table 5). Only two of the stars in *AMS 1* were under 40: Hrdlička and Alfred Kroeber. That this first group of stars was generally older is not unexpected; they were the established figures in the discipline. Their reputations had been earned and established years—sometimes decades—earlier. But already obvious in *AMS 1* are the signs of what will later become a trend: the stars among the Washington anthropologists were, as a group, slightly older than the stars outside the Washington arena. Through time, and as their numbers diminished, the cohort that remained aged considerably.

Most of the stars in *AMS 1* lacked academic training (Boas and his students, Kroeber and Alexander Chamberlain, were among the exceptions). Typically, they had earned their anthropological credentials in the field, often coming into anthropology from other disciplines (Hinsley, 1976:41–42; Meltzer, 1985). This was a by-product of the nineteenth century tradition of anthropology as natural history and of the very few opportunities then available for specialized training. Advanced degree programs in anthropology did not exist in American universities when most of the stars in *AMS 1* were rising through the ranks. Indeed, over the seven editions of *AMS* in which stars were awarded, only 15 anthropologists earned a star while lacking either a baccalaureate or a doctoral degree (Visher, 1947:366, 369). Of those 15, fully 11 appeared

TABLE 2.—Ranking of anthropologists for *AMS I*; voting was conducted in 1903. Names in parentheses were handwritten on Cattell's tally sheets; the remainder were typed. Several of the names that were on the voting lists do not appear in *AMS I* (Carr, Gordon, Matthews, Mindeleff, Peabody, Phillips, Russell). (Data are from the James McKeen Cattell Papers, box 61, folder 3, Library of Congress, Washington, D.C.)

Voting list ¹	Judges									
	Boas	Chamberlain	Culin	Dorsey	Farrand	Fletcher	Hodge	Holmes	McGee	Mason
Baker, Frank	43	24	11	20	23	11	30	33	13	28
(Bandelier, Adolph)	19	18	6	40	6	—	12	10	—	—
Beauchamp, William	30	—	—	—	—	15	—	55	37	49
Blackmar, Frank	—	—	—	—	—	41	—	47	41	45
Boas, Franz	?	1	1	4	1	3	1	2	2	1
(Bowditch, Charles)	6	—	—	—	—	—	—	—	40	—
(Butler, Amos)	64	—	—	—	—	47	—	63	4	52
(Carr, Lucien)	59	—	—	—	—	—	—	44	—	—
Chamberlain, Alexander	17	?	20	19	20	9	8	23	26	20
Culin, Stewart	22	19	38	13	29	20	18	20	8	23
(Curtin, Jeremiah)	44	—	—	—	—	—	—	59	—	—
(Dall, William)	62	—	—	—	—	—	—	—	—	16
(Dellenbaugh, Frederick)	—	—	—	—	—	—	44	—	—	—
Dixon, Roland	21	30	27	17	21	16	28	26	22	26
Dorsey, George	20	12	2	8	31	14	15	14	7	11
Farrand, Livingston	10	31	26	18	?	30	25	26	20	25
Fewkes, Jesse	9	10	13	6	10	5	11	6	5	9
Fletcher, Alice	8	6	18	7	12	?	14	13	11	10
Fowke, Gerard	—	—	—	—	—	28	—	—	43	47
Gatschet, Albert	25	7	10	10	3	4	7	7	25	6
Giddings, Franklin	13	32	37	41	—	35	13	17	16	44
(Gordon, George)	51	—	—	—	—	—	42	39	—	—
(Grinnell, George)	41	—	—	—	—	—	—	35	—	—
(Hagar, Stansbury)	55	—	—	—	—	46	—	65	35	48
Hewitt, John	31	29	21	30	26	17	37	34	9	22
Hodge, Frederick	34	17	12	11	25	22	?	15	6	16
Holmes, William	2	2	7	2	4	13	2	?	1	2
(Horsford, Cornelia)	63	—	—	—	—	49	—	66	—	51
Hough, Walter	11	33	15	15	33	50	23	18	24	15
Hrdlička, Aleš	26	20	14	23	16	8	16	22	19	31
Jenks, Albert	49	35	39	32	35	44	24	38	21	39
Kroeber, Alfred	14	21	23	16	11	25	21	27	23	37
Lamb, Daniel	32	36	41	21	39	38	46	48	14	42
(Laufer, Berthold)	12	40	28	36	15	—	35	61	—	40
(Lumholtz, Carl)	40	13	29	35	17	31	22	21	—	36
MacCurdy, George	54	37	31	39	38	48	40	54	29	29
Mason, Otis	1	4	4	3	2	—	3	1	3	?
Matthews, Washington ²	7	8	5	12	14	2	10	12	10	5
(McGee, WJ)	4	3	3	5	7	7	5	3	?	4
(McGuire, Joseph)	42	28	36	31	28	51	19	45	36	24
Mercer, Henry	61	—	—	—	—	53	—	41	33	53
(Mills, William)	46	—	—	—	—	—	—	58	—	—
(Mindeleff, Victor)	48	—	—	—	—	—	—	51	—	—
Mooney, James	23	9	9	9	5	12	4	5	4	8
(Moore, Clarence)	24	27	24	29	9	10	17	11	—	17
Moorehead, Warren	60	—	—	—	—	26	34	46	32	43
(Morse, Edward)	38	—	—	—	—	—	—	24	—	—
(Murdoch, John)	50	—	—	—	—	24	—	52	—	34
(Nelson, Edward)	58	—	—	—	—	23	—	49	—	13
(Newell, William)	16	11	33	24	18	32	20	19	—	12
Nuttall, Zelia	27	26	35	25	27	19	31	37	40	14
(Peabody, Charles)	—	—	—	—	—	—	45	—	—	—
Peet, Stephen	39	—	—	—	—	52	—	64	44	55
Pepper, George	53	34	34	37	37	45	41	56	31	38
(Perkins, George)	37	—	—	—	—	42	—	60	—	54
(Phillips, William)	—	—	—	—	—	—	—	57	—	—
Putnam, Frederic	3	5	8	1	8	1	9	4	18	3
Ripley, William	—	—	—	—	—	34	—	—	—	18

TABLE 2.—Continued.

Voting list	Judges									
	Boas	Chamberlain	Culin	Dorsey	Farrand	Fletcher	Hodge	Holmes	McGee	Mason
Russell, Frank	18	25	25	33	30	43	39	30	15	35
Saville, Marshall	33	22	16	26	32	18	27	29	17	21
Smith, Harlan	36	23	30	34	24	27	29	31	27	33
Starr, Frederick	29	14	32	14	34	40	25	28	28	32
Stevenson, Matilde	35	—	—	—	—	29	32	36	38	27
Stevenson, Sara	28	—	—	—	—	37	—	50	39	50
Swanton, John	15	38	40	22	13	39	36	53	30	41
Thomas, Cyrus	56	15	19	38	22	36	6	9	12	7
(Thruston, Gates)	57	—	—	—	—	—	—	62	—	—
(Tooker, William)	52	—	—	—	—	33	—	32	—	46
(Uhle, Max)	5	16	17	28	19	—	38	43	—	—
(Ward, Lester)	47	—	—	—	—	6	—	8	—	19
Willoughby, Charles	45	39	22	27	36	21	43	42	34	30

¹The greater number of votes cast by Boas and Holmes likely reflects their write-in votes and, perhaps, their receiving their voting lists after they had already been expanded by write-in votes of others.

²Washington Matthews was voted a star during the judging for *AMS I* (in 1903) but died in 1905, as *AMS I* was going to press. He died early enough to be dropped from the volume but too late for the judges to take another vote.

in *AMS I*. These were the last of the self-trained amateurs (Hinsley, 1981:264).

In general, there was little disagreement in *AMS I* about who deserved a star, although with the broad criteria for judging merit and Cattell's "personal equation" coming into play, there were telling differences among the rankings. Boas, for example, was top-ranked by many of his peers but not by all of them. Fletcher and George Dorsey put Boas third and fourth, respectively, placing him significantly lower than did the other judges. Yet those lower rankings are not necessarily a reflection of their views of Boas, for Fletcher declared him to be "the best ethnologist we have" (Mark, 1980:74), and Dorsey likewise deeply respected Boas and constantly sought to ingratiate himself in his inner circle (it didn't work; see Boas's ranking of Dorsey (Table 2) and Hinsley, 1981:272). Those views notwithstanding, Fletcher and Dorsey each put Putnam—under whom they had both trained—first on their lists. Filial loyalty obviously took precedence.

Not for all of the judges, however. Boas himself hadn't been trained by Putnam, but Putnam had befriended him soon after he arrived in America and been instrumental in securing desperately needed employment for Boas at critical moments early in his career (Mark, 1980:32, 36–39). Still, Boas—who did not vote for himself—had to rank someone first, but instead of Putnam he selected Otis Mason, against whose ideas he had launched his critique of evolutionary theory and his advocacy of the importance of understanding history, context, and cultures (e.g., Boas, 1887; Stocking, 1974:2–4). Clear-eyed critic that he was, Boas well appreciated the fact that Putnam's contributions were more administrative and institutional than intellectual (Mark, 1980:41, 45–46; Swanton, 1944:9) and that Mason's contributions—although occasionally wrong-headed in his view—were nonetheless the more significant.

There was possibly another factor involved as well: most of the other judges had ranked Holmes second behind Boas. For

that matter, Holmes also put Mason first, and he ranked Boas second. The fact that Mason was top-ranked on Boas and Holmes's lists—and only their two lists—says more about the politics of compromise than about Mason's contributions to anthropology (for by then Mason's once-considerable influence was on the wane (Hinsley, 1981:100)). Although Boas and Holmes recognized each other's considerable contributions, their views and interests were utterly incompatible, and their personal and professional relations were often strained (Meltzer and Dunnell, 1992:xvii–xviii, xxiii–xxiv). It would appear they were unable to give the other the top rank and independently selected Mason as a compromise candidate, ensuring that he finished second in the overall ranking and that Holmes finished third (this is not to suggest Holmes would have come in second had Boas only ranked him more highly; given the low rank (13) Holmes received from Fletcher, it is surprising he did not rank any lower).

Just as Putnam seems to have been highly ranked for his administrative contributions to the field, so too were others, such as McGee and William Newell (permanent secretary and long-time editor of the American Folklore Society). Their presence among the stars reflects the wide latitude in Cattell's definition of what constituted a contribution to the field. McGee's case is particularly illuminating, for his intellectual contributions were remarkably slender and short-lived. He clambered into the top ranks of anthropology by virtue of ambition, position (as de facto director of the BAE during Powell's long-declining last years), organizational abilities, an extraordinary capacity for hard work, and an even greater capacity for self-promotion and "leg pulling"¹² (Hinsley, 1981:238–244, 250; Swanton, 1944:33–34). Yet, his heavy-handed role within the BAE and as self-appointed spokesperson for government science had bred resentment among anthropologists in and outside of the Washington arena (Hinsley, 1981:231–234; Meltzer, 1983),

TABLE 3.—Ranking of anthropologists for *AMS 2*; voting was conducted in 1909. The names in parentheses were handwritten on Cattell's tally sheets, and it is presumed these were the write-in votes. As in *AMS 1*, several of the names that were on the voting lists do not appear in *AMS 2* (Bushnell, Gordon, Grinnell, Lewis, Owen, Thomas). (Data are from the James McKeen Cattell Papers, box 61, folder 4, Library of Congress, Washington, D.C.)

Voting list	Judges									
	Boas	Chamberlain	Dixon	Dorsey	Farrand	Holmes	Hodge	Hrdlička	Kroeber	McGee
Baker, Frank	—	32	—	—	—	—	47	—	—	—
Bandelier, Adolph	15	15	5	23	3	19	19	6	12	27
(Barrett, Samuel)	—	—	32	—	—	40	33	—	37	—
Boas, Franz	?	1	1	1	1	1	1	3	1	1
Bowditch, Charles	34	41	23	24	21	39	40	11	15	8
(Bushnell, David)	—	—	—	—	—	46	39	—	—	—
Chamberlain, Alexander	24	?	14	33	12	16	15	16	24	14
Culin, Stewart	26	13	33	3	22	12	12	28	25	7
Dixon, Roland	9	12	?	6	10	13	14	20	9	9
Dorsey, George	7	11	11	?	19	2	6	12	3	2
Farabec, William	35	25	38	35	40	41	45	38	39	28
Farrand, Livingston	13	20	40	17	?	23	21	30	28	10
Fewkes, Jesse	5	7	4	10	8	6	9	5	11	6
Fletcher, Alice	14	9	17	27	7	3	7	10	8	4
(Fowke, Gerrard)	—	—	—	—	—	35	41	—	—	43
Goddard, Pliny	22	36	19	30	25	22	17	31	18	17
(Gordon, George)	10	—	26	31	41	25	28	24	40	19
(Grinnell, George)	—	26	—	—	—	—	—	—	—	—
(Hewett, Edgar)	27	—	—	—	33	32	27	25	16	24
Hewitt, John	42	19	36	40	32	15	22	32	42	38
Hodge, Frederick	23	8	22	12	6	10	?	9	6	26
Holmes, William	2	2	7	11	2	?	2	1	2	11
Hough, Walter	11	24	13	16	20	17	32	15	26	31
Hrdlička, Aleš	19	22	21	13	18	11	11	?	14	29
Jenks, Albert	31	35	31	38	28	28	16	37	30	20
Kroeber, Alfred	6	10	6	4	5	5	3	13	?	5
Lamb, Daniel	—	39	—	—	—	—	48	—	—	—
Laufer, Berthold	4	16	12	2	15	26	25	34	17	34
(Lewis, A.B.)	40	—	—	29	—	47	—	—	—	40
(Lowie, Robert)	17	—	37	37	38	—	—	41	38	—
Lumholtz, Carl	36	23	25	28	31	38	36	33	27	18
MacCurdy, George	37	31	39	22	27	29	29	26	33	25
McGee, WJ	3	5	28	7	9	9	10	14	21	?
McGuire, Joseph	43	38	34	42	29	43	43	17	29	42
Mooney, James	18	4	16	14	11	4	4	8	13	15
Moore, Clarence	25	34	3	25	16	18	18	4	19	32
(Moorehead, Warren)	—	—	34	—	—	42	42	—	—	—
Nuttall, Zelia	29	40	27	34	30	37	35	35	31	22
(Owen, C.L.)	—	—	—	39	—	—	—	—	—	—
(Peabody, Charles)	—	—	—	—	—	44	44	—	—	—
Pepper, George	39	43	41	41	36	45	46	39	44	30
Putnam, Frederic	1	3	2	5	4	7	5	2	4	3
(Sapir, Edward)	20	37	24	21	23	36	37	40	36	35
Saville, Marshall	21	17	29	18	26	24	24	18	20	12
Smith, Harlan	28	29	10	19	24	30	31	19	32	33
(Speck, Frank)	32	42	—	—	37	33	38	—	35	41
(Spinden, Herbert)	—	—	—	—	—	—	—	—	41	—
Starr, Frederick	30	6	15	20	35	27	26	29	5	23
Stevenson, Matilde	33	33	18	26	34	21	23	23	23	37
Swanton, John	8	14	9	8	13	8	8	21	7	16
Thomas, Cyrus	41	18	8	32	17	20	20	7	22	36
(Tooker, William)	—	28	—	—	—	—	—	—	—	—
(Tozzer, Alfred)	12	27	30	15	39	34	34	36	34	21
Willoughby, Charles	38	30	35	36	42	31	30	27	43	13
(Wissler, Clark)	16	21	20	9	14	14	13	22	10	39

TABLE 4.—Rank orders, degrees, and institutional affiliations of the stars in anthropology in *AMS 1-4AMS 3*, as determined by Cattell. Because of vagaries in Cattell's calculations, these rank orders are slightly different than those that result from ranking the means of the data in Tables 2 and 3. Names in bold above a rank of 20 are new stars for that edition; names in bold below a rank of 20 are gratuitous stars. (Ranking data from unpublished lists; James McKeen Cattell Papers, box 61, folder 1; box 62, folder 1; and box 171, Library of Congress, Washington, D.C. Data on degrees and institutional affiliations from the respective volumes of *AMS*) (AMNH = American Museum of Natural History, BAE=Bureau of American Ethnology, FM=Field Museum, PM=Peabody Museum, USNM=United States National Museum.)

Rank	AMS 1 (1906)			AMS 2 (1910)			AMS 3 (1921)		
	Name	Degree	Institution	Name	Degree	Institution	Name	Degree	Institution
1	Boas, F.	Ph.D.	Columbia U., AMNH	Boas, F.	Ph.D.	Columbia U.	Boas, F.	Ph.D.	Columbia U.
2	Mason, O.	Ph.D.	USNM	Putnam, F.W.	-	Harvard U., PM; U. California	Kroeber, A.L.	Ph.D.	U. California
3	Holmes, W.H.	-	BAE	Holmes, W.H.	-	BAE	Wissler, C.	Ph.D.	AMNH
4	McGee, W.J.	-	St. Louis Exposition	Kroeber, A.L.	Ph.D.	U. California	Laufer, B.	Ph.D.	FM
5	Putnam, F.W.	-	Harvard U., PM	Fewkes, J.	Ph.D.	BAE	Fewkes, J.	Ph.D.	BAE
6	Mooney, J.	-	BAE	Dorsey, G.	Ph.D.	U. Chicago, FM	Holmes, W.H.	-	BAE
7	Fewkes, J.W.	-	BAE	Fletcher, A.	-	PM	Lowie, R.	Ph.D.	AMNH
8	Matthews, W.	-	BAE	Mooney, J.	-	BAE	Dixon, R.	Ph.D.	Harvard U., PM
9	Gatschet, A.	-	BAE	Swanton, J.	Ph.D.	BAE	Swanton, J.	Ph.D.	BAE
10	Fletcher, A.	-	PM	Dixon, R.	Ph.D.	Harvard U., PM	Hrdlicka, A.	M.D.	USNM
11	Dorsey, G.	Ph.D.	U. Chicago, FM	McGee, W.J.	-	USDA; Inland Waterways Comm.	Spinden, H.J.	Ph.D.	AMNH
12	Bandelier, A.	-	Columbia U.	Hodge, F.W.	-	BAE	Hodge, F.W.	-	BAE
13	Hodge, F.W.	-	BAE	Bandelier, A.	-	Columbia U.	Mortley, S.G.	-	Carnegie Institution
14	Chamberlain, A.	Ph.D.	Clark U.	Hrdlicka, A.	M.D.	USNM	Tozzer, A.M.	Ph.D.	PM
15	Moore, C.B.	-	-	Wissler, C.	Ph.D.	AMNH	Nelson, N.C.	-	AMNH
16	Culin, S.	-	Brooklyn Museum	Culin, S.	Ph.D.	Brooklyn Museum	Goddard, P.E.	Ph.D.	PM
17	Hrdlicka, A.	M.D.	USNM	Laufer, B.	Ph.D.	FM	Fletcher, A.	-	PM
18	Newell, W.	-	American Folklore Society	Chamberlain, A.	Ph.D.	Clark U.	Speck, F.	Ph.D.	U. Pennsylvania
19	Kroeber, A.L.	Ph.D.	U. California	Moore, C.B.	-	USNM	Kidder, A.V.	Ph.D.	Andover Foundation
20	Thomas, C.	-	BAE	Hough, W.	Ph.D.	USNM	Mills, W.C.	-	Ohio Historical Society
21	Dixon, R.	Ph.D.	Harvard U., PM	Saville, M.	-	Columbia U.	Mooney, J.	-	BAE
22	Farrand, L.	M.D.	Columbia U.	Starr, F.	Ph.D.	U. Chicago	Moore, C.B.	-	New School for Social Research
23	Baker, F.	Ph.D.	Georgetown U.	Thomas, C.	-	BAE	Goldenweiser, A.	Ph.D.	-
24	Hough, W.	Ph.D.	USNM	Farrand, L.	M.D.	Columbia U.	Parsons, E.C.	Ph.D.	-
25	Saville, M.	-	Columbia U.	Goddard, P.E.	Ph.D.	AMNH	Saville, M.	-	Columbia U.
26	Hewitt, J.N.	-	BAE	Smith, H.I.	-	AMNH	Hough, W.	Ph.D.	USNM
27	Lumholtz, C.	-	-	Bowditch, C.	-	PM	Michelson, T.	Ph.D.	BAE
28	Starr, F.	Ph.D.	U. Chicago	Gordon, G.B.	-	Archaeological Institute of America	Cole, F.-C.	Ph.D.	FM
29	Uhle, M.	Ph.D.	U. California	Hewitt, E.L.	-	BAE	MacCurdy, G.G.	Ph.D.	Yale U.
30	Nuttall, Z.	-	PM	Stevenson, M.C.	-	BAE	Culin, S.	-	Brooklyn Museum
31	Smith, H.I.	-	AMNH	Tozzer, A.M.	Ph.D.	PM	Jenks, A.	Ph.D.	U. Minnesota
32	Giddings, F.	-	Columbia U.	Jenks, A.	Ph.D.	U. Minnesota	Radin, P.	Ph.D.	U. California
33	Swanton, J.	Ph.D.	BAE	Lumholtz, C.	Ph.D.	-	Gordon, G.B.	Sc.D.	U. Museum Pennsylvania
34	Laufer, B.	Ph.D.	Columbia U.	MacCurdy, G.G.	Ph.D.	Yale U.	Harrington, J.P.	-	BAE
35	Willoughby, C.	-	PM	Sapir, E.	Ph.D.	U. Pennsylvania	Hewitt, J.N.	-	BAE
36	McGuire, J.	-	-	Hewitt, J.N.	-	BAE	Sullivan, L.R.	-	AMNH
37	Jenks, A.	Ph.D.	Ethnological Survey, Philippines	Nuttall, Z.	-	PM	Bassett, D.S.	-	-
38	Lamb, D.	M.D.	Army Medical Museum	Willoughby, C.	-	PM	Dorsey, G.	Ph.D.	-
39	MacCurdy, G.G.	Ph.D.	Yale U.	Lowie, R.	Ph.D.	AMNH	Farrand, L.	M.D.	American Red Cross
40	Pepper, G.	-	AMNH	McGuire, J.	-	-	Hooton, E.	Ph.D.	PM
41	-	-	-	Farabee, W.	Ph.D.	Harvard U.	-	-	-
42	-	-	-	Speck, F.	Ph.D.	U. Pennsylvania	-	-	-
43	-	-	-	Pepper, G.	-	U. Museum Pennsylvania	-	-	-

TABLE 5.—*AMS* stars and mean ages of Washington anthropologists compared with non-Washington anthropologists. Mean age is calculated as of the year of the edition. The table does not include gratuitous stars. (See also Visher, 1947:373, who gives tallies in median ages.)

<i>AMS</i> edition	Washington Anthropologists		Non-Washington Anthropologists		All Anthropologists	
	Number	Mean age	Number	Mean age	Number	Mean age
Stars						
1 (1906)	10	57.4	10	51.7	20	54.8
2 (1910)	8	50.6	12	49.7	20	50.1
3 (1921)	5	60.6	15	48.8	20	51.8
4 (1927)	4	64.0	17	51.0	21	53.5
5 (1933)	4	57.2	18	53.6	22	54.3
New stars						
1 (1906)	10	57.4	10	51.7	20	54.8
2 (1910)	2	44.0	3	37.0	5	39.8
3 (1921)	—	—	9	44.2	9	44.2
4 (1927)	—	—	5	51.8	5	51.8
5 (1933)	1	36.0	4	44.5	5	42.8
6 (1938)	—	—	5	42.4	5	42.4
7 (1944)	—	—	5	49.8	5	49.8

and the strong feelings he engendered, coupled with an apparent ambiguity over how to credit his contributions, were visible in his rankings. Despite an overall high rating, the z scores reveal that the majority of the judges thought highly of McGee or cared little for him: five of the nine (McGee did not vote for himself) rated him either significantly above or below his mean rank. Ultimately, he ranked fourth after Boas, Mason, and Holmes. It was a ranking he would not hold.

Putnam followed McGee in the ranking and may have come ahead of him were it not for the fact that McGee lowballed Putnam (one of the worst instances of this in the *AMS I* rankings). They had battled for years over the American Paleolithic (Meltzer, 1983) and McGee—who had strong opinions and viewed the world in stark and dichotomous terms—could scarcely see Putnam beyond those differences.

The remainder of the top 10 was rounded out by several of the Washington anthropologists, who as a group (and this includes all the BAE and USNM anthropologists) sparked among the judges some of the sharpest disagreements in the ranking process. The BAE's Thomas, for example, whose highly acclaimed Mound Survey of the 1880s and 1890s had broken the back of the moundbuilder myths, received a mean rank of 8.5 from his four Smithsonian colleagues but a mean rank of only 31 from the non-Washington judges.

Viewed from another angle, the *AMS I* rankings reveal that a couple of the judges—notably Boas and Fletcher—were consistently out of step with their peers. Boas's differences were most directly expressed in his rankings of the Washington anthropologists, though there is an unevenness to the pattern. Those with whom he had the most pronounced and (often) public differences over theoretical and institutional matters, as well as those who fundamentally opposed his ideas about the future of anthropology—Holmes, Mason, and, in certain particulars, McGee—he nonetheless ranked highly (on their differences, see Stocking, 1960:11–12, 1974:1–8; Hinsley, 1981: 98–100, 223–224). The rest of the Washington anthropologists he low-

balled. Thus, among the 10 who were starred in *AMS I*, Boas ranked seven of them below their mean rank (five of them significantly so; see Table 2 and the comparatively low ranks he gave Albert Gatschet, Hodge, Hrdlička, James Mooney, and Thomas).¹³ The following year he would do as much publicly in his "History of Anthropology," which pointedly ignored virtually the entire Washington group (Boas, 1904). Of course, all this was necessarily colored by the very public dispute Boas had in 1903 with the Smithsonian over the leadership and direction of the BAE following the death of its founder, John Wesley Powell (see "Rising and Falling Stars," below; Hinsley, 1981:250–253, 269–277).

Fletcher had an equally low opinion of some of the other Washington anthropologists. Most noticeably, she had a significantly lower opinion of Holmes and McGee and gave no rank at all to Mason. Fletcher's rankings also tended to support the observation that "the few women consulted put more women on their lists and ranked them higher than did the men in the same fields" (Rossiter, 1982:107). Including Fletcher and Zelia Nuttall, there were five women in the pool of anthropologists being ranked in 1903. The others were Cornelia Horsford, Matilde C. Stevenson, and Sara Yorke Stevenson, whom Cattell listed in *AMS I* by her husband's name, "Mrs. Cornelius Stevenson." In voting, Fletcher consistently gave this group higher ranks than they received from the other judges—with the exception of Mason, who gave higher ranks to Nuttall and Stevenson, and Boas, who did likewise in the case of Sara Stevenson.¹⁴ Fletcher herself was ranked tenth; she was one of only 19 women awarded a star in *AMS I*. That is more than would be expected from a discipline with as few practitioners as anthropology. Women anthropologists (5) constituted just 3.3% of the total number of women scientists in *AMS I* yet netted 5.2% of the stars awarded women in that edition.

Rounding out the *AMS I* stars are two individuals—Farrand and Kroeber—who failed to make the top 20 on at least half the ballots but were nonetheless awarded stars. Their cases are not

alike, however. Farrand, in fact, failed to make the top 20 altogether (he was twenty-second), but he still received a star (over the unstarred but higher-ranked Dixon). His *AMS 1* entry implies his star was for anthropology (and he was so identified in Cattell's unpublished lists), but it was clearly unearned there, and it may partly reflect his status in psychology, where he is cross-listed. Hodge deemed Farrand quite capable but not a legitimate star, since "he has not produced so much thus far."¹⁵ Cattell, however, Farrand's Columbia colleague, nonetheless ensured that he received one. This is the only case I found in which Cattell deliberately manipulated the data to change the outcome of the rankings (a manipulation that, in the absence of publishing the ranking data, he was able to carry out).

In contrast, Kroeber was still in his 20s at the time of the voting for *AMS 1* and had just received his doctorate from Columbia two years earlier. He was at best an unknown quantity to most of the judges, which likely explains why he was not in the top 20 on seven of the ballots. Still, high ranks from Boas, Dorsey, and Farrand were enough to ensure he was starred. By the next edition of the *AMS* there would no longer be ambiguity about his star status, and, indeed, he was asked to serve as a judge. There would continue to be doubts about Farrand's status.

Rising and Falling Stars: Anthropology in *AMS 2*

Because of the relatively brief period separating the first and second editions of the *AMS 1*, there is considerable carry-over in the stars: 15 of those from *AMS 1* made it into the top 20 of *AMS 2* (Table 4). As before, Boas is first, this time with near unanimity among the judges—including Holmes. And as before, Boas grappled with the problem of whom he should rank first—Mason having died in the intervening years. He ultimately selected Putnam, a choice that was defensible enough, even if he didn't really mean it. Boas's vote (the highest Putnam received) was enough to vault Putnam into second place. That put him ahead of Holmes, the perennial also-ran, who was again second on Boas's list and third overall (Holmes may have passed Putnam were it not for low rankings from Dorsey and McGee; see below).

But if the stars of *AMS 2* are a familiar group overall, with a few exceptions their rank orders changed, partly as a necessary consequence of the appearance of new stars in this edition. These were, in rank order, John Swanton, Dixon (now himself a judge), Wissler, Berthold Laufer, and Walter Hough. The number of new stars (five), was restricted by the number of stars of *AMS 1* who had since died (and thereby freed slots under the star cap imposed on anthropology).

Those changes in rank orders were also a result of the aging and altered status of older stars and the changing intellectual fashions within the discipline, all of which made such perturbations the norm rather than the exception. This was particularly the case when rankings were determined at multi-year and sometimes decadal intervals (were these rankings done more

frequently, one would see more constancy in the rankings unless, of course, the scientific community was on the cusp of major change). In the face of this, the fact that Boas' rank remained unchanged over the course of at least three decades is striking testimony to his status within the field.

Given that relative rank is expected to change, it is important to distinguish a significant from an insignificant change in rank. Such a determination can be made statistically using adjusted residuals (Everitt, 1977:47), and that analysis identifies four individuals whose ranks rose from *AMS 1* to *AMS 2* significantly more than expected: Dixon, Kroeber (who had been starred in *AMS 1* and was now fifth overall), Laufer, and Swanton. Wissler should probably be added to this group. He went from no mention in *AMS 1* to a rank of fifteenth in *AMS 2*, although his absence from *AMS 1* makes the actual statistical calculation impossible. Kroeber and Swanton were ethnologists, held doctorates from Columbia, and were students of Boas. Dixon was an archaeologist with a doctoral degree from Harvard (and was a contemporary of Swanton); Laufer, a Sinologist with a doctorate from Leipzig, had not been trained by Boas but during these years worked with him at the AMNH, Columbia University, and on the Jesup Expedition to Siberia (Freed et al., 1988:12–15; Swanton, 1944:22).

What is characteristic of these rising stars, aside from their relatively young age, is that each possessed an advanced degree (raising the total in this edition to 11), all were in a direct or indirect sense "Boasians" (Stocking, 1976:29), and with one exception (Hrdlička), the judges were nearly in agreement as to their merits. There was little question that these were the rising stars of the field. In 1903 Boas was already describing Laufer as "one of the best among the younger anthropologists," ranking him ahead of Kroeber, Swanton, and Dixon.¹⁶ This view evidently was shared by Farrand as well, for in *AMS 1* Boas and Farrand gave significantly higher marks to this group than did any of the other judges (Table 2). By the time the rankings were made for *AMS 2*, the reputations of these four had spread, hence their significant rise in relative rank among almost all the judges except Hrdlička (Table 3).

That Hrdlička ranked these individuals significantly lower than the mean is not altogether surprising, nor is it a coincidence that he significantly underrated Boas's contributions (Table 3 and unpublished z scores). Hrdlička's loyalties were to Holmes and Putnam, both of whom had nurtured his career, and then to his fellow physical anthropologists and archaeologists with shared interests. He thought less of the contributions of ethnologists and linguists, and his relationship with Boas (and with some of Boas's students) was always a cool one (e.g., Stocking, 1976:24–25; Spencer, 1979:633, 693, 702). Hrdlička had his prejudices, and Swanton (1944:36) observed that these "were so much a part of him that he did not realize he had any." All of this is reflected in Hrdlička's rankings.

There is only one individual whose star fell significantly between *AMS 1* and *AMS 2*: WJ McGee. His plunge from the heights of the discipline began in late 1902 with Powell's death

and McGee's subsequent aggressive effort to be named the next director of the BAE (Hinsley, 1981:249–250).¹⁷ For a variety of reasons, McGee was passed over and Holmes was appointed instead, sparking a bitter dispute among anthropologists and scientists. At issue was not administrative succession but the struggle between museum and research-oriented anthropology and the relationship of the federal government to the anthropological community. The latter was spearheaded by Boas, who, disliking Holmes's sympathies toward an object- and museum-oriented anthropology, aided McGee (Boas to Jacoby, 2 Sep 1909, in Stocking, 1974:303–306; Hinsley, 1981:249–253; Meltzer and Dunnell, 1992:xx–xxi).

It was to no avail. A bitter McGee attacked the secretary of the Smithsonian but that only served to provoke a more forceful reprisal. In the summer of 1903 a minor BAE official was charged with embezzlement, and as the ranking administrator from the Powell era, McGee became the prime target of the investigation (Hinsley, 1981:253; Meltzer and Dunnell, 1992:xxi). McGee was finished in Washington, and in the late summer of 1903 resigned from the BAE. As his professional and personal life disintegrated, he moved off to the periphery of the discipline and began to work (quite capably) in the fledgling conservation movement (Stocking, 1960:15; Lacey, 1979; Hinsley, 1981:255). By *AMS 2* he was making few contributions to anthropology, though Boas, who had vigorously promoted and defended McGee in the months after Powell's death (for not entirely altruistic reasons), still ranked him significantly higher than did the other judges.

McGee served as a judge for *AMS 2*, and it is symptomatic of his distance from Washington and the center of the discipline that his rankings for 25 of the 43 names differed significantly from the mean scores of those individuals. No other judge's rankings were so divergent. A comparison of his *AMS 1* and *AMS 2* rankings (using adjusted residuals), indicates that by *AMS 2* his former BAE colleagues received from him significantly lower marks than he had given them previously (compare, for example, his rankings for Hewitt, Hodge, Holmes, Mooney, and Thomas, Tables 2, 3). And, in an utterly transparent reversal, he gave Boas's students and even Putnam significantly higher marks than he had given them in *AMS 1*.

Boas's rankings had the next highest variance among all the judges. This was in keeping with the independence he had established in *AMS 1* and perhaps reflected the fact that "he was not always right nor always just in his estimate of men" (Swanton, 1944:34). But unlike McGee, Boas at least was consistent from one edition to the next. In only one case—that of George Dorsey—did Boas significantly change his ranking. In 1909 he thought much more highly of Dorsey than he had earlier. But it wasn't just Dorsey's obvious efforts to ingratiate himself with Boas that had met with some small success. His status rose in the opinion of most of the other judges as well, likely in response to a steady stream of publications through the first decade of the century.

There were changes visible in *AMS 2* aside from dramatic changes in rank. The number of starred Washington anthropologists slipped from 10 in *AMS 1* to seven in *AMS 2*, with Thomas joining Farrand among the ranks of the gratuitous stars (Tables 4, 6). That said, a few of them rose slightly in the ranks, including Jesse W. Fewkes, whose contributions historians have judged poorly (Hinsley, 1981:281) but which obviously were better thought of by contemporaries. Only one of the nominally Washington group rose significantly in rank, and that was Swanton. Of course, Swanton must be counted among the Boasians, as was clearly the perception of the time: "as a 'Boas man' I shared the opposition to Boas entertained by many, if not most, of the Washington anthropologists at that time" (Swanton, 1944:34).

Swanton excluded, in *AMS 2* the Washington anthropologists as a group once again triggered considerable disagreement among the judges as to the merits of their contributions. By this edition, however, fewer of the Washington group were among the judges (just three this time), and although they continued to vote mostly as a bloc, their influence was proportionately less.

The Washington anthropologists may not have realized their *AMS* stars were falling, but they were acutely aware of the underlying institutional limits they were increasingly facing. The BAE and USNM anthropologists did not and could not perpetuate themselves by training and producing students (Stocking, 1976:9). The best they could do was accept the trained students of others, and the opportunity for that was rare enough because, as Holmes joked, in the museums and research bureaus of the government "few die and none resign" (Holmes to G. Stanley Hall, 25 Apr 1905, in Noelke, 1974:313). Their inability to add to their numbers was, however, only a symptom. The deeper cause was their lack of ties to the expanding university system, the effects of which would become much more visible with the rankings of the stars in *AMS 3*.

The Stars after a Decade of Change: Anthropology in *AMS 3*

Only six years separated the voting for *AMS 1* and *AMS 2*, but the subsequent intervention of World War I and other delays caused *AMS 3* to appear in 1921, over 10 years after *AMS 2* (the rankings for *AMS 3* were apparently submitted sometime in 1920). In that time, there had been some substantial changes within the discipline (indeed, within all the sciences). The modern graduate school—the means of professional self-consciousness, legitimization, and inculcation—had appeared (Weibe, 1967:121). In the first decades of the twentieth century, many of the major university programs in anthropology were established and began producing trained anthropologists and archaeologists (de Laguna, 1960:102–103; Darnell, 1969; Stocking, 1976:11). The university was rapidly becoming the central institutional venue of American anthropology (as it was for many other fields), and as Boas saw particularly clearly, that was where the future of the discipline lay, not in museums (Weibe, 1967:121; Stocking, 1974:284). He anticipated—as,

TABLE 6.—Rising and falling stars in anthropology by rank order in *AMS 1* to *AMS 3*. Names in bold are individuals whose ranks significantly rose (+) or fell (-). No rank = individual was present in a prior edition of *AMS* but was unranked. No entry = individual was absent from prior editions of *AMS*. (Data are from Cattell ratings in Table 4 and sources therein).

Rank	<i>AMS 1</i> (1906)	<i>AMS 2</i> (1910)	Rise or fall	<i>AMS 3</i> (1921)	Rise or fall
1	Boas, F.	Boas, F.	0	Boas, F.	0
2	Mason, O.	Putnam, F.W.	+3	Kroeber, A.L.	+2
3	Holmes, W.H.	Holmes, W.H.	0	Wissler, C.	+12
4	McGee, WJ	Kroeber, A.L.	+15	Laufer, B.	+13
5	Putnam, F.W.	Fewkes, J.	+2	Fewkes, J.	0
6	Mooney, J.	Dorsey, G.	+5	Holmes, W.H.	-3
7	Fewkes, J.W.	Fletcher, A.	+3	Lowie, R.	+32
8	Matthews, W.	Mooney, J.	-2	Dixon, R.	+2
9	Gatschet, A.	Swanton, J.	+24	Swanton, J.	0
10	Fletcher, A.	Dixon, R.	+11	Hrdlička, A.	+4
11	Dorsey, G.	McGee, WJ	-7	Spinden, H.J.	no rank
12	Bandelier, A.	Hodge, F.W.	+1	Hodge, F.W.	0
13	Hodge, F.W.	Bandelier, A.	-1	Morley, S.G.	no entry
14	Chamberlain, A.	Hrdlička, A.	+3	Tozzer, A.M.	+17
15	Moore, C.B.	Wissler, C.	no rank	Nelson, N.C.	no entry
16	Culin, S.	Culin, S.	0	Goddard, P.E.	+9
17	Hrdlička, A.	Laufer, B.	+17	Fletcher, A.	-10
18	Newell, W.	Chamberlain, A.	-4	Speck, F.	+24
19	Kroeber, A.L.	Moore, C.B.	-4	Kidder, A.V.	no entry
20	Thomas, C.	Hough, W.	+4	Mills, W.C.	no rank
21	Dixon, R.	Saville, M.	+4	Mooney, J.	-13
22	Farrand, L.	Starr, F.	+6	Moore, C.B.	-3
23	Baker, F.	Thomas, C.	-3	Goldenweiser, A.	no entry
24	Hough, W.	Farrand, L.	-2	Parson, E.C.	no entry
25	Saville, M.	Goddard, P.E.	no rank	Saville, M.	-4
26	Hewitt, J.N.	Smith, H.I.	+5	Hough, W.	-6
27	Lumholtz, C.	Bowditch, C.	no rank	Michelson, T.	no entry
28	Starr, F.	Gordon, G.B.	no rank	Cole, F.-C.	no rank
29	Uhle, M.	Hewett, E.L.	no entry	MacCurdy, G.G.	+5
30	Nuttall, Z.	Stevenson, M.C.	no rank	Culin, S.	-14
31	Smith, H.I.	Tozzer, A.M.	no entry	Jenks, A.	+1
32	Giddings, F.	Jenks, A.	+5	Radin, P.	no entry
33	Swanton, J.	Lumholtz, C.	-6	Gordon, G.B.	-5
34	Laufer, B.	MacCurdy, G.G.	+5	Harrington, J.P.	no entry
35	Willoughby, C.	Sapir, E.	no entry	Hewitt, J.N.	+1
36	McGuire, J.	Hewitt, J.N.	-10	Sullivan, L.R.	no entry
37	Jenks, A.	Nuttall, Z.	-7	Bassett, D.S.	no entry
38	Lamb, D.	Willoughby, C.	-3	Dorsey, G.	-32
39	MacCurdy, G.G.	Lowie, R.	no entry	Farrand, L.	-15
40	Pepper, G.	McGuire, J.	-4	Hooton, E.	no entry
41	-	Farabee, W.	no rank	-	-
42	-	Speck, F.	no entry	-	-
43	-	Pepper, G.	-3	-	-

presumably, did those in Washington—that such a program would concentrate in his own hands a considerable amount of the anthropological work done in the country, and he was right. He sought to link his future to the universities, disassociate them from museums, and establish a well-organized machine for specialized academic training in the field. Without such a foundation, he felt that “we can never hope to thoroughly investigate and explore all the numerous problems of American anthropology” (Boas to Nuttall, 16 May 1901, in Stocking, 1974:286–287).

By the second decade of the twentieth century, questions about the academic format of training in anthropology had

reached a critical mass, and Boas organized a meeting at Columbia University to plan the “objects and methods” of teaching anthropology. Perhaps out of courtesy more than sincerity (given that they did not teach anthropology), representatives from the BAE and USNM were invited to participate. None, however, was subsequently appointed to the permanent committee on the subject, symptomatic of the fact that they made little effort to seek links with the university programs (Boas, 1919, MacCurdy, 1919; see also Darnell, 1969; Meltzer, 1985:256). None of that particularly mattered to Boas, who did not think museum-oriented anthropology was the way the field was or ought to be headed. Academic anthropology re-

quired “men of quite different training and bent of mind” than those in museums.¹⁸

As a result of all this, the intellectual character of university training was indelibly shaped by Boas, who was able to train a generation of students committed to his general anthropological orientation. But beyond the sheer force of his own ambition and drive, changing demographics, expanding employment opportunities within the university for trained professionals, and a “sharp decline” in the proportion of anthropologists employed “in government work” or private research institutions (Visher, 1947:79, 481–482) enabled Boas and his students by the early to mid-1920s to head every major department of anthropology in American universities, occupy most of the offices of the American Anthropological Association, and dominate the intellectual landscape (Stocking, 1960:13, 1968:296, 1976:9; Hinsley, 1981:273).

Whether those changes were felt among the judges of *AMS 3* is unclear, since their identities remain unknown. It is evident, however, that because of these changes there was considerably less carry-over from *AMS 2* to *AMS 3* than there had been between *AMS 1* and *AMS 2* (Tables 4, 6). Only 11 of the top 20 stars in *AMS 2* maintained their place in that group in *AMS 3*. The loss of the other nine was not due solely to death (which accounted for all the attrition between *AMS 1* and *AMS 2*) but also to five stars falling out of the top 20 ranking.

Continuing their downward spiral were the Washington anthropologists, of whom only five, one of them Swanton, now had legitimate stars. Indeed, of all the Washington anthropologists who had been in *AMS 2*, only four managed to maintain or improve their rank in *AMS 3*, while the remaining seven dropped in rank or died. Those who survived weren't getting any younger: the average age of the starred Washington anthropologists, of whom Swanton was still the youngest, increased by 10 years from *AMS 2*; their non-Washington counterparts were, on average, nearly 12 years younger (Table 5). Most telling of all, however, is that there were no new Washington stars in *AMS 3* among the nine added in that edition.

The new stars that were added (Table 4) were Boas's students—Goddard, Robert Lowie, and Frank Speck—along with several Harvard and (partly) Boas trained and influenced archaeologists: Alfred V. Kidder, Sylvanus G. Morley, Herbert Spinden, and Alfred M. Tozzer (Stocking, 1976:29). Of that group, the rise in the ranks of Lowie, Speck, and Tozzer proved to be significantly rapid (Lowie especially so: his rank went from 39 in *AMS 2* to seven in *AMS 3*) (Tables 4, 6). With the exception of Morley, all possessed doctorates. With their inclusion among the stars, for the first time in the several editions of the *AMS* a significant majority (14) of the top 20 stars possessed advanced degrees.

The other rising stars of this edition were two of Boas's colleagues: Laufer, whose meteoric rise in the rankings of *AMS 2* went unchecked in *AMS 3*, and Wissler (Table 4). Wissler held a doctorate from Columbia (1901) and “should have been one of the Boas circle, but was not” (Freed and Freed, 1983:802).

Instead, his degree was in psychology, under Cattell, but for a number of reasons he wandered into anthropology and never left. Since it has been reported that Wissler's “students and colleagues either ignored his influence or took it for granted” (Freed and Freed, 1983:807), it is noteworthy that his rank rose significantly between *AMS 2* and *AMS 3* to where in *AMS 3* he ranked third behind Boas and Kroeber. Not coincidentally, between 1910 and 1920 Wissler published a suite of substantial and well-regarded monographs and papers on the Blackfoot, as well as his classic *The American Indian* (Wissler, 1917), with its widely hailed articulation of the culture-area concept and its effort to elevate Boasian historical ethnology to a more general level (Kroeber, 1918; see also Stocking, 1976:14; Freed and Freed, 1983:813–814).

One student of Boas's, Edward Sapir, was noticeably absent from *AMS 3*. By 1920 Sapir had already burst on the intellectual scene (Darnell, 1990), perhaps even more brilliantly than Lowie or Speck. That he did not receive any notice in the balloting that year may merely reflect a technicality. Stars were only awarded to residents of the United States (Cattell and Cattell, 1933:1261), and Sapir was then living in Canada. He would be the highest ranked of the new stars in *AMS 4*, when he again returned to the United States.

The strong emergence of Boas's students and colleagues, and Boas's own top ranking among the stars, takes on considerable significance in the light of a set of events that occurred in the years leading up to the *AMS 3* rankings. Over that time there had been increasing tension between the Washington and New York groups. Tempers flared over the control of the *American Anthropologist* (which in 1914 was transferred from Washington to New York and put in the editorial hands of a Boasian: Goddard); the composition of the National Research Council Committee on Anthropology, formed during World War I to mobilize American anthropologists as part of a program for national preparedness; and the nominations of Kroeber and Hrdlička to the National Academy of Sciences (Stocking, 1968:284–292). Matters exploded with the publication by Boas, in late 1919, of a letter to *The Nation* denouncing unnamed anthropologists whom he believed had worked as spies (Stocking, 1968: Chapter 11, 1976:1–2; Spencer, 1979; Meltzer and Dunnell, 1992:xxiv).

The accumulated resentment of the Washington anthropologists toward Boas and his students' increasing control of the field exploded in a rage of patriotic indignation. In December 1919, at the American Anthropological Association meetings in Cambridge, Boas was publicly censured, stripped of his membership in the Association's governing council, threatened with expulsion from the very organization he had helped found, pressured into resigning from the National Research Council, then denied even the courtesy of a public explanation—ostensibly because to do so would have allowed the identification of and thus endangered the spies (Stocking, 1968:292–293; 1976:1–2).

Holmes was delighted with the outcome, though not with those of his colleagues who refused to support the censure. As he wrote to one of them:

I have your recent favor and am surprised that you should wish the continuance of the Prussian regime, the vicious, scheming, minority of the association has ruled long enough, and if it is to continue I shall close my connection with anthropology for good. (Holmes to Hodge, Dec 1919, in Sturtevant, 1975:6)

The heady drama at Cambridge represented the swan song of nineteenth century anthropology: a desperate effort on the part of the Washington anthropologists to assert their political dominance over the profession. But that dominance was then on the wane both politically and theoretically, as the change in the rankings from *AMS 2* to *AMS 3* attest.¹⁹ The fact that they chose to flex their political muscles is indication that they hadn't any intellectual muscle to flex, and the shrillness of their political stand at Cambridge could hardly disguise the ultimate weakness of their position within the profession.

Just how weak their position was is evident in who was denied a star in this edition of *AMS*, for the number of new stars added in *AMS 3* would have been much higher had there not been an overload of gratuitous stars monopolizing the allotted slots. Six of the 26 stars awarded in this edition went to individuals whose stars had fallen "below the horizon," as Cattell put it, though he continued to show them as starred. The gratuitous stars in *AMS 3* included Stewart Culin, Dorsey, Farrand, Hough, Mooney, and Clarence Moore (of that group, the rankings of all but Hough and Moore had plunged significantly since *AMS 2*).

What makes this group noteworthy is that it includes none of the Boas students, and in fact the group's presence effectively blocked many of his students from obtaining stars. Had these gratuitous stars been dropped, the new stars for *AMS 3* would have included Alexander Goldenweiser, Elsie Clews Parsons, Marshall Saville, Truman Michelson, Fay-Cooper Cole, and George Grant MacCurdy (Table 4). With the exception of MacCurdy, all had connections to Boas and Columbia. All of them would have to wait for a subsequent edition to be starred (Saville had just missed in *AMS 2* as well; instead of receiving a star at age 43, he received one at age 60).

A few of those who missed the list in *AMS 3* were angry about it, for the stars by then conveyed considerable status and were highly coveted (Visher, 1947:4-7; Rossiter, 1982:289-290). An incensed MacCurdy complained to Hrdlička, "Don't you think that a list [of names] which includes Culin, Dorsey, Kidder, and Nelson should also include mine?"²⁰ Hrdlička duly wrote to Cattell to champion MacCurdy's cause.²¹ Goldenweiser didn't bother sending his complaint through a third party but snarled directly at Cattell about the injustice of including those such as Dorsey and Farrand, who had long since ceased to contribute to the field, or (worse in his eyes) Culin and Hough, who "cannot be taken seriously as anthropologists."²² Their pleas went unanswered, at least until the next edition of *AMS*.

Later Rankings and Final Thoughts

The trends set in motion in the earlier editions continued through several later editions, although except for the ranks of the new stars (Table 7), data is limited from those later editions on judging or on overall rankings. By then, death had removed the gratuitous stars Cattell would not, and five new stars were added in each of the three subsequent editions (*AMS 4* through *AMS 6*). Several of them were holdovers from earlier rankings: MacCurdy, Parsons, and Saville each earned a star in *AMS 4* (after coming close in *AMS 3*), and Cole was starred in *AMS 5* (12 years after coming close in *AMS 3*). With the exception of some of those holdovers (e.g., MacCurdy and Saville), all of them were relatively young and mostly were Columbia-trained ethnologists.

Parsons represented the first additional woman among the stars of anthropology since *AMS 1*, when Fletcher received a star. Fletcher had maintained her presence among the top 20 (and her star) through *AMS 3*, but by then she had begun to slip significantly in the rankings (Table 4). Her death, in 1923, removed her from the list of stars in *AMS 4*, although Parson's star in that edition meant there was no net loss in the number of women stars in anthropology. But there was no net gain, either. Only with Ruth Benedict's award in *AMS 5* would there be an additional star among women anthropologists. Parsons and Benedict would remain the only women stars in anthropology through *AMS 7* (the last edition in which stars were awarded). Even so, women were starred in anthropology at a relatively greater percentage than in all the other fields of science (Rossiter, 1982:291). Although there is ample evidence to indicate women were "understarred," especially in the early editions of *AMS* (Rossiter, 1982:106-109, 289-294), their chances of receiving a star in anthropology were at least better than they were elsewhere in the sciences.

There would be no new stars among the Washington anthropologists in *AMS 4* and only one in *AMS 5*, that awarded to Frank Roberts, who held a doctoral degree from Harvard in archaeology. Although an employee of the BAE, he was not in any sense a product of the intellectual tradition that had dominated Washington anthropology at the turn of the century, for by then the principals of that tradition had nearly all died. And with that, the changes in the intellectual and institutional landscape of American anthropology were complete.

TABLE 7.—Rank orders of the newly starred anthropologists for *AMS 4* through *AMS 6*. Dates are the year voting was done for that edition. (Data are from the James McKeen Cattell Papers, box 63, Library of Congress, Washington, D.C.).

Rank	<i>AMS 4</i> (1926)	<i>AMS 5</i> (1932)	<i>AMS 6</i> (1937)
1	Sapir, E.	Spier, L.	Strong, W.D.
2	Hooton, E.	Roberts, F.	Valliant, G.
3	MacCurdy, G.	Benedict, R.	Herskovitz, M.
4	Parsons, E.	Cole, F.-C.	Schultz, A.
5	Saville, M.	Linton, R.	Lothrop, S.

Charting the stars of the *AMS* shows the progressive deterioration of the once-dominant Washington anthropologists and their inability to maintain their preeminence and rank in a rapidly changing field. It also shows the steady rise of Boas and his students. By the end of the second decade of the twentieth century, the net result of the expanding university programs, the ascendancy of Boas's vision for anthropology as the intellectual underpinning of those university programs, and the shift of power from Washington to New York produced a nearly complete reversal of the star pattern from what it had been in *AMS 1*. Then, seven of the top 10 and 10 of the top 20 anthropologists had been centered in government museums and research bureaus. By *AMS 3*, five of the top 10 and over 10 of the top 20 stars were relatively young, highly educated anthropologists who for the most part had academic appointments and affiliations with Boas himself.

The starring procedure of *AMS* was, as Cattell insisted, a generally reliable device for measuring status, but it never was a valid one. Quirks in his procedures and questions about the representativeness of his samples ensured that it was not—and ultimately ensured the demise of the star system (Rossiter, 1982:290). But as a blunt instrument for measuring what select judges thought of their peers, for mapping the field of competence, and for tracking changes in status and rank among the practitioners of the field, the *AMS* rankings provide a unique window into the workings of early twentieth century American anthropology and anthropologists.

Notes

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It gives me great pleasure to acknowledge Bill Sturtevant's long-standing interest and support of my research efforts in the history of archaeology, some of which I've much enjoyed doing in collaboration with him. Knowing Bill's fondness for the little ironies of history, I close with the observation that he himself is a scion of Cattell's star system: his father, geneticist Alfred Sturtevant, first appeared in *AMS 3* and was accompanied by a star—precociously so, for he was then only 30.

In the notes, frequently cited archival materials are identified by the following acronyms:

AH/NAA	Aleš Hrdlička Papers, National Anthropological Archives, National Museum of Natural History, Smithsonian Institution, Washington, D.C.
FB/APS	Franz Boas Papers, American Philosophical Society, Philadelphia, Pennsylvania.
JMC/LC	James McKeen Cattell Papers, Library of Congress, Washington, D.C.
WHHRR/NMAA	William Henry Holmes, <i>Random Records</i> , National Museum of American Art, Washington, D.C.

1. Today, nearly a century and 20 editions later, Cattell's *American Men of Science* survives as the more inclusively named *American Men and Women of Science*, having also spawned specialized directories of physical and social scientists.

2. The archival material that serves as the basis for the subsequent discussion is housed in boxes 61–63 and 169–171 of the James McKeen Cattell Papers, Library of Congress, Washington, D.C. Few letters between Cattell and the *AMS* judges regarding their voting survive in the Cattell papers; however, I have found letters pertaining to the *AMS* rankings among the papers of the judges themselves.

3. For example, Cattell to Holmes, 28 Feb 1903; McGee to Cattell, 18 and 30 Nov 1909, JMC/LC.

4. For example, Mason to Cattell, 11 Mar 1903, JMC/LC.

5. More specifically, despite the variation in the numbers of individuals being ranked by particular judges, when Cattell tallied the votes he did not standardize all the judges' rankings to a constant numerical scale, which effectively raised or lowered an individual's mean score, depending on how they were ranked by a particular judge and by what scale. Also complicating matters was that Cattell made no distinction by the number of judges ranking an individual. Thus, the mean score of a person ranked by, say, three judges, was given equal weight to the mean score of a person ranked by all 10 judges. One could rectify these biases by transforming all of the ranking data to a constant scale and differentially weighting the number of rankings received, but doing so produces a far different rank-order list than Cattell's—even to the point of producing a different set of stars. Given that Cattell's lists were, ultimately, the ones that mattered and the ones that were used in those decades, I have also used them—at the risk of losing a certain degree of statistical integrity. It was also necessary, before analysis, to clean Cattell's data, which mostly involved rectifying his errors in transcribing a judges' rankings on the master tally sheet. The details of this data-cleaning are not of great significance and are available on request.

6. Delabarre to Cattell, 23 Oct 1909; Woodbury to Cattell, 5 Feb 1932, JMC/LC.

7. Hodge to Cattell, 15 Nov 1903; see also Mason to Cattell, 11 Mar 1903, JMC/LC.

8. Holmes to Cattell, 3 Mar 1903, JMC/LC.

9. More precisely, he classed the 50 psychologists into five groups of 10 each and then determined the mean rank each judge gave that group of 10. He likely proceeded in this manner to reduce the number of required computations. In effect, he calculated a rudimentary z score, although because he merely summed the deviation from the mean and took no account of the variance, the figures he provided (e.g., Cattell, 1910: table IV) cannot be read as standardized normal deviates.

10. Curiously enough, Holmes and McGee were the outliers in this analysis for both *AMS 1* and *AMS 2*. In each case theirs were the rankings most and least correlated (respectively) with the overall mean rankings.

11. As a psychological exercise, Cattell deliberately said nothing about whether a judge should vote for himself or herself (Cattell, 1906:663, 1910:542), and most did not. The rankings of those who did—Culin and Dorsey—say a great deal about their respective perceptions of self. Culin was obviously either very modest or very insecure. Dorsey was neither (see Table 2).

12. Branner to White, 2 Jan 1903, WHHRR/NMAA.

13. It is odd—to me at least—that given the lasting quality of Mooney's work (Hinsley, 1981:210, 219), his deep interest in history, his divergence from the BAE's party line, his deep sense of political reform, and his politics (Swanton, 1944:43–44) Boas would have ranked him so low.

14. A corresponding observation of Rossiter's (1982:107), that men who included women in their rankings tended to cluster them together at or near the bottom of their lists, is more difficult to test, but it seems at least partly true, at least as measured by the overall results of the ranking. Among the 71 individuals who appeared on the judges' ballots for anthropology (and were thus potentially eligible for stars), nearly 30% of the men (19/66) were selected as stars, versus just 20% of the women (1/5). Further testing of this issue will require a more detailed look at voting patterns among the individual judges (as presumably not all acted alike in this regard, as Boas and Mason's rankings suggest).

15. Hodge to Cattell, 15 Nov 1903, JMC/LC.

16. Boas to Walcott, 7 Dec 1903, FB/APS.

17. See also the FB/APS, Dec 1902.

18. Boas to Carl Schurz, 12 Aug 1903, FB/APS.

19. Although the anti-Boas faction won the battle at Cambridge, they lost the war. Goddard (a Boasian and star since *AMS 3*) still controlled the *American*

Anthropologist for another year, and through some shrewd maneuvering Boasians retained at least indirect control of it when Swanton was appointed editor in 1920. Within a few years they resumed their overwhelming influence on the direction of American anthropology (Stocking, 1976:10).

20. MacCurdy to Hrdlička, 5 Nov 1926, AH/NAA.
21. Hrdlička to Cattell, 9 Nov 1926; Hrdlička to MacCurdy, 10 Nov 1926, AH/NAA.
22. Goldenweiser to Cattell, 7 Jan 1920, JMC/LC.

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V. Collections in Anthropological Research

At the Cutting Edge: Patchwork and the Process of Artistic Innovation

Sally Price

In the context of Westerners' interest in the arts of the Suriname Maroons, textiles have received relatively little attention, and as an art form marketable outside the home territory of Maroons, they are nearly nonexistent. But in the context of the culture they are made for, these same textiles have played an important aesthetic and social role since the late nineteenth century. During the 1960s and 1970s, Maroon women and men helped me piece together the history of their textile arts, and later I was able to complement the information and insights they provided by working in museums, libraries, and archives elsewhere—principally in Suriname and Holland, but also in France, Germany, and the United States. That research forms the core of the present paper.

Flow Charts and the Steps of Production

In 1967 the Proceedings of the American Ethnological Society included an analysis of Seminole men's clothing by Bill Sturtevant that begins with a plea for treating costume as a bona fide domain of art (Sturtevant, 1967). The text of that article, and even more strikingly the figures, reflect attention not only to visual detail but also to two diachronic dimensions of clothing fashion. First is the process by which, in a matter of hours or days, raw materials pass through the steps of production. Second is the more gradual process, strung out over years and decades, by which old styles are replaced by new ones. The article's conclusion proposes that the most privileged indicator of chronological changes in Seminole men's and women's clothing is to be found in patchwork.

When Bill became my dissertation adviser, he shared with me a paper he had presented orally in 1969 that further explored the potential of patchwork textiles for elucidating Seminole artistic techniques and aesthetic principles (Sturtevant, 1969). That was in the mid-1970s, when even our children were not yet playing with computers, and the text was rendered in that now-obsolete form known as handwriting. The heart of the argument appeared as a flow-chart, which Bill and his research assistant Donna Kathleen Abbas had constructed on the basis of his field notes and a large collection of Seminole patchwork (Figure 1). As they worked, they tried out their ideas on a sewing machine, limiting the variables being tested

by using black and white pieces of cloth rather than colors. Flow charts (which in this case grew out of structural linguistics via ethnographic semantics) were in a sense the early ancestors of voice-mail menus or Microsoft Windows drop-down lists. They provided an ordered sequence in which an initial decision opens the next level of options, which lead to a third level, and so on. (Anyone old enough to have accessed data by jabbing long metal skewers into the perforations of file cards will also remember this as the era of "edge sort cards" or "punch card sorting.") In the case of Seminole patchwork, Bill argued, the seamstress's decision to rip a single stripe (for example) annulled the need for decisions about insertions and joinings that would have followed upon a decision to rip multiple stripes. The next step would be cutting, which led to a further sequence of choices about slashes, inversions, and arrangements before any actual sewing was done. The whole chain of decisions could then be reinitiated (something along the lines of a *da capo* form in music), treating the composite product of the first run-through as a single element. Although Bill's primordial drop-down list took the form of pen-on-paper, the underlying principles of sequential, embedded decision-making that people like Bill Gates began marketing some years later were already solidly in place.

My own subsequent investigation of African-American patchwork took off, in an important sense, from ideas like that flow chart. There was, for example, the suggestion of using a Polaroid camera to document the patchwork layouts that women tried out and then interviewing the women about why they rejected the ones that never got to the stage of being sewn up. In a similar way, the unpublished manuscripts, scribbled bibliography slips, and one-line research suggestions that Bill fed into my project from time to time tended to be esoteric references to highly particularistic ethnographic materials, but when they were all put together, they constituted a powerful argument for the importance, in studying art and material culture, of paying close attention to the production process—the ordered sequence of options that a given cultural setting allows for a given artistic form and the culturally conditioned influences that shape decision-making at each stage. The idea was to back up from an analysis of finished forms in order to uncover the logic of the steps by which those forms are created. And this would have the effect, in turn, of shedding light on the mechanics of creative innovation and hence the longer-term phenomenon of stylistic change.

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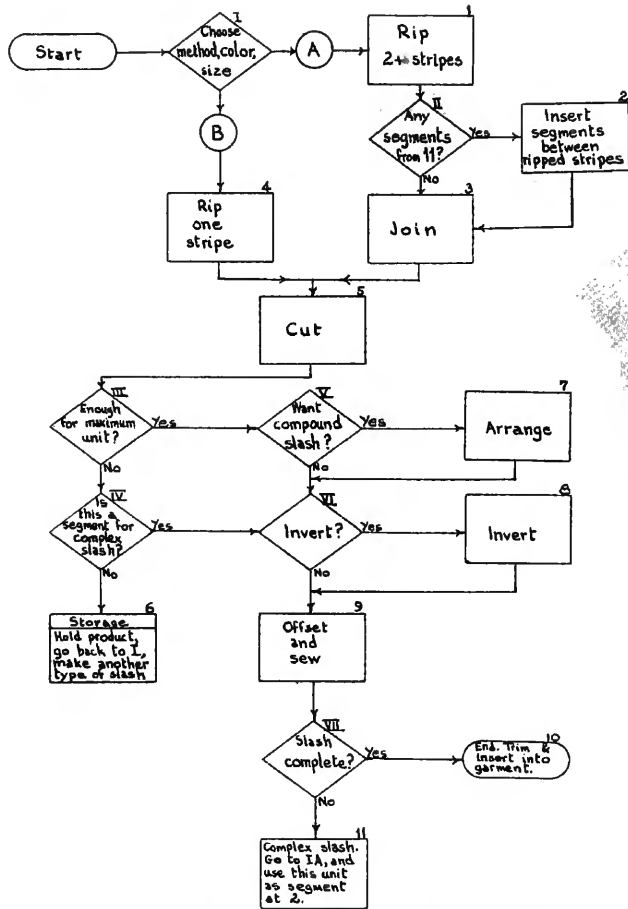


FIGURE 1.—Flow chart constructed by Donna Kathleen Abbas and William C. Sturtevant for Sturtevant, 1969.

In this paper, which focuses primarily on the colorful patchwork clothing made by Saramaka Maroons in the Suriname rain forest, I try to show how attention to the production process can help us understand the nature of artistic change. The essence of my argument is that in Saramaka, individual textiles are composed from the center out, that experimentation with new ideas or techniques generally happens only at the end of this process, and that stylistic change consists of a gradual migration of those new ideas from compositional edges to compositional centers. We can also see this principle operating, with slight variation, in the history of Maroon calabash carving, which is the second main form of artistic expression for women. If we follow styles as they come and go through time, we see innovations cropping up in margins, borders, leftover spaces, and even what we tend to refer to as the “wrong side” of decorated objects, and moving center stage only over the years, as they become mainstream, standard fare. (It is as if, in Western art history, new movements got started not in the paintings themselves but in the frames around them, not in the sculptural

forms but in the pedestals that held them.) But before getting into the main argument, it is necessary to clear up some misconceptions about the origins of Maroon patchwork and to look briefly at the social environment in which it is produced.

The Origin of Maroon Textile Arts

The earliest mentions of Suriname Maroon clothing and textiles are frustratingly sparse in their descriptive detail. Missionaries who lived for many years among the Saramaka during the second half of the eighteenth century, for example, wrote that their hosts had “no clothing except a small covering over the abdomen” (Staehelin, 1913–1919, 3.2:141) and the drawings made by one of them corroborate this description (see Price, 1990:161). John Gabriel Stedman (1988:390–392, 405, frontispiece, pl. 53) described the clothing and accessories of two Maroons he encountered in eastern Suriname in the 1770s, but he neither mentioned nor illustrated any kind of patchwork or decorative sewing. Although other historical accounts go into some detail about costume, including both ritual accessories and coastal imports such as shirts and trousers (see Price, 1984:125–129), the only suggestion that Maroon patchwork or decorative sewing may have existed in any form before the late nineteenth century comes not from Suriname but from Cayenne, French Guiana. There, a recaptured 15-year-old runaway slave declared under interrogation in 1748 that in the enclave where he had been living over an 18-month period, three of the men “make cloths from cotton, which provide *tanga* for the women and loincloths for the men, and that this cotton material is woven piece by piece which they assemble and which is marked with Siamese cotton” (Price, 1973:317’).

The art historian Robert Farris Thompson has made a valiant effort to unearth historical evidence showing that Suriname Maroons had developed decorative patchwork art before the late nineteenth century. “There must have been,” Thompson (1983:215) argued after citing the 1748 testimony from Cayenne, “similar memories of West African multi-strip cloth among black runaways of neighboring Suriname, for in 1823 Ferdinand Denis describes and illustrates an article of dress, given as Carib.” Thompson reproduced Denis’s illustration and went on to reason that the garment, by virtue of its “two patterned narrow strips...separated by a single band of continuously unpatterned cloth,” embodies an aesthetic that “points back to early Asante cloths of the nineteenth century, when weavers were working under Mande influences radiating from Kong and from Bonduku, northwest of the Akan and north of Cap Lahou, whence sailed to Suriname 50 percent of a sample of Dutch slaving ships” (Thompson, 1983:215).

Although the object in Denis’s sketch is captioned as an Indian loincloth (“*Camiza Indien*”), Thompson dismissed this attribution on the grounds that Suriname Indians “in general make and prefer solid red loincloths” (no reference given), and that therefore the garment must have been made by Ndyuka Maroons, who must then have sold it to the Indians from whom

it was collected. In any case, he asserted (with no reference given), “the word *camiza* is not Amerindian” (Thompson, 1983:296). In fact, however, this word *is* (and was even in the eighteenth century) Amerindian, and the garment it referred to was not necessarily red, as any number of sources make clear.² Stedman (1988:306), writing of the 1770s, noted: “The only dress Wore by these Indians consists in a Strip of black, or blew Cotton, worn by the Men to cover their Nakedness, and cal’d *Camisa* [while the equivalent for women is] a Girdle made of human Hair around their Waste, through which before, and behind, they fasten a Square broad Piece of black Cotton.” Benoit (1839:42) described Suriname Indian loincloths in the 1830s as being “red or blue,” and the venerable *Encyclopædie van Nederlandsch West-Indië* reported that “the Caribs of both sexes wear a loincloth, or *kamiesa* (Sp. *camisa*) of dark blue cotton” (Benjamins and Snelleman, 1914–1917:102). Given all this, one might be tempted to suspect that Thompson’s dismissal of the original observer’s Amerindian attribution because the cloth is not red reflects nothing more than an unharnessed eagerness to find Africanisms in the Americas.³

Returning credit for the 1823 loincloth, then, to Amerindians, we are left with no picture of Maroon decorative sewing that predates the second half of the nineteenth century. Indeed, despite clear evidence that earlier Maroons had women’s wrap-skirts, two styles of men’s loincloths (wider and narrower), an impressive range of jewelry and accessories (much of it intended for ritual protection), and Western-style clothing purchased in coastal Suriname (see Price and Price, 1999:53–129), we do not even have reason to believe that Maroon dress included shoulder capes of any sort before the second half of the century. Schumann’s (1778) eighteenth-century Saramaka dictionary gives no word for cape. Coster (1866) reported that among the Maroons of eastern Suriname both men and women wore multiple “cloths” over the shoulders, but his detailed frontispiece—which shows eight men in loincloths, chest sashes, neckerchiefs, hats, leg bands, jewelry, and more—does not depict capes. Crevaux’s (1879) description and numerous illustrations of Aluku Maroons 10 years later confirmed this picture; and Bonaparte’s (1884) book on the Ndyukas and Saramakas brought to Amsterdam for the Colonial Exposition of 1883, who were systematically photographed in native garb, shows all of them bare-chested.

It seems likely that men’s capes and decorative sewing made their first appearance in Maroon costume at roughly the same time, in the late nineteenth century. That is, in any case, the point at which both of them first appear in museum collections and written documentation. And current Maroon ideas about dress might well reflect their simultaneous introduction, since men’s shoulder capes are by far the most consistently and most elaborately decorated of any type of garment. This paper, which considers clothing styles through the 1970s, therefore covers roughly 100 years of Maroon textile history.⁴

The Social Context of Maroon Textile Arts

Because most textile art is fashioned by women as gifts for men, I begin my exploration of the dynamics of creativity and the history of textile fashions by sketching in the main lines of the gendered division of labor and cultural notions about the material interdependence of men and women. Women assume primary responsibility for supplying and processing food from gardens (rice, tubers, bananas, peanuts, okra, etc.) and the forest (most importantly, the palm nuts used to make cooking oil). Men hunt and fish, purchase imported goods (including pots and pans, cloth and soap, sugar and salt, guns and machetes, radios and tape recorders) with their earnings from wage labor, and fashion wooden objects, such as houses, canoes, paddles, stools, combs, and cooking utensils. With marriage serving as the main institution through which these foods and goods pass from male to female hands, a woman without a husband is at a significant disadvantage in terms of material comfort. For various demographic reasons, including earlier first marriages for women and, since the 1870s, heavy outmigration by men, there are many more women of marriageable age than men. Both because of and in spite of the fact that most men have more than one wife, there is vigorous competition among women for the available pool of husbands. These (and a number of other) demographic and economic factors come together to produce a cultural environment in which women spend a great deal of energy trying to please men. In this setting, their artistic production plays an important role. (Further detail on these aspects of Saramaka life is provided in Price, 1984.)

In terms of textile arts, the great bulk of patchwork and decorative sewing appears on the vibrantly designed shoulder capes, which represent the most prominent item of men’s formal dress; second in importance are men’s breechcloths.⁵ Even when women decorate their own skirts and capes, there is a general understanding that it would be inappropriate to devote as much aesthetic attention to this kind of sewing as to that on a man’s garment.⁶ In the 1970s, for example, when narrow-strip patchwork capes were declining in popularity but women still had large accumulations of strips (edge pieces trimmed from the cloths they had hemmed to make their own wrap-skirts), they sometimes used the strips to make patchwork skirts for themselves. They were quick to explain, however, that they simply threw together, with an explicit avoidance of pre-planning, whatever strips they had on hand.

Similarly, although the handsomely carved calabash bowls that women produce belong to them and not to the men, the most important use of these bowls is at men’s meals, a highly charged site for competition among each man’s several wives. The calabash forms destined for men’s meals (water-drinking and hand-washing bowls) are embellished with more elaborate and carefully executed designs than are those destined for use by women (spoons, spatulas, ladles, and rice-rinsing bowls), where the carvings are sparse and simple. (Calabashes intended for use in rituals are completely undecorated.)

Stylistic Developments, 1880–1980

The oldest type of textile art that present-day Maroons remember, and that photographs and museum collections document, consists of embroidered figures on a monochrome or subtly striped cotton backing. Figure 2 (left) shows a representative example from 1908. The shapes tend to be curvilinear, their placement is roughly symmetrical around a vertical axis, and they are executed as linear outlines, often filled in with dense stitching in a contrastive color (see Figure 3).

The absence of vibrantly colorful patchwork textiles during this early period does not mean, however, that Maroons would not already have developed both the aesthetic principles and the cutting-and-piecing technique that were to go into its creation. Not only is color contrast already present in the nineteenth-century embroidery designs, but many other domains of daily life attest to its importance as a central feature of Maroon aesthetics. Gardens are laid out in patchwork-like alternations of red and white rice varieties, even though the different kinds



FIGURE 2.—A Dutch explorer with his Saramaka guides, 1908 (Eilerts de Haan, 1910, fig. 24).

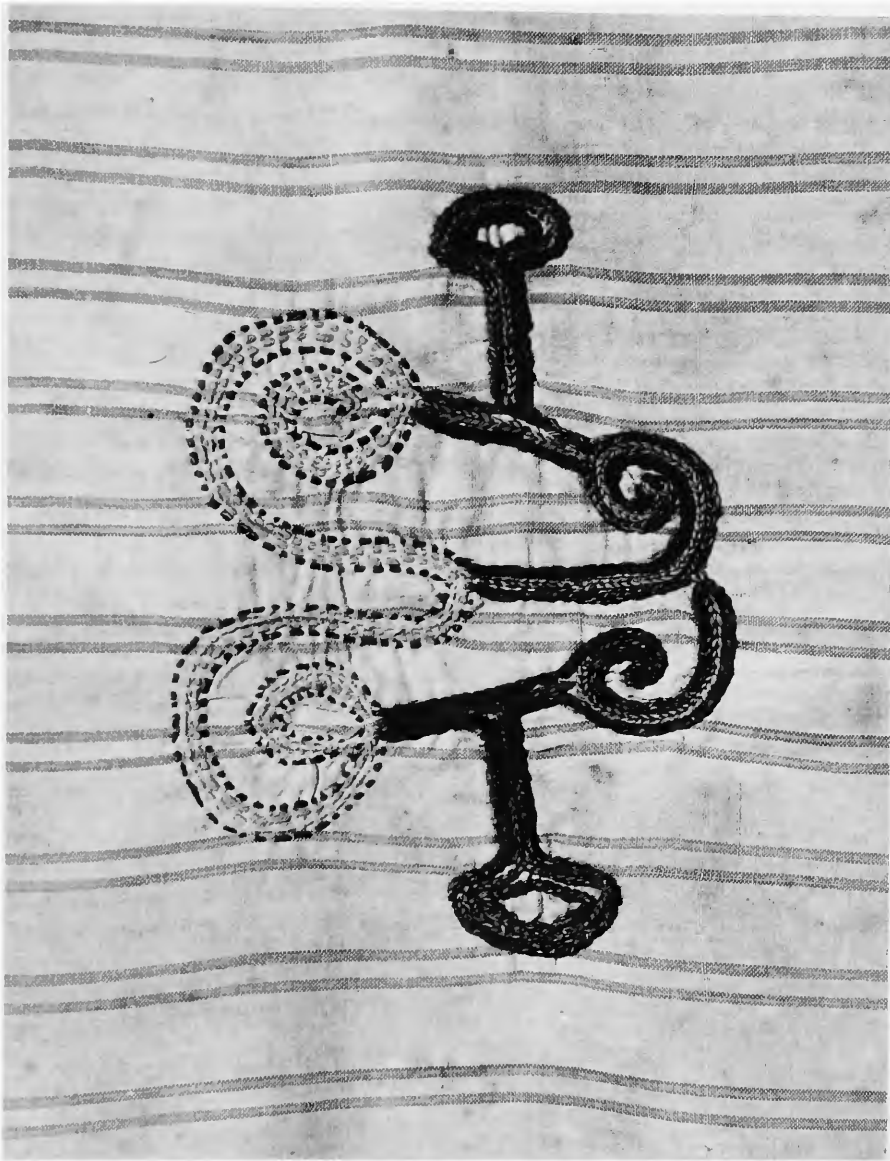


FIGURE 3.—Detail of cloth collected in the 1890s. Tropenmuseum, Amsterdam (cat. no. H2475). Photograph by Antonia Graeber.

look and taste the same once they get to the cooking pot. Dress reflects an explicit preference for wearing colors that contrast rather than blend with each other (for example, a red waistkerchief on top of a yellow and green wrap-skirt). Ideals of physical beauty include admiration for bright white teeth against jet black skin and dark (“green”) cicatrizations on an albino woman. And the inlays of men’s woodcarving introduce tonal contrast into an otherwise monochrome art.

In terms of the technical dimension, there exist Maroon garments that are made by cutting cloth into pieces, repositioning them, and seaming them back together without incorporating any of the vibrant color contrasts that later came to dominate the art of patchwork. In a cape construction popular in the 1920s,

for example, a length of striped cloth was cut into five pieces that were then repositioned and sewn back together in three vertical panels (see Price and Price, 1999: fig. 4.34, and, for a similarly pieced breechcloth construction, Price, 1984: fig. 46). Here, no pattern of contrastive colors or cross-cutting stripes results; both the initial cloth and its pieced-together follow-up are characterized by uniform stripes running in a single direction. But while a cape made from the uncut cloth either would have displayed horizontal stripes (which Saramakas say they don’t like on capes) or would have been too long and narrow, the cut-and-pieced version forms a cape of the preferred orientation and appropriate proportions. Similarly, the cape in Figure 4 has been

discreetly pieced together simply because the seamstress did not have a single piece of cloth that was large enough.⁷

Lingering a bit longer with this same cape, we can discern other aspects of Maroon textile arts as well. First, the design spread over its center is an excellent illustration of a very common color scheme in which the basic threesome of red, white, and navy or black predominates but is complemented by yellow, blue, and orange. Second, it displays the imperfectly realized bilateral symmetry that characterizes the bulk of women's art—calabashes as much as textiles. Saramakas explicitly esteem symmetry more than off-balance visual effects, but they are quite unanimous in the belief that women are less skilled at producing it than are men. A layout of motifs such as the one on this cape seems almost designed to prove their point as it is clearly conceptualized in terms of a vertical axis but has been executed with its elements a bit off-center. Third—and this is the heart of my argument in this paper—the composition is framed on the sides and bottom with patchwork strips in red, white, and black, appliquéd onto the edges of the white cape.

The steps of production lend us help as we try to read the implications of such a textile's aesthetic features. Having had the opportunity of watching Saramaka women plan out many dozens of textile compositions, I have been struck that each time they begin at the center and then work out to the edges. This or-

der governs every other kind of decorative sewing as well. Furthermore, when adjustments are made on a cape that has already been worn, they are introduced at the edges, which means that a cape's borders sometimes postdate its center by many years. It is not unusual, for example, for a cape to be enlarged in response to changing fashion by extra strips sewn onto the sides and bottom (see Price and Price, 1999: fig. 4.32). So, whether or not the patchwork strips in the embroidered cape in Figure 4 were present the first time the cape was worn, they would not in any case have been sewn on until after the embroidery design was completed. This means that the masterful realization of an established embroidery art filling the center of the cape was complemented, somewhere lower down on the flow chart, by a three-sided frame in which the seamstress was experimenting with something new. Over the years, as both the makers and the wearers of capes began to get tired of the same old thing, the once fashionable embroidery style faded out and the technique and aesthetic represented in its edge strips moved center stage.

Although the new style, which made its debut in the early years of the twentieth century, first appeared on garments with the earlier curvilinear embroidery, it was constructed by a completely different process, used different raw materials, and produced a different aesthetic effect. Small strips, squares, and tri-

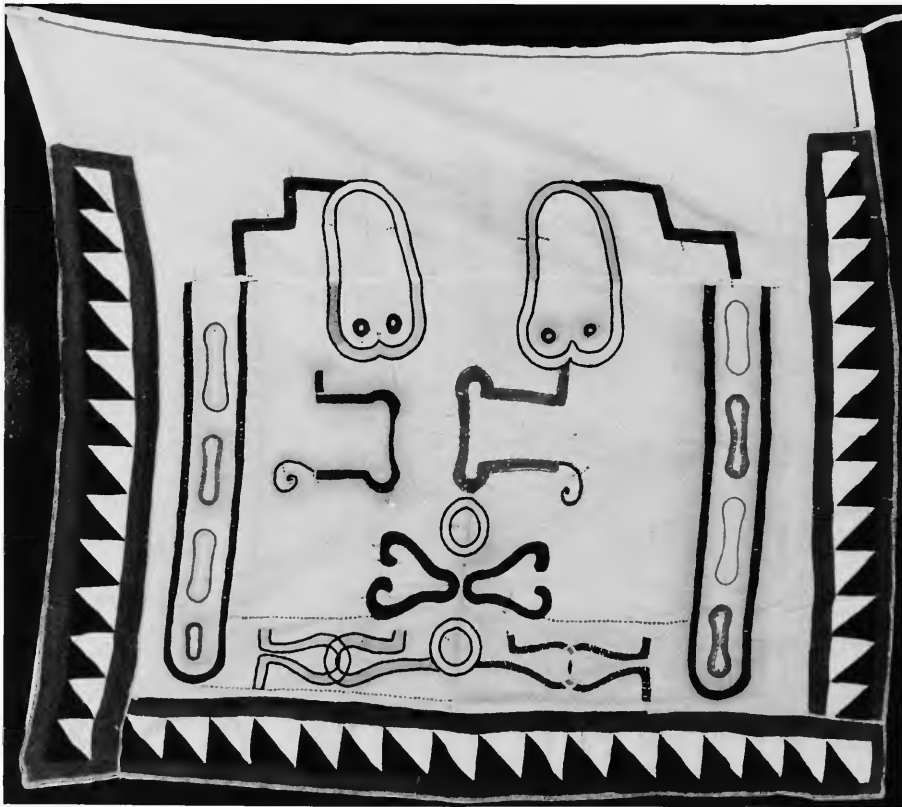


FIGURE 4.—Saramaka cape, sewn between 1900 and 1910. For more on this cape's life history, see Price and Price, 1991:iv. Photograph by Richard Price.

angles were cut with a knife from monochrome red, white, and black/navy cloth and were sewn together to produce a patchwork strip, which was then appliquéd onto the backing cloth. This technique/style eventually became known variably as *bè-ku-baáka* (red-and-black), *pèndê koósu* (striped/patterned cloth), or *pispísi* (pieced-together, or bits and pieces) sewing.

Over time, the new bits-and-pieces strips began to upstage the older curvilinear embroidery as women became more proficient at designing and producing them and as men acquired a taste for clothes that featured them. With embroidery's fall in popularity in Maroon fashion, bits-and-pieces patchwork literally took the center. On breechcloths (see Price and Price, 1999: figs. 4.37, 4.39) this meant a patchwork composition covering the broad rear flap—the most noticeable portion, since the front flap is small in comparison, and the rest of the garment simply passes between the wearer's legs. This rear panel received special attention because of the swinging motion it makes when its wearer walks. As Saramakas explained to me, the design itself is colorfully spread out, *wangaa!*, on the breechcloth, and it swings jauntily, *lióliólió*, as the man moves—hence their term for a breechcloth sewn in this style: *awangalió*. For shoulder

cap (which, unlike breechcloths have no nonvisible parts), the construction in which a solid piece of cloth served as a backing for decorative patchwork dropped out, and the entire garment came to be formed exclusively of bits and pieces, the seams of which were tucked under with a needle and meticulously hemmed to hide the raw cut edges. At the same time, the standard red, white, and navy color scheme of earlier textiles was embellished with accents of yellow.

Figure 5 illustrates this development (see also Figure 2 (right)). Here, the production process begins with the construction of composite strips, much in the fashion of Seminole patchwork. One of these strips (often of a unique pattern—in this cape, for example, the central patchwork strip is black and white, while all the others are black, white, and red) is chosen for the garment's "spine" (*báka mindi*), thus defining its vertical center. The spine is then flanked by a pair of matching strips, one to the right and one to the left, followed by another and another until the center of the composition achieves the proper size. The strips are laid out on the ground without being sewn so that changes can be made at any point. In this cape, once the patchwork strips were in place, the seamstress contin-



FIGURE 5.—Bits-and-pieces cape, sewn between 1920 and 1940 by Peepina, village of Totikampu. Collection of Richard and Sally Price (cat. no. T78.4). Photograph by Antonia Graeber.

ued the process with strips of the multi-colored cotton that was being sold in coastal stores, beginning with a pattern she had in duplicate (a red and yellow striped cloth), attempting to continue the symmetry (with white and then black cloth), and then finishing with a more random sequence of three strips in which only the middle one produces a left-right match. The next steps would be to select one or two warp strips (strips running in the same direction as the cloth's selvage) for the lower edge, and to attach a final warp strip, to be tied at the man's shoulder, on the top. A single stitch or two would fix the chosen order of strips, and the time-consuming process of seaming and hemming could begin.

We now follow these multicolor strips as they migrate from the edges to the center of fashion (see Figure 6). Capes composed exclusively of narrow strips cut from multicolored striped cotton had completely driven out the bits-and-pieces style well before I first arrived in Saramaka in the mid-1960s. Despite their visual resemblance, if viewed by non-Maroon eyes, to West African traditions of edge-sewn textiles, this style

of narrow-strip composition emerged directly from experimentation by Maroon seamstresses, who were tiring of the older form of sewing, discovering the properties of raw materials recently made available in coastal stores, and elaborating in novel ways an aesthetic preference of contrasts and interruptive patterning that already ran through many dimensions of their daily lives.⁸

Note that in this new textile art, the raw materials themselves are marginal in the most literal sense, since the strips are the leftover trimmings from women's wrap skirts, which were made by cutting the ends and sides off two-ell lengths of trade cotton, and even the sewing thread was sometimes salvaged from such scraps when store-bought thread was in short supply. The earlier bits-and-pieces had by this time dropped out completely, and the entire composition consisted of multi-chrome narrow strips, although the procedure of establishing a vertical spine and flanking it, from the center out, with matching strips remained constant. (For examples of narrow-strip capes, see Price, 1984: figs. 48, 55.)



FIGURE 6.—Saramaka cape, probably sewn in the 1960s or early 1970s. Collection of Richard and Sally Price (cat. no. T78.32). Photograph by the Photographic and Illustrations Department, Johns Hopkins University.

While the narrow-strip capes were dominating men's fashion in the villages of the Suriname interior, young Maroon women in the villages closest to the city, where Moravian missionaries had set up churches, medical clinics, and schools, were learning the refined art of cross-stitch embroidery, conscientiously following diagrams in women's magazines provided by the missionaries. Saramaka men from upstream villages, who traveled the river frequently on wage-labor trips and developed romantic ties with downstream women, were often presented with gifts of capes and breechcloths decorated in this cross-stitch embroidery. They wore these garments with pride at community events in their home villages upriver, where their wives were not insensitive to the admiration they inspired. And although the upriver women, who had never had an opportunity to go to school, were at first profoundly intimidated by this competition from their more formally educated rivals, they realized they would have to learn to embroider cross-stitch if that's what their men wanted.

So they did. For the first several years women worked on thin trade cotton like that their mothers and grandmothers had used for embroidery, first setting up a grid of horizontal and vertical guidelines for the crosses by pulling threads out of the cloth with a pin. The designs on these early "pull-the-thread" capes were linear motifs, and they were executed at the very edges of the cape (Figure 7). Both because the thread pulling was labor intensive and because the women didn't yet feel confident in the medium, these early cross-stitch designs were rather minimal. Later on women discovered that a heavier weave of commercial cotton, which they called *lapu*, allowed them to skip the time-consuming preparation, because its knobier texture supplied a ready-made grid. And with time, they mastered the new embroidery technique at least as well as the downstream women had. By the mid-1970s, when men had packed almost all of their narrow-strip capes away in trunks or started using them for rags, the cross-stitch art had broken out

of the edges and taken over the centers of capes, and sometimes breechcloths (see Figure 8).

That, in a very small nutshell, is the art history of Saramaka Maroon textiles and a summary demonstration of the idea that particular styles—defined in terms of materials, techniques, and design principles—take root in the margins, migrate to the centers as they evolve into mature arts, and eventually cede their privileged place when the next style begins moving along the exact same route.⁹

Calabash Carving

Does this progression characterize the road from experimentation to established styles in other media as well? The second major medium exploited by Maroon women is calabash carving, an art form that has neither warp nor weft, neither selvages nor raw edges. Where, then, are the marginal areas and what do we see happening in them?

The earliest Maroon calabashes—as represented in historical documents, museum collections, and the memories of late twentieth-century Maroons—were decorated in a technique and style that followed the general model of carvings done by their ancestors in many parts of Africa. In spite of the fact that the fruits utilized in Suriname came from the American calabash tree and the fruits known as "calabashes" in Africa were picked from vines in the botanical family of pumpkins and squashes, the bowls made out of the two fruits looked a lot alike.¹⁰ That is, the raw materials were totally unrelated, but the objects made from them, and the artistic style of their decoration, were at first strikingly similar. Early Maroon calabash carvings were, like many of their African precedents, composed entirely of intricate designs carved on the exterior surfaces of bowls and covered containers. In early Maroon communities, it was the men who made these carvings, using manufactured metal tools imported from the coast.

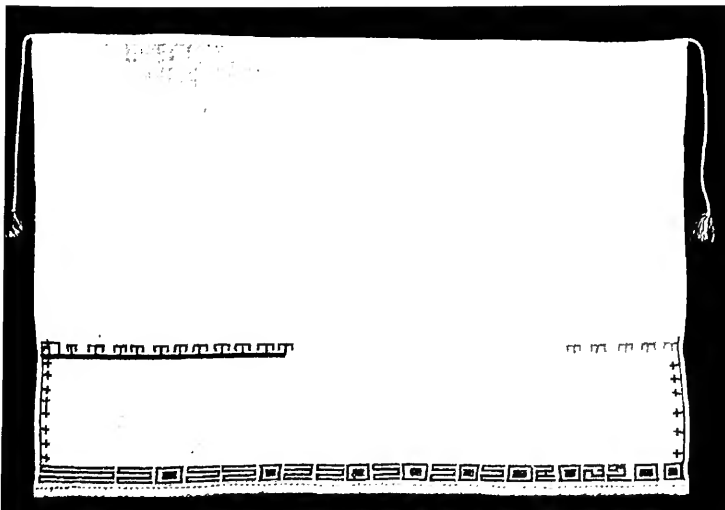


FIGURE 7.—Cape with cross-stitch embroidery, sewn between 1960 and 1965. Collection of Richard and Sally Price (cat. no. T68.2). Photograph by David Porter.



FIGURE 8.—Cape (left) and breechcloth (right) with cross-stitch embroidery, sewn in the 1970s. Photographs by Richard Price.

The history of women's calabash carving begins with a discreet takeover of this traditionally male artistic domain. Although the innovative styles that young women developed in textile arts eventually replaced those of their mothers and grandmothers, the artistic territory that their calabash experimentation invaded was that of their fathers and grandfathers. If, in examining early examples of Maroon calabash carving in museum collections, we look beyond the published photographs, pick the bowls up off the shelf, and turn them over, we notice markings in their leftover spaces. These crude incisions

were clearly not made by a knife or chisel or gouge, were clearly not designed with rigorous attention to symmetry or geometry, were clearly not products of a coherent artistic style, were clearly not executed with full manual control, and were clearly not carved by the same artist who worked the bowl's exterior (see Figure 9 and, for seven nineteenth-century examples, Price, 1984: figs. 15–18). The authorship of these irregular, off-center, bas-relief markings is not identified in publications or museum records, but by following them through time, we can see them evolving into a highly refined carving style.

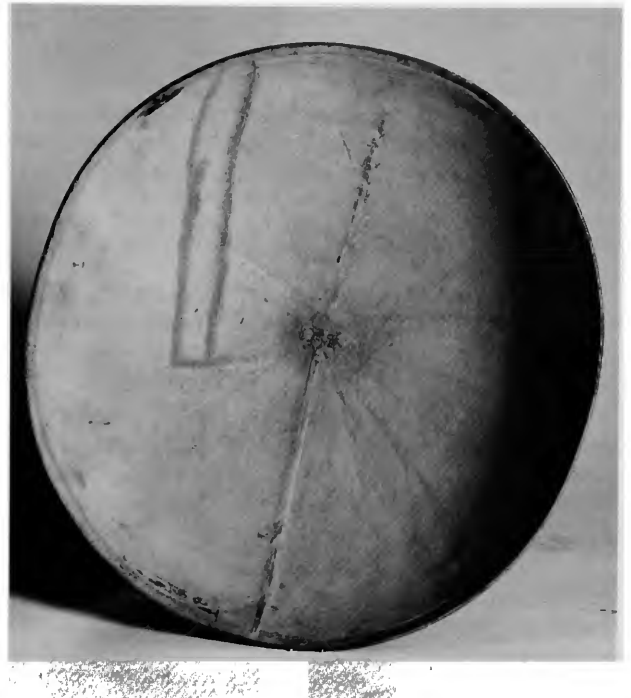


FIGURE 9.—Top of two-piece calabash container, carved on the outside (left) with woodcarving tools and on the inside (right) with a piece of broken glass. Collected in Aluku at an unknown date; acquired by the Musée de l'Homme in 1935 (cat. no. 35.72.74). Courtesy of the Musée de l'Homme, Paris (neg. nos. D-82-727-493, D-82-728-493). Photographs by D. Ponsard.

As the women gradually redefined their artistic terrain, abandoning bowls that had already been carved by men and taking over artistic control of the entire object, their designs spread over the bowl's whole interior surface, and their art became a pervasive presence in the material culture of the Maroons. In short, both the carvings themselves and the role of the art in daily life moved from the wings to center stage (Figure 10), like each of the styles and techniques in the series of textile arts discussed above.

The tools for the calabash carvings also emerged from the margins of Maroon material culture in much the same sense as the edge trimmings that women used to make narrow-strip textiles. Maroon men had long been working on calabashes with manufactured instruments they bought in the city. But women (who had no personal access to money or city stores) discovered that by setting a bottle on the ground and breaking it with a rock, they could produce small, very sharply pointed pieces of glass, some of which, with a little experimentation and a lot of practice, served as effective tools for the carving of their bowls.

Elsewhere (Price, 1999:219–221) I have proposed a relationship between Saramaka gender ideology (which prescribes, for example, that women eat their meals out of cooking pots with the children while men are served carefully molded rice in a handsome bowl covered with an embroidered cloth) and the



FIGURE 10.—Saramaka calabash bowl carved with a piece of broken glass by Keekete, village of Asindoopo, 1970s. Collection of Richard and Sally Price (cat. no. K78.12). Photograph by Antonia Graeber.

fact that women develop their arts in marginal spaces and on the backs of male-carved objects, or that the materials they employ are scraps and pieces of broken glass. Here, however, I end on a more methodological note, with some reflections on the kinds of questions that need to be asked if we are to arrive at a full understanding of art in cultural context.

The Ethnography of Art and the Art of Ethnography

The increasing dialogue between anthropologists who study art and art historians who study what might, by extrapolation from the conventional terminology, be called the not-so-fine arts (this is, after all, a world that distinguishes its “insiders” from its “outsiders”) puts all the participants in a position to arrive at more enlightened understandings of art in its cultural settings. And as that dialogue includes more and more voices from the communities whose arts are in question it takes on greater nuance, texture, and authority.

But whatever our formal training, and whether we use one, two, or three slide projectors to illustrate our findings, it's important to make sure that the ethnography of art takes maximal advantage of the art of ethnography. This means sometimes setting aside the contemplation of finished forms, the spotlight on masterpieces, the authentication of signatures, the celebration of star-status artists, and the promotion of master narratives, such as intercontinental continuities, in order to devote conscientious attention to the humbler dimensions of art. In the spirit of those innumerable references to esoteric archival documents and auction catalogues and travel journals and unpublished dissertations that Bill scribbled onto 3 × 5 inch slips of paper and passed on to me while I was undertaking research on Maroon textile arts, it means following up on every lead.

It also means asking all the little questions. With textiles, for example, where do Maroons get their thread, what do they use to tuck under the raw edges in preparation for hemming, and how close are their stitches? What words do they use to label these cloths as they pass from men's coastal purchases to conjugal presents to women's skirts to sacks of edge trimmings to unsewn patterns on the ground, then back to conjugal presents, men's formal wear, pieces of laundry, and finally threadbare rags? What roles do textiles play in marriages, in worship, in political investitures, in popular songs, in legal disputes, in funerals? Do people talk among themselves about aesthetic principles? What, if anything, do they have to say about symbolism? Why do they always fold clothes, wrong side out, into little wallet-size packets? Why do seamstresses sometimes lather up a newly-sewn textile with bar soap and leave it in the sun before rinsing out the suds? Why, after carefully concealing the tiny stitches used to make a seam, do they lead the thread onto a part of the cloth where it shows clearly before cutting it off? How do they deal with slips, errors, and botched designs? What features of a textile inspire praise (from men, from women) and what features are disparaged? Are clothes mended when they tear, and if so how? What tone do people

adopt when they critique a six-year-old's first attempt to sew a patchwork apron? How do they talk about the obsolete arts of their grandmothers? What parts of a garment do women use to test out new ideas and how do their experiments affect fashion trends? Do they cut the cloth with scissors, knife, or razor? Or is it ripped?

Whether focusing on patchwork or on the less-kind art of scalping (Axtell and Sturtevant, 1980), Bill Sturtevant has demonstrated the importance of trying to understand, in as much close detail as we can muster, the historical, cultural, and material environment in which each cut is made. His scholarship stands as a reminder that solid ethnography, like a fine patchwork textile, reflects an overall vision but is executed through hundreds and thousands of tiny, meticulous stitches.

Notes

My argument about “the centrality of margins” was first developed for an April 1996 conference entitled “The African Diaspora: African Origins and New World Self-Fashioning” at Binghamton University, Binghamton, New York (see Price, 1999). I am grateful to Isidore Okpewho for inviting me to participate in that conference and for permitting portions of that argument to be used in the present paper.

1. I have reworded the translation of this passage very slightly, rendering the verb *marquer*, for example, as “mark” rather than “decorate.”

2. The adoption, by Maroons and Amerindians, of the Portuguese word *camisa* to designate “loincloth” parallels the adoption in English of the French word *entrée*. In each case a word (*camisa*, *entrée*) and its general context (an article of men's clothing, a category in restaurant menus) have been retained, but the specific meaning (shirt, appetizer) has been shifted (loincloth, main course). Stedman's documentation of its standard use among Amerindians in the 1770s is complemented by another observer's documentation of its standard use among Saramaka Maroons by 1778 (Schumann, 1914, s.v. *kamissa*).

3. In addition to the errors in Thompson's Amerindian ethnography, there are troubling sleights of hand in his representation of Ndyuka history and in his use of demographic figures on the slave trade to Suriname. The sequence of events he proposes is the transfer of influence from Mande to Asante weavers of the early nineteenth century; the capture, trans-Atlantic voyage, and selling into slavery of some of these Asantes; their escape into the interior of Suriname; their acceptance by the Ndyuka Maroons; the production by them (or under their influence) of the loincloth in question; its sale to Carib Indians; and its collection from those Indians by a French explorer, who then sailed back to Europe, wrote up his travel account, and published it in Paris in 1823. He capped this scenario with a claim that the African area that influenced the hypothetical Asante weavers was one “whence sailed to Suriname 50 percent of a sample of Dutch slaving ships” (Thompson, 1983:215), giving as his source pages 14–15 of *The Guiana Maroons* (Price, 1976).

Note, however, that (1) the Ndyuka Maroons had been virtually closed off to new runaways since their treaty with the Dutch Crown in 1760, and (2) the statistical sample he alludes to does not refer simply to “a sample of Dutch slaving ships,” but rather to a (56-ship) sample of those Dutch slaving ships that transported Africans from the Windward Coast—an area that, as those pages make clear, supplied between 0% and 49% of Suriname slaves (depending on the particular moment) during the course of the eighteenth century.

4. A word of explanation about the 1980 end date. In 1986 Richard Price and I went to Suriname after a six-year absence. The evening before our trip upriver, we were placed under house arrest in our Paramaribo hotel and then, at midnight, brusquely put in the back seat of a Volkswagen and escorted to the border by two military policemen. Although we did not know it at the time, the Maroon-led Jungle Commando had just had its first skirmishes with the Suriname military, and a civil war, which eventually lasted six years, was in the

making. In the wake of that experience, compounded by encounters that Richard Price had after testifying on behalf of the Saramaka people in a 1992 Human Rights trial against the Suriname government (see Price, 1995), our Maroon fieldwork has been conducted in neighboring French Guiana.

5. This may be the appropriate place to note that the decision to translate the Saramaccan word *kamisa* as “breechcloth” was made on the basis of discussions in which Bill Sturtevant helped Richard Price and me weigh the alternatives. Rejecting “loincloth” on the grounds that the Saramaka garment can either be held up by sewn-on ties or draped over a separate waist string, and “breech clout,” on the grounds that it carried a connotation of aesthetic crudeness inappropriate for the Saramaka garments, “breechcloth” eventually emerged as the favored term.

6. This observation applies specifically to Upper River Saramakas. Eastern Maroons and Saramakas living in the villages closest to Paramaribo have sometimes decorated skirts.

7. In like manner, if the 1823 evidence for assembling pieces of cloth in French Guiana (see above) was referring to patchwork, it could easily have been made without producing color contrasts, given that nothing is said in that testimony about dyes.

8. This view of discontinuous textile arts emerging from continuous aesthetic principles and sewing techniques, which reflects a perspective first elaborated in terms of Afro-Caribbean culture in general (Mintz and Price, 1992), explicitly contests the more narrowly medium-specific continuity proposed by Thompson (1983:214–219, pl. 141). Thompson’s preferred scenario would link Saramaka narrow-strip cloths “of the twentieth century,” Ndyuka equivalents “of the late nineteenth century,” and coastal black patchwork “of even earlier facture” in order to explain Afro-American continuities without recourse to “a mysterious black consciousness.” The “earlier facture” of coastal patchwork

rests on a single verbal statement by a man Thompson met in Paramaribo in the 1980s and is unsupported by illustrations, examples, or written documentation. The basis for the Ndyuka dating is not indicated. Plate 141, Thompson’s illustration of the “culmination [of this progression] in Djuka and Saamaka multi-strip expressions of the early twentieth century,” is a patchwork hammock sheet that was made by Apúmba, wife of Saramaka Tribal Chief Agbagó, as a gift to her sister-in-law Naai, who gave it to Richard Price and me in 1968 when we left Saramaka after having been her close neighbors for two years. Photographed hanging from a rod in our living room, it is presented by Thompson without acknowledgment or attribution of any kind, perhaps because our published documentation of the textile (Price and Price, 1980:77) made clear that it may have been sewn as late as the 1950s.

9. Like any nutshell history, this one has left out numerous details. I cite just three. Even after the early curvilinear embroidery style gave way to bits-and-pieces patchwork, a more linear, less textured version of it continued as a secondary kind of decoration for many kinds of clothing (see, for example, Price, 1984: figs. 49–50; Price and Price, 1999: figs. 4.46–4.48). During a transitional period between bits-and-pieces and narrow-strip patchwork, some textiles displayed bits-and-pieces composition executed with the multicolored cloth more typical of the narrow-strip art (see, for example, Price and Price, 1999: figs. 4.73, 4.74). And in the 1980s, women were already focusing their efforts on a new patchwork art that displayed striking visual similarities to the textiles of the early twentieth century but employed a distinctive range of colors and made significantly heavier use of appliqué (see, for example, Price and Price, 1999: figs. 4.52–4.54).

10. For further discussion of the these two botanical species (*Crescentia cujete* L. and *Lagenaria siceraria* (Molina) Standley), and the implications of differences between them for Maroon art history, see Price, 1982.

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European Motifs in Protohistoric Iroquois Art

Edmund Carpenter

If, in a protohistoric Iroquois grave, someone found a sixteenth-century French spoon, its handle shaped like a modest nude, we would say, "Of course: obviously the model." But no such find has ever been reported. To date, we can only guess what inspired sixteenth- and seventeenth-century Iroquois to carve figurines of modest nudes, wear them as pendants, and bury them with their dead (Figure 1).

Thirty years ago I worked in a remote part of the Western District, Papua New Guinea. Trade objects were rare: isolated villages treasured a steel knife or scrap of mirror. Few men and no women had ever before seen a European, yet for at least 60 years they had heard stories about strangers beyond their borders, and a few men, visiting distant villages, had actually seen them. Decades before the first government patrol entered those villages, their inhabitants began to change, perhaps partly because of trade objects, which reached them through intertribal routes. I thought of sixteenth-century Iroquois villages.

"September Morn" Figures

As early as 1529, European maps included accurate details of the Atlantic Coast (Anonymous, 1981, maps 27–29). These maps were drawn, their authors acknowledge, with the help of native pilots and cartographers. Once ashore, European trade objects moved rapidly inland, carried over vast distances via ancient networks of trade and alliance. Indians served as middle men.

European goods reached the Iroquois well ahead of Europeans, as did ideas. Long before missionaries arrived, graves were reoriented, and burial offerings multiplied. Mortuary changes began with the first appearance of trade goods. Behind these changes lay, I suspect, fear of death.

Traditionally, the Iroquois believed in reincarnation. Life, they asserted, was an unending trail of rebirths. Their first encounter with the concept of death, that is, death as full stop, must have been traumatic. Those who take reincarnation for granted seem to be especially vulnerable. Resurrection offers the possibility of a single, spiritual reawakening in another world—hardly reassuring to anyone expecting endless rebirths

in this world. The shift from belief in reincarnation to belief in resurrection produced profound changes for many, including the Iroquois.

Paleolithic peoples across Eurasia, as well as later peoples in the Arctic and Oceania, wore pendants of inverted female figures, the notion being, presumably, that the World Beyond is an inverted world where ancestors await rebirth (Schuster and Carpenter, 1996:268–277). The Iroquois, like many North American Indian tribes, shared this view, although they never wore inverted figurines. They did, however, sometimes depict the dead as inverted (O'Callaghan, 1849:7).

Iroquoian figurines were suspended through or around the figurine's neck; thus, they hung feet down. This appears natural to us, but it may not have appeared that way to those who depicted their dead as inverted.

All Iroquoian figurines are post-Contact. No prehistoric example is known. Many look so Hellenistic, so non-Indian, that a European model seems required. Except for a rare headdress and a possible mask, they seem completely alien to Iroquois iconography, even to North American Indian art generally. Such figures occurred in prehistoric Arizona, possibly under Mexican influence (Gumerman and Haury, 1980:79), but nothing even remotely like them occurred in the Northeast until after European contact.

Two Iroquoian examples come from the Adams site, Livingston County, New York. The Adams site is the earliest known Seneca village with trade goods (ca. 1570–1590), yet no European is known to have reached the Seneca before 1616 (Wray et al., 1987; Noble, 1994:27). The Jesuits arrived 60 to 80 years after the Adams figurines were carved, and by then this tradition was fading.

How popular were Iroquoian figurines? In 1938 I circulated a questionnaire to museums and avocational archeologists in the northeastern United States, requesting information and photographs of examples in their possession or known to them. I located nearly 100 (Carpenter, 1942). Iroquois tribes were widely represented, although most examples came from Cayuga and Seneca sites. Genoa Fort, a Cayuga site (ca. 1600–1620), Geneva County, New York, produced an exceptionally large number of fine examples.

My survey missed many specimens, especially Susquehannock ones. Among those in private hands, some have disappeared, at least for the moment. Others were mislabeled. (I saw

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FIGURE 1.—Two antler figurines from the Genoa Fort site (Gna 002), Cayuga Iroquois, ca. 1600–1620, Geneva County, New York. Rochester Museum and Science Center (cat. nos. 6515/205 (left) and 6065/205 (right)). Courtesy of the Rochester Museum and Science Center, Rochester, New York. Graphite drawings by Gene Mackay. Photographs by James Osen.

one in a Near Eastern museum labeled “Cycladic.”) Fakes entered the market, principally through Paul Mann, a forger active in the 1930s. His creations, mistakenly identified as authentic, grace several recent publications. Unfortunately, I recorded no discoveries after 1938. Still, many documented examples survive.

Not all enjoy Hellenistic posture, but enough do that local archaeologists call all of them “September Morn” figures. This refers to Paul Chabas’s coy bather, whose Venus-like portrait—one hand over breasts, the other over genitals—appeared in the 1930s as a White Rock beverage advertisement.

NUDITY IN SEPTEMBER MORN FIGURES

None of the figurines exhibits clothing. Gender reveals itself by a vulva or phallus, breasts, enlarged abdomen, or perhaps a tress or braids, but these are by no means always present. Several figurines in classic Venus posture exhibit headgear unsuitable for Iroquois women. One figurine covers its face with its hands. Others have hands at their sides. This variety, this lack

of uniformity, suggests to me an alien motif, newly introduced. I miss the conformity I associate with traditional iconography.

Were the figurines carved by men, women, or both? Who wore them? Most documented examples come from the graves of women and children. Some bear evidence of use; others may have been made for the dead. Whatever role they played, their nudity sets them apart from aboriginal Iroquois art.

The Vatican Library contains sixteenth-century engravings of Adam and Eve modestly posed. Some were intended for overseas proselytizing. Nudity became a theological issue when Columbus discovered America. Had he come upon humans not of Adam’s seed? Such a discovery would challenge biblical authority. It would also deny American Indians human redemption. The Vatican supported monogenism, noting that St. Augustine had scornfully dismissed antipodal humans as “exceedingly absurd” (Carpenter, 1950:5). The Indians of the New World, it declared, were naked innocents, created in God’s image. And that is exactly the way they were first represented, beginning with the announcement of Columbus’s discovery.

Adam and Eve images served as models for American Indian images. The frontispiece in the 1590 edition of Theodore de Bry’s *America* shows Adam and Eve as New World representatives. So does the frontispiece of Champlain’s *Les Voyages* (1613).¹

THE PRINT MODEL

Were September Morn figurines print-inspired? Prints served as trade goods in the sixteenth century. During the winter of 1596–1597, the Dutch explorer Willem Barentz, seeking a northern route to Asia, laid over on Novaya Zemlya, off the northern coast of present-day Russia (Braat et al., 1980), and abandoned his cargo there. Recovered centuries later, it included bales of prints, some depicting Adam and Eve, both nude (Braat et al., 1980, figs. 21, 22).

Barentz’s cargo survived thanks to arctic conditions. Prints in Iroquoia were far more vulnerable, although a copper kettle in a grave on the Dann site (ca. 1655–1675), Livingston County, New York, preserved a baptismal certificate² signed by Father Julien Garnier, who is believed to have served this Seneca village between 1668–1674.

A brass frame, recovered from the Hamilton site, a Neutral village (1635–1651) in southwestern Ontario, has the scalloped interior common to seventeenth-century religious picture frames (Lennox, 1981:328–329, 401).³

Frames appear in seventeenth-century mission sites in Florida and elsewhere in the Southeast. In my imagination, I envisage framed biblical scenes gracing the walls of rustic chapels. This was certainly true of Southwest missions and elsewhere in the Spanish empire. There, we know, prints served as models for local artisans (Lange, 1974, 1991).

Prints nearly everywhere inspired local copies. In West Africa, sixteenth-century Benin craftsmen used them as models for Sapi-Portuguese ivories (Bassini and Fagg, 1988). This seems to have been widely true, no less so in Iroquoia.

Although we have no actual prints—no smoking gun—circumstantial evidence exists, much of it assembled by George Hamell, New York State Museum, Albany (Hamell, pers. comm., 1980–1990). The most remarkable document he found, dated 1712, describes the intended distribution of 12 framed and 192 unframed prints of four Iroquois who visited England in 1710 and became widely known as the “Four Indian Kings” or “Four Kings of Canada.” One framed print was assigned “to the 5 Nations to be placed in y^e Onondagas Castle where the 5 Nations meet.” Unframed prints were assigned to

“Each of y^e 5 nations & y^e River Indians” and “to y^e 4 Indians who went to England.”⁴

Noble Hunter Figures

One of those four prints shows Sa Ga Yeath Qua Pieth Tow, “King of the Maquas” (also known as Brant), in classic European pose as lord of the manor, gentleman hunter, Indian chief (Figure 2). Ideally, this posture shows the right foot forward, the right hand raised and grasping the barrel of a grounded



FIGURE 2.—Mezzotint *Sa Ga Yeath Qua Pieth Tow, King of the Maquas* (Mohawks), London, 1710. Based on a painting from life, made in London by John Verelst in 1710.

musket, the left hand pointed down, and a dog opposite the musket. Here a bear substitutes for the dog.

It is a noble pose. It is the one that Emperor Charles V (1500–1558) chose to represent himself to posterity. So did other nobles. It derives from the classic posture of divine nobility: right foot forward, right hand pointing toward the upper world, left hand pointing to the lower world, body aligned with the *axis mundi*. When seated, the figure holds a scepter, symbolizing the *axis mundi* as climbing pole, in the right hand.

Whirling Dervishes are even more explicit: right hand up, left hand down, body as *axis mundi*, with cap and gown symbolizing the sky dome in planetary rotation. In the European Renaissance this symbolism became less explicit yet basically survived. Each of the Four Kings of Canada was painted in this Western posture reserved for divine royalty, although one portrait reverses left and right.

Mezzotints of the Four Kings, dated 1710, were distributed to the Iroquois in 1712, as noted, yet a figure in Dutch dress assumes this posture on an effigy comb from Ganondagan, a large Seneca village that burned in 1687 (Figure 3). His outstretched (left) arm holds a grounded musket, his right hand touches his hat, and his dog, opposite the musket, jumps up beside him.

The Dutch established at Albany, New York, first a trading post (1614–1617), then Fort Orange (1624). I searched for a print of one of the Princes of Orange in that pose, imagining such a print posted in Albany or in Iroquois villages, but I failed to find one. Still, I think some such print—Dutch, French, English—inspired seventeenth-century Iroquois artists. By 1712 that posture may already have been old hat to the western Iroquois.

European visitors to America, after returning home, loved to strike this pose bedecked in Indian garb, when standing for their portraits. This is how we remember several prominent visitors, and it is also how we remember Meriwether Lewis, from a 1807 watercolor.⁵

This pose served as a model for Indians themselves. An example, wearing a horned headdress, was cut into rock at Esopus Landing, on the west bank of the Hudson River, New York. True, both hands are raised and it lacks a dog, but otherwise it conforms to the European format (Figure 4).

George Catlin painted his 1838 portrait of Osceola, the Seminole chief, from life. It shows Osceola, who was left-handed, grasping his grounded musket in his outstretched left hand.⁶ An Ottawa mission-school artist later embroidered that portrait on leather, presumably from a Catlin lithograph.⁷ Full circle is achieved in *A Pictorial History of Costume* (Bruhn and Tilke, 1973:199), which offers Osceola's pose as typically American Indian.

Horse and Rider Figures

I also see European images as likely models for Seneca and Cayuga “horse” combs. In 1677, Wentworth Greenhalgh, sent by Colonial New York Governor Edmund Andros to assess Iro-



FIGURE 3.—Effigy comb of antler, Ganondagan State Historic Site (Can-2-1), Canandaigua County, New York. New York State Museum (cat. no. 13-4B). Courtesy of the New York State Museum, Albany, New York, and the Rochester Museum and Science Center, Rochester, New York. Graphite drawing by Gene Mackay.

quois military strength, rode a horse into their villages (Brodhead, 1853:250–252). He said the Seneca at “Canagorah,” probably Ganondagan,⁸ “were very desirous to see us ride our horses, w^{ch} wee did” (Brodhead, 1853:251). He also visited what is now the Kirkwood site (ca. 1675–1687), Livingston County, New York. That site produced at least one comb depicting a horse and rider.⁹

Yet such combs existed prior to Greenhalgh’s visit. The Dann site (ca. 1655–1675), Monroe County, New York, produced one of a mounted man brandishing a sword overhead



FIGURE 4.—Petroglyph (from Schoolcraft, 1853, pl. 18) at Esopus Landing, on the west bank of the Hudson River, New York. Undated, perhaps undatable, but conceivably late seventeenth century.

(Figure 5). Its counterpart (and possible model) appears on a white-clay trade pipe from a Seneca village (ca. 1619–1636), now the Warren site, Ontario County, New York, (Figure 6).

The Mohawk used horses to haul timber. Seneca who traveled east may have these animals, but I think they saw images of horses before they saw actual horses. Seneca horse and rider images look more noble than realistic. I doubt that Europeans rode around Seneca villages brandishing swords overhead.

In 1684 the English placed an escutcheon, or placard of protection, in each of the three Upper Iroquois Nations in anticipation of de la Barre's punitive expedition. That escutcheon featured a man on horseback.

Figures in the Round

Recently a cast-iron handle for a spoon or (less likely) a dagger, with pewter shaft, was recovered from the Fox site, a Seneca village (ca. 1658–1672), Ontario County, New York. It depicts a hunter grasping the muzzle of a grounded musket, with

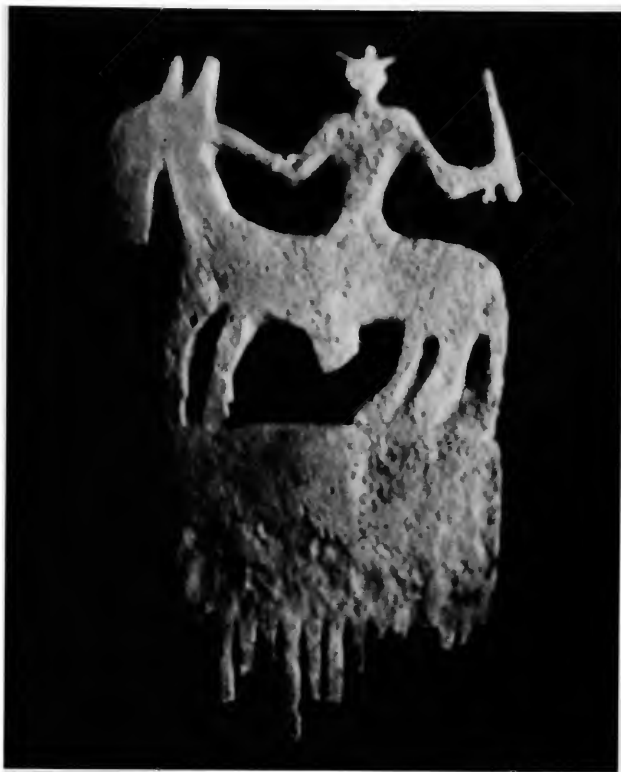


FIGURE 5.—Effigy comb of antler, horse, and rider, Dann site, Monroe County, New York. Rochester Museum and Science Center (cat. no. 794.28). Courtesy of the Rochester Museum and Science Center, Rochester, New York.



FIGURE 6.—European white clay pipe, Warren site, Ontario County, New York. Rochester Museum and Science Center (cat. no. 10,038.89). Courtesy of the Rochester Museum and Science Center, Rochester, New York. Drawing by Patricia Miller. Photograph by Brian Fox.

a dog on the opposite side (Figure 7). The musket presses against the hunter's body, and the dog presses against his leg.

This tightly rendered sculpture bears little artistic similarity to silhouette examples. More than any other specimen, it makes me suspect that the model for September Morn figures was sculptural. Iroquoian figurines are three dimensional, a form that is not easily derived from flat art. We do not know what Jacques Cartier left behind in Canada (1534–1543), but we do know that spoons with modest maiden handles were then popular in France.

The outstretched arm of the gentleman hunter, rendered flat, provides a noble gesture that vanishes when hunter, musket, and dog squeeze together to form a functional handle. Septem-

ber Morn figures don't walk freely through Eden, as they do in prints, but hug themselves. I think spoons, not prints, provided the model for Iroquoian figurines.

Northeastern tribes, including Iroquoian, had flat art, such as quill work. But that art lacked three-dimensional perspective with its mysterious vanishing point. That perspective, which arrived with European prints, must have come like a revelation to the Iroquois. I've watched tribespeople elsewhere stand before such renderings, totally absorbed. Mary McCarthy, describing the Renaissance invention of perspective, wrote:

The vanishing point, toward which all the lines of a painting race to converge, as if bent on their own annihilation, exercised a spell like that of the ever-disappearing horizon toward which Columbus sailed, with his mutinous crew—the brink of the world, as it was then thought to be. The vanishing point, if contemplated steadily, can induce a feeling of metaphysical giddiness, for this point is precisely the center at which the picture ought to disappear, a zero exerting on the "solid" realities of the canvas a potent attraction, as though it would suck the whole—old, young, maidens, women, small dogs, sheep, buildings, provinces down the funnel of its own nothingness. That is, the very fulcrum on which the picture rests, the organizing principle of its apparent stability, is at the same time the site at which the picture dissolves. (McCarthy, 1959:49)

Prints of the Four Kings of Canada employed perspective. Earlier prints may have done so as well. Iroquois artists did not immediately imitate this technique, but my guess is it fascinated them when they first saw it. It may have conferred a special quality on the figures portrayed.

Conclusions

Protohistoric Iroquois saw a wide variety of European images on coins, medallions, banners, escutcheons, perhaps porcelains, and even embroideries, and recycled some of them. Not all came through friendly trade. One Seneca grave contained a guidon. Back from battle, Europeans and Indians alike exhibited trophies and copied one another's clothing.

Recently an archaeologist, scanning a seventeenth-century Seneca site with an improved metal detector, recovered 29 lead seals used to safeguard and identify shipments.¹⁰ These bear the markings of Dutch, French, English, and possibly other depots. Clearly trade goods arrived from many sources. Piracy, on sea and land, surely contributed to this complexity.

I think the Iroquois recycled many European images. Hamell found successive prototypes for the Bundle of Arrows motif, a very ancient symbol of union (strength of union). Seventeenth-century Iroquois borrowed this symbol from the Dutch, just as the Dutch borrowed it from earlier times (Hamell, pers. comm., 25 Mar 1996).

Few artists, even great artists, view the world with pristine eyes. Most recycle. In the Marquesan Islands, Paul Gauguin visited the Taipi Valley, set up his easel before a massive, ancient sculpture, then copied a sketch of that sculpture printed in a travel book he had taken along. He improved on that sketch but retained several of its errors.

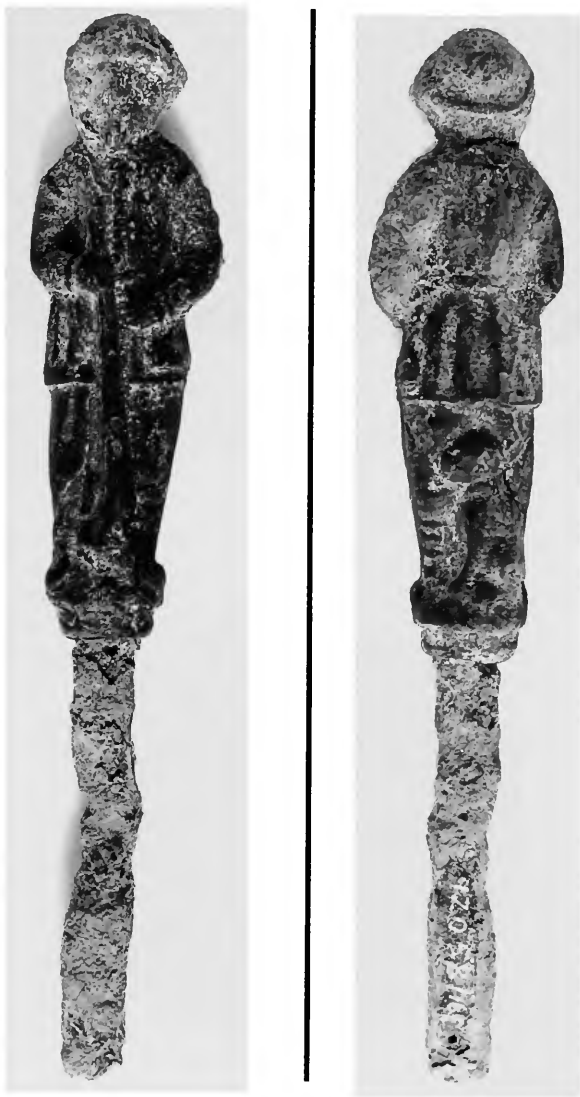


FIGURE 7.—Cast-iron handle with pewter shaft, Fox-Voorhees site, Ontario County, New York. Rochester Museum and Science Center (cat. no. 12058/161). Courtesy of the Rochester Museum and Science Center, Rochester, New York. Photograph by Paul Porell.

We may never know what inspired the Iroquois to carve images of a modest maid. One study concluded that Seneca examples symbolized witchcraft, not modesty, and power, not chastity (Mathews, 1980). I prefer to imagine that somehow, by means unknown to me, Eve, perhaps even the Virgin herself, appeared among the Iroquois, bringing solace and comfort to mothers in days of sorrow and anxiety.

Notes

1. I am indebted to François-Marc Gagnon, Montreal, pers. comm., 21 Jul 1996, for both examples. See Gagnon (1975, 1984) and Martin (1989).
2. Catalog no. 3962/28, Rochester Museum and Science Center, Rochester, New York (hereafter RMSC).
3. "In all probability," wrote Noble (1994:32), "a few Jesuit artifacts (nine to date) from historic Neutralia represent items distributed by proselytizing Huron converts in 1642 or shortly afterwards." The brass frame measures about 8 x 8 cm. Its design resembles those used for religious pictures or icons.
4. New York Colonial Manuscripts 58:4, New York State Archives and Records Administration, Albany, New York.

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5. Watercolor portrait of Meriwether Lewis by C.B.J. Fevret de Saint-Mémin, 1807. Missouri Historical Society, St. Louis, Missouri.

6. *The Black Drink*, oil portrait of Osceola, Seminole chief, by George Catlin, 1838. National Portrait Gallery, Smithsonian Institution, Washington, D.C. See Wickman, 1991:100-120.

7. Feest, 1968: cover.

8. Hamell and John (1987:23) identify that Seneca village as probably Gandonagan (1677-1687). On 5 Sep 1687, Robert Livingston wrote to Governor Dongan: "The french of Canida seem to be much incensed at a picture which they found in the Sinnekes country made by us as they say, viz: one a horse-back the horse has an ax in his mouth and under his belly abundance of Ropes, two Indians smoaking together and an Eagle between them. The man on horse-back is Arneut [Arneut Cornelisse Viele], bidding the Sinnekes to kill the french, the ropes is to tye the french prisoners. The two Indians are the Sinnekes and Cayouges united to war with he french, the Eagle is the Onnondages flyeing to and again and is not fixed with whom to joyn" (Brodhead, 1853:481). Here, and in a dozen other ways throughout the text, I am indebted to George Hamell.

9. Catalog no. 156/27, RMSC.

10. Beale site, a small Seneca village (ca. 1670-1687), Ontario County, New York, catalog nos. 12368-12383/98, 15331-15345/98, RMSC.

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Quilled Knife Cases from Northeastern North America

Christian F. Feest

Ethnographic museums as places of educational displays often struggle with the interpretation of the artifacts they preserve, since although ethnographic museums are primarily archives of cultural documents, their role as places for material culture research is all too often sadly neglected. One of the recurrent problems in working with historically collected material is the fact that the forms, functions, and meanings of artifacts from periods significantly predating modern ethnographic fieldwork often cannot be interpreted in terms of the standard ethnographic accounts.

Knife cases, for example, suspended from the neck or attached to the belt to hold hunting or scalping knives, were no longer used by the Iroquois when Lewis Henry Morgan (1851) published his classic account of Iroquois culture. Scalping had ceased with the end of armed conflicts, hunting had lost much of its former importance for the reservation communities, and clothing styles had changed to an extent that made neck-worn knife cases an unlikely dress item. Nonetheless, Morgan was able to collect two “scalping” knives for the New York State Cabinet of Natural History (now the New York State Museum, Albany, New York), which were listed among the museum’s acquisitions but were not dealt with in Morgan’s accompanying report (Tooker, 1994:102, 279), but he did not collect or describe any knife cases. Even quillwork, which is found on virtually all knife cases from northeastern North America, is noted only in passing as having given way to beadwork (Morgan, 1851:384, cp. 360).

The present essay reviews the written and pictorial sources as well as the material evidence for quilled knife cases in the Northeast in order to illustrate the importance of artifacts for our knowledge of the historical ethnography of this region.

Construction and Terminology

Knife cases are pouches with a specific function, which determines their basic shape. They are usually made of one or two pieces of leather or rawhide folded and/or sewn to produce a long, flat sheath that encases the knife blade. Folded knife cases (often made of rawhide) are asymmetrical, with a straight

edge produced by the fold and a curved side sewn to accommodate the knife’s cutting edge. Leather cases are usually made of two pieces (panels) sewn together along both edges and at the bottom, which may be pointed, rounded, or squarely cut off. Often the back panel projects beyond the front wall of the knife case to form a protective layer between the knife handle and the body of the wearer. These projecting back panels come in a variety of distinctive shapes, ranging from square to rounded and from straight to flaring; many have “ears” (i.e., two rounded or pointed projections). The ears commonly serve as points of attachment for carrying strings or straps. The upper edge of the front panel is sometimes reinforced by a strip (cuff), either a folded-down flap of the wall itself or a separate piece of leather.

The reverse of knife cases (whether suspended from the neck or attached to a belt) always faced the wearer’s body, so decoration was limited to the obverse and the edges. Discrete decorative areas are formed by the front panel, the cuff (which is sometimes treated as a decorative unit even in the absence of a structural reinforcement), and the projecting back panel. Fringes may be attached to the edges or to the lower edge of the cuff. A wide variety of quillwork techniques (including appliqué, embroidery, plaiting, weaving, and wrapping) was used to decorate knife cases from northeastern North America, but none of them is limited to this artifact type (e.g., Orchard, 1971; Feest, 1980:118–119, 138–141, 154–155).

Written and Pictorial Evidence

The earliest unequivocal written reference to Native American knife cases so far located appears in Peter Kalm’s *Travels*, where the traveler described a group of Mohawks(?) encountered on 23 June 1749 north of Albany, New York: “Round their necks were ribbons from which bags hung down to their breast, containing a knife” (Kalm, 1987:354). A few years later, a French observer reported from Canada that the local native people when going to war usually carried three knife cases, “one hanging on a chain from the neck, one stuck into the belt and the third along the leg on the outside of the garter”¹ (J.-C.B., 1978:181). In his *History of the American Indians*, written in the 1760s, James Adair, an English trader among the Cherokee, Catawba, and Chickasaw since 1735, included a generalized discussion of the practice of scalping in which he noted that “the barbarous artists speedily draw their long sharp-

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pointed scalping knife out of a sheath from their breast" (Adair, 1930:415–416).

In 1757 the Jesuit missionary to the St. Francis Abenaki witnessed an intertribal war dance of the allies of the French in which the warriors were wearing "a large knife suspended over the breast"²² (Thwaites, 1900:96). At about the same time, Major Robert Rogers (1765:228) reported that "that horrid weapon, the scalping-knife hangs by a string which goes around their necks," but like the Jesuit father he did not explicitly mention a knife case. Thomas Anburey, a British officer stationed in the St. Lawrence Valley at the time of the American Revolution, also omitted specific reference to the knife case in his description of scalping, reporting that "they... twist their left hand in the hair, ... and with the other hand draw their scalping knife from their breast, which is always in good order, for this cruel purpose" (Anburey, 1789:399). A Brunswick mercenary stationed in the same region at the same time more specifically reported the use of pouches made of the "fur of wildcats" worn on the breast and used as knife cases (Schlözer, 1779:300).

To this can be added for the late eighteenth century some descriptions supplied by field collectors of knives and knife cases. In 1778 for example, Major August Wilhelm Du Roi, another Brunswick mercenary stationed at St. Charles, on the Chambly River, collected from a local Indian (presumably either Mohawk, Huron, or Abenaki) "a knife with a sheath, which hangs from the neck and is worn in front of the breast."²³ And an undated Latin label from about the same period, on a knife case now in a German museum, describes the object as a "sheath, which the Iroquois Indians have suspended from their neck, containing a knife with which they cut and tear away the scalps of the vanquished as a trophy of war, made from porcupine quills."²⁴

Neck-worn knife cases may have appeared exotic enough to have been noted at least occasionally. Belt-worn knives or knife cases, on the other hand, are hardly considered at all, although they do appear in illustrations. A report from the eastern Great Lakes region (Weld, 1807: letter 35) appears to be one of the earliest written accounts of belt-worn knives; Frederick Baraga's (1837:61) description of belt-worn knife cases probably referred to the Ottawa and/or Chippewa, and about the same time their use was observed among the southwestern Chippewa (Nicollet, 1970:162). Much later, Penobscot knife cases were described as plain and attached to the belt (Speck, 1940:129).

Pictures of native people wearing knives or knife cases also began to appear shortly before the middle of the eighteenth century. The earliest known European illustration of a Native American knife case is seen in an anonymous French drawing (ca. 1730) of a Fox warrior wearing a small, simple knife case on his chest (Peyser, 1989:82). An early and rather isolated depiction of a South American asymmetrical neck-worn knife case appears in an illustration in André Thevet's *Singularitez de la France Antarctique* (Thevet, 1558:101 recto). Based on the appearance of the blade and of the handle, the knife could have been a European metal knife, which suggests the possibility of an independent case of parallel adaptation or diffusion to

South and North America from a European (most likely French) source. Nothing, however, appears to be known about early modern European neck-worn knife cases. In 1759 George Townshend, General Wolfe's successor as commander of the British army in Canada, made several drawings of an Indian (once identified as "of y^e Outewas Tribe" (Honour, 1975:128)) with a knife case hanging from his neck. On the monument erected in Westminster Abbey in 1761 for Townshend's brother Roger, two Indians are wearing clearly defined neck-worn knife cases, which are perhaps based on artifacts in George Townshend's collection (Figure 1; see also Honour, 1975: 128–129, figs. 119–120).⁵

The oil painting *Savage Warrior Taking Leave of His Family* (ca. 1760), by Benjamin West, includes the image of a rather small knife case suspended from the warrior's neck by means of a narrow band with beaded edges; the sheath itself lacks a projecting back panel and is bordered with tufts of deer hair. A laterally reversed engraving after this painting was published in Italy in 1763 (Erffa and Staley, 1986:57, 420–421, no. 452). The same knife case reappears in West's *General Johnson Saving a Wounded French Officer from the Tomahawk of a North American Indian* (ca. 1764–1768) (Erffa and Staley, 1986:210–211, no. 92). Another sheath, again rather small and simple, is more hidden than shown by West both in *The Death of General Wolfe* (1770) and in the slightly earlier portrait (ca. 1767–1770) now thought to depict Sir William Johnson, Bt., rather than Guy Johnson; it is at least possible that this is the knife case once belonging to West and now in the Museum of Mankind in London (Erffa and Staley, 1986:58–59, 211–216, nos. 93–100, 523–525, no. 647; King, 1991:39, fig. 8).

A quilled knife case with tin cones, red-dyed deer hair below the top of the front panel, and a rounded extension of the back panel is depicted as being worn by "Sir John Caldwell as an Ojibwa Chief" (ca. 1782) (Honour, 1975:134, fig. 127; another version in Brasser, 1976: cover, 180). Caldwell's ethnographic collection was dispersed in the 1970s; if the knife case shown survives, its present whereabouts are unknown. What may be a neck-worn knife case appears in the engraving *Indian Warrior Entering his Wigwam with a Scalp*, representing an unspecified Indian as seen by Thomas Anburey (1789: facing 291), whose description of scalping has already been noted. Like the one illustrated by West, the sheath is small and has lateral tufts of hair, but the knife looks almost like a clay pipe.

Jonathan Carver's drawing (ca. 1770) of a Fox man wearing a triangular knife case with lateral fringes or tin cones was re-used in Carver's depiction of a Dakota warrior (or vice versa). The drawings of the knife cases were probably done from memory as the earliest versions by Carver show only a double-edged dagger with a triangular blade (Carver, 1976:85, 96–97).

Belt-worn knives with or without cases are depicted in the eighteenth and early nineteenth century both from the Southeast (e.g., Fundaburk, 1958:110, figs. 113, 115) and from the Northeast. Some knives are shown stuck behind the belt (e.g., Phillips, 1984:87, 92, figs. II, 2, 3); otherwise, knife sheaths are suspended from it (e.g. Phillips, 1984:90, 92, figs. I, 2, II, 6).



FIGURE 1.—“An Indian dress’d for war with a scalp.” Drawing presumably illustrating an Ottawa wearing a knife case, by George Townshend, 1759. Picture Library, National Portrait Gallery, London (reg. no. 4855(69)). Courtesy of the National Portrait Gallery, London.

Few details of the construction or decoration of belt-worn knife cases are discernible in these depictions. The best illustration is supplied by late eighteenth-century dolls mostly made by French-Canadian nuns to represent Hurons of Lorette or Abenakis, which show simple, asymmetrical knife cases suspended from the belt (e.g., Benndorf and Speyer, 1968:62, 74, nos. 69 and 106, pls. 29, 41; Phillips, 1984:80, nos. 85–87; cp. Phillips and Idiens, 1994:24–26, 33: notes 19–20, fig. 1).⁶

In addition to contextualized depictions, visual representations of knife cases also include pictures of actual specimens in collections. In view of the widespread use by artists of props from their own collections (as in the case of Benjamin West), this distinction between contextualized depictions and representations of artifacts from collections is not a sharp one. The earliest such images are among the drawings produced in the 1780s by the amateur artist Sarah Stone of artifacts in the collection of Sir Ashton Lever (King, 1993:33, fig. 1), where two types of knife cases are represented. A different type of case figures both among the 1816–1817 drawings of Charles Hamilton Smith, a British spy (King, 1994:59, fig. 1), and in the lithographs illustrating the English edition of Giacomo Costan-

tino Beltrami’s travel account, where it is identified as of “Cypowais” (Chippewa) origin (Beltrami, 1828, pl. 1: fig. 3; cp. Feest and Kasprzycki, 2001). Yet another type of quilled knife case is found in Seth Eastman’s illustrations for Henry Rowe Schoolcraft’s monumental *History, Conditions and Prospects of the Indian Tribes*. In general, these drawings not only adhere rather closely to the actual objects they were made to represent, but they share with them the lack of information on use and provenance. Of three quite different specimens illustrated, all that Schoolcraft (1852:515, pl. 77) said was, “His knife sheath...is ingeniously ornamented.”

Meager as the results of this survey are, they are interesting because they suggest that the origin of knife cases in northeastern North America may have occurred during the first half of the eighteenth century as a result of the increased use of metal knives. Since neck-worn tobacco pouches or pipe bags are described and illustrated as early as the seventeenth century (e.g., Swan, 1973:243–244, figs. 115–116; Williams, 1973:187), there is no good reason why knife cases should not similarly have been reported had they been part of the Native American accoutrement at that time. Additional support for this view may

be found in the linguistic evidence. David Zeisberger's (1887) manuscript *Indian Dictionary; English, German, Iroquois—the Onondaga, and Algonquin—the Delaware*, compiled in the second half of the eighteenth century, is the earliest source citing a Native American term for "knife case." He supplied an Onondaga term for this artifact but gave no Delaware term (Zeisberger, 1887:171). Algonquian words for "knife case" recorded in the nineteenth and twentieth centuries provide no indication of an ancient, common origin, and some are obviously newly coined words (e.g., Baraga, 1878–1880(1):151, (2):357; Lemoine, 1901:135; Trumbull, 1903:121; Skinner, 1921:141; Uhlenbeck and van Gulik, 1930:119; Speck, 1940:129; Northern Cheyenne Language and Culture Center, 1976:60; Bloomfield, 1984:222; Goddard, 1994:251).

Although the written and pictorial evidence for northeastern neck-worn knife cases in the period between 1750 and 1800 focuses on the Iroquois, Ottawa, and Fox, most of the information for the nineteenth century relates to the Menominee. The only author of this period actually describing neck-worn knife cases was the Swiss Capuchin missionary Antoine-Marie Gachet, who at one point noted that the men wore on their breasts both beaded or quilled tobacco pouches and "a case ornamented in the same manner, into which he slips his large knife or scalpel" (Gachet, 1890:270). On another occasion, Gachet spoke of Chief Iometah, who "wore his great knife in a sheath suspended from his neck" (Gachet, 1890:149).⁷ This fleeting reference is appropriate inasmuch as Iometah was one of several Menominees portrayed with a neck-worn knife case. Thus were painted Machekakat and Makometa by James Otto Lewis in 1827, Iometah by George Catlin in 1831, and Kitcheogimaw by Paul Kane in 1845, and there is a daguerreotype from before 1856 showing Chief Oshkosh, as well as a wood engraving apparently based upon another take from the same session (Figure 2; Kasprzycki, 1990:67–71, 79–80, 94, 103–104, figs. 1–7, 21, 50, 64, 65). Of these artists, only Kane provided a realistic and detailed image of a knife case. The daguerreotype is lacking in clarity and resolution, and the engraving based upon it used a great deal of artistic license in interpreting the photograph.

Thus, the documentary evidence from the first half of the nineteenth century is clearly sufficient to prove that Menominee men wore different types of knife cases suspended from their necks. In contrast, the striking absence of pictorial and written evidence for knife cases among other tribes in the western Great Lakes region during the same period is highly surprising. Yet it remains a remarkable fact that among all the native peoples from the Great Lakes region and the adjoining eastern Prairies shown on the lithographs published by Lewis (1835–1836) and by McKenney and Hall (1838–1844), as well as on the paintings by Catlin (Truettner, 1979), Kane (Kane, 1971), and the other artists depicting American Indians in the nineteenth century, only Menominees are seen wearing knife cases suspended from their necks—with one exception discussed below. Similarly, knife cases are discussed in Alanson Skinner's (1921:127–128) monograph, *Material Culture of the Menomini*, but they are not even mentioned in the accounts by

Skinner and others of the Prairie Potawatomi, Sauk, Fox, Winnebago, or Chippewa. This may be an indication that most Native American groups in the Great Lakes region had abandoned the use of neck-worn knife cases significantly earlier than the Menominee had.

The exception to the apparent Menominee monopoly on neck-worn knife cases in nineteenth-century illustrations is provided by a recently discovered drawing by the German artist Adolf Hoeffler, which shows a double knife case worn by the Dakota chief Bad Hail, a headman of Wabasha's Village, drawn near Fort Snelling in 1852 (Figure 3; see also Andreas, 1981:145, fig. 32). The double knife sheath clearly carries a stylized floral decoration, which would not instantly be recognized as typical for the Dakota. It is likely that even this knife case had ultimately been obtained from some of the Dakota's eastern neighbors, such as the Menominee.

Material Evidence

A survey of Native North American artifacts documented in European collections before 1750 has disclosed no convincing evidence for the presence of knife cases. A knife (apparently without a sheath) was obtained by Ralph Thoresby of Leeds from the three Mohawks among the "Four Kings of Canada" visiting London in 1710, and there is no record of knife cases in collection catalogs of that period (Feest, 1992:82). A knife with a quilled sheath is said to have been collected in 1697 by Pierre le Moyne, sieur d'Iberville, the founder of French Louisiana, for his cousin Le Moyne de Martigny (Vitart, 1980:131), but neither an illustration nor any supportive documentation has ever been published. More recently, an unusual type of quilled knife case has surfaced in Besançon in association with eighteenth-century Iroquois and Mississippi Valley material, which so far can be traced back only to 1853 (Lagrange and Dubois, 1992:111, no. 241).

Quilled knife cases from northeastern North America preserved in European and American collections would thus appear to date from the middle of the eighteenth to the middle of the nineteenth century. In preparing this report, almost 100 knife cases, published and unpublished, from this region and period were studied (references for these are provided largely in the end notes). Less than one-third of these have a documented history before 1850, and less than 10% of them have reasonably well-documented provenances, including the Iroquois, Huron of Lorette, Ottawa, Menominee, and Winnebago. It is estimated that this sample exceeds well over 50% of the knife cases from northeastern North America that have been preserved in collections. This leaves some hope that additional material yet to be studied will further our understanding by adding more dated and/or provenanced examples.

Various types of quilled knife cases occur all over an area ranging from the northeastern United States to the western Canadian Subarctic. Like other pouches of this region, knife cases were either suspended from the neck or attached to a belt. Unlike other pouches, no examples worn with bandoleer-



FIGURE 2.—Chief Oshkosh wearing a double knife case. Daguerreotype by James F. Harrison, before 1856. Oshkosh Public Museum, Oshkosh, Wisconsin (cat. no. 141.11). Courtesy of the Oshkosh Public Museum (neg. no. 1455).

type shoulder straps are known, and an eighteenth-century French source is unique in reporting knives stuck behind the garters (J.-C.B., 1978:181). The distinction between symmetrical and asymmetrical shapes of knife cases in part corresponds to the manner in which they were worn.⁸ All asymmetrical cases were belt-worn, whereas all neck-worn cases were symmetrical; symmetrical, belt-worn knife cases are known to have existed and could possibly be distinguished from neck-worn examples by their shorter straps. Symmetry thus pro-

vides a useful primary distinction, but decorative techniques and patterns, rather than outline shapes, generally produce better results on lower taxonomic levels and are therefore used in the following typological survey. Due to the small size and random nature of the sample, some of the proposed types are highly tentative; types are summarized in Table 1, which follows the typological survey. At present, it is not known whether these types represent regional, ethnic, local, family, or personal styles or fashions.



FIGURE 3.—“Bad Hail, Chief of the Sioux nation Minnesota Territory October 1852.” Part of a pencil and watercolor drawing by Adolph Hoeffler. Privately owned. Courtesy of Kunsthandel J.P. Schneider Jr., Frankfurt.

Typology

SYMMETRICAL KNIFE CASES

Woven-Quillwork Group

Because woven quillwork has a wide distribution in the central and western Subarctic, sheaths decorated in this technique are sometimes misattributed to the western Subarctic. Thus, for example, all Northeast knife cases with woven quillwork are misidentified as Chipewyan in the catalog of the Smithsonian Institution’s National Museum of the American Indian (NMAI), New York. Truly Subarctic examples with woven quillwork include a type thought to be of Red River Ojibwa or Plains Cree

origin. These are much larger than the Northeast type; have more complex designs, cuffs with woven quillwork, and fringes at the top of the front panel; and lack the lateral quill appliqué typical for the Northeast.⁹

NO CUFF.—Like the Subarctic types, the Northeast examples are almost parallel-sided to slightly tapering in shape and have a projecting back panel. Their front is decorated with a strip of woven quillwork flanked by quill appliqué, but they lack a cuff. One type is made of black-dyed buckskin and has a bilobed top on which outcurving double curves in moosehair embroidery are framed by moosehair lines. Quill colors are orange, blue, and white, and the woven pattern consists of linked diamonds. The average length of this type is 21 cm. None of the pieces has a documented provenance.¹⁰ A specimen in the Museum für Völkerkunde, Basel (Figure 4), was acquired by Lukas Vischer in 1825 from a dealer in Quebec. On purely stylistic grounds, a Lorette Huron origin of the type is likely. Another example, more distantly related and equally undocumented, is made of brown buckskin and has moosehair and quill appliqué on the projecting back panel. It features a woven design of single and double diamonds as well as a pair of facing triangles, and the quillwork (black, orange, blue, yellow, white) includes triangle bands in overlay.¹¹

CUFF WITH QUILL APPLIQUÉ.—A second type is almost parallel-sided, with a square lower end and a slightly bilobed top. As in the first type, the woven design consists of linked diamonds and is flanked by line and overlay band-quill appliqué. A cuff above the woven strip is also decorated with quill appliqué. In addition to the black, red, yellow, and white quillwork, white glass beads edge the knife case. Carrying straps are made of multiple quill-wrapped leather thongs. Associated knives have handles wrapped with quill-plaited strings. Lengths are within the range of the first type.¹²

Transitional forms with stylistic links to this second type are represented among drawings of knife cases from the no longer extant Leverian Museum (King, 1993:33, fig. 1) and among the specimens in the NMAI (e.g., cat. no. 19/6340). None of these sheaths is documented, but the second type is obviously related to the first one and may have the same or a similar provenance. The combination of woven quillwork and delicate overlay quill appliqué suggests an Algonquin origin (cp. Feest, 1968:42–43, fig. iii/18, pl. 1).

CUFF WITH WOVEN QUILLWORK.—A third type of similar size tapers to a rounded lower end and has a pointed, bilobed top. As on the Subarctic examples, the cuff is decorated with woven quillwork. Designs are made up of single diamonds and/or chevrons on the front panel and of triangles on the cuff. The woven panel is flanked by (wavy) line quill appliqué; edging is also in quillwork. Quill colors are red, blue, yellow, and white. Neither of the two known examples of this type is documented, and both have been attributed to the Subarctic, although they are closer to the previously described Northeast types.¹³ Distantly related may be an otherwise rather unique item in the Museum für Völkerkunde, Berlin. Said to be from an “old col-

lection," it was probably obtained at the 1819 sale of Bullock's Museum in London. The cuff features woven quillwork with the initials TM and the figure 1811. It has no documented provenance, but it has been attributed to "Algonquians north of the Great Lakes" (Krickeberg, 1954:130, table 30c).

Another unique knife case with woven quillwork, from the Arthur Speyer collection in the Canadian Museum of Civilization, Quebec,¹⁴ has a trapezoidal top and tapers to a squarely cut-off lower end. Its cuff is decorated with appliqué quillwork, and the woven design consists of pairs of triangles facing a vertical line. It has a neck strap of the netted-fringe type. Quill colors are green, blue, yellow, and white. It has been attributed to either the "Ojibwa, c. 1800" or the "Eastern Great Lakes, before 1800." In view of the similarities with central Subarctic woven quillwork and the more westerly distribution of netted fringes, the Ojibwa attribution is a little more likely.

Netted-Fringe Group

This group is represented by only two undocumented examples, both of which are of tapering shape with a rounded lower end, a projecting back panel, and a cuff at the top of the front panel. The front itself is covered with a netted fringe consisting of quills wrapped around alternate pairs of threads to form a pattern. The designs are composed of triangles.¹⁵ Quill colors and overall length, as well as other features, differ considerably. Attributions include "Red River Ojibwa, before 1840," "Northern Ojibwa type, c. 1800," and "Western Great Lakes, before 1880." Netted fringes have a wide distribution, especially in the western Great Lakes region and further to the west and north; they also occur as decorations on cuffs in the quill-appliqué group, described below.

Quill-Embroidered Group

The only known example of this group tapers in outline and has a rounded lower end, a projecting back panel, and a cuff at the top of the front panel (Figure 5). Both front and cuff are reinforced with birch bark embroidered with porcupine quills in the manner of bark boxes (Feest, 1968:43–44, no. 19, table 2b). It probably was collected by Johann Georg Schwarz, an Austrian fur trader and diplomat, in 1820–1821 among the "Alegonk" (Algonquin). It has a short, commercially woven carrying strap.¹⁶

Two-Lane Quill-Wrapped Group

This group is represented by 17 specimens and one early drawing of an actual specimen, all of them of tapering shape with a pointed, rounded, or squarely cut-off lower end. The front is decorated with two narrow, quill-wrapped slats of wood, bark, rawhide(?), or cardboard(!), with additional quills interwoven to form plaited geometric designs. The three to five colors of the quills always include red (used only for wrapping); white is

used primarily for plaiting (but also for wrapping), black is employed mostly for wrapping, yellow is the major fourth color, and green occurs rarely (and only in wrapping). Designs include diamonds, chevrons, X-shapes, and hourglasses. Since more than one color is used for wrapping, all specimens feature horizontal stripes arranged in various patterns (Figure 6).

A common feature of this group is the use of pairs of tin cones with (mostly red) deer hair symmetrically attached to the sides of the knife cases; sometimes the pairs are replaced by single cones. Carrying straps are generally made of a pair of strings of quills plaited over two threads. Knives have often been preserved, most of them with quill-decorated handles (mostly wrapped, some also plaited). Lengths of two-lane quill-wrapped knife cases range from 18 to 30 cm, with an average length of 21 cm (see also Feest, 1987:292–296). Despite the rather large number of specimens, no clear types are discernible: outline shapes and specific decorative details, which include white glass bead edging of two styles, appear to combine freely.¹⁷ Two of the specimens of this group stand somewhat apart. One of them has only one wide rather than two narrow quill-wrapped splints (Phillips and Idiens, 1994:28–29, fig. 6), and in the other one (identified as the most recent example of this group by the violet bead appliqué on the cuff), the quills are wrapped but are not plaited (Feder, 1965, no. 27e).

This group has commonly been attributed to the Iroquois in general or to the Mohawk in particular. Evidence for this attribution is poor and is ultimately based on an old, undated label on one of the specimens, whose German inscription reads: "Scalper knife of the Iroquois...—Scalping knife of the Chiroquois"¹⁸ (Dräger et al., 1992:64). The history of the dubious Mohawk or Iroquois attribution has been discussed by Feest (1987:296), yet the label continues to be used without new or convincing proof (see Phillips in Harrison, 1987:50, no. W62; Phillips and Idiens, 1994:29, fig. 6; cp. Zender and Dale, 1995:109–110). Attributions to the Ojibwa (National Museum of Scotland), Delaware (NMAI), or the Upper Missouri (National Museum of Natural History (NMNH), Smithsonian Institution) are not based upon documentary or comparative evidence.

The use of birch-bark slats in most of the known examples should be regarded as a strong indication of an Algonquian origin. Quill-plaiting, the principal technique employed in this group, has a wide distribution from the Canadian Maritimes to the western Subarctic and cannot be regarded as especially characteristic of Iroquois work, although it may have occurred there as well (Feest, 1987:295–296, including note 62).¹⁹

Information on collection dates provides a clue as to where the collecting took place: the earlier the recorded dates, the more likely would be an eastern Great Lakes provenance. Unfortunately, few of the pieces can be dated reliably. The Townshend Monument of 1761 in Westminster Abbey and Sarah Stone's drawing of a two-lane quill-wrapped knife made during the 1780s provide the earliest secure dates (Honour, 1975:128–129, figs. 119–120; King, 1993:33, fig. 1). Published



FIGURE 4.—Knife case with decoration of woven and appliqué quillwork and moosehair purchased in 1825 in Quebec by Lukas Vischer. Museum für Völkerkunde, Basel (cat. no. 1Va39). Length 22.5 cm. Photograph by Christian F. Feest.

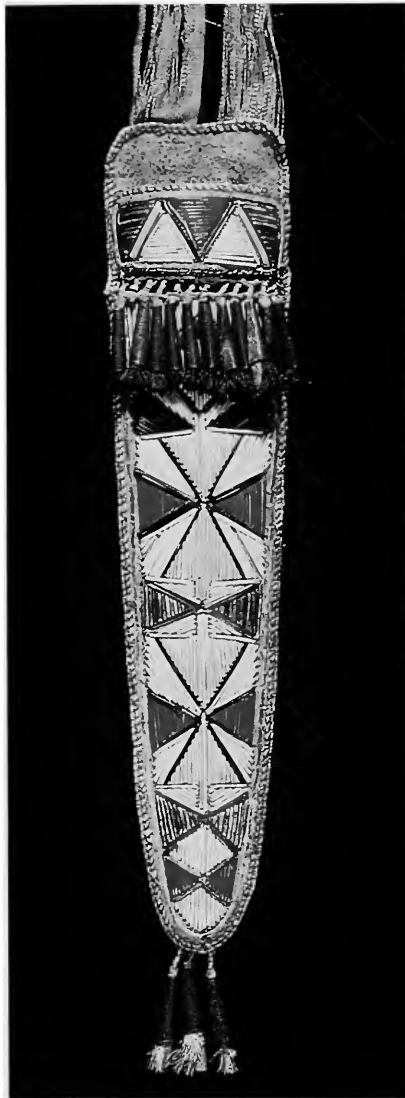


FIGURE 5.—Knife case with porcupine quill embroidery on birch-bark reinforcement. Probably collected by Johann Georg Schwarz in 1820–1821 and identified as “Alegonk” (Algonquin). Museum für Völkerkunde, Vienna (cat. no. 11965). Length 18 cm. Photograph by Christian F. Feest.

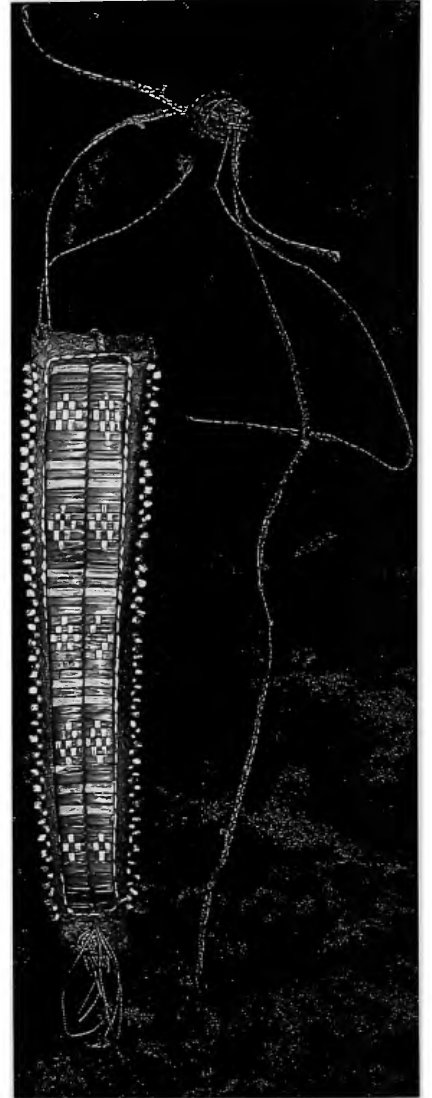


FIGURE 6.—Knife case with two-lane quill-wrapped panel and “zipper” edging in white glass beads. Undocumented (eastern or northern Great Lakes region, ca. 1800). National Museum of Scotland, Edinburgh (cat. no. 1956.664; ex National Museum of Antiquities of Scotland). Photograph by Christian F. Feest.

earlier dates are speculative and are at best suggestive, while stylistic considerations do not preclude an early nineteenth-century date of manufacture. The fact that almost all of the knife cases in this group were once or are still in European repositories is, however, indicative of a relatively early collection date. Less than half of the double knife cases referred to below have a European collection history; documented collection dates begin around 1820, or 60 years after the earliest pictorial evidence (Feest and Kasprzycki, 2001).

Quill-Appliqué Group

Among Northeast knife sheaths decorated with appliqué quillwork, those made to hold two knives rather than one clearly stand apart from the rest. Both single and double knife cases can further be distinguished according to the symmetry or asymmetry of their basic design.

SINGLE KNIFE CASES.—The sample includes about 40 knife cases with appliqué quillwork from the Northeast (including one nineteenth-century drawing based on an actual specimen),

of which nine have documented collection dates before 1850. Almost all of their provenances (Iroquois, Huron, "Huron or Cherokee," Delaware, Menominee, Ottawa, Chippewa/Ojibwa, Cree, Dakota) are based on secondary attribution, but at least one Ottawa, one Menominee, and one Winnebago sheath are rather reliably documented.

Available measurements range from 20 to 28 cm in length (with an average length of 24.5 cm). Outline shapes are generally similar to those described above, but there are also several cases with a concave upper edge, and an additional bilobed type may also be distinguished. Bilobed specimens slightly outnumber the rest. There is only a partial correlation between the shapes of the knife cases and their mostly symmetric decorative patterns. The main designs are usually made up of one, three, or five patterned triangle bands of quillwork running the length of the knife case and forming compact blocks, often in combination with straight or wavy linework; netted fringes occur on the cuff of some of the specimens. Most of the proposed subtypes are represented by just a few examples, and several of the knife cases studied may be considered unique.

One Triangle Band: The most common design (with variations) consists of one patterned triangle band flanked by wavy lines, with either a piece of netted fringe or horizontal triangle bands on the cuff.²⁰ Variants lack the wavy line (Ewing, 1982:151, no. 124) or have more than one of them (Phillips, 1984:44, 82, no. 30).

Multiple Triangle Bands: One subtype has two or three vertical triangle bands flanked by wavy lines (Figure 7).²¹ None of these specimens has a reliable ethnic attribution, but several of them have documented eighteenth-century collection histories; the Du Roi knife case (see "Written and Pictorial Evidence," above) collected in 1778 within easy reach of Mohawks, Hurons, and Abenakis, would appear to place the subtype in the upper Saint Lawrence River region.²² Wavy lines framing the main design do, of course, also occur on sheaths in the woven-quillwork group and on the slats of the two-lane quill-wrapped group. The general pattern may be compared to a common style of Iroquois moccasins, in which the toe seam is covered by a triangle band of quillwork flanked by wavy lines (see Ewing, 1982:259–260).

Another subtype is decorated with three to seven triangle bands forming distinctive patterns. One subtype with five bands features a central line with pairs of facing lateral triangles (cp. the similar pattern in a unique, no-cuff woven-quill type, described above); both known examples have a cuff of netted-fringe quillwork and a neck strap in the same technique.²³ Other three- or five-band sheaths display the central line without the triangles or show crosses or horizontal lines.²⁴ Another subtype is characterized by a pattern of diagonal, indented black lines on orange, blue, and yellow backgrounds (Figure 8).²⁵ None of these pieces is reasonably well documented, and attributions have ranged widely, from Huron to Northern Ojibwa.

Shaped Triangle Bands: Other single knife cases have more or less complex patterns made up of shaped triangle bands; more than one style and provenance is represented in this subtype. Based on the similarity of its unusual lower end with an identical form illustrated for the Menominee,²⁶ a western Great Lakes origin may be suggested for a knife case in the Museo Nazionale di Antropologia ed Etnologia in Florence, collected before 1828 (Bushnell, 1906:249, pl. XXa). Affinities with designs on sheaths with woven quillwork and the use of moosehair in combination with porcupine quills suggest a possible Huron or upper Saint Lawrence River Valley origin for a knife case from the John Painter collection, Cincinnati, Ohio.²⁷

Asymmetrical Design: Only three of the known single knife cases with quill appliqué have asymmetrical designs, but they are too different from one another to be regarded as a separate subtype. All are from the western Great Lakes and Prairies region (Figure 9),²⁸ adjoining the distributions of asymmetrically shaped sheaths and double knife cases with asymmetrical designs.

DOUBLE KNIFE CASES.—The 16 presently known double knife cases have been discussed in detail by Feest and Kasprzycki (2001) (cp. Figures 2, 3). These knife cases are unusual not only in shape, but also in that a substantial number of them can be shown to have been collected among the Menominee and their neighbors in Wisconsin between ca. 1820 and 1850. Stylistic relationships exist between double knife cases with symmetrical and asymmetrical designs, but they also exist between double knife cases and single knife cases. A closer analysis of both single and double knife cases may in the long run help to clarify the distribution of styles and thus provide a safer ground for attributions.

ASYMMETRICAL KNIFE CASES

Belt-worn knife cases are best known from the Prairies and Plains. Most of them are asymmetrical, such as a distinctive Santee(?) Dakota type with a bird-quill decorated sheath and porcupine appliqué-quilled cuff,²⁹ related Dakota types with an undecorated or appliqué-quilled sheath and appliqué-quilled cuff,³⁰ or an Upper Missouri type with a quilled sheath and cuff.³¹ But there are also symmetrical types, such as a northern Plains group of sheaths that are sometimes referred to as Eastern Sioux,³² and transitional types with a symmetrical or asymmetrical sheath and asymmetrical fringes.³³ The distribution of Plains and Northeast asymmetrical belt-worn types may not be contiguous; similarities in overall construction appear to be based on shared Euroamerican models.

Floral Moosehair-Embroidery Type

One type of belt-worn knife cases from the Northeast has an asymmetrical sheath of black-dyed buckskin with a rectangular loop of leather sewn to its top, which allows attachment to the belt. Floral-design moosehair embroidery extends the



FIGURE 7.—Knife and knife case with appliqué quillwork. Undocumented. Merseyside County Museum, Liverpool (cat. no. M 12885; Joseph Mayer coll.). Length 20 cm. Photograph by Christian F. Feest.

length of the front and is flanked by quilled triangle bands; on the curved side of the sheath, metal cones with dyed hair are attached. The upper border of the front panel is edged with red ribbon. Quill and moosehair colors are orange, blue, yellow, and white. Lengths (without loop) range from 20 to 22 cm (Figure 10).³⁴ None of the specimens is reasonably well documented, but based on comparison with related pouches and moccasins the common attribution to “Huron [of Lorette] type” is certainly correct.

One example of a slightly variant type features moosehair-embroidered borders instead of the quill appliqué and red wool tassels instead of dyed hair.³⁵ It is not clear whether the in-

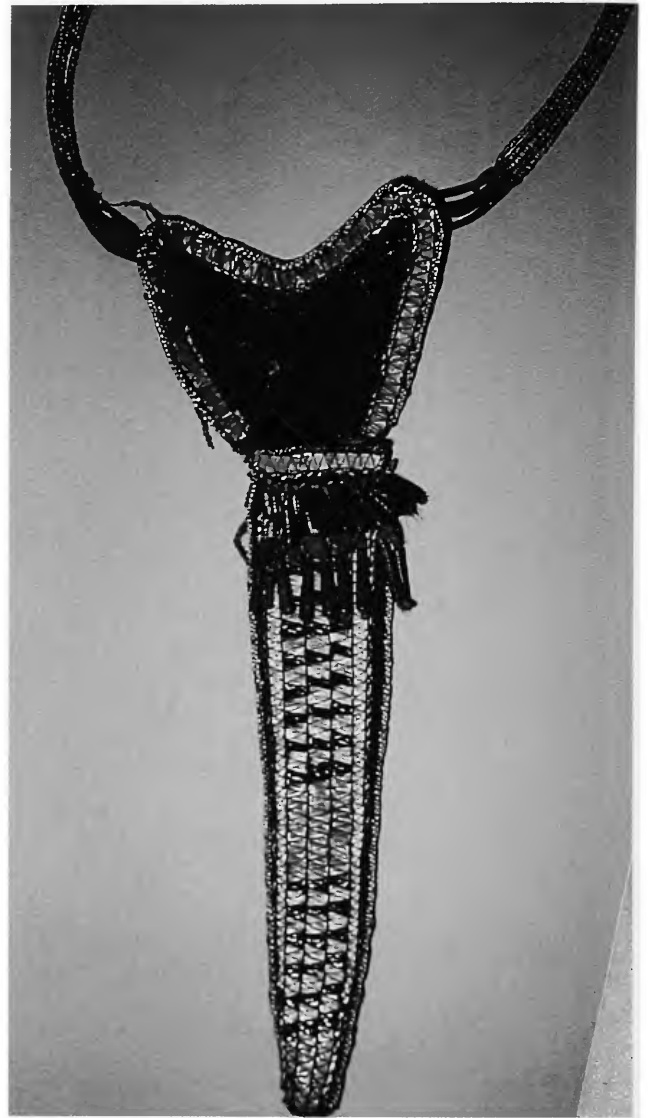


FIGURE 8.—Single knife case with appliqué quillwork. Undocumented. NMNH, Department of Anthropology (cat. no. 8654; A.J. Comfort collection). Length 27 cm. Photograph by Christian F. Feest.

scription “Lorette Inds. Quebec, Can.” on the birch-bark lining of this specimen represents a note by the collector or a later attribution.

Caribou-Skin Type

A second type of belt-worn knife case is made of a piece of caribou skin to which a moosehair-embroidered buckskin cuff has been added; below this cuff and along the curved edge of the knife case are tassels of beads, metal cones, and hair. The Average length is about 28 cm.³⁶ A Huron origin is generally assumed and is likely (but apparently is not documented). If the



FIGURE 9.—Single knife case with asymmetrical appliqué quillwork. Probably collected by Bishop Résé of Detroit, ca. 1830, among the Ottawa of Harbor Springs, Michigan. Museum für Völkerkunde, Vienna (cat. no. 11966; J.G. Schwarz collection). Length (without appendage) 26.5 cm. Photograph by Christian F. Feest.



FIGURE 10.—Belt-worn knife case with moosehair embroidery and porcupine quill appliqué. Undocumented (Huron of Lorette style, first half of nineteenth century). NMNH, Department of Anthropology (cat. no. T-7373). Length 21.8 cm. Photograph by Christian F. Feest.

attribution is correct, this type is probably later than the floral-design moosehair-embroidery type. A subtype, represented by two examples, both in the Museo Nazionale di Antropologia ed Etnologia in Florence (Bushnell, 1906:249, pl. 21b), is smaller in size and is made of a scaly beaver tail instead of caribou skin.

Reptile-Skin Type

A unique, asymmetrical sheath of reptile skin with a quilled buckskin cuff and quill-wrapped leather fringes along the

curved edge of the case was collected by R.B. Hough at Oka, Quebec, and accessioned by the Smithsonian Institution in 1878.³⁷

Conclusions

The present survey has attempted to demonstrate the importance of artifacts preserved in public and private collections for a historical ethnography of northeastern North America. Descriptions and depictions alone indicate hardly more than

TABLE 1.—Major types of Northeast knife cases and some of their relatives: distinctive features of outline shape and decoration. Unevenness of taxonomic levels reflects limitations of sources and need for further research.

A. Symmetrical	B. Asymmetrical
A.1 Woven quillwork flanked by quill appliqué	
A.1.1 No cuff (Lorette Huron)	B.1.1 Moosehair embroidery, no cuff (Lorette Huron)
A.1.2 Cuff with quill appliqué (Algonquin?)	B.1.1a Flanked by quill appliqué
A.1.3 Cuff with woven quillwork (Northern Great Lakes Algonquians?)	B.1.1b Flanked by moosehair embroidery
	B.1.2 Caribou skin (Lorette Huron?)
(Various types of symmetrical knife cases with woven quillwork (Subarctic (neck-worn) and Northern Plains (belt-worn))	Cuff with moosehair embroidery
A.2 Netted-fringe quillwork	B.1.3 Reptile skin (Oka)
Cuff with woven quillwork or quill appliqué	Cuff with quill appliqué
A.3 Quill embroidery on bark backing (Algonquin)	
Cuff with quill embroidery	(Various types of asymmetrical knife cases with quill appliqué (Dakota to Upper Missouri))
A.4 Quill-wrapped slats (Northern Great Lakes?)	
Lateral metal cones	
A.5 Quill appliqué	
A.5.1 Single case	
A.5.1.1 Triangle band flanked by wavy line(s) (Iroquois?)	
A.5.1.2 Patterned multiple triangle bands	
A.5.1.3 Shaped triangle bands	
A.5.1.4 Asymmetrical design (Western Great Lakes)	
A.5.2 Double case (Menominee and neighbors)	
A.5.2.1 Symmetrical design	
A.5.2.2 Asymmetrical design	

that knife cases were used. Although inspection of the material documents vastly increases our appreciation of the technical and stylistic diversity of knife cases, it is primarily the poor documentation of most early collections that accounts for the problems of tribal attribution (and thus cultural contextualization) of specific forms. It is fairly characteristic for historic collections of Native American material culture that a substantial portion of the knife cases that have been preserved were specifically made for sale. This is certainly true of the symmetrical knife cases with woven quillwork attributed to the Huron of Lorette and the belt-worn sheaths which presumably were also made there. It is equally characteristic that provenances of such items are more easily identifiable than are those made for actual use. Further research into the collection histories of artifacts and comparisons of undocumented with documented objects should help to approach a solution of the jigsaw puzzle representing the distribution of ethnic, local, and temporal styles.³⁸

The tentative conclusion regarding a post-European origin of knife cases in northeastern North America, on the other hand, calls for an investigation of the possible European models and their adaptation by native peoples of North America in the process of the cultural exchange known as acculturation.

Notes

I thank Sylvia Kasprzycki for allowing me to draw upon results of her unpublished work on Menominee historical ethnography and material culture. The help extended to me over the years by the staff of the various museums whose objects are discussed herein is herewith likewise acknowledged with gratitude.

Part of the present essay was itself part of a paper entitled "Material Evidence, Critical Reasoning, and the Identification of Styles: A (Knife) Case in Point," coauthored with Sylvia S. Kasprzycki and read at the symposium on American Indian Art at the Fenimore House Museum, Cooperstown, New York, 12 Jul 1995 (cp. Feest and Kasprzycki, 2001).

1. "un pendu au col en sautoir, un passé à la ceinture et le troisième le long de la jambe en dehors dans la jarrettière...."
2. "un grand couteau suspendu sur la poitrine...."
3. "Ein Messer mit der Scheide, welches an dem Hals gehängt und vor der Brust getragen wird." A copy of the original collection list, now lost, is in the catalog of the Braunschweigisches Landesmuseum, Brunswick.
4. "Vagina, quam collo suspensam habent Indi Iroquaeenses, continendo pugione quo secant & avellant capillitium derrotorum, belli trophaeum, ex pennis hystricis confecta." This label is attached to catalog number 3 of the ethnographic collection of the Hessisches Landesmuseum Darmstadt, which is on permanent loan to the Deutsches Ledermuseum, Offenbach.
5. The Indians on this monument served in part as models for an artist's reconstruction of a "Mohawk Warrior, 1750" in Taylor and Sturtevant (1991:230). The knife case is correctly interpreted as a two-lane quill-wrapped type (see under "Woven Quillwork Group"), but the colors do not match those found in the actual specimens preserved. In the accompanying text, the artifacts (including the knife case) are implicitly identified as of Mohawk origin, although it is equally possible that they were collected from other groups, such as the Ottawa (cp. Figure 1).
6. On some of these dolls, the knife case is suspended from the shoulder or neck by means of a rather long string. This may represent a mistake on the part of the nuns or it may represent a later repair.
7. The nineteenth-century Menominee practice is confirmed by Skinner's (1921:127-128) early twentieth-century ethnography.
8. Another factor is, of course, the shape of the knife, but asymmetrical knives and symmetrical daggers were carried in symmetrical knife cases.
9. See, for example, Orchard, 1971, pl. 14; King, 1982:19, fig. 7c; same, Brasser in Harrison, 1987:82, no. P51, and Acevedo et al., 1983:29, fig. 27; Vincent, 1995:43.

10. Examples are in the Museum für Völkerkunde, Basel, cat. no. 1Va39 (L. Vischer coll.); National Museum of the American Indian (NMAI), New York, G. Heye coll., cat. no. 10/9716; and in the British Museum, London (King, 1982:82, fig. 87).
11. NMAI, cat. no. 19/6339.
12. Orchard, 1971, pl. 29; Ewing, 1982:152, no. 125.
13. Boden, 1995:199, no. 701; NMAI, cat. no. 18/9432.
14. Benndorf and Speyer, 1968:92, no. 159, fig. 62; same, Brasser, 1976:100, no. 66.
15. Benndorf and Speyer, 1968:48, no. 35, fig. 15; same, Brasser, 1976:144, no. 142; Phillips, 1984:75, no. 57.
16. Two knife cases made completely of birch bark and ornamented with moosehair, now in the Städtisches Museum, Brunswick, were obviously intended for the late eighteenth-century tourist trade. They were collected by J.L. Unger, a Brunswick mercenary in the American Revolution. Although of a style usually associated with the Huron or Lorette, they were probably made by French-Canadian nuns (Phillips, 1991:20, fig. 2).
17. National Museum of Scotland, Edinburgh, cat. no. 1956.664; University of British Columbia Museum of Anthropology, Vancouver; NMAI, cat. no. 19/6356; Krickeberg, 1954:129, table 30a; Benndorf and Speyer, 1968:66, no. 83, fig. 31; same, Lyford, 1989:51; Völger, 1976: no. 4.50.20; Brasser, 1976:102; same, Lyford, 1989:50; Sotheby's, 1982: no. 286; same, Acevedo et al., 1983:28, fig. 25; Sotheby's, 1982: no. 288; Feest, 1987:292–296; Phillips in Harrison, 1987:50, no. W62; Sotheby's, 1988: no. 68; same, Zender and Dale, 1995, pl. 5; Dräger et al., 1992:30, 31, 64; Turner, 1992, fig. 22; King, 1993:33, fig. 1.
18. "Scalper-Messer der Irokesen...—Skalpeer-Mes. der Chirokeesen."
19. Phillips and Idiens (1994:29, 31, figs. 6, 9) identify a knife case of this type in a Scottish collection as "Iroquois" but have no problem attributing a technically and stylistically very similar headdress as "possibly Ojibwa."
20. Orchard, 1971, pl. 29e; King, 1982:18, fig. 7a (left); Acevedo et al., 1983:28, fig. 24; same, Batkin, 1995:54–55; Sotheby's, 1986, no. 125; Museum voor Volkenkunde, Rotterdam, cat. no. 35049.
21. Two bands: Braunschweigisches Landesmuseum, cat. no. VM 7249 (W.A. Du Roi collection). Three bands: Hotz, 1975:43, 154, fig. 16 (cp. Orchard, 1971, pl. 29a; Figure 7); King, 1991:39, fig. 8; Sánchez Garrido, 1992:22, figs. 3, 27; Merseyside County Museum, Liverpool, cat. no. M 12885.
22. The problem with using the Du Roi knife case to anchor the whole group in time and space is the singularity of the two-band decoration, which structurally resembles the two-lane quill-wrapped type.
23. Krickeberg, 1954:129–130, pl. 30; Benndorf and Speyer, 1968:91–92, no. 158, fig. 62, where the sheath is identified as Ojibwa; cp. the knife case shown on the portrait of Sir John Caldwell (see under "Written and Pictorial Evidence").
24. Orchard, 1971, pl. 29d; Benndorf and Speyer, 1968:78, no. 116, fig. 43; same, Ewing, 1982:150, no. 123; Vitart, 1980:133; same, Phillips in Harrison, 1987:41, no. W19; King, 1982:18, fig. 7a (right); Dräger et al., 1992:30; Painter, 1992:41, fig. 21a; Karl May Museum, Radebeul, uncataloged.
25. Feder, 1965, fig. 27d; Thompson, 1977:188–189, fig. 145; King, 1982:18, fig. 7b; see also Peabody Museum, Harvard University, cat. no. 99-12-10, shown as a reproduction in Zender and Dale, 1995:110.
26. The painting, by Paul Kane, is reproduced in Brasser (1976:190), King (1982:83, fig. 88), Kasprzycki (1990:94, fig. 50), and elsewhere. Compare Bushnell, 1906:241, pl. 21a, and Schoolcraft, 1851–1857, pl. 77: fig. 4.
27. Painter, 1992:37–38, no. 18; Zender and Dale, 1995, pl. 6 (right). Other more or less isolated forms of single knife cases are found in Orchard, 1971, pl. 5; Völger, 1976(4): case 20, item 34; Feest, 1987:290–292 and color plate; Lagrange and Dubois, 1992:111, no. 241; and Zender and Dale, 1995, pl. 6 (bottom).
28. Feest, 1968:44, table 2a (Ottawa); Acevedo, et al., 1983:29, fig. 26; same, Batkin, 1995:68–69 (attributed to Eastern Sioux); Zender and Dale, 1995:124 (Winnebago).
29. See, for example, Feder, 1965, fig. 27a, 1987:50–51, 53, figs. 6, 7; Hartmann, 1973:344, fig. 100; Flint Institute of Arts, 1973:5, no. 9; Thompson, 1977:185, fig. 127; Tippecanoe County Historical Association, Lafayette, Indiana, Wetherill collection.
30. Feder, 1964:53, figs. 37, 39; Casagrande and Ringheim, 1980:80, fig. 77 and color plate; Painter, 1992:35, 37, no. 17; Náprstek Museum, Prague, cat. no. 21328; Peabody Essex Museum, Salem, Massachusetts, cat. no. E 53445.
31. Schulze-Thulin, 1976:69, no. 54 (Mandan); see also Painter, 1992:41, fig. 21 (collected at Pine Ridge, South Dakota, but attributed to the Hidatsa).
32. Feder, 1965, fig. 27f; same, Maurer, 1977:164, and Brasser in Harrison, 1987:82, no. P50; Museo Luigi Pigorini, Rome, cat. no. 3654; the British Museum, London, cat. no. 1944.Am2.241.
33. Bushnell, 1906:249, pl. 21c; Brasser, 1976:174, no. 188 and color plate; Best and McClelland, 1977:10, no. 7; Laurencich-Minelli, 1990:246–247, figs. 11–12 (cp. Beltrami, 1828, pl. 2: fig. 5); Painter, 1992:35–37, no. 16; Batkin, 1995:70–71; Folkens Museum-etnografiska, Stockholm, cat. no. 1854.2.14.
34. NMNH, Department of Anthropology, collections housed at the Museum Support Center, Suitland, Maryland, cat. no. T-7373; Feder, 1965, fig. 27c; Maurer, 1977:109, no. 98; same, Vincent, 1995:24.
35. NMNH, Department of Anthropology, collections housed at the Museum Support Center, Suitland, Maryland, cat. no. T-10721.
36. Benndorf and Speyer, 1968:63, no. 73, fig. 27; Ewing, 1982:153, no. 126; King, 1982:92, fig. 98a,b; Acevedo et al., 1983:47, fig. 75.
37. NMNH, Department of Anthropology, collections housed at the Museum Support Center, Suitland, Maryland, cat. no. 18814.
38. While comparisons on the level of specific artifact types should be undertaken first, comparisons between such types can be very elucidating. Thus, the further study of quilled knife cases will greatly profit from a better understanding of quilled pouches and moccasins. As long as provenances for these other types of artifacts are themselves mostly based on dubious attributions, however, comparisons will only lead to equally dubious results.

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Pabookowaih Unmasked

William N. Fenton and Donald B. Smith

Ethnographic specimens in older museum collections seldom are accompanied by documentation of the time and place they were acquired or manufactured and who made or collected them. Even when such documentation is available, it often is inaccurate. Identifying such specimens thus presents intriguing opportunities for combining research in museums, libraries, and the field. Here we describe this identification process in relation to a mask found in the collections of a Scottish museum. Once attributed to Africa, the mask was later suspected to be of American Indian origin. On examination and comparison with other known masks, it proved to fall within the genre of North-east Indian masks and is most likely of Delaware origin.

The Search Begins

At the 1987 meetings of the American Anthropological Association in Chicago, Professor Thomas Ablor of the University of Waterloo, Ontario, Canada, showed several Iroquoianists photographs of an old mask housed in the David Livingstone Centre at Blantyre, Scotland (Figure 1). He then gave the photographs to Fenton, who was keenly interested in older examples of such masks, his book on Iroquois masks having just appeared (Fenton, 1987). The Blantyre mask has a figure-eight design circling the mouth and eyes, which makes it somewhat unique. Otherwise the general form of the mask is not unlike that found on Iroquois masks in collections dating from the mid-nineteenth century.

Ablor had been given the photographs by Dale Idiens of the Royal Museum of Scotland, in Edinburgh. Idiens regarded the mask of great importance and hoped that experts in North America might be able to provide some information on an item seldom encountered in Scottish collections. She told Ablor that there was no information at all relating to the mask, noting that local people often donated such items to the museum at the Livingstone Centre because they perceived it to hold non-European collections (Dale Idiens, pers. comm. to Fenton, 16 Dec 1987).

The mask came to the museum during the early 1950s "as an African Witch Doctor's mask," and it was exhibited as such until Idiens began to suspect it was of Native American origin.

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She informed the warden of the collection, Bill Cunningham, of her suspicions, and Cunningham had the mask taken off exhibit and placed in storage (Bill Cunningham, pers. comm. to Roland Force, 24 Jan 1989).

In 1988 Fenton, eager to study the mask, recommended that it be acquired by the Museum of the American Indian-Heye Foundation in New York, where he was a trustee. The director of the Museum of the American Indian at the time, Roland W. Force, contacted Cunningham, who was advised to price the mask at not more than 1200 £ (approximately \$1850). The sale was never completed because other, more pressing matters intervened. The ultimate affiliation of the Museum of the American Indian and the destiny of its collections just then preoccupied the director and the trustees.

Not until 1994 was Fenton able to examine the mask firsthand. In May of that year, he travelled to Glasgow and then arranged to visit the Livingstone Centre in nearby Blantyre, where he had the opportunity to handle the specimen and photograph it from several angles.¹ The following October he showed his photographs and described the mask at the Conference on Iroquois Research, held in Rensselaerville, New York. One of the Iroquois people present, Nora Carrier of the Onondaga Longhouse at Six Nations Reserve, volunteered to Dr. Hanni Woodbury, who was sitting with her: "My grandmother had a mask like that. It was Delaware." Later, Carrier repeated this remark to both Fenton and Woodbury, which set all the conference participants wondering.

Peter Jones, Father and Son

These events inspired William C. Sturtevant to dig up his notes on the objects and illustrations of objects collected by the Reverend Peter Jones (William C. Sturtevant, pers. comm. to Fenton, 5 Oct 1994). Jones was a Mississauga (eastern Ojibwa) Indian who served as a Methodist missionary among the Ojibwas and Munsee Delawares of Southwestern Ontario from 1823 until his death in 1856 (Smith, 1985). In his book *History of the Ojebway Indians*, which appeared posthumously in 1861, Jones illustrated three masks, or carved faces. Two of these are on a single plate with the heading "Me-Zeengk is the name of this God;" the left one is labeled "A Muncey Idol," and the right one is labeled "A Muncey Devil Idol" (Jones, 1861, opposite p. 83, left and right) (Figure 2). The third mask is on a second plate labeled "Pabookowaih" (Figure 3), explained in a



FIGURE 1.—Full-face (*a*), profile (*b*), and back (*c*, opposite) views of the mask at Blantyre, Scotland: *a*, note the square chin, the fringe of dew-claw rattles, the handle resembling a dance wand, and especially how the lower of five supra-orbital wrinkles incised on the forehead is extended to frame the eyes and mouth in a figure-eight motif; *b*, note the remains of feather tufts at the crown and handle and the skin cover at the back; *c*, the skin cover is lashed to 11 holes drilled in the rim, which is thicker than it is on Iroquois false faces. Photographs by Angus Mills, Glasgow.

note as “the God that crushes or breaks down diseases” (Jones, 1861, opposite pp. 85, 87, note).

Nearly 50 years later, M.R. Harrington purchased a mask backed by a turtle-shell rattle (Harrington, 1908, pl. 26, opposite p. 417) from Rev. Peter Jones’s son, the physician Peter E. Jones of the New Credit Reserve of the Mississaugas, which borders on the Six Nations Reserve of the Iroquois on Grand

River, Ontario. Peter Edmund Jones (1843–1909) was born at his father’s Muncey Mission near London, Ontario, and spent the first eight years of his life there and in nearby London. The family then moved to Brantford, a largely non-Indian town adjacent to Six Nations Reserve, where the elder Jones died in 1856. A governess prepared the younger Jones for Brantford Grammar School, and after completing his studies, he entered



the University of Toronto medical school. There and at Queen's College, Kingston, he completed his medical studies, and in 1866 he became the first Canadian Indian to attain a doctor of medicine degree (Smith, 1994).

Following his father's example, Dr. Jones opted to practice in an Indian community. He settled at Hagersville, adjacent to New Credit Reserve and just west of Six Nations Reserve. Although but one-quarter Indian by birth and married to an English woman, he apparently identified himself as Mississauga. During the 1880s he served two terms as head chief of the Mississaugas, and he worked to improve their legal status with the Province of Ontario, where they resided, and with the Indian

Department in Ottawa. He later served as agent for the Mississauga Band of Eastern Ojibwa. He also edited and published a newspaper, *The Indian*, and collected both books on Indians and Native American artifacts, which he occasionally traded or sold to museums (Smith, 1994).²

When Harrington bought the mask from Dr. Jones, Jones told him that his father had collected the mask. Harrington considered the mask to be very old and of Mississauga derivation, a conclusion reinforced by Jones's claim that it was the Pabookowaih illustrated in his father's book (Jones, 1861, opposite p. 85).³ Jones attributed differences between the mask he sold Harrington and the illustration in his father's book to liberties taken by the artist (Harrington, 1908:417).

The mask that Harrington purchased and reported on (Harrington, 1908:416–417, pl. 26; American Museum of Natural History (AMNH), New York, cat. no. 50.1/1447) derives from the same genre as the Blantyre mask, but it is not the mask illustrated in Rev. Peter Jones's book (1861). Although it has the same square chin, elliptical mouth, straight nose, eye sconces, and supraorbital wrinkles, it lacks the distinctive figure-eight motif surrounding the eyes and mouth. No traces of feathers, hair, or fringe of dew-claw rattles, if ever present, remain, although Dr. Jones told Harrington that he remembered when the mask he sold had its crown of feathers, deer hair, and fringe of rattling deer hoofs, all of which appear on the Blantyre mask. Moreover, the Harrington piece is backed by a turtle-shell rattle, the handle protruding beneath the chin, whereas the handle to the Blantyre mask resembles a dance wand. And the Blantyre piece retains the now-disintegrating remains of feather tufts at the crown and the remains of a deer-hair fringe with dew-claw rattles attached, features amply illustrated in the drawing in Rev. Peter Jones's book.

The statement by Jones's son initially led Sturtevant to believe that the Muncey Idol mask illustrated in the elder Jones's book might be the one that M.R. Harrington bought from Jones's son, which is now in the AMNH (see Jones, 1861, opposite 83, left; Fenton, 1987, pl. 18-1B).⁴ A close comparison of the two revealed, however, that they are different. Upon learning about the Blantyre mask from Fenton, Sturtevant identified it as the Pabookowaih mask illustrated in Rev. Peter Jones's book. Thus the mask sold by Dr. Jones to Harrington is not among the three in the elder Jones's book.

The Transatlantic Journey

Our establishing a definite association of the Blantyre mask with the Reverend Peter Jones resolves only one of three puzzles surrounding this mask. The second puzzle is how the mask got from Ontario to Scotland.

After the 1994 Conference on Iroquois Research, Donald Smith of Calgary University, biographer of both Rev. Peter Jones and his son, supplied Fenton with documentation on Ojibwa-Mississauga travelers abroad (Smith, 1976, 1987, 1994). He noted that Rev. Peter Jones made three extended trips to Britain—in 1831–1832, 1837–1838, and 1844–1846—to

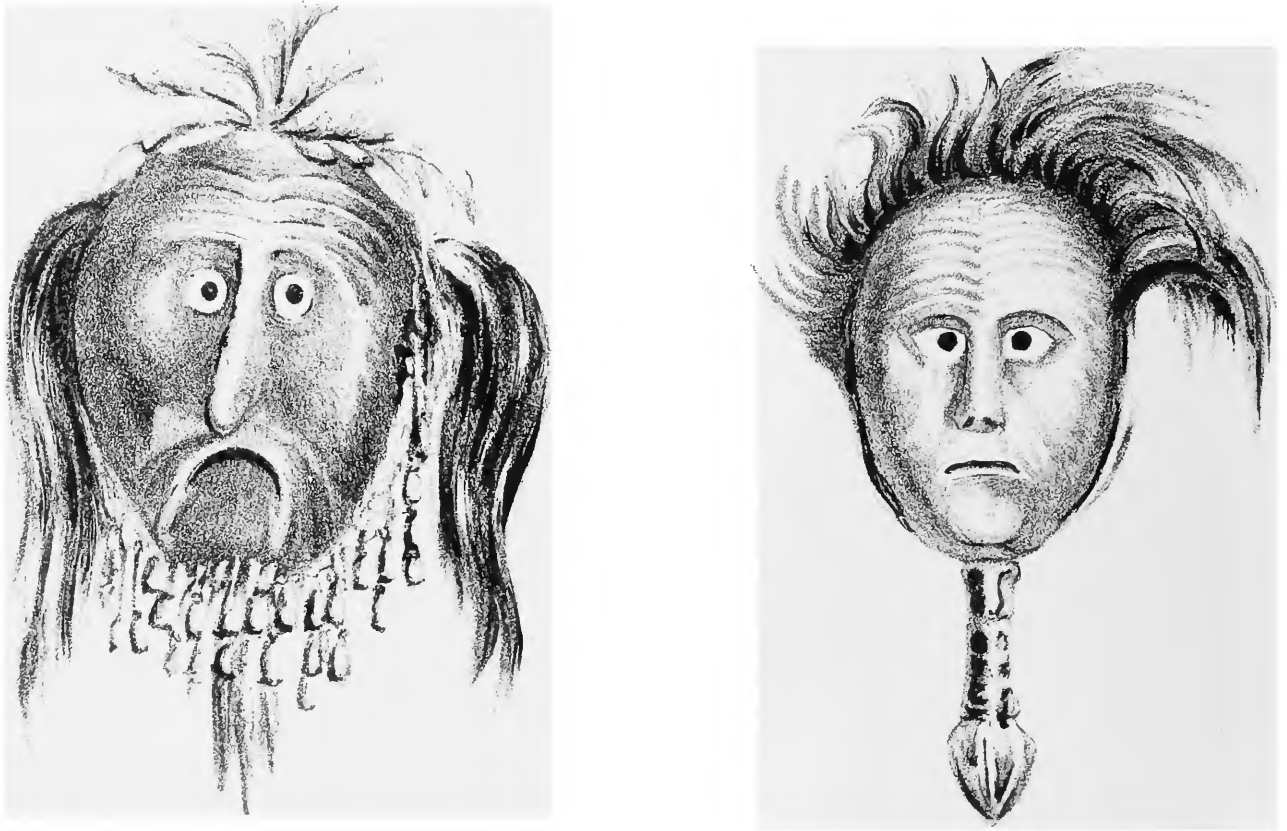


FIGURE 2.—Two comparable Munsee (Delaware) masks, the Me-Zeenk of Rev. Peter Jones (1861): left, “A Muncey Idol;” right, “A Muncey Devil Idol.” The Edward E. Ayer Collection, Newberry Library. Courtesy of the Newberry Library.

promote and raise funds for his mission among the Mississauga and the Munsee Delaware. Jones visited Glasgow, among other cities in Great Britain, and sat for his photograph wearing Indian dress (King, 1982:65). He also brought with him an array of native artifacts, including masks, to illustrate his lectures on Native Americans.⁵ These performances evidently attracted large audiences.

It thus is conceivable that Jones left the mask in Scotland on one of his trips there. It also is possible that the mask was taken there by Jones’s half-brother Maungwudaus, or George Henry. Maungwudaus carried a collection of Indian artifacts with him when he led an Ojibwa troupe to Paris in 1845, and two years later he visited Scotland (Maungwudaus, 1848). The American painter George Catlin produced a series of sketches of this Ojibwa troupe. There is a further illustration in a sketchbook, attributed to George Catlin and now in the Gilcrease Institute in Tulsa, Oklahoma, labeled “PA-BO-KO-WAIGH, the Crushing God”—Indian Idol,” that bears a certain resemblance to the mask at Blantyre (Hamilton, 1972, opposite p. 44). The mouth is different, but the crown of feath-

ers, dew-claw fringe, square chin, suggestion of a protruding and lashed handle beneath the chin, and hint of a figure-eight motif on the left cheek all point to the same genre (Hamilton, 1972, opposite p. 44).

Other evidence suggests, however, that the Blantyre mask reached Scotland after 1861. There is no doubt that this mask is the same as the Pabookowaih illustrated in Jones’s 1861 book, which was published 11 years after he concluded his Scottish tour. It is possible, of course, that Jones’s wife, Eliza, a trained artist, made sketches of the mask many years prior to the publication of the book that could have served as the basis for the published illustration.

There were several people in the area of the New Credit Reserve who had connections to Scotland and specifically Blantyre. N.H. Livingston (1835–1926), “a nephew of the celebrated explorer Dr. Livingston [sic],”⁶ lived in Hagersville, Ontario, near Six Nations Reserve, where he was manager of a branch of the Bank of Hamilton. In 1886 Livingston was listed among the guests at a tea held at the council house on Credit Reserve to honor a visiting Mississauga chief, John

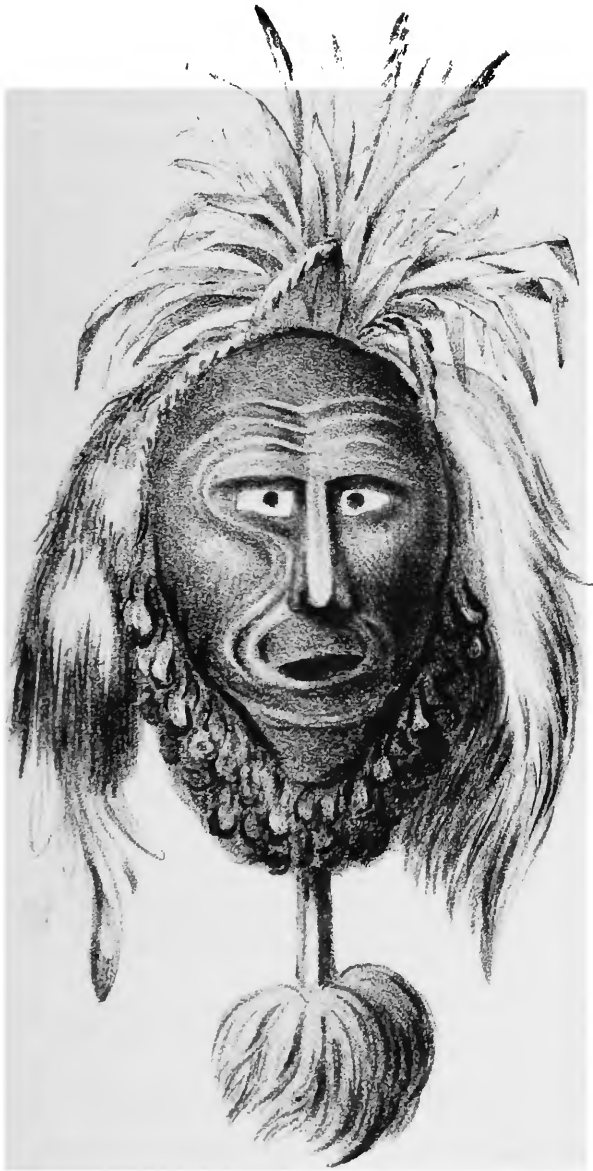


FIGURE 3.—Pabookowaih of Rev. Peter Jones, after the original (Jones, 1861). The Edward E. Ayer Collection, Newberry Library. Courtesy of the Newberry Library.

Tecumseh Henry, the son of Maungwudaus (*The Indian*, 3 Feb 1886).

N.H. Livingston's father, John (1811–1899), elder brother of the African explorer, emigrated to Canada around 1856, settling at Listowel, northwest of Waterloo, Ontario, where he died in 1889 (Martin and Simpson, 1989:103; Stratford-Perth Archives, Ontario, pers. comm. to Donald Smith, 17 Aug 1995). Several other Livingston kindred from Blantyre also emigrated to Canada and the United States, and, like other

loyal Scots, revisited Scotland, some of them to stay (J.C. Cunningham (Keeper of Manuscripts, National Library of Scotland), interview with Fenton, 1995).

Any one of these people who had contact with either Dr. Jones or N.H. Livingston could have brought the mask to Scotland, and one of their descendants, supposing the mask was of African origin, could have donated it to the Livingstone Centre. There are, however, no records at the Livingstone Centre or among the Livingstone Papers at the National Museum of Scotland, Edinburgh, of donations from either N.H. Livingston or Dr. Peter E. Jones. It is unlikely that documentation will ever be found that would reveal how the mask reached Scotland.

Establishing A Cultural Affiliation

The third puzzle is the cultural origins of this mask. Because the elder Jones labelled the mask he illustrated in his book—the mask we now know is the Blantyre mask—with the Mississauga term “Pabookowaih,” the implication is that it is of Mississauga origin. It is likely, however, that all of the masks definitely associated with Rev. Peter Jones were created by the Munsee Delaware.

Jones certainly would have had the opportunity to collect such masks among the Munsee Delaware. When he served as a missionary among them, from 1841 to 1849, the Munsee Delaware were abandoning their native religion and converting to Christianity. We know that Christian zealots obtained such masks when the Delaware Big House at Six Nations was dismantled, about the same time. Nora Carrier's instant recognition of the Blantyre mask as Delaware lends further credence to this hypothesis.

It is likely that the Munsee Delaware used the Blantyre mask for curing purposes and that Jones converted the Delaware concept of the spirit face, “Me-Zeengk” (Jones, 1861, opposite p. 83), into his native Mississauga concept of Pabookowaih, possibly because the power to “crush” disease was attributed to such masks or spirits in both contexts.⁷ The conclusion that it was associated with curing is supported in M.R. Harrington's 1908 report of field collecting among the Canadian Delaware the previous year. Harrington (1908:416, pl. 25, opposite p. 414) collected only one mask (“mīzīnk”) among the Delaware, and it had been used for healing the sick. Although the hair is attached in the Iroquois manner, the lines are burned in instead of painted or carved, and Harrington noted that it was much cruder than Iroquois masks.

Unlike Iroquois False Face masks used to treat disease, the Blantyre mask was designed to be held in the hand and not to be worn as a face mask. Harrington quoted other Mississauga informants as saying that such masks were rarely worn but were carried by the handle while curing the sick. The handle to the Blantyre face is permanently lashed and protrudes beneath the chin to enable carrying it, and the hide cover at the back appears to be lashed in place so that it could not be worn. Shamans sometimes used such masks for divination and kept

them hanging in little bark houses when not in use (Harrington, 1908:417).

Conclusions

Following Harrington's lead, Rolf Krusche of Leipzig completed a thorough canvas of the literature and reported the results of his research in a critical monograph in which he argued for the existence of an ancient mask tradition in the Eastern Woodlands (Krusche, 1986). He proposed a genetic connection between masks carved on trees in the Iroquois manner and portable masks hung on posts and carved on posts in the Delaware Big House. Noting that portable masks sometimes became stationary icons, he concluded "that the wooden masks of the Woodland Indians evolved out of tree and post faces" (Krusche, 1986:24). He found confirmation of this metamorphosis in contemporary Iroquois mask carving. Iroquois carvers regularly fully develop the face before hollowing out the back to the depth required for its intended use. Delaware masks, which may be carried and, as in the case of the Blantyre specimen, even fitted with a handle, are not finished to the depth of Iroquois False Faces, which are invariably worn. They may thus be seen as continuing an earlier stage in the development of masks from carved posts.

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Notes

Sheila Watt, Education Officer, and her staff welcomed Fenton at the Livingstone Centre and could not have been more cooperative in affording him freedom to examine the mask and photograph it without interruption.

1. The catalog entry for the Blantyre mask reads "Wooden Mask, # 871. 31 cm. x 18 cm.; overall: 70 cm."
2. In 1898 the United States National Museum acquired pieces from the collection of Peter E. Jones (William C. Sturtevant, pers. comm. to Fenton, 5 Oct 1994). Also, M.R. Harrington (1908) collected a mask from Peter E. Jones, now in the American Museum of Natural History, New York, which is illustrated and discussed in Fenton, 1987:466-467, pl. 18.2.
3. Harrington (1908:417) recorded Jones's term "Pabookowaih" from a Mississauga Ojibwa as "Pabokowāi."
4. The mask with the turtleshell-rattle backing illustrated at the right of the same plate ("A Muncey Devil Idol," Jones, 1861, opposite p. 83, left) is in the Karl May Museum at Radebeul, Germany, where Sturtevant studied it (Krusche, 1986:38). The rattle is present but is separated.
5. Handbills and press cuttings, National Library of Scotland, Dep. 360/146/273, no. 4/360. *Edinburgh Advertiser*, 5 Aug 1845; *Aberdeen Journal*, 13 Aug 1845.
6. *The Indian*, 3 Feb 1886. Later generations of Livingstones in Canada dropped the final "e."
7. We have no other information on Mississauga masking or on beliefs regarding masks except what was used by Harrington (1908:417); see text below. "Me-Zeengk" is Munsee *māsi-nkw* "spirit face," literally "all face;" "Pabookowaih" appears to be Ojibwa rather than Munsee but cannot be definitely analyzed (Ives Goddard, pers. comm. to Fenton, 1997).

The Linguistic Writings of Alfred Kiyana on Fox (Meskwaki)

Ives Goddard

The Meskwaki writer Alfred Kiyana, who lived from 1877 to 1918, produced a large number of manuscripts in the last seven years of his life. These manuscripts were written for Truman Michelson of the Bureau of American Ethnology between July 1911, when Michelson first visited the Meskwaki Settlement in Tama, Iowa, and November 1918, when Kiyana died in the Spanish Influenza pandemic. They are now in the Smithsonian Institution's National Anthropological Archives (NAA) at the Museum Support Center (MSC), Suitland, Maryland. As to content, they are mostly traditional narratives and ceremonial texts, but they include some ethnographic descriptions and other miscellaneous compositions.

One category of Kiyana's literary output that seems of broader significance is his linguistic writings. These are mostly collections of or commentaries on words, but there are also occasional passages that reflect an awareness of and interest in grammatical and derivational patterns. Like the rest of his writings, they are all written entirely in Meskwaki (Fox), in the Meskwaki version of the Great Lakes Algonquian Syllabary (Kinkade and Mattina, 1996:250; Walker, 1996:168–172; Goddard, 1997).¹ An inventory of Kiyana's linguistic writings in the NAA follows; they are referred to in the text by their identifying letter.

- A [Commentary on "The Story of the One That Was Blessed by the White Buffalo" (NAA ms. 2062).] NAA ms. 2819: 85 pages [ca. 1912].
- B [Commentary on "What Green Buffalo Did When he Bestowed a Blessing" (NAA ms. 1786).] NAA mss. 2763:1–49, and 2952 [part, at the end, separately paginated]:50–54, 54 pages [ca. 1912].
- C Kinship Terminology and Archaic Vocabulary (*čī-nawe-ti-weni, e-yi-ki-meko ašawaye-kanawi-ni*). NAA mss. 2232:1–40 and 2277:41–72, 72 pages [1916].
- D [(Lists and Ethnographic Notes:) Medicines, Tohkan and Kishko, Names of Dogs Belonging to the Different Clans, Insects, Fish and Other Water Creatures, People Who Had Spiritual Power Long Ago, Early Indian Foods, Words.] NAA mss. 2841:[1–58] and 2778: [1–48], 106 pages [1917].

The Meskwakis became literate in their language in the nineteenth century. In 1880 the Indian agent for the

Meskwakis, the sympathetic George L. Davenport, reported that they "prefer to teach one another to read and write in their own language, and great progress has been made in their education in that way. They understand well the use of postal cards and post-office money-orders, and carry on a large correspondence with themselves and the Indians of Kansas and Indian Territory" (ARCIA, 1880:97). The earliest examples of Meskwaki writing that I have seen are the month and day names in dates carved on a hole-and-slot heddle in the MSC, which indicate the years [18]87 and [18]88 (Sturtevant, 1977:335). Historical manuscripts are extant that bear dates earlier than 1880, but the existence of a manuscript actually written before this date has not been confirmed. Thus when Alfred Kiyana learned to write, he was in one of the first few generations of his people to be literate. His English was rudimentary and apparently not fluent,² and most Meskwakis in his day probably knew even less. A few of his near age-mates were the first Meskwakis to attend school. Not surprisingly, then, Meskwaki writing in Kiyana's day consisted essentially of varieties of traditional oral discourse transferred directly to paper. Yet, in the context of the very inception of literacy, Kiyana's linguistic writings show secondary considerations of language as an object of study in itself already taking hold and finding expression in the written medium.

Archaic Vocabulary

The most polished of Kiyana's writings, apparently written in 1916, is a manuscript he called "Kinship Terminology and Archaic Vocabulary" (C). This is in two school notebooks that have been separately cataloged but that clearly form a unit. It begins with a long section in which kinship terms are defined and explained, and the rest of it is a list of unusual or archaic words and their definitions. There is also a collection of various lists, in two notebooks, some with ethnographic commentary (D). Most of what is in the first of the two notebooks of manuscript D is ethnographic rather than linguistic in nature, though the lists of items in certain semantic fields, such as plants used for medicine, have some linguistic significance as lexical collections. More intriguing is Kiyana's wordlist, a stream-of-consciousness listing with no definitions or explanations, which begins at the end of the first notebook and continues through the whole of the second notebook. Although Kiyana indicated no page numbers, and there are separate

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archival paginations of the two notebooks, the second appears to be a continuation of the first. Despite the fact that the listing in manuscript D contains words that appear with explanations in manuscript C, it appears that D was written after C, in 1917.³ Finally there are two commentaries, of a type that may be called scholiastic, which Kiyana wrote on two of his ceremonial texts, those referred to for short as White Buffalo and Green Buffalo (A and B, respectively). These explain passages in the text and especially songs, often adding ethnographic and exegetical commentary. The White Buffalo text was probably written in 1912, and in any event the commentary on it must have been written before Horace Poweshiek, one of Michelson's Meskwaki assistants, wrote out his English translation of the main text in September and October of 1915. The Green Buffalo text and commentary were probably written about the same time as the White Buffalo text and commentary, but later.⁴

Kiyana's presentation of the kinship terminology in manuscript C does not cover all kinship terms and lacks an overall framework, but the individual entries reflect careful thought in their selection and organization. They also neatly put to rest any notion that the abstract structure of a kinship terminology is inaccessible to its native-speaking users without referring to actual genealogies. This source was used extensively in my paper on Fox kinship terminology (Goddard, 1992). An example will make clear the general style of Kiyana's explanations (C: 1B–G):⁵

- (1) neniwa otehkwē-mani oni-ča-nesani, onekwa-hani.
man 3(°)s+sister 3(°)s+child 3(°)s+nephew
'A man's sister's child is **his nephew**.'
- onekwa-hani okwisani neniwa e-h=o-šise-miči.
3(°)s+nephew 3(°)s+son man have.grandchild+3s(-3°)/AOR
'And his nephew's son is the man's **grandchild**.'
- o-šise-mani e-h=oni-ča-nesani-či,
3(°)s+grandchild have.child+3°/AOR
'And when his grandchild has a child,
ni-šo-nameki o-šise-mani.
for.the.second.time 3(°)s+grandchild
it's **his grandchild of a second kind**.'
- neniwa okwisani oni-ča-nesani, ope-hki-o-šise-mani.
man 3(°)s+son 3(°)s+child 3(°)s+real-3(°)s+grandchild
'A man's son's child is **his real grandchild**.'
- neniwa ope-hki-o-šise-mani oni-ča-nesani, še-ški=meko
man 3(°)s+real-3(°)s+grandchild 3(°)s+child only=EMPH
o-šise-mani.
3(°)s+grandchild
'And a man's real grandchild's child is just plain **his grandchild**.'

Here, having introduced extensional uses of 'grandchild', Kiyana then explains how 'real grandchild' (literally) can be differentiated from these. Characteristically, however, he does not

give a complete account of the terminology for all kin-types referred to as 'grandchild'.

Another example attests additional features of Kiyana's style (C: 4F):

- (2) neniwa ošise-hani (nekya otawe-ma-wani)
man 3(°)s+maternal.uncle (1s+mother 3(°)s+brother)
owi-wani, okaka-či-osekwisani.
3(°)s+wife 3(°)s+joking-3(°)s+maternal.aunt
'A man's maternal uncle's wife is **his teasing aunt**.'

The phrase *nekya otawe-ma-wani* 'my mother's brother', given in parentheses in the edition excerpted in example 2, is written in the manuscript above the word *ošise-hani* 'his maternal uncle' as an interlinear gloss. The casting of this phrase in the first person attests a perhaps ad hoc metalinguistic style and shows that its intended function is as a gloss, a comment standing outside the sentence as an explanation of a word, and not as an expansion added in apposition within the sentence.

In another entry, Kiyana defines a kinship term scantily attested in Fox but with cognates in Cree and Ojibwa. The form used is a reciprocal verb based on a derived transitive verb of possession, which gives the meaning 'to have each other as...' (C: 10A, B):

- (3) owi-weti-haki okwiswa-wani owi-weti-haki ota-neswa-wani
married.couple 3p+son married.couple 3p+daughter
owi-weti-nite, oni-ča-neswa-wahi,
get.married+3°/SUBJ 3p+children
†otata-wa-ti-wa-či.
have.each.other.as.co-parents-in-law+3p/CONJ
'If their children get married, one couple's son and one couple's daughter, **they would be co-parents-in-law to each other**.'

In his notes on this manuscript, Michelson recorded the underlying kinship term as *netatawa-wa* 'my son-in-law's or daughter-in-law's parent', and a Meskwaki speaker interviewed in 1998 pronounced this as *netata-wa-wa* (compare Cree *nitih-ta-wa-w* 'my fellow-parent-in-law' (Bloomfield, 1984:6) and Ojibwa ⟨indindawaa⟩ (Nichols and Nyholm, 1995:66)).⁷ Fox shows assimilation in the quality of the second vowel; the length of the third vowel varies. Kiyana's entry is a third witness that supports the recognition of a kin term not previously recognized for Algonquian, which can be reconstructed as Proto-Algonquian (PA) **netenta(°)wa-wa* 'my fellow-parent-in-law, one whose child is married to my child'.

After the section on kinship terminology, manuscript C has a section headed "Archaic Vocabulary" (*našawaye-e-yo-te-ki-kanawi-ni* 'words used long ago'). Each word is typically given at the end of a separate entry after an often discursive explanation, sometimes with a plural or other additional inflectional form. A survey of several examples will make clear the nature of this material and its value, as well as Kiyana's method and presentational style.

The briefer entries in this section typically state the equivalence of an archaic word with a current synonym (C: 13H):

- (4) aye e-šitehka-sowa-či mači·hkiwesaki,
 earlier be.called.(so)+3p/PPL(OBL) eldest.brother+ANpl
 kehkiwesa.
 eldest.brother+ANsg

'In earlier times what eldest brothers were called was *kehkiwesa*.'

The words equated in example 4 are used interchangeably in some traditional texts, but Kiyana's definition makes clear for us which was the ordinary word and which was the synonym used for variety.

Some entries have longer explanations (C: 35A–D):

- (5) neniwa ne-hi-ši-ša-ta ne-wa-te|
 man know.how-hunt+3s/PPL(ANsg) see+3s-3'/SUBJ
 pešekesiwani, nekotahi kahkisoči,
 deer+OBVsg somewhere hide.self+3s/CONJ
 e-nowe-niči pešekesiwahi inowe-či,
 utter.(so)+3'/PPL(obl) deer+OBVpl utter.(so)+3s/CONJ
 me-me-kime-wa.
 call.(animal)+3s-3'/IND

'If a man who is a skilled hunter sees a deer, he would hide somewhere and would give a call the way deer sound: **He calls it.**'

The word defined here, *me-me-kime-wa* 'he calls him (an animal)', was not generally known in the 1990s, but on the basis of this entry, it was possible to elicit it from a single elderly speaker in 1997.

This entry is notable for attesting two plain conjunct forms (another was in example 3). The modes of the conjunct order in Algonquian languages are used in various kinds of subordinate clauses, differentiated by suffixes and other morphological processes, and the aorist conjunct in Fox, marked by the aorist proclitic preverb (*e-h-*), is also the usual mode in narrative. Plain conjuncts have the pronominal inflection of the conjunct order with no modal preverb or suffix. They are exceedingly rare in narrative texts; the apparent examples seem to function as aorist conjuncts and most likely are to be considered recent or colloquial forms of the aorist with the aorist proclitic preverb dropped (Voorhis, 1971:75; Goddard, 1988:197, 205–206). The true plain conjunct shows up in expository prose, as in examples 3 and 5, with the meaning 'would' in consequential clauses. The use of this form in Kiyana's definitions provides a good demonstration of the need to examine a variety of genres of discourse in order to document the use of the full grammatical resources of the language.

Definitions are a genre of discourse that brings out the use of precise, carefully monitored language and that ideally requires indefinite, non-specific phrasing that lacks the specificity of reference characteristic of ordinary narratives. As a result of these unusual demands on the expressive resources of a language, definitions may exhibit constructions that are otherwise observed rarely, and then only in the highest stylistic registers (C: 30H–J):

- (6) ihkwe-wa e-šite-he-ki neniwiweni
 woman+ANsg think.(so)+X/PPL(OBL) being.a.man+INsg
 e-h-inekikhkwiwkwe-wiči,
 be.woman.(so).big+3s/AOR
 e-h-makihkwe-we-hiči, mesawina-kosiwa.
 be.big.woman/DIM+3s/AOR appear.desirable+3s/IND

'When a woman is the size men like, a larger woman, **she looks desirable.**'

Here *e-šite-he-ki* 'what one (indefinite) thinks, the way one likes' is a verb inflected for an indefinite subject. An indefinite-subject form cannot occur with an ordinary, overt subject noun phrase, but this one has as a nominal adjunct the abstract inanimate noun *neniwiweni* 'maleness; men (collective)'. This noun is derived from a verb of being, *neniwi-* 'be a man', which is in turn derived from the animate noun *neniwa* 'man' (plural *neniwaki*). This construction, with an abstract noun in effect agreeing with the indefinite subject of the verb, permits the subject to remain appropriately indefinite while also being specified as male.

This entry is also notable for giving insight into the meaning and use of a word that goes beyond the usual dictionary definition.⁸

Examples 7–30 summarize the entries rather than giving them in full. Single quotes mark my generic glosses of the preceding word or phrase, single quotes in parentheses indicate a direct gloss of Kiyana's Meskwaki definition or explanation (the equivalent of ordinary double quotation marks except for the difference of language), and parentheses without single quotes are used where what Kiyana wrote is summarized.

- (7) *nepe-kwite-he* 'I'm thirsty'; also *nepe-kwinawe*, *neka-hkinawe*.

These are three synonyms for 'thirsty', the ordinary one and two others. The third word probably meant 'I'm parched', like the matching Sauk form (Gordon Whittaker, pers. comm., 1996). The second word seems to blend the elements of the other two.

In example 8 an ordinary word is given together with a rarer, high-register synonym:

- (8) *maneseno-hi* 'war'

('A big fight, when they have a great battle with their enemies, when they fight dangerously.')

mešihkatwi, *i-ni-meko* = 'ni = 'ni *kanawi-ni*. "Hostilities": that's the same word.'

This attests a more explicit way of stating that one word is synonymous with another.

Example 9 gives the synopsis of an entry that contains perfectly ordinary words but reflects Kiyana's insight into the structure of the lexicon of his language:

- (9) *anenwi-wa* 'he bathes, takes a dip, washes himself'

ko-kenike-wa 'he washes things, does the wash'

The first word is explained as 'when anyone goes and washes himself', using the compound verb *mawi-ko-kenowa* 'he goes

and washes himself'. The second is explained as 'when anyone washes anything', using the phrase *ke·ko·hi ko·kenamwa* 'he washes something'. Kiyana had noticed that among the ordinary words referring to washing, the verb meaning 'to bathe, to wash oneself' is not part of the derivational set based on the stem *ko·ken-*, which covers most kinds of washing and can, in fact, be used to define it. In noticing that this verb belongs in this semantic set, he perceived and formulated a semantic linkage lying beneath the overtly divergent forms of the lexical items.

In some cases he recorded an idiomatic meaning for an ordinary word:

- (10) *ahkawa·piwa* 'he guards, acts as a guard, is on guard'

('When a man's wife looks somewhere and he right away looks there, and he also goes around with her all the time.')

The examination of a series of Kiyana's various entries illustrates the topical areas that drew his interest and the kinds of lexical items that he thought merited comment. For convenience in this survey, the words Kiyana defined can be classified according to their degree of obsolescence in the language. Some are words that are known today by some speakers but are unusual, either rare synonyms or words for items or practices that have fallen out of use (11–15).

- (11) *mehtašaye·wa* ('A man is naked.')

This is an unusual word, with a stem consisting of an initial *meht-* 'bare' and a medial *-ašay-* not found elsewhere.⁹ Perhaps the medial is derived from *owašayi* 'his foreskin', a word that, like a lot of sexual vocabulary, is attested only in William Jones's manuscript notes.¹⁰

- (12) *pehkwapiso·haki* 'green-corn dumplings'

papakena·waki ['comcakes (fried green-corn dumplings)']

This entry describes a traditional way of preparing corn. The second word is not explained by Kiyana but is listed as if it were a synonym, hence my added gloss is in brackets.

- (13) *opehkwina·naki* 'skewered pieces of meat roasted next to the fire'

This is another traditional food.

- (14) *netoma·wašiha·wa* 'I give him my kill' ('If a man goes hunting and kills a deer, and after killing it he skins it, and another man who is hunting comes, and he gives it to him.')

This refers to a traditional food-sharing practice.

- (15) *awanwi* 'there's a thick fog; thick fog'

This is a now obsolete word that has cognates elsewhere; cf. Ojibwa *awan* 'there's a fog', Munsee *awân* 'there is fog, mist'. It preserves a cognate that is otherwise found only in two places in Kiyana's texts, once as a noun and once as a verb, and it makes the meaning precise.

Some words Kiyana listed are known passively by some speakers today but without a clear idea of their meaning (16–21):

- (16) *nanawa·tesiwá* ('If a man never kills animals for food when he hunts, if he fails every time, coming back empty-handed.')

This word was glossed in the 1990s as 'he lives for nothing', which literally translates the component elements; Kiyana's gloss makes the idiomatic use of this expression precise.

- (17) *anawiwaki* ('When people go on an easy hunt, when they go and try to kill animals for their food for a short time, just a few campers.')

This obsolete word for 'hunt' (cf. Munsee *aláwi·w* 'he hunts', Unami *alái·*) was recalled by one man in the 1990s as an old word for 'go on the warpath'. It seems to apply to small-scale hunting trips, between a day's hunt and a major seasonal hunt.

- (18) *ana·hpawe·wa* 'he recounts his dream of spiritual blessing for power' ('Any person who was blessed by a manitou [a god or spirit] when fasting while growing up, who had a significant dream about something, if he is in dire straits in any way—for example, hard pressed by his enemies—and he recounts that dream he had long before.')

This word was recognized in the 1990s as having to do with a dream that was helping a person by being "stuck on to his life," but the details about recitation in times of peril were not known.

- (19) *a·pano·wa* (A murderer whose relatives expiate the crime by a payment of wergild to the relatives of the victim.)

This word was explained in the 1990s as applying to someone who was relied on or relied on others, but the specific reference to the practice of the payment of wergild was unfamiliar.

- (20) *ne·nawih·to·wa* 'marshal' ('one who gives orders to a war party and gives the signal to attack')

This word is found in texts (Michelson, 1925b:588–589, 594–595); Kiyana's definition gives a clear picture of the ethnographically significant meaning, which is confirmed by its translation in Sauk as 'camp or hunt policeman' (Skinner, 1923:140) and by cognates in other languages: Menominee *ne·nawe·htaw* 'great warrior, hero' (Bloomfield, 1975:154) or, more likely, 'camp policeman' (Skinner, 1921:143); Narragansett (nanouwétea) 'an overseer and orderer of their worship' (Williams, 1936:128).

- (21) *wahkwí* 'sky'

Kiyana's definition of this word as 'sky' corresponds to the way it is used in songs, though one man in the 1990s thought it was 'clouds'. Both meanings are found in other Algonquian languages, reflecting PA **waskwi*: Cree *wasko·w* 'cloud', Severn Ojibwa *wahkwí* 'cloud', and Munsee *wáhkonk* 'in heaven'.

- (22) *na·hkatešima·pi* 'he is left behind in flight'

na·hkataho·na·pi 'he is left behind by a canoe party'

The two stems in example 22 are found in texts but were generally unknown in the 1990s, and the transitive inanimate corresponding to the first stem was incorrectly analyzed and phonemized in Goddard (1994:112). Subsequently, one speaker was found who knew both words.

For words of a third set, the phonemic shape is recoverable from Fox sources, but even if the word is familiar, the meaning is completely unknown except for Kiyana's explanation (23, 24).

(23) †*si-sepe-ha* ('warrior')

This word was glossed by James Geary, working with Harry Lincoln, as 'person with prophetic vision'.¹¹

(24) †*nawači-waki* ('If campers are moving and stop to cook, and after they eat they would break camp.')

In his discussion of this word, Kiyana glosses *e-h-nawači-ki* 'when one stops-to-cook' explicitly as *e-h-nawači-wača-hoki* 'when one stops to cook' (preverb *nawači* 'stop and' + stem *wača-ho-* 'cook'). Clearly the verb in example 24 is formed with the initial that appears in the preverb *nawači* 'stop and, first', and Kiyana recognized this connection. The Fox word appears to be the exact cognate of the Cree word *nawaci-w* 'he roasts food' and suggests an etymology for it. Roasting would be the most convenient and hence the preferred method of cooking for a traveling party.

The words of a fourth set are recoverable with some confidence on the basis of comparative evidence, with the usual caveat that cognates do not always behave as we would like them to (25–29):

(25) †*ošiwa* ('When a divorced woman marries a man.')

This is a word that dates back to the Proto-Algonquian kinship terminology, reflecting PA **weʔšiwa* 'she marries' (a verb of possession derived from **neʔθa* 'my husband'). In Fox it has a much narrower meaning, basically 'she marries without wedding ceremonies'.

(26) †*oko(-)wa* 'doe' ('female deer')

There is a single textual occurrence of this word. It is the cognate of Menominee *oko-w* 'female animal' (Bloomfield, 1975:173), 'doe' (Skinner, 1921:196), and Shawnee *hokowa*, 'doe'.

(27) †*a-te-koškwe-h* ('crow')

This is the match of Ottawa ⟨*aandegshkwenh*⟩ 'crow' (Rhodes, 1985:8).

(28) †*wa-wa-htehkohike-wa* ('if anyone is signaling with the hand(s)')

For this word we have Harry Lincoln's pronunciation as recorded by Michelson; the proposed phonemicization is a compromise between this and what the cognates point to: Eastern Ojibwa ⟨*waawaatkohged*⟩ 'beckons' (Rhodes, 1985:354), Southwestern Ojibwa *wa-ttikwaʔike* 'nod, beckon, wink (as a signal)' (Baraga, 1880:404, phonemicized).

(29) †*kenaho-čikani* ('What they use when they tie up their enemies when they capture them.')

No cognates are known for this ethnographically precious word for prisoner tie, but the etymological analysis is transparent; it is an instrumental noun derived from a stem combining PA **keθ-* 'restrain' and Fox *-aho-t* 'drag (it)'.¹²

One word looks like a restructured or misremembered archaism:

(30) (ninopwa) ('I shall smoke')

This is clearly related in some way to PA **wespwe-wa* 'he smokes' (stem **wespwa-*). Descriptively, the form in example 30 appears to consist of the first person future preverb *ni-h-* and a Fox stem †*no-hpwa-* (word-final shape †*no-hpwa*). Fox †*no-hpwa* would be precisely the expected reflex of the first-person singular form PA **no:spwa* 'I smoke' (PA **ne-* first person + **wespwa-*), with fossilized preservation of the old morphophonemics of prefix contraction otherwise eliminated in Fox. Whether the apparent reanalysis of the contracted prefix as part of the stem and concomitant restructuring of the form is an old feature or results from its having been slightly misremembered is an open question.

Finally, Kiyana defined some archaic words that are simply unknown:

(31) (kanimewa) ('when an old man smokes')

⟨mekwatetihā⟩ ('young of buffalo')

⟨anepitesia⟩ 'male milk'

⟨ketikešaki⟩ 'rabbits'

⟨aponewa⟩ (he gives things to his bride)

⟨atomepyani⟩ (carved-out container for maple sap)

⟨tokesenwi⟩ (the wind is calm)

⟨mašinepwewa⟩ (he doesn't share his food)

⟨ayasekote⟩ (one after another, time after time)

⟨akwawewa⟩ (he has a lot of prepared meat hanging up)

⟨tetewa⟩ (big fire-log)

⟨nektwenene⟩ *aho-wa* ('he or she has many children')

⟨kečinektwenenawe⟩ *ki-h-po-ni-pena* (we'll pass a potential camping place and camp further on)

⟨šenewiwa⟩ (he or she has a venereal disease)

⟨tetakoa⟩ (ceremonial attendant).

Information on any cognates of the words in example 31 would be welcome.¹²

Kiyana also has a separate section labeled *wa-waneška-hi-kanawi-nani* 'naughty words' (32–39). These are terms for sexual behavior but are usually not in themselves indecent. In the explanation given in full in example 32, however, information on the propriety of words is given (C: 63B–E):

(32) neniwa owi-wani ahpene-či kotakani neniwani
man 3s+wife all.the.time other+OBVsg man+OBVsg

manamana-nite.^[1] ke-ko-hi-'pi

RED.copulate.with+3'-3"/SUBJ something=HRSY

to-tato-tamawa-pi owi-wani.^[2]

RED.do.(so).to.(SEC.OBJ).of+X-3/IND 3('s)+wife.

^[1]mya-neteki kanawi-ni.

be.bad+0/PPL(INsg) word

^[2]we-wenhki- kanawi-ni

be.good+0/PPL(INsg) word

'If another man always keeps fornicating¹ with a man's wife:
Something is being done² to his wife.'

¹Improper word

²Proper word.

In this entry there are notes in balloons, attached to the words with lines (here rendered as numbered notes), that distinguish proper and improper expressions for ‘to copulate’.

(33) *nenano-čiha-wa* (‘I’m bundling with her (sleeping with her secretly without having sex).’)

This is really just an ethnographic explanation of the practice of courtship by bundling (to adopt the early American English term). A young man sneaks into a wickiup at night and lies with a young woman on her sleeping platform, anything further being subject to negotiation.

(34) *mehči=pi pakine-wa* ‘he raped her’ (‘When any man “does something” to a woman, and she is unwilling but the man goes ahead and attacks her.’)

This is an ordinary expression, literally meaning ‘it is said he threw her to the ground’.

(35) *wapašihapi, menipopi* ‘she is gang-raped’ (‘When a woman drinks whisky and is very drunk, and many men repeatedly “do something” to her.’)

These are also common expressions, the first meaning literally ‘she is disparaged, treated as of no worth’. Both are proper.

(36) †*pi-htawa-hkwisečike-wa* ‘she has lovers on the side’ (‘A married woman who lets any men fornicate with her.’)

(37) †*pi-š-awi-kocini* ‘the one who first had sex with her’

The third person indicative form would be *pi-ši-awiwe-wa*.

(38) †*aški-paši-škičnwa* ‘he has sex for the first time’

The literal meaning is ‘he gets peeled back for the first time’

(39) †*ki-mahike-wa* ‘he copulates with a sleeping woman’

The literal meaning of this is ‘he strikes secretly’. Examples 36 through 39 have not yet been confirmed by field work, but their analysis seems clear.

Wordlist

Kiyana’s apparently later unglossed wordlist (D, ms. 2841:49–58, ms. 2778:1–48) gives words at times in semantically related sets, in some cases in formally related sets, and sometimes without discernible order. There are pairs and sets of similar words and expressions: the list begins with two questions with *kaši* ‘what?’ followed by two derivationally related negative forms (‘I didn’t find it’ and ‘he doesn’t remember it’), and two semantically similar phrases with the emphatic pronoun *wi-na* ‘himself’ (‘he kept instructing him or them himself’ and ‘he conversed with someone else himself’). Two words illustrate the suppletive stem and final for ‘sing’. A set of four verbs all have the initial *pi-n-* ‘clean’. A set of eight illustrates *aniw-* ‘fast, intense, etc.’; perhaps Kiyana’s interest was caught by the different meanings it has depending on the following final: ‘he walks fast’; ‘he has big, bright eyes’; ‘he has a loud voice’; ‘she has children one right after another’. There are some words for cooking and some for riding horses and some for types of dogs. A page of verbs relates to court-

ship, including different inflected forms of the important verb for ‘to snow someone’ (literally ‘hit with a missile’), and a set of verbs with the medial for ‘wife’ or ‘girlfriend’. There is a set of aorist forms for seasons and types of weather. A list of body-part terms beginning with the third person or indefinite prefix *o-* is followed by a list with the indefinite prefix *me-* (Goddard, 1995:126). Another short list is of articles of clothing. And so forth, page after page.

Some special topics are treated. There is one page each listing standard pick-up lines used by young men, conventional responses to these used by young women, and common or culturally normative remarks made by jealous husbands to their wives. There is a list of characters in winter stories, including some for which stories have not yet been found in the corpus. There are a few sets of forms with certain preverbs and sentences with certain idiomatic particles, though why these were selected is not evident. There is one page of relational forms of the verb, a subtle, obsolescent, and paradigmatically defective verbal inflectional category (Goddard, 1995:141–146).

Some examples of the pick-up lines (40–43) and responses (44–46) attest to Kiyana’s interest in this semantic field:

(40) *ta-ni-ča-h a-mi-š-šikeki ni-šiyakwe*.| ‘How would it be if we went together?’

(41) *ta-tepi-ča-h e-ya-yani*. ‘Where are you going?’

(42) *kepo-si-ki-waki-wa-čika-pa*. ‘You’re always standing around all by your lonesome.’

(43) *anike-me-h-pe-taka-wi šišineno*. ‘Could you slide in a little?’

(44) *neša-kwe-nemo-ča-h wi-h-ki-hkišina-ni*. ‘I don’t want to move over.’

(45) *meše, ‘na-kwa-no, inena-ne, ki-h-na-kwa?* ‘When I tell you to go, will you go?’

(46) *hwi-, pe-hki-š’kwe-, me-me-nešimiwane-ni*.| ‘Gee, you’re really embarrassing me.’

Example 43 is a line used to initiate bundling (the practice referred to in example 33), and examples 44 and 45 are presumably listed as two possible responses (although not arranged by Kiyana to make the connection explicit).

A set of expressions illustrates the idiomatic use of the preverb *nana-hi*, derived from an initial that usually means ‘arrange, order, position suitably’:

(47) *nenana-hi-owi-hka-ni* ‘I shouldn’t have made him my friend.’

There is a full page each of examples illustrating the particles *na-pi* ‘better’ and *pe-hki* ‘really’:

(48) *na-pi-wi-na natawi-mawi-kyawamenakwe*. ‘Why don’t we try to go and get someone jealous at us?’

(49) *o-, pe-hki-meko nenano-čimekwa*. ‘Oh, he really maligned me unjustly.’

The subject matter reflects a continuing interest in matters revolving around courtship, jealousy, and their consequences.

The Commentary on the White Buffalo Text

Kiyana's two scholiastic commentaries provide explanations and exegesis for two of his ceremonial texts. These are written in sections that are marked with the numbers of the pages where the words or passage being commented on appear in the original manuscripts. As the designation chosen for them here implies, they may be compared to the scholia that provide explanations and commentary in manuscripts of classical texts, though Kiyana's commentaries perform this function for texts he wrote himself.

The examples below are from the commentary written on the White Buffalo text (A). Some of the explanations are for words that are fairly ordinary:

- (50) P. 11 [of the text]:
e-h=ki-ma-hečī 'he is approached unobserved'

[Explanation:]

ki-mo-čī e-h=ne-wočī. ...wi-na e-h=pwa-wi-ne-wowe-čī
'He was secretly seen. ...He himself saw no one.'

This is a straightforward gloss of an ordinary but semantically complex word. It is part of an explanation that expands on the text.

Many of Kiyana's explanations are glosses of songs. There exist three sets of interpretations of the songs given in the account of the White Buffalo ceremony and its origin. Horace Poweshiek included translations of the songs in the translation he wrote of the entire text. After this translation was typed up, Michelson reviewed the meaning of the songs with Kiyana directly, using Willie Poweshiek, Horace's younger brother, as an interpreter. (Michelson dated this interview to 1917.) Kiyana's written commentary seems to be independent of both of these. He must have written it with the text he was commenting on in front of him, and yet it would be reasonable to think that its composition was a reaction to inquiries about the meaning of the text and its songs. And curiously, in his edition of the White Buffalo text (Michelson, 1925a), Michelson uses the information he obtained in the 1917 interview but makes no reference to Kiyana's written commentary.

A simple example from a song is the following:

- (51) P. 120 [of the text; from a song]:
mehtekwineniwaki, _neniwaki.
tree-men men

[Explanation:]

ča-ki=šī- mehteko-ni,...
all (such)- trees (= 'all kinds of trees')

Here Kiyana had apparently noticed that the noun final *-in-eniw-* in the ceremonial term *mehtekwineniwaki* 'tree spirits, trees' does not mean specifically 'man, male', like the corresponding noun *neniwa*, whose plural echoes it in the song. He explained that 'all kinds of trees' was meant.

Somewhat more obscure is the following:

- (52) P. 125 [of the text; from a song]:
ahkwičī_nenosō-ki (yo) owa-nata-kanwa-ki.
on.top buffalos VCBL 3p+mound(s)+LOC

[Explanation:]

ahkwitahkamiki taši-kohpičī_nenosō-ki ahki
on.top.of.earth (there)-buffalos earth
e-h=wa-nehkwā-wakeškamowa-čī.
hollow.out.as.earth.by.foot+3p-0/AOR

Taking these words at face value, the word-for-word translation of this song line is 'on top' / 'buffalos' / (vocalic) / 'on their mounds'. The reference would appear to be to the underground buffalos worshipped in the ceremony, whose representations are placed on an earthen mound having an indented top. Kiyana's written explanation, however, is 'above-ground buffalos hollow out the earth with their hooves' "Above-ground buffalos" would presumably be ordinary buffalos, as opposed to manitou buffalos, who live underground.

Kiyana's interpretation in example 52 differs from the one that Michelson obtained from him orally and published in his edition: "The buffaloes are standing so much there, in their holes" (Michelson, 1925a:100). In this reading, manuscript (<akwičī) was taken not as *ahkwičī* 'on top' but as a song variant of the ordinary word Michelson perceived as [a-yahkwi-čī] (his narrow phonetics are here normalized), presumably *a-yakwi-čī* 'more and more, to excess'. The last word in the line was taken as referring to 'holes' rather than to 'mounds', hence 'in their holes', and was explained as "the place which the buffaloes have dug up with their horns."

On the face of it, however, the first and last words of the song line must be taken as a discontinuous constituent meaning 'on top of their mounds'. The concave top of the mound, and its iconic association with the wallows of the manitou buffalos, may account for why it was described to Michelson as a hole, but the oral explanation that horns were the instrument for digging is inconsistent with the stem-final element in the verb in Kiyana's written explanation, which must mean 'by foot'. The literal meaning of the whole line is presumably 'buffalos on top of their mounds'. Kiyana's explanations seem like exegetical commentary, inspired by expansive folk-linguistic analyses of the words that make up the syntactically unusual discontinuous locative phrase.

Divergent analyses are also found in the case of example 53:

- (53) P. 130 [of the text; from a song]:
ki-wihkanawe-wa inenoswa
make.trail.about+3s/IND buffalo (archaic form)

[Explanation:]

omahomye-miwa_ wa-peški|-kohpičī-nenos_okima-wa.
RED.have.a.trail+3s/IND white-upland-bovine-chief

This line means, literally, 'The buffalo makes his trails about.' The word for 'make trail(s) about' is glossed with the ordinary verb of possession derived from the word for trail, reduplicated (hence, 'have trails'). The word for buffalo in the song, *inenoswa*, is an archaic form of *nenoswa*. The form *nenoswa* can still be used to mean 'buffalo' in ceremonial contexts or with reference to buffalo manitous and the ceremonies for

them, but today it ordinarily means 'cow, ox' Kiyana explained this as 'buffalo chief'.

Kiyana's written explanation of the verb in the song is obviously preferable to the one he gave Michelson orally (Michelson, 1925a:102–103). In the oral explanation the medial+final *-ihkanawe-*, meaning 'make or have a trail', was interpreted as the equivalent of the ordinary word *kanawiwa* 'he speaks', hence 'goes about speaking'. Kiyana explained *inenoswa* to Michelson as *maneto-wi-nenoswa* 'spirit buffalo', which has the same reference as the 'buffalo chief' of the written explanation.

Conclusion

Alfred Kiyana's linguistic writings are significant in several ways. They, of course, provide a great deal of information on the Fox language, especially on archaic and unusual vocabulary. But they also give us insight into how Kiyana thought about the work he was doing for Michelson. The general impetus for this work came from Michelson, who was paying Fox writers to write texts. The specific selection of material, though, seems clearly to be due to Kiyana. The evidence is that Kiyana wrote his lexical lists in 1916–1917, after working with Michelson for several years, and although the commentaries must have been written earlier, when he still had access to the manuscripts, he evidently did not discuss them with Michelson.

Within a few decades of the inception of literacy in the Meskwaki community, here was a writer compiling lexical materials, developing a metalinguistic style for defining words, commenting on difficult passages of texts, and assembling sets of related forms and expressions that illustrated systematic patterns of derivation and inflection. Kiyana was thinking like a linguist. His work goes beyond that of an informant or consultant. It belongs in the corpus of linguistic writings on Algonquian that form the basis for the understanding of these languages. We should recognize him not just as a source of marvelous data, but as one of our intellectual grandfathers.

Notes

1. "Kiyana" is the spelling he used as an English rendition of his Meskwaki name, *kya-na-wa*, in the materials he wrote for Michelson. This is a name of the War Chief lineage, the highest ranking division of the Fox Clan. His descendants spell the family name "Keahna."

The people of the Meskwaki Settlement and the language they speak (which is shared with the Sauk) have generally been referred to as Fox. The name Meskwaki (the now-preferred spelling) is especially appropriate for the modern community and its residents.

2. Early in his fieldwork Michelson elicited a vocabulary from Kiyana in English (NAA ms. 2647), but the manuscript shows that Michelson misunderstood Kiyana's pronunciation of English "silver fox" as "son of Fox," and "God" as "card." The word *kehci-pi-hi* 'belt; belt-line, waist' is glossed "back-side," and *okota-kani* '(his) throat, gullet' is glossed "Adam's apple," indicating that at least some body-part terms were elicited by pointing.

3. Michelson wrote "1916 Consanguinity 1916" on the cover of the notebook containing the translation of NAA ms. 2232 (the first half of manuscript C) that he recorded from Harry Lincoln, a Meskwaki of part Potawatomi descent who was his chief interpreter and go-between. NAA ms. 2778, containing the second part of manuscript D, has annotations by Kiyana on the inside

of the front cover that apparently calculate the number of hours worked each day. (Perhaps this was intended as a basis for payment, but for the most part, at least, Michelson paid Meskwaki writers by the page.) Kiyana listed the days of the week by their Meskwaki names, beginning with Friday, and Michelson added the month and the date, beginning with September 7. During the years that Kiyana was working with Michelson, September 7 fell on a Friday only in 1917.

4. The White Buffalo text, one of the first texts edited and published by Michelson, presents the origin myth and songs of the buffalo ceremony of the Fox Clan, Kiyana's clan (Michelson, 1925a). The somewhat shorter Green Buffalo text, published in English only, similarly treats the buffalo ceremony of the Wolf Clan (Michelson, 1937). It seems likely that Kiyana described the ceremony of his own clan first. The unique similarity of the two commentaries supports the conclusion that they were written at very nearly the same time, and the fact that they must necessarily have been written while Kiyana still had the original texts also narrows the possible time span for these writings.

5. References to manuscript C are by original page numbers and by line (indicated by capital letters), as edited.

In the interlinear glosses the following abbreviations are used: AN=animate; AOR=aorist conjunct; CONJ=plain conjunct; DIM=diminutive; EMPH=emphatic; HRSY=hearsay; IN=inanimate; IND=independent indicative; OBL=oblique; OBV=obviative; pl=plural; PPL=participle; RED=reduplication; SEC.OBJ=secondary object; sg=singular; SUBJ=subjunctive; VCBL=vocable. (The obviative is the secondary or thematically subordinate third-person category of Algonquian. Nouns with third-person possessors are always obviative, and "OBV" is omitted from the interlinears for these.)

3=third person animate; 0=third person inanimate; 3'=third person animate obviative; 3(′)=third person animate obviative or non-obviative; 3″=third person animate further obviative; p=plural; s=singular; X=indefinite person.

Certain editorial conventions are used in the textual material cited. A space or a hyphen in the edited text corresponds to a word divider in the manuscript. A bar (|) indicates an absent word divider at line end. A link (·) or linked hyphen (·) indicates an absent word-divider line-medially. A double hyphen (=) sets off proclitics and enclitics (and cliticized words), which are always written as one word with their hosts. A hyphen or linked hyphen sets off or earmarks the parts of a compound stem, one consisting of more than one phonological word. (The preposed elements in compound verbs are called preverbs in Algonquian grammar, and those in compound nouns are pre-nouns.) The linked hyphen is also used to join the elements of a compound fused by elision, where no word divider would ever be used.

The asterisk (*) marks reconstructed words, assumed to have existed but not attested.

6. The dagger (†) marks obsolete forms whose exact phonemic shape is inferred.

7. Shallow-pointed brackets ({...}) mark words that are simply transcribed literatim from the source rather than being given in a standardized transliteration or phonemicization. Words in this transcription lack some phonemic distinctions and may be ambiguous.

8. Fox *mesawina-kosiwa* reflects Proto-Algonquian **mweʔθawina-kwesiwa*; related words are Cree *mostawinawe-w*, *mostawinam* 'he sees and desires him, it', Menominee *moʔnawenawe-w*, *moʔnawenam* 'he admires him, it', and Ojibwa *nimissawe-nima*; *nimissawe-nta-n* 'I desire, want him, it'.

9. The formative elements of primary Algonquian stems are initials, medials, and finals.

10. William Jones was a one-quarter Meskwaki who grew up among the Sauks (Oklahoma Sac and Fox). He learned to speak Meskwaki from his father's mother, though it is clear that he did not have native fluency as an adult. As a student of Franz Boas he worked on the language with his father, Henry Clay Jones, and his father's relatives in Tama, whom he never named. Kiyana told Michelson that his grandmother was Jones's father's mother, *keti-hkwe-wa* (NAA ms. 2647); she was not the mother of either of Kiyana's parents, however, and her exact relationship to Kiyana (among the various kin types called 'grandmother' in Meskwaki) is not known. Jones's Fox manuscripts are in the NAA.

11. For a decade after Michelson's death, in 1938, James A. Geary, a professor of Celtic Languages at Catholic University, worked with Harry Lincoln to edit

Kiyana's 1110-page text of the story of the culture hero Wisahkcha. Geary's manuscripts are in the NAA.

12. For example, ⟨tetewa⟩ 'big fire-log' could be the cognate of the Delaware words for 'fire', Munsee *tānte-w* and Unami *tāntay*, which otherwise have no

etymology. The Fox word would then be †*tete-wa*, from PA **tente-wi* (assuming original inanimate gender). This would make sense as **tem-* 'truncated' + *-(e)te-* '(inanimate) affected by heat or burning', i.e. 'one that has or gets its end burned off'

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The Munich Chukchi Collection

Jean-Loup Rousselot

The Staatliches Museum für Völkerkunde (SMV), München (the Munich State Museum of Anthropology), is one of Europe's major anthropological museums, with collections comprising about 350,000 specimens from all parts of the world. About 500 items are from Siberia; a few of these date from the eighteenth century, and the remainder date from the nineteenth and twentieth centuries. Acquired over the years from several different donors, the museum's Siberian collection includes objects from various regions of Siberia, but the majority are from the Chukchi and the Yuit Eskimos of the Chukotka Peninsula in northeastern Siberia.

The Chukchi collection, apparently assembled in the late nineteenth century, was officially received by the museum on 14 January 1899 (SMV Archives, 1899), as a single donation from Eugen Wolf, a German philanthropist and journalist. Although the precise origins of the collection are unknown (it is doubtful that Wolf made the collection himself), it is of considerable significance both because it attempts to document systematically all aspects of the life of these reindeer herders of the tundra and because very little material from the Chukchi of that period has been preserved in museums (for information on the few other extant collections, see Stein, 1881; Krause and Krause, 1882; Nordenskiöld, 1882; Bogoras, 1901, 1904–1909; and Abel, 1969:76). Like the remainder of the museum's Siberian collection, however, a description of the Wolf collection has never been published, and only a small portion of it has appeared on public exhibition, displayed in a single exhibit case in the museum's permanent exhibits during the early decades of the twentieth century (Scherman, 1922:11).

The Donor

Eugen Wolf was born 24 January 1850 in Kirchheimbolanden, a small city in the Palatinate region of southwestern Germany,¹ and died in Munich six decades later, on 10 May 1912, of typhus (Muth, 1912; Dreyer, 1915). Independently wealthy, he devoted most of his life to traveling, first within

Europe and then to the New World, Africa, and the Far East. In 1873 he journeyed to South America, followed by trips to central Africa (1884–1885), the United States (1887), East Africa (1889–1890), South Africa (1891–1892), and Madagascar (1895) (Wolf, 1905). Between August, 1896, and June, 1898, he traveled extensively in China, Japan, and Siberia. In 1909, three years before his death, he visited Oceania (Wolf, 1915).

Both in Germany and abroad, Wolf, a polyglot, met with politicians, businessmen, and diplomats to promote an expanded role in world affairs for the newly founded German Empire. He envisioned international trade as a key element in Germany's development as a world power and lobbied for increased exchange of German manufactured goods for raw materials from other countries, particularly China. An ardent nationalist, he feared that the growing presence of Russians, Americans, and Japanese in China would limit the possibilities for German expansion there. He expressed his concerns to former Chancellor Otto von Bismarck during a series of meetings before departing for China in 1896 and repeated them in his account of his travels, *Meine Wanderungen, I: Im Innern Chinas* ("My Travels, I: In Inner China"),² published in 1901. While in China he visited many foreign concessions, and, based on this firsthand experience, he developed a plan for German expansion there, which he proposed to the Ministry of Foreign Affairs in Berlin upon his return (Wolf, 1904).

Wolf was a prolific writer, publishing narratives of these and other travels in several books and in a number of newspaper articles, which appeared primarily in the *Berliner Tageblatt*. He used his writings to promote the approach to international relations, both political and economic, that he believed Germany should follow.³ He also linked his extensive collecting activities to his international agenda, commenting that he "donated ethnographic collections to Bavarian and Prussian museums to instruct young Germans about foreign cultures" (Wolf, 1901: 288).⁴ It is known that he made collections in both the New World and Africa and that his ethnographic collection from Madagascar included around 600 items (Wolf, 1901, 1904:189–191). He did not, however, mention in any of his publications the Chukchi collection donated to the SMV in 1899.⁵

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History of the Collection

When Wolf delivered his Siberian collection to the SMV, he provided an inventory of the collection that included brief comments on each item and on various aspects of Chukchi life; I refer to this inventory as the "Collector's List" (SMV Archives, 1899). It is likely that Wolf personally prepared this inventory because it is signed by him and is handwritten in German using Roman characters. It is known that Wolf wrote German with Roman characters rather than employing the German alphabet current at the time and also that he completed the inventory of his ethnographic collection from Madagascar, in which he displayed a rare sensibility for its value (Wolf, 1904:25–26, 189). Nonetheless, based on my review of Wolf's travel accounts, I have concluded that he could not have personally assembled the Chukchi collection.

In 1897 Wolf visited Vladivostok, where the construction of the eastern section of the Trans-Siberian Railway had just begun, and also Kamchatka, but he did not indicate that he traveled north of Kamchatka or Sakhalin Island or otherwise had direct contact with the Chukchi (Wolf, 1901, folded map, 1904: 212–214). It is therefore very unlikely that Wolf acquired his Chukchi collection directly from the Chukchi, although he may have gathered or purchased some of the zoological specimens that appear on the list—those from the Anadyr region, Indian Point (Mys Chaplino), and East Cape (Mys Dezhneva), collected between 1894 and 1897—during this trip to the Russian Far East.

If Wolf had no direct contact with the Chukchi, he must have acquired the collection from someone else. Given the choice of objects included in the collection, as well the quality of information that appears in the Collector's List, the original collector (and also the source of information on the collection, if this was a different person) must have known the culture and customs of the native population well. I have found no information, however, either in the museum's records or elsewhere, that would reveal the identity of this person or these persons.

I originally suspected that the Russian ethnographer Waldemar Bogoras had assembled the SMV Chukchi collection. Bogoras was exiled to Yakutia between 1890 and 1898 and during the last three years of that period conducted research on the Kolyma River and in northern Chukotka as a member of an expedition financed by the merchant Innokenty Sibiriyakov (Bogoras, 1904–1909:1). Also, the information that appears in the Collector's List corresponds closely to that included in Bogoras's (1904–1909) monograph on the Chukchi, and the collection that he made for the American Museum of Natural History in New York, although larger than the SMV's, includes specimens that are of the same type and manufacture. The Collector's List was, however, prepared no later than 1899, and the objects in the collection are all from southern Chukotka (Anadyrsk and the Pacific Coast), a region that Bogoras did not

visit before 1900–1901. Bogoras also did not mention the Munich collection in his publications.

Another possible source of the SMV collection was Nikolai Sliunin, who spent 1892–1893 on an official mission studying the fauna of Kamchatka and the northern Pacific Coast. As Sliunin's (1895, 1900) publications reveal, he was also interested in ethnography, but it appears that all his collections were deposited in Moscow.

A more likely possibility, which needs further investigation, is that the collection was made by the Russian civil servant Nikolai Lvovich Gondatti, governor of the Anadyr District from 1893 to 1897 (Bogoras, 1901:1; Stephan, 1994:318). The Anadyrsk *ostrog*, a military post dating back to 1649, had been abandoned in 1764 or 1770 because of violent Chukchi resistance to the Russian conquest of Chukotka. Cossack efforts to subject the adult male population to the fur tribute, or *yasak*, was a failure, and the post was not reopened until 1889, when the Anadyrsk District was created (Stephan, 1994:88). By that time, peaceful trading relations had been established with the Chukchi, and Anadyr had become a trading post of considerable importance, visited primarily by American fur traders and New England whalers (Dittmar, 1856:37; Knox, 1870:84, 91; Kennan, 1871:286–287, 328–330; Bogoras, 1904–1909:697). Gondatti, an able and energetic administrator "partly moved by scientific interests" (Bogoras, 1904–1909:710), made extensive natural history collections during his time in Siberia, and ethnographic material that he collected was deposited in 1898 at the Imperial Academy of Sciences in St. Petersburg (Bogoras, 1901). Before moving to his next assignment, on the Ussuri, in 1899, Gondatti probably traveled to western Europe, where he sold specimens collected in Anadyr to a gentleman of Paris. These specimens were acquired in 1911 by the Musée d'Ethnographie du Trocadero in Paris (Falck and Falck, 1963: 51; Bucher, 1994:119).

If Wolf did not acquire the Chukchi collection from Gondatti, he may have purchased it in Europe from an unknown collector. In 1890 a large and well-documented Chukchi and Koryak collection was displayed in Leipzig and was sold the same year to a collector from southern Germany (Schmeltz, 1890a, 1890b, 1891). Wolf was in Germany in the summer of that year and could have been the buyer (Wolf, 1904:42–60).

At the Museum

The Royal Museum of Ethnography, the predecessor of the SMV, received nine crates containing the Wolf collection in January of 1899. At the time, the museum was located in the Hofgartenarkaden, a very narrow building situated north of the King's Palace, whose small rooms were already filled with permanent exhibitions. Probably due to lack of space, the crates were soon moved to another government building, the Oberlandesgericht, or Upper State Court, where, in March of the same year, the crates were opened and the accessioning of the collection begun (SMV Archives, 1894–1902:95).

The original collection consisted of about 600 specimens. During the long journey to Munich, vermin destroyed some organic material, especially some skins and skin clothing, which were discarded, and one crate (crate 10 on the Collector's List), with more than 145 ethnographic specimens, did not arrive. The vast majority of the objects came from the Chukchi, but a few items were collected among Siberian Eskimos of Indian Point and East Cape (a dog-sled harness, a sling to catch aquatic fowl, and some fishing rods). Other objects—jewelry from the “Kereks” (Koryaks), a large saddlebag from the “Lamuts” (Evens), and a box imported from China—appear to have been trade goods. Also present were eight human skulls (two from the Coastal Chukchi, four from Reindeer Chukchi, and one each from the Eskimo of Indian Point and East Cape) and some zoological and botanical specimens.

Dr. Max Buchner, the director of the museum,⁶ served as the registrar for this collection. He returned some duplicates to Wolf (SMV Archives, 1899:16, 18) and, in 1902, transferred the nonethnographic materials to Munich's museums of physical anthropology, zoology, and botany. Other items (inventory no. 99-226) were exchanged with the Reiß-Museum of Mannheim.

Four decades later, soon after the first bombing of Munich in 1942, the museum hastily moved its collections, including the Wolf collection, to two castles south of the city, where they were safely stored for years (Harrer, 1993:67). Reaccessioning and re-storage started in the late 1940s but was not completed until several decades later. The Wolf collection, which reappeared in 1981, showed almost no signs of damage. Proper storage in large crates throughout this period and the fact that it has never been used extensively in public exhibits contributed to the preservation of the collection.

Description of the Collection

The Chukchi ethnographic collection housed in the museum today includes almost all of the items accessioned 100 years ago. Totalling around 330 items (frequently, the same inventory number is used to label several different objects), most aspects of Chukchi life and material culture in the late nineteenth century are represented.

The rich variety of Chukchi clothing is documented in sets of garments for adults and for children of all ages as well as clothing used for special purposes, such as spring sealing. Recreational items include children's toys (a leather ball, a drum, a stuffed mouse, buzzers, toy reindeer (Figure 1)), a chess set (wooden board and figures), two musical instruments (a homemade violin and a balalaika), and a pipe and tobacco bag (the tobacco is mixed with wood dust). Important ceremonial items also form part of the collection: fire lighters and fire boards (Figure 2), an offering plate, a ritual drum, and different kinds of gloves used in the summer and winter to perform the dissection of the dead.

Weapons and tools for hunting, fishing (Figure 3), and trapping are well represented in the collection. These include armor made of iron plates sewn to a hide, bows, arrows, various kinds of spears (including a buoyant bird spear used to hunt water fowl and a leister for small fish), a spear thrower, harpoons and harpoon floats (which were also used as fenders), fishnets made from hair and sinew, fishing lines, hooks for large fish, and a series of traps or snares for ermine, fox, and mice. A full-size kayak made of reindeer skins—a rare item in museum collections—is equipped with a combined paddle-spear used to hunt wild reindeer as they crossed the Anadyr River. Tools used by



FIGURE 1.—Two reindeer toys that pulled a child's sled. According to the Collector's List (SMV Archives, 1899:12), “both antlers or at least the one on the inner side, were sawn off to avoid entangling.” The bodies of the reindeer are carved from wood and are covered with the fur of unborn reindeer. Reindeer fur, wood; height, 12.5 and 13.5 cm. Staatliches Museum für Völkerkunde München (inventory nos. 99-269 and 99-273). Photograph by Veronika J. Grammer.



FIGURE 2.—Fire board. The Collector's List (SMV Archives, 1899:23) describes this as a "complete sacred Gürgür, shaped like a human body with head, on which an Okkamak is hanging, bow, drill and mouth piece. This Gürgür is used on holidays to light the sacred fire. The Okkamak is a little spirit protecting a lake, a creek, etc." This apparatus is made mainly by the Chukchi themselves and is carried under the belt. Wood, leather; length 55 cm. Staatliches Museum für Völkerkunde München (inventory no. 99-210). Photograph by Veronika J. Grahammer.

men include snowshoes, snow beaters, drills for iron and wood, crooked knives, a bodkin, a chisel for woodworking, an adze, a hoe, reindeer harnesses, a lasso, a whip, and a bucket for collecting urine (Figure 4). Women's tools and domestic items used in the completion of household chores include spoons of wood, bone, and antler, wooden plates, a goblet fashioned from the horn of a mountain sheep, mauls made from walrus bacula, a work bench, skin scrapers, and a needle case (Figure 5). Also present is a stone kettle with wooden stand and a leather pouch containing a stone anvil and two stone hammers used to break open reindeer bones to extract the marrow (Figure 6).

Sets of samples of different kinds of materials are included for didactic purposes, for example, to demonstrate the stages of preparation of sinew for thread, or to document the tree fungus from which the ashes to mix with tobacco are prepared. Also, samples of hides and skins (from seals, walrus, wild and domesticated reindeer, mountain rams, otters, ermine, blue and white foxes, and mice) document both variation between species and seasonal variation within species that make them appropriate for different applications. These variations can be seen in the evaluation of alternative sources of leather thongs. Thongs made from the hides of young walrus were the strongest and were particularly appropriate for use in making fishnets and harpoon lines for hunting walrus and whale, while hides of adult walrus were converted into thongs for lashing boats and sleds. Thongs prepared from a reindeer hide just after skinning were regarded as stronger than the white thongs made from the hides of young bearded seals, which were nonetheless used to drag boats upstream. Similarly, sealskins were made into shoes and mittens, seal and walrus intestines and walrus bladders were used for waterproof clothing, wild reindeer skins served as boat skins, and smoked reindeer hides provided tent covers. Skins of young domesticated reindeer killed in August were used as windows.

An interesting feature of the collection is its inclusion of models of artifacts, only some of which are present in full scale. For example, there is a model of a tent (known in Chukchi as *yaranga*), complete with a cooking pot and tripod, various containers, plates, spoons, knives, axes, shovels, a lamp, skin scrapers with work boards, snow scrapers, tobacco boxes, a urine vessel, mosquito nets, a leather bag with stone anvil and hammer, and a scaffold for drying fish. The residents are represented by dolls. Also present are models of at least eight different kinds of sleds, umiaks for whaling and transportation, hunting and fishing equipment (a bow and four arrows, a sling, a fox trap, harpoons for sealing and walrus hunting, snowshoes, and fishnets), and some ceremonial paraphernalia (a drum, a fire drill bow, two offering cups with spoons, two reindeer shoulder blades used in divination, and paddles associated with the *байдара* festival).

Although the importance of this collection is beyond debate, one cannot help but regret the loss of crate 10, which never arrived at the museum. According to the Collector's List, this crate contained five wooden and 50 bone toys, five dolls, a cradle, four parkas, four spoons, a crooked knife, a woman's knife,

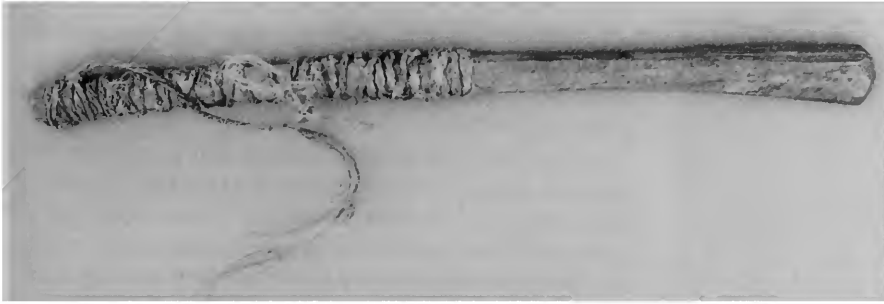


FIGURE 3.—Fish lure with rod to attract fish; used for ice fishing in combination with a fish leister. Bone (baculum), sinew, fish skin, wood; length of fish, 22 cm. Staatliches Museum für Völkerkunde München (inventory no. 99-125). Photograph by Veronika J. Grahmmer.



FIGURE 4.—Leather pouch to carry human urine. The herder uses urine, which reindeer like to drink, to attract reindeer to be harnessed (Bogoras, 1904–1909:85–86). The Collector's List (SMV Archives, 1899:17) describes it as a "little leather bucket for urine, from which reindeer will be drinking on the road. Essential to reindeer herders." Leather, wood; height 15 cm. Staatliches Museum für Völkerkunde München (inventory no. 99-175). Photograph by Veronika J. Grahmmer.

FIGURE 5 (right).—Needle case with thimble holder; this apparatus was attached to a woman's belt. The Collector's List (SMV Archives, 1899:5) mentions only a "needle case" belonging to a woman's coat. Bogoras (1904–1909:225) represented slightly different types of needle cases. Metal, leather; length of tube 14.5 cm; total length 47 cm. Staatliches Museum für Völkerkunde München (inventory no. 99-112). Photograph by Veronika J. Grahmmer.





FIGURE 6.—A cook at work at a Chukchi camp. A similar leather pouch with stone anvil is part of the collection of the Staatliches Museum für Völkerkunde München (inventory no. 99-197). The Collector's List (SMV Archives, 1899:23) gives the following information: "Leather bag with stone and two stone hammers; used to break reindeer bones before their marrow was cooked." It also is used to crush all kinds of frozen meat (Bogoras, 1904–1909: 188). Laguna Getlyangen, Chukotka, September 1995. Photograph by Jean-Loup Rousselot.

a stone hammer, an adze, a work bench, a tobacco box, a mosquito net, a sling, resin for chewing and for caulking boats, a ceremonial fire drill, a drum, four paddles used in the spring *baidara* festival, and a set of eight amulets which were hidden in the owner's tent and only occasionally displayed, during private rituals. It is especially unfortunate that all examples of Chukchi art in the collection (more than 50 whale, reindeer, and cod carvings) were packed in this crate.⁷

Conclusion

The Chukchi collection of the SMV is an important resource for Arctic ethnography. It provides an incomparable record of the Chukchi's sophisticated adaptation to their Arctic environment as well as information on the social and religious dimen-

sions of their society. Despite the loss of the carved works of art, the beauty of the skin garments reveals the importance they placed on aesthetics, and the presence of foreign goods substantiates reports that the Chukchi were passionate traders (Dittmar, 1856:36).

As is the case with museum collections in general, the value of the Chukchi collection is enhanced by the fact that it is both systematic and well documented. The person who made the collection and prepared the original description of it undoubtedly was an intelligent and informed observer of Chukchi culture, with a scientist's concern for detail and a naturalist's understanding of the complexities of the relationships between humans and their natural world. The collector also had a clear vision both of what should be collected and of the purpose of natural history museums as these were conceived at the time. The approach to collecting was global in that an attempt was made to document all facets of life and also in that items collected included not only artifacts but raw materials and specimens of the plants and animals from which they derived. The collector also appears to have been aware of the problem of creating a distorted view of a culture that can emerge if only goods produced for outsiders are collected: except for some skin garments, all items were used before they were acquired.

The SMV never organized a scientific expedition to the Chukchi region of Siberia, and it is doubtful that it would have ever acquired a major Chukchi collection if Eugen Wolf had not donated his. Because of Wolf's generosity, the museum is in a position to contribute to a more profound understanding of the native people of Siberia. This contribution has been delayed in part because the collection was unavailable to researchers and the general public for decades; however, a detailed, accurate catalog and public exhibition of the collection are now being prepared, both of which will include all items from the original collection except those lost or destroyed.

Notes

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1. This area has been known as Rheinland-Pfalz since 1946, but in the nineteenth century it was called Rheinpfalz and formed part of the Kingdom of Bavaria. Eugen Wolf considered himself Bavarian.

2. Although the "I" in the title of this work suggests that Wolf intended to publish one or more additional volumes on his travels, I have found no evidence that he ever did.

3. Through his writings, Wolf also expounded his very conservative views on politics, education, race, and social inequality. Although unpopular today, his perspectives reflected his times and do not differ significantly from those of many other European and American travelers of the period.

4. This and all other translations from German are the author's.

5. The Museum für Völkerkunde of Berlin received 148 Chukchi specimens from Eugen Wolf in 1899 and 1900 but returned 101 of them to him in 1902. The motivation for this unusual return is not revealed in the museum's records (Claudius C. Müller, pers. comm., 23 May 1997), and the whereabouts of these specimens is not known.

6. Max Buchner had some experience as a collector in the Far East, spending two years there before returning to Munich in 1890 (Schmeltz, 1890c).

7. These missing items do not correspond to those donated by Wolf to the Museum für Völkerkunde of Berlin in 1899–1900.

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VI. Nature in Culture

Totemism Reconsidered

Raymond D. Fogelson and Robert A. Brightman

The work and days of William C. Sturtevant provide an important link between the final breath of the Bureau of American Ethnology and a revived Americanist anthropology in the new millennium. Sturtevant's multifaceted interests combine rigorous description and thorough scholarship with an openness to entertain, if not always embrace, contemporary international developments in anthropological method and theory. One major theme that connects much of his work is a constant concern with classification; another is his abiding interest in exploring curious corners of the hidden history of anthropology. This essay bears on both these interests.

Categorial Impositions

The use of animal categories as classificatory devices is an old, largely unconscious convention haunting much of the history of Western ideas. These categories were pushed into consciousness with the "discovery" of totemism as a recurrent feature of "primitive" societies by the late nineteenth-century evolutionary theorists. Totemism was taken as a primary diagnostic feature that distinguished the "primitive other" from "civilized us." In 1903 Durkheim and Mauss (1963) set forth an agenda for what would become their version of the sociology of knowledge by means of a comparative analysis of Aristotelian categories of thought—such concepts as space, time, number, person, and classification itself. The notion of totemism as a classificatory device was then already assumed, at least implicitly. We, as well as the "primitive others," classify our worlds by many means, for example, by number (both Roman and Arabic numerals), by letter (both upper and lower case), by color (color coding for race being a particularly pernicious example), by various measures of time (chronological, cosmological, diurnal, and life cyclical, among others), by space or location (e.g., left, right, center, or cardinal directions), and, as we have come to recognize, by animals (political groups, nations, sports teams, and automobiles being conspicuous examples.)

As a culture we are very concerned about the consequences of animal extinction. The cause of conservation attracts both

Green Peace activists and segments of the religious right who see the preservation of nature as keeping faith with God's divine plan for the universe. Biological scientists worry about shrinking gene pools and diminished genetic data banks. Animal extinctions might even affect early childhood learning—mastery of the alphabet might be imperiled without books containing pictures of animals ranging from Aardvarks to Zebras.

Anthropology has long recognized native people's use of animal categories. Such peoples claimed descent from, named social groups after, arranged marriages according to, and observed taboos with respect to certain animal species, more rarely plants, still more rarely natural phenomena (stars, lightning, etc.), and most rarely manufactured cultural objects (projectile points, baskets, etc.). These categories were usually applied to classes or species of animals, but less frequently they were applied to individual members of particular species. Sometimes, as with peoples in northern North America, partnerships between specific representatives of animal species and human individuals are mutually established through vision quest or dream experiences. In parts of Latin America and elsewhere certain individual animals are believed to be sympathetically connected to particular human beings, such that what happens to the animal may literally or figuratively affect the related human individual. This kind of relationship usually is referred to as *nagualism* rather than as totemism, although the two phenomena may possess intrinsic similarity. Sometimes, rather than an entire species or animal, subclasses of animal species or particular body parts are recognized as totems. Thus we may have the Bear Paw clan or the Falcon Wing lineage; or color qualifiers may be employed, as in Red Deer versus Brown Deer groups, White Wolf and Gray Wolf clans, or as in Black Cockatoo versus White Cockatoo moieties in Australia.

At the end of the nineteenth century and during the first two decades of the twentieth century, anthropologists and their friends confabulated, mystified, reified, transmogrified, and exoticized the notion of totemism to reflect, in Lévi-Straussian terms, the deep chasm between nature and culture, between the primitive and the civilized, between "them" and "us." (Lévi-Strauss, 1963:3). Totemism, or the totemic complex, was seen as a primordial form of social organization, an elementary form of religion, a primitive system of philosophy, and an essentialized manifestation of the primitive mind at work and play.

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The origins of totemism, its diffusion, and its logical types were subjects of learned debate. Even Sigmund Freud (1952) took part with his still-interesting book *Totem and Taboo*, in which he drew analogies between the behavior of primitive peoples vis à vis their totemic rites and taboos and the obsessive, compulsive behavior of some of his patients. That Freud himself was not beyond the temptation to utilize animal categories may be recognized in his labeling of two of his classic case studies the Wolf Man and the Rat Man.

As noted by French structuralist anthropologist Claude Lévi-Strauss (1963:3–10), the anthropological obsession with totemism peaked by 1920 and soon became a moribund topic. It is widely believed that Alexander Goldenweiser, a brilliant, first-generation student of Franz Boas, refuted the validity of the concept of totemism in his analytic critique of 1910. In slightly over 100 tightly argued pages, Goldenweiser took issue with the enormous literature that regarded totemism as a unitary phenomenon. He maintained that the assumed totemic complex did not cohere, could not be essentialized, that it was an artificial anthropological construct, a snare, and an illusion. Shapiro (1991), however, noted that Goldenweiser's feat may itself have been mythologized and that Goldenweiser avowed a fundamental unity of totemism through structural and historical relations and metaphysical connections between animals and clans in his important later articles "The Origin of Totemism" (1933a) and "Form and Content in Totemism" (1933b). Indeed, as Shapiro cogently argued, Goldenweiser's rehabilitated totemism anticipated the Lévi-Straussian structural version in many crucial respects.

It is certainly the case that the subject of totemism was resurrected and given new direction by Lévi-Strauss's *Totemism* (1963; the full French title of which translates as "Totemism Today") and *The Savage Mind* (1966). Lévi-Strauss tried to understand the fascination with animal-human relations that so preoccupied some of the best minds of the West. As condensed in his critique of Malinowski's utilitarian theory of totemism, Lévi-Strauss maintained that it isn't that animals are good for eating, but that they are good for thinking. Animal categories serve as logical operators in that they encompass naturally occurring gradational differences.

Lévi-Strauss's intellectualist theory has considerable power and appeal. Yet totems are more than bloodless cognitive categories good for thinking; totems are also good to feel, to experience, to react to. The World War I soldier is expressing more than his divisional affiliation when he says "I am Rainbow" (Linton, 1924:296). The outrage felt by Chicago policemen during the 1968 Democratic Convention at being called "pigs" by Yippie protesters reflected more than categorical boundary transgression. Totemic identification has an affective dimension that is based on more than analogy or metaphoric extension. In this paper we search out and reconsider the original sources responsible for introducing the concept of totemism into scholarly discourse.

Native North American Totemism Retransformed

The original homeland of the term "totemism" is the north-central Algonquian-speaking area of North America. Much confusion revolves around the meaning of the term "totem." In the original published reference to the phenomenon by the English trader John Long, in 1791, totemism clearly refers to an individual tutelary or personal guardian spirit (Long, 1904). The more usual scholarly usage of the term denotes a relationship between descent groups—sibs, clans, phratries, moieties—and their eponymous natural species. Such units are often referred to in early documents as a tribe, family, or, more rarely, a nation; moreover, in native usage the Ojibwa stem *-do-de-m* and its close cognates can also refer to a local territorial group, or what may be technically classed as a deme. In the case of the Ojibwa, *-do-de-m* referred both to one's fellow patrilineal clan members and to an eponymous animal from which the clan was held to be descended or with which its ancestors were otherwise associated (Warren, 1957:41–53; Jones, 1970:138). Thus, for example, the trader Nicolas Perrot recorded in the seventeenth century an Ojibwa myth describing the creation by the Great Hare (presumably Nenabozho, the Ojibwa trickster-culture hero) of the clan ancestors from the corpses of different animals: "Accordingly some of the savages derive their origin from a bear, others from moose, and others similarly from various kinds of animals" (Blair, 1996:37). In academic practice the sociological referent of totemism has become the unmarked category, while the guardian-spirit complex, when associated with totemism, is marked or qualified as individual totemism (cf. Schoolcraft, 1855:196; Tylor, 1898; Durkheim, 1965; Goldenweiser, 1910; Frazer, 1934; and Lévi-Strauss, 1963).

Long's introduction of the terms "totem" and "totemism" into the literature was duly noted by Wundt, Tylor, Durkheim, Frazer, Freud, Boas, and Lévi-Strauss, among others, but it appears that few of these scholars had actually read the original 1791 Long account, *Voyages and Travels of an Indian Interpreter and Trader*, despite its eventual accessibility in reprinted editions published in Reuben Gold Thwaites's *Early Western Travels* series (Long, 1904) and in the later Lakeside Classics series published by R.R. Donnelley (Long, 1922). John Long worked out of Caughnawaga and had a close familiarity with North American Indians. He spoke French and possessed a working knowledge of some variety of Northern Iroquoian and of a variety of Ojibwa used in the fur trade, and his book contains appended word lists. Long spent several winters with Indian companions in the bush; he was even adopted into an Ojibwa band and given the appropriate name *Amik*, or "Beaver." Given that Long's account of totemism remains a source of considerable misunderstanding and ignorance, it deserves to be quoted in its entirety:

One part of the religious superstition of the Savages, consists in each of them having his *totam*, or favorite spirit, which he believes watches over him. This *totam* they conceive assumes the shape of some beast or other, and therefore they never kill, hunt, or eat the animal whose form they think this *totam* bears.

The evening previous to the departure of the band, one of them, whose *totam* was a bear, dreamed that if he could go to a piece of swampy ground, at the foot of a high mountain, about five days march from my wigwam, he would see a large herd of elks, moose, and other animals; but that he must be accompanied by at least ten good hunters. When he awoke he acquainted the band with his dream, and desired them to go with him: they all refused, saying it was out of their way, and that their hunting grounds were nearer. The Indian having a superstitious reverence for his dream (which ignorance, and the prevalence of example among the Savages, carries to a great height), thinking himself obliged to do so, as his companions had refused to go with him, went alone, and coming near the spot, saw the animals he dreamed of; he instantly fired, and killed a bear. Shocked at the transaction, and dreading the displeasure of the Master of Life, whom he conceived he had highly offended, he fell down, and lay senseless for some time: recovering from his state of insensibility, he got up, and was making the best of his way to my house, when he was met in the road by another large bear, who pulled him down, and scratched his face. The Indian relating this event at his return added, in the simplicity of his nature, that the bear asked him what could induce him to kill his *totam*; to which he replied, that he did not know he was among the animals when he fired at the herd; that he was very sorry for the misfortune, and hoped he would have pity on him: that the bear suffered him to depart, told him to be more cautious in future, and acquaint all the Indians with the circumstance, that their *totams* might be safe, and the Master of Life not be angry with them. As he entered my house, he looked at [me and said] "Amik, hunjey ta Kitchee Anniscartissey nin, O Totam, cawwicka nee wee geossay sannegat debwoye:"—or, "Beaver, my faith is lost, my *totam* is angry, I shall never be able to hunt anymore." (Long, 1904:123–125)

Long (1904:125) then explained:

This idea of destiny, or, if I may be allowed the phrase, "*totamism*" however strange, is not confined to the Savages; many instances might be adduced from history, to prove how strong these impressions have been on minds above the vulgar and unlearned. To instance one, in the history of the private life of Louis XV, translated by Justamond, among some particulars of the life of the famous Samuel Bernard, the Jew banker, of the court of France, he says, that he was superstitious as the people of his nation are, and had a black hen, to which he thought his destiny was attached; he had the greatest care taken of her, and the loss of this fowl was, in fact, the period of his own existence, in January, 1739.

Long's account prompts several comments. First, as is generally agreed, Long's description seems to refer to the guardian-spirit complex rather than to sociological or clan totemism, a point emphasized by Schoolcraft (1855:196) and Tylor (1898), neither of whom seems to have read Long directly. They believed that sociological totemism and individual totemism were different phenomena, with different origins, that should not be conflated. Apparently, Long confused the two ideas by subsuming them under a single term.

It should be mentioned, however, that Long's presumed error in linking the term *totem* with "guardian spirit" was independently repeated by the famous Jesuit missionary, Pierre de Smet. In an article dating to 1838–1839 (and reprinted in his collected *Life, Letters and Trips* (1905)), de Smet mentioned a Potawatomi naming ceremony in which a 17-year-old male's name, obtained from previous dream experience, is publicly proclaimed. To quote de Smet (1905:1093), "The animal which presents itself to him will become his manitou or totem (dodeme), and all his life long he will carry about him a badge of it, whether a claw, a tail, a feather, it matters not." Further, the image of the animal, or dodeme, the guardian spirit, was

painted in red on the top of the grave post of the deceased person (de Smet, 1905:1091–1092).

Linguistic evidence is crucial in demonstrating that in native usage the root from which Long constructed his terms *totam* and *totamism* refers to kinship. In Algonquian languages, the nouns corresponding to English *totem* are dependent stems, which occur only in possessed nouns: thus "my totem" or "our totem" but not "[a] totem." According to Ives Goddard (pers. comm. cited in Callender, 1978:621), the Proto-Algonquian (PA) verb stem **o-te-* can be translated as 'to dwell together as a group, village'. Through regular derivation, **o-te-* would have formed a noun **o-te-wa* 'dweller', the possessed form of which would be (here with third-person possessor) **wet-o-te-m-ali*, with third-person prefix **wet-* (incorporating the intervocalic connective **t*), possessive ending **-m*, and animate obviative singular ending **-ali*. Ojibwa *-do-de-m* and its cognates in other Algonquian languages are reflexes of this form. PA **weto-te-mali* can then be glossed as 'his or her co- or fellow dweller'. It is also interesting to consider that the reflexes of **o-te-* together with the final abstract noun-forming suffix **-naw* formed such Algonquian words for "village, town, or settlement" as Ojibwa *o-de-na* and Woods Cree *o-ti-naw*. We observe, then, two lexical affinities of **o-te-*, one with coresidential kinship groups and the other with residential localities.

The semantics of the reflexes of PA **weto-te-mali* refer to diverse kinds of consanguineal, affinal, and coresidential relationships. In Ojibwa, *-do-de-m* clearly exhibits the dual meanings of 'patrilineal clan' (or 'member of patrilineal clan') and 'eponymous clan animal'. For Algonquin at Lake of the Two Mountains, Jean-Baptiste Thavenet (in Cuq, 1886:312–313) glossed the first-person possessed form *nindo-de-m* ("*nind otem*") as "ma tribu" (possibly the patrilineal clan), or an individual member of the same. Thavenet correctly identified the stem as *o-te-* and glossed it as 'family', but he erred in seeking to derive it from *-te-?* 'heart'. He also remarked on the use of such constructions as *makwa nindo-de-m* 'the bear is my clan', which clearly exemplify the association of kin groups with eponymous animals. For the same dialect (historically the Nipissing variety of Eastern Ojibwa), Cuq (1886:312) gave *-o-te-* both as the stem of *nindo-de-m* 'my totem' and as having the meaning 'village'; he also gave *o-de-naw* 'village' but did not connect the two forms. J.N.B. Hewitt (1910:787–788), who relied heavily on Cuq, rendered *o-de-* in Ojibwa "and other cognate Algonquian dialects" as referring to kinship between siblings and, by extension, to all members of the exogamic kinship group; he thus glossed *odo-de-man* (with third person affixes) as "his brother-sister kin." Hewitt also glossed the Ojibwa abstract noun-forming final *-na(w)* (which he gave with incorrect vowel length) as "dwelling place" and thus glossed Ojibwa *o-de-na* or *o-de-naw* 'village' as "dwelling place of the clan." Note that not all Algonquian societies possessed patrilineal descent groups—the probable referent of

Hewitt's "clan"—and that this institution, therefore, cannot be assumed to have been a feature of Proto-Algonquian social structure or to have been a meaning of PA **weto-te-mali*. We can infer that Ojibwa *-do-de-m* (singular) meant 'clan relative' (and, by abstraction, the membership of the clan as a collectivity) and that *-do-de-mag* (plural) meant 'clan relatives'. In a unilinear system, it would make little sense to gloss the plural of the dependent noun *-do-de-m* as 'clans', although rhetorical contexts for such a meaning are imaginable. The usage of two nineteenth-century Ojibwa historians of their own people, William Warren and Peter Jones, makes clear that *-do-de-m* referred both to the patrilineal clan and to its eponymous animal emblem (Warren, 1957:42; Jones, 1970:138; see also Nichols and Nyholm, 1995:66, *indo-de-m* 'my totem, my clan').

Fox exhibits a similar range of meanings. Goddard (1973, pers. comm., 1997) gave *o-te-* as the stem of *o-te-weni* 'town'. For *neto-te-ma*, the meanings 'my sibling' and 'my same-sex sibling' are attested and can possibly be extended to 'my fellow clansperson'. The Fox noun for patrilineal clan, however, is not *-to-te-m* but *mi-so-ni* 'name'. For Fox *oto-te-mani*, Jones (1911:810) gave the meanings "his eldest brother" (specificity dubious), "his master" (incorrect), "his clan tutelary," and "his giver of supernatural power." The last gloss shows affinities with Long's Ojibwa usage, although no textual exemplifications exist; however, *neto-te-ma* does occur in texts as a term of reciprocal address between clan members and their totemic animals.

For Menominee, Bloomfield (1962:260–261, 397, 1975:6) gave the medial *-o-te-* as 'household, family', and glossed *-(t)o-te-m* as 'my totem animal, my totemic ancestor'. In Woods Cree, singular *nito-ti-m* is 'my relative or friend', and plural *nito-ti-mak* is 'all my relatives and friends'; Crees lack patrilineal descent groups, and *-to-ti-m* has no reference to eponymous animals.

The form is also present in the eastern branch of the Algonquian language family, where, however, the social denotata are commonly friends or affines. Western Abenaki *-dodam* is 'animal ancestor, totem' (Day, 1994), i.e., the eponymous animals of patrilineal clans. Penobscot *-totem* is 'friend' and, more specifically, 'affine' (Frank Siebert, pers. comm., Aug 1997). Micmac *-tuttem* is glossed as "totem" (somewhat anomalously given the absence of Micmac descent groups) and also possesses the meanings "friend of another nation," "gentleman friend," and (as *-tuttemisqw*) "lady friend" (Rand, 1888; DeBlois and Metallic, 1984).

Individual totems, guardian spirits, or personal tutelaries are often regarded as an extended type of kin. The vision quest experience can be likened to an initiation adoption ritual in which a new kinship relationship is established. Many Algonquian-speaking peoples refer to the individual totem as a specific manitou, a term that has historically defied precise translation but can be loosely associated with power, medicine, or a divine being. The Ojibwa *bawa-gan*—an other-than-human

person encountered in dreams or visions (Hallowell, 1976:369)—could function as a guardian spirit, as could *a-dizo-ka-nag*, ancestral spirits or primordial myth beings called grandfathers (Hallowell, 1976:365). Adrian Tanner (1979:95, 103) described Mistassini Cree beliefs about winds associated with the cardinal directions and with seasonality. These winds are personified as spiritual beings who control weather and can be propitiated or magically manipulated to insure successful hunting. In particular, the spirit connected with the north and winter, *Ciiwetinsuu* (and secondarily the spirit of the west wind), can affect the hunting of nonhibernating, non-migratory game upon which humans depend for their winter survival. This being is honorifically referred to as "your grandfather," suggesting not only esteem and kinship but also descent. These directional and seasonal spiritual beings thus function as masters of generalized classes of game animals. In effect, they mediate between the souls of humans and those of animal spirits. As such, these beings represent a more general and inclusive power than that obtained through the species-specific guardian spirit or individual totem. "Grandfather" is also a common term of address or reference for spirit entities among the Saulteaux Ojibwa and Woods Cree of Manitoba (Hallowell, 1976:369; Brightman, 1993:109) and perhaps among other Algonquian groups.

Seldom mentioned in connection with Long's celebrated discovery of totemism is the fact that he clearly recognized the existence of descent-group or sociological totemism. In the beginning of his book, Long (1904:10) cited Cadwallader Colden in reference to the Five Nations Iroquois: "each of these nations is again divided into three tribes or families, who are distinguished by the names of the Tortoise, Bear, and Wolf." These three family totems are recognized among the Mohawk-Oneida and some Eastern Algonquian groups. Visual evidence of totemic affiliation is afforded in the famous full-length portraits painted by John Verelst during the visit to London of one Mahican and three Mohawk chiefs (or "kings") in 1710. At the foot of the Mahican is a turtle, two of the Mohawks are accompanied by wolves, and the third Mohawk, the grandfather of the late eighteenth-century political leader Joseph Brant, sports a small bear (Editors of Time-Life Books, 1992:74–75). Clearly, Long knew of descent group or sociological totemism as well as individual totemism, and he presumably recognized the presence of both among the Ojibwa.

Returning to Long's narrative, in the recorded Ojibwa speech of the accidental bear slayer is the form "nin, O Totam" for 'my totem'. The expected form would be *nindo-de-m*. Recent research indicates that Long possessed, as he claimed, a certain Ojibwa fluency, but it was in the grammatically simplified trade language known locally as broken Ojibwa rather than in *Ojibwe-mowin* or "Ojibwa proper" (Nichols, 1992; Bakker, 1994). The lexical forms (and presumably the semantics) of broken Ojibwa, however, were clearly Algonquian, and Long's "nin, O Totam" is recognizable as a pidgin rendering of Ojibwa

nindo·de·m. The supposition that Long's interlocutor used the word *-do·de·m* as a pidgin improvisation for *manido·* or *bawa·gan* 'individual spirit guardian', although possible, is not especially probable. Long's reputed error may be less the result of faulty diction (Hewitt, 1910:789) and more the lack of a native appreciation of the subtle analytic and conceptual distinctions posited by late-nineteenth- and twentieth-century arm-chair anthropologists and social theorists.

We find it especially difficult to believe that Lévi-Strauss, who, along with most everyone else, dutifully cited Long as the first to enter the term totemism into our discourse (thus becoming *our* totemic ancestor), would fail to mention the French Jewish banker's black hen as an anticipation of his thesis about the universal manifestations of totemic thought. Samuel Bernard was, indeed, an important personage, and his portrait hangs in the French chateau Chenonceau. Without trying to advance a Jewish theory of totemism, we do think that had Lévi-Strauss directly consulted Long's account we might have been spared the tortuous genealogy of metaphoric totemism from Rousseau out of Bergson through Linton's A.E.F. Rainbow Division and Radcliffe-Brown's second theory of totemism (Levi-Strauss, 1963:7–8, 83–104)!

The question of the relationship between so-called individual totems and hereditary clan totems is interestingly posed by the classicist and religious anthropologist James Frazer (1934) in the third volume of his massive, if not magnum, opus *Totemism and Exogamy*, originally published in 1910. Although he accepted Tylor's objection that individual totemism and clan totemism are different phenomena that should not be classed together under the same rubric, Frazer sensed a similarity in attitudes of respect and affection toward the totemic being. He suggested that American ethnologists with direct field experience—most notably the Canadian Charles Hill-Tout, who worked primarily with the Coast Salish—tended to derive clan totems from guardian spirits. The essential argument is the belief that the guardian spirit of the ancestor of the clan is transmitted by social inheritance to his or her descendants, either in the male or the female line. While Frazer granted a certain plausibility to this position, he noted that other scholars, with equal plausibility, inverted the argument by maintaining the priority of clan totemism. For these theorists, guardian-spirit beliefs emerged "at a time when the totemism of clans was falling into decay, and when consequently individuals, deprived of the protection of the clan totem, looked about for a personal guardian of their own to supply its place" (Frazer, 1934(3):371). Frazer attributed the latter position to E.S. Hartland, A.C. Haddon, Henri Hubert, Marcel Mauss, and to personal communication with a young Cambridge colleague named A.R. Brown. It is tempting to see in Frazer's counter-positions American individualism matched against European social apriorists, but we won't pursue this here.¹

Frazer was skilled at posing dilemmas in studying problems of origins, but he was less successful in resolving them. By tracing the distributions of guardian spirit complexes and clan totemism over Native North America, he found an expectable overlap; yet some areas clearly possessed totemic clans without guardian spirits, and in other areas the situation was reversed. Temporal priority cannot be established on the basis of distribution. Although the question of origins is probably moot, if not mute, in modern anthropology, it is possible to examine the relationships of clan totems to guardian spirits from other angles besides the kind of distributional analyses favored by Frazer and others, or, at another extreme, the elegant types of analogical exercises championed by structuralists. The diverse semantics of Algonquian words for totem allowed us to reexamine the late ethnohistorian Harold Hickerson's (1970:42–50) argument that the basic social structure of the Chippewas at first contact comprised localized clan groups possessing animal eponyms. He employed an etymology identical to that followed above to interpret the term *o·de·na* as signifying "clan village" (he cited Landes, 1937:33–35, who in turn credited Truman Michelson). Hickerson amassed impressive data to demonstrate the equivalence between Chippewa clan names and their names for local groups. One key quotation from Nicolas Perrot appears to clinch Hickerson's argument for original clan settlements: "You will hear [the Indians] say that their villages bear the name of the animal which has given its people their being—as that of the crane, or the bear, or of other animals" (quoted in Hickerson, 1970:47).

We find difficulties with the notion of exclusive clan villages. It was desirable, if not necessary, to have lineal relatives residing in other settlements.² As Tylor (1888:266) speculated long ago, "Again and again in the world's history, savage tribes must have had plainly before their minds the simple practical alternative between marrying out and being killed-out." Such localized kin groups would seem to be extremely fragile, volatile, and vulnerable to processes of segmentation or amalgamation, or both. Yet there is less difficulty in recognizing clan villages as a kind of cultural fiction, a positive ideology stressing the dominance or ascendancy of a particular clan or lineage in a particular locale. Here the Stewardian patrilineal bands imagined by Hickerson converged with the Ojibwas' own representations. Such a group might provide political or religious leadership in the local community, serve as the bearers of collective sacred bundles or traditions, or possess special rights or relations to a particular territory and its resources.

We agree with Hickerson that, going back in time, distinctions between clan and local group become blurred. We part company in interpreting the nature of the relationship between the totemic group and the local territory. Here, spatial coordinates defining locality and generational time inherent in descent intersect. Ideological factors, especially notions of guardian spirits or masters or mistresses of the game, or both, become relevant.

Hickerson's neglect of ideological factors is understandable given his strong ecological and cultural-materialist orientation. Moreover, he seemed to accept unquestionably Eleanor Leacock's (1954) seemingly persuasive evidence, generalized from the Montagnais, that family hunting territories came into existence only after the establishment of the fur trade (1954). The advent of a fur-trading economy, along with devastating epidemic disease, certainly brought profound changes to traditional systems of territorialism, but many recent fieldworkers and ethnohistorians, such as Robert Brightman (1993), Harvey Feit (1973, 1991), Charles Bishop and Toby Morantz (1986), Adrian Tanner (1979), and David Turner (1978), view this transformation as less abrupt and absolute. They have also detected certain continuities in ethnoecology and in deeply ingrained cultural belief structures about relations between animals and humans and between each of these and the land—a kind of symbolic ecology, as it were.

Leacock (1954) and her followers tend to emphasize the establishment of sharp boundaries, rules against trespass, and strict notions of ownership in the sense of private property as defining features characterizing the appearance of family hunting territories with commercial commodification of animals. Boundaries, although marked and ethnocartographically mapped, probably were always somewhat fluid, and few legal sanctions other than threats to mutual understanding and fears of divine punishment protected them against trespass.

Ownership and private property seem to be the sources of contention. The overriding principle is not so much that land is owned by individuals or by a local group, but that these persons have rights to the resources, particularly fur-bearing animals, present in that area. In a more than casual remark, the German émigré ethnologist Julius Lips once stated that among the Mistassini Cree, animals, not humans, were the true owners of the territory (cited in Tanner, 1979:107).

Adrian Tanner's (1979) study of modern Mistassini modes of production and religious ideology, *Bringing Home Animals*, took the comment of Lips seriously and described in detail the cultural constitution of Cree spatial orientations, the complexities of divinatory communication with animal spirits and forces of nature, the rituals regulating relations between hunters and their quarry, and the ritualized respect owed to slain animals. Successful hunting, from the native perspective, depends as much upon these ritual relations as upon technical proficiency in hunting. It is thought that certain individuals, usually elder males and nominal heads of the local group, have established special enduring relations of friendship with local spirits who control the numbers of animals and access to them. It is further believed that these spiritual connections can be inherited by a successor, normally a son or an adoptive member of the group who has resided in the territory for an extended period of time.

We suggest that the beliefs and practices that Tanner has so cogently described for the Mistassini Cree may formerly have

been much more widespread and that clan totemism, locality, and individual totemism were more integrally connected than has generally been supposed. Two independent attestations exist—Long's and de Smet's—of Ojibwa-Potawatomi *-do-de-m* with the meaning 'individual totem or spirit guardian' (see above). Thus, for these groups at least, the noun possessed multiple meanings. In Long's time, Ojibwa *nindo-de-m* 'my totem' could mean 'my clan relative' and 'my individual spirit guardian' and 'my clan animal'; pluralized, it meant 'my clan relatives' or 'my clan'. Thus, eponymous group totems and individual guardian spirits need not succeed one another in lock-step evolutionary succession but could coexist in the same societies (Goldenweiser, 1910). An Ojibwa might have Wolf as clan totem and Beaver as an individual spirit guardian, or the reverse, or might imaginably possess as guardian spirit a being of the same type as the clan totem.

Consider, for example, Speck's (1917) reflections on "totemism" among the Penobscots of Maine. Lacking the unilineal-descent groups of the Ojibwas and other Great Lakes Algonquians (and of other Atlantic Coast Algonquians to the south), the Penobscots nonetheless were "totemically" organized: 13 out of 22 residential bands present in the late nineteenth century were named for an individual animal species (beaver, otter, moose, eel, etc.) deemed to be distinctively and exceptionally populous on their traditional winter foraging territories. The individual guardian spirit, which Speck gave as "baohigan" (*pāwāhikan* 'token, talisman, fetish object used for magic purposes by shamans' (Frank Siebert, pers. comm., Aug 1997; Ives Goddard, pers. comm., 27 Aug 1997)) was not commonly an animal of the type of the group totem. In contrast to Ojibwa, the Penobscot word *-totem* lacks known reference to spirit or animal beings, and neither does it refer to a primary kin group; the sociological reference excludes "consanguines" and seems rather to refer to such "solidary nonkin" as friends, acquaintances, and relatives by marriage (Frank Siebert, pers. comm., Aug 1997). Speck compared the group totemism of the Penobscot with the individual spirit-guardian practices of the Québec Mistassini Cree to the north.³ Mistassini bands lacked zoonymic band totems, and Penobscot group totems were distinct from individual spirit guardians. Speck, nevertheless, identified a significant parallel between the Penobscot totems and the individual dream spirits of the Mistassini: both involved animal species that were dietary staples and were commonly hunted on the winter foraging territory. In hindsight, we can say that it is a question of Penobscot attentiveness to *la différence*. Although the same project of dream-inspired foraging confronted both groups, the Penobscot improvised upon regional disparities in particular game species to develop a genuinely totemic system in which otherwise homogeneous winter bands were differentiated on the basis of statistical heterogeneity in the densities of particular spe-

cies on otherwise homogeneous foraging tracts. Thus, with analogies to Lévi-Strauss:

species ₁	≠	species ₂	≠	species ₃
tract ₁	≠	tract ₂	≠	tract ₃
band ₁	≠	band ₂	≠	band ₃

Here also are parallels between the dietary regulation of human-animal relationships in the clan totemic and guardian-spirit relationships. Nowhere in the literature on the Ojibwa or other Great Lakes Algonquians is there evidence that individuals practiced a classically Australian Aboriginal dietary renunciation of their clan totem animal. Indeed, such renunciation would entail impracticable hardship for groups whose eponymous clan totems were dietary staples; consider, for example, the winter prospects of bands exclusively composed of male hunters of the *Mo-z* 'moose' or *Amik* 'beaver' clans. Thus, for example, the testimony of an Ojibwa member of the *Awa-zisi* 'Bullhead' (*Aspidophorus cataphractes*) clan: "We have great respect of our *dō'dām*, but we eat them; I have often eaten bullhead" (Hilger, 1992:155; see also Landes, 1937:33). Speck (1917:10) long ago observed that Subarctic Algonquian "band totemism" of the Penobscot type formed a dietary inversion of Australian totemism: the Australians "re-produced" the eponymous species by renouncing it as food, whereas the Algonquians accomplished the same end by ritual consumption (see Brightman, 1993:213–243). The same could perhaps be said of the dietary relation between the patrilineal clans of the Great Lakes Algonquians and their eponymous animals.

The dietary relation to the individual spirit guardian is more variable. Speck (1917) emphasized the dietary parallels between Penobscot bands and their animal totems and between Mistassini hunters and individual guardian-spirit animals (both were eaten), but there is evidence that guardian-spirit animals sometimes imposed dietary taboos on animals of the same species. Thus the individual renunciation of the animal-spirit guardian attested in Long's eighteenth-century text is paralleled in the contemporary practices of some Woods Cree hunters of Manitoba. Among the latter, the logistical difficulties are sometimes adjusted by exempting from the taboo the meat of animals of one or the other sex or of those killed by others. In cases known to us, for example, a man whose *pawa-kan*, or guardian spirit, is a female beaver abstains from eating them, and a man who survived drowning by dreaming of a sturgeon thereafter renounced sturgeon meat.

The semantics of totemism in Algonquian languages and cultures thus exhibits a rich and geographically variable network of associations that reaches from the Atlantic Coast to the Great Lakes and north into the boreal forest: coresidential kin groups (patrilineal or bilateral), winter foraging tracts, eponymous animals, and individual animal spirit guardians. Even though the distribution of the cultural forms in question does not permit us to reconstruct a Proto-Algonquian totemic system, if such ever existed, we observe in them associations

counter to the rigid typological separation of group from individual totemism and counter to the assumption that Long got it all wrong at the beginning. In Penobscot totemism we observe clear parallels to Tanner's (1979) Mistassini themes of animal ownership of the foraging tracts and of enduring transgenerational relations between the humans and animals habitually subsisting in the same landscapes. Groups, even nomadic ones, dwelt in places, and—Leacock notwithstanding—there was intergenerational continuity between groups and places before the fur trade and thus possible relationships with theriomorphic spirits-of-place that could be both individual guardians and group totems. Curiously, but perhaps not coincidentally, it is with the meaning of individual spirit guardian rather than group eponym that "totem" has been assimilated to the pop Indian spirituality of New Age devotees (Steiger, 1997).

Conclusion

This exploration of totemism in its native homeland may not have resolved the "problem" of the origin of totemism (Jewish or otherwise) that has perplexed the minds of so many social scientific theorists. We can, however, perhaps discern in the situation of parallel existence and reciprocity obtaining between humans and animals, and their mediators, a possible solution to the classic classificatory problem of reconciling so-called individual totemism with the hereditary totemism of descent groups. The radical dissociation between individual and group totemism may be more an analytic dream, and anthropological nightmare, than a native reality.

Finally, if nothing else, we hope to have indicated the relevance of Native American data to past and continuing debates in anthropology, something Claude Lévi-Strauss always appreciated and Bill Sturtevant always knew.

Notes

This paper has been in process a long time. Those we can recall who made useful comments on it include Anne Chien, Raymond DeMallie, Ives Goddard, Robert McKinley, Toby Morantz, Jay Miller, Frank Siebert, and George Stocking.

1. The American, or better, Canadian, evidence for deriving sociological totemism from group totemism was partly dismissed by Frazer in noting that Northwest Coast cultures were already highly developed and could not be placed on a comparable evolutionary plane as the Australian Aborigines, who were regarded by most contemporary scholars as embodying a pristine form of totems. Paradoxically, totemism, a Native American phenomenon, came to be measured and evaluated against Australian Aboriginal standards and was found to be anomalous.

2. Indeed, Landes (1937:33) pointed out that the *dodems* at Eno and Manitou Reserve "are not localized, whether as villages or within the village," although she noted that Radin reported clan localization for the Winnebago, and Michelson believed in its former existence among the Fox.

3. The spirit-guardian experiences of the Québec Algonquians differ substantially from those of the Crees and Ojibwas to the west. The being was not acquired at puberty during a formal protracted fast but through involuntary dreams during early adulthood.

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Coyote, Acorns, Salmon, and Quartz: Verse Analysis of a Karok Myth

Dell Hymes

The Karok myth analyzed in what follows was transcribed from dictation in 1951 by the American linguist William Bright and later published in his invaluable account of the Karok language (Bright, 1957:204–207). Bright joined in the early work in ethnopoetics, and this myth was one of those he analyzed (Bright, 1982, 1984). Herein I hope to carry further recognition of the form of the text and therefore its meaning. Because I discussed this myth at a seminar at the Smithsonian Institution,¹ it seems especially appropriate as part of this tribute to William Sturtevant.

The text of this myth is one of the few for which alternate analyses of form have been offered. Two other analyses of texts—one Zuni (Hymes, 1982), the other Hopi (Hymes, 1994)—compare a focus on spoken contours to a focus on lexical and syntactic relationships. In this paper the comparison is within a shared framework. Bright and I share the assumption that such a narrative is “measured verse”; his analysis differs from mine in how units beyond line and verse are distinguished.

Bright distinguished four levels of units: lines, verses, scenes, and acts. I follow his identification of lines; the line numbers in our accounts are the same. I also follow his identification of verses, with minor modifications (explained in “Analytic Notes,” below). In particular, I accept his treatment of forms with the prefix *pa-* as distinct lines (supported by a personal communication from Victor Golla (20 Mar 1985) about neighboring Hupa); however, the manner of our presentations differs. Bright showed lines by spatial relationship within verses, and he identified verses by preceding parenthetical numbers that are continuous throughout the text. I identify lines throughout the text by consecutive numbering of every fifth line at the right, but I do not number verses. Instead, verses begin flush left within stanzas, and stanzas are separated by space within scenes. In the profile, I identify verses by lower-case letters and stanzas by upper-case letters, with the sequence of letters beginning anew within each part. Scenes [ii] and [iii] in Act I each have three stanzas, and these are labeled (ABC). The other scenes in this narrative have only a single stanza each, which could be labeled (A) in each case, but that seems redundant.

Bright found acts to correspond to major changes in the locale of action, but he found scenes to be definable most consistently in terms of the participants involved (Bright, 1984:138). Bright did not constrain or have an expectation of the number of units a larger unit might contain. His three acts contain five, three, and two scenes, respectively, and they vary in the number of verses the scenes contain. I distinguish stanzas within scenes and look for consistent patterns in the grouping of verses within stanzas and stanzas within scenes. The differences can be summarized as follows:

	Bright	Hymes
Acts	3	3
Scenes	5, 3, 2	3, 3, 5
Stanzas	–	(1, 3, 3) (1, 1, 1) (1, 1, 1, 1, 1)
Verses	I (3, 6, 4, 6, 7)	(2) (5, 3, 3) (3, 3, 3)
	II (5, 4, 4)	(3×2) (3×2) (3×2)
	III (7, 2)	(2, 3, 3, 3, 4 (chiasmus))

In effect, Bright’s definitions bifurcate the text. Verses are identified by linguistic features, notably initial particles. Scenes and acts are identified by content, by a consistent set of actors for scenes, and by a major change of location for acts. Such features matter, but they seem to take into account only the nouns of the plot, as it were, and to omit the verbs. As the narrative continues, constellations of actors may recur, but actions change.

It does not seem sufficient to identify several segments of the text in terms of the presence of the sisters (see profile, below, Bright IA, ID, IIC, IIIA), or the sisters and Coyote (Bright IC, IE, IIA), apart from the action, or more important, apart from the goal of the action involved. When the goal of the action is taken into account, the appearance of unity of the segments changes. Covariation of form and meaning becomes more evident in recurrent relations, here threes and fives. Changes among them have dramatic meaning (as with Act II). One begins to recognize a repertoire of rhetorical patterns (two such are a situation, an action, something said, or a sequence of onset, ongoing, outcome). One can grasp the story as ongoing arousals and satisfactions of formal expectation.

These differences can be seen in the side-by-side presentation of profiles of the two analyses that follows the presentation of the text and its translation. Notice that Bright’s headings

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show locations for acts and participants for scenes, whereas my headings show actions as well.

By now such relations and patterns have been found in narratives in about 60 Native American languages, in American English, and in several Old World languages, including Bulgarian, Finnish, Japanese, and Koryak. There appear to be two basic alternatives, relations of two and four or relations of three and five. The hypothesis of such relations seems reasonable for any oral narrative.

Karok is an interesting case, in that its narrators may make use of both basic patterns. Two of Bright's sources, Nettie Reuben and Julia Starritt, told him the story of "Coyote's Journey." Reuben grouped verses, and stanzas, in pairs, whereas Starritt grouped them in threes and fives. On the other hand, Reuben grouped scenes in threes and fives, and in "Coyote's Homecoming," she used three and five grouping throughout. In sum, two narrators told the same story with different principles of patterning. Different stories with different principles of patterning were told by the same narrator (cf. Hymes, 1985).

A Karok narrative told by Phoebe Maddux to J.P. Harrington (of the Bureau of American Ethnology), "Two girls apply for marriage with Spring Salmon" (Harrington, 1932:22-24), also shows both principles of patterning (Hymes, 1997). The first scene has three stanzas, each with three verses. The second and third scenes each have two stanzas but have sets of verses in threes and fives. The fourth scene reverses that relationship, having five stanzas, each with a pair of verses. The fifth scene has one stanza with five verses. The second and third scenes stand out, probably because the two girls initiate the action. In the fourth and fifth scenes that relation is reversed. Others in the village are heard speaking, and the girls respond.

This information makes a theoretical point, but it barely scratches the surface as to Karok. Analysis of all the texts and narrators (including those in Harrington, 1930, 1932) might disclose motivations for choices and connections among them. Such work is possible because of Bright's careful, comprehensive foundation. I am glad to have been able to discuss some aspects of Karok with Bright in correspondence when beginning work with Karok texts.

Coyote Gives Salmon and Acorns to Humans

The text was dictated to Bright by Mamie Offield, a Karok woman, at her isolated home on the slopes of sacred Offield Mountain, in northern California (Bright, 1984:136). (The title was supplied by Bright.) She also told it, in Karok, to the anthropologist E.W. Gifford in 1939. All Gifford's texts were told in Karok and then were translated into English, either by the narrator or "in most cases [by] a younger bilingual Indian" (Kroeber, 1980:xv). The translator of this story probably was Offield herself, because with Gifford she translated for older Karok and narrated as well (Bright, 1984:136). The result is little more than a summary, perhaps 18 lines, with only one instance of quoted speech.

Offield's dictation to Bright is far superior. The text has some 111 lines with 15 instances of quoted speech and thought. There is a neatly shaped central dialogue. The story leads to a transformation and the origin of a basic rite.

These two versions from Offield are the only ones we have. Kroeber, Gifford, Harrington, and Bright have accounts of the creation of salmon (Kroeber and Gifford, 1980:360), but only Gifford and Bright have accounts of their release. In 1940 another Karok woman, Georgia Orcutt, told Gifford separate accounts of the provision of acorns and of salmon. The first, on how acorns came to be available only in the fall, against Coyote's wishes, evidently was popular, because she heard it "from Red Cap Tom and others." But her telling of it, or the translation, was summary (Kroeber and Gifford, 1980:156). Her account of Coyote making salmon available, however, was performed with distinct details (Kroeber and Gifford, 1980:156-157).

Gifford reported that Offield learned from her mother the story of Coyote releasing both acorns and salmon. Perhaps this integrated version, making Coyote central to the availability of both major foods, was specific to her family. In any case, the difference as to acorns shows that, as elsewhere, Coyote was a contested figure among the Karok (cf. Hymes, 1996).

In the text that follows, each of the three stanzas of Act II has three pairs of verses. A brace () at the end of a line calls attention to the end of such a pair, which is enclosed in paired braces in the profile. An exclamation point (!) line-internally in English corresponds to the Karok word *vúr(a)*, which lends emphasis to a line without having a translatable meaning.

[Coyote gives Salmon and Acorns to Humans]

Mamie Offield

I. [Coyote discovers the hidden salmon]

[i] [Two sisters hide the salmon]

ʔáxxak ʔasiktá·n kunʔí·nanik,
kustá·ras,
ʔAme·kyá·ra·m.

Kári xás kunpi·p,
"Púra kára vúra ʔá·ma ʔa·mtíhe·šara.
"Yukún, tánupíššunva,
paʔá·ma."

Two women used to live,
sisters,
at Amekyaram.

Now then they said,
"Nobody ! will eat salmon.
"See, we've hidden it,
that salmon."

5

5

[ii] [Coyote pretends to have salmon]

Kári xás Pihñê-fič ʔuxxus, “Púxay vúra va· kupítíhe-šara.”		Now then Coyote thought, “They’re not going to get away with that!”	
Kári xás ʔuxxus, “Čími kanimússan.”	10	Now then he thought, “Let me go look.”	10
Kári xás muvíkkapu ʔuppê-čip. Kári xás ʔé·pa·x ʔúkruh. Kári xás víkkapuhak ʔuθθa·námnih.		Now then he picked up his quiver. Now then he peeled alder bark. Now then he put it in the quiver.	
Kári xás va· ká·n ʔuʔu·m, yô·ram ʔukrí·š.	15	Now then he arrived at that place, he just sat in the corner.	15
Kári xás kunpatánvaniš, “Fâ·t kumáʔi· ʔivúrayvutih?”		Now then they asked him, “Why are you roaming around?”	
Kári xás ʔuppi·p, “Káruk ʔiθivθane·nʔíppan nivâ·ramutih.”	20	Now then he said, “I’m going to the upriver end of the world.”	20
Xás vúra ká·n ʔúkri·.		Then ! there he sat.	
Kári xás ʔuppi·p, “Tánaxxúriha. “Tî· matê· ʔá·ma kanʔam.”		Now then he said, “I’m hungry. “I shall eat a bit of salmon.”	
Kári xás ʔuʔê·θrišuk, paʔé·pa·x.	25	Now then he took it out, the alder bark.	25

[iii] [The sisters reveal the real salmon]

Kári xás taʔittam ʔuʔávahe·n. Kári xás kunxus, paʔasiktáva·nsas, “Hô·y ʔu·m po·ʔaramsî·privtihirak? “Ká·n hínupa ʔá·ma kunʔá·mtih.”	30	Now then already indeed he’d eaten it. Now then they thought, those women, “Where is it he comes from? “It looks like they’re eating salmon there.”	30
Kári xás ʔuppê·r, pamukústa·n yíθθa, “Čími numni·ši.”		Now then she told her, that one sister, “Let’s cook.”	
Taʔittam yíθθa pamusvírik mû·k mâ·ka ʔuʔi·k, θivrihvassúruk. Kári xás ʔiššaha ʔuvuniššuk. Kári xás ʔá·ma ʔúkyi·mnišuk.	35	Already indeed one struck uphillward with her elbow, under a wall-plank. Now then water flooded out. Now then salmon fell out.	35
Kári xás taʔittam kunimníššahe·n. Kári xás tákunʔav. Pihñê-fič vúra va· ʔúmmu·stih.	40	Now then already indeed they had cooked it. Now then they ate it. Coyote ! is watching that.	40

II. [Acorns and salmon for all]

[i] [Coyote gets them to take him for acorns]

Kári xás kunpi·p, “Čími ʔíppaho·. “Yakún nu· tánuʔiffíkar, xuntáppan.”	}	45	Now then they said, “Go on your way. “See, we go to pick them up, acorns.	}	45
Kári xás ʔuppi·p, Pihñê-fič, “Xâ·tik niθívke·.”			Now then he said, Coyote, “Let me go along.”		
Kári xás kunpi·p, “Pû·hara.”	}	50	Now then they said, “No.”	}	50
“Vúra xá·tik niθívke·. “Miník niθθâ·viš.”			“Let me go along! “I’ll knock down the acorns for you.”		
Xás kunpi·p, “Čímmi man.”	}		Then they said, “Come on then!”	}	

[ii] [He scatters the acorns]

Kári xás ká-n kunívyi-hma.	55	Now then there they went.	55
Taʔíttam kunʔíffikahe-n, paxuntáppan, paʔasiktáva·nsa. }		Already indeed they had gathered them, the acorns, the women did. }	
Kári xás Pihnê-fič ʔuʔíffik.		Now then Coyote picked it [a stick] up.	
Kári xás kô-kaninay vúra ʔúktir, paxunyê-p, máruk, sáruk, yúruk, káruk. }	60	Now then he beat them everywhere!, those tan-oaks, uphill, downhill, downriver, upriver. }	60
Kári xás ʔuθáha-sha, paxuntáppan.	65	Now then he scattered them, those acorns.	65
Víri va· kúθ payê-m paxuntáppan kô-kaninay vúra ʔuʔi-ftih. }		So that's why it is now, acorns are growing everywhere! }	

[iii] [He frees the salmon]

Kári xás ʔupíkvip, sáruk, ká-n paʔasiktáva·nsa kunʔi-nirak,	70	Now then he ran back, downhill, there where the women lived.	70
Kári xás ʔusxáxxaripa·, paθi-vri-hvar. }		Now then he ripped them out, those wall-planks. }	
Kári xás paʔíššaha ʔuvuniššuk.		Now then water flooded out.	
Xás ko-vúra paʔá·ma kunívyi-hrišuk. }		Then all those salmon came out. }	
Víri va· kúθ sâ-m ʔussa-mnúputih.	75	So that's why it flows downriver.	75
Káru va· kúθ ʔá·ma ʔukvíripa-tih. }		And that's why salmon run upriver. }	

III. [Transformation and renewal]

[i] [The sisters accept the world to come]

Kári xás kunpirúvi-š, paʔasiktáva·nsa.		Now then they came back down, the women.	
Kári xás kunpi-p, “Tá hínupa ʔutá-yva-r, paPihnê-fič. “Víri čô-ra, čémmi, xâ-tik nupkê-viš. “Yakún, yíθ ʔára-r ʔuʔi-níšrihe-š.”	80	Now then they said, “Look, he's spoiled it, that Coyote. “So let's go, all right, let us be transformed. “See, another (kind of) person will come to exist.”	80

[ii] [One returns for her knife]

Taʔíttam kunpiyâ-ramahe-n.	85	Already indeed they had gone off again.	85
Kári xás kunʔíffukra·, ʔasanamkaráyurukam.		Now then they climbed uphill, downriver from Stony Flat.	
Kári xás yíθθa ʔuppi-p, “Tánipipšítta-ni, nanisímsi·m.	90	Now then one said, “I forgot it, my knife.	90
“Čími kanpávan. “Čími ʔi·m ʔô-k ne-krû-ntih.”		“I'd better go back for it. “You'd better wait here for me.”	

[iii] [Her sister and their dog become quartz]

Víri po·pkíya·vrin. Sâ-m to·ppárihfak.		So she turned back. She started downhill.	
Víri po·píttiθun, yánava pamukústa·n, ʔasaxyíppit tó·ppárihiš.	95	So when she looked around, behold her sister, she'd turned into quartz.	95
Xás sâ·mvanihič, pamukunčíšši— va· káru ʔasaxyíppit tó·ppárihiš.		Then a little downhill, their dog— the same way too it had already turned into quartz.	

[iv] [Witnessing the Jump Dance, she becomes quartz]

Yakún, yíθ ʔára tuʔi-niš— ʔIθyáruk po·tkáratih, yánava pavuhvúha tuʔíššipva, ʔuθítti·mti pakuníhyi·vtih. Kári xás va· ká·n ʔasaxyíppit ʔuppárihiš.	100	See, a different (kind of) person had come to exist— As she looks across river, behold the Jump Dance lined up, she hears that they're shouting. Now then the same way there she turned into quartz.	100
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[v] [The salmon knife]

Vírí hū·tva kó· ʔiθívθa·ne·n ʔuʔi·náha·k, va· vúra kó· kuníhru·vtihe·š, pasímsi·m, va· kummû·k kuníhvi·θtihe·š, pe·šyá·t, pe·θívθa·ne·n tákunpikyâ·ha·k.	105 110	So just as long as the world exists, just so long they will use it, that knife, that's what they'll clean it with, that spring salmon, as they make that world again.	105 110
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Profiles Side By Side

The profile on the left, for Bright's analysis and presentation, enables one to infer the ways in which the text is given shape by his analysis. The parallel profile is my own analysis, showing the points of difference. For example, Bright took lines 4–7 as two verses, but I take them as one verse. Bright took the next sequence of verses to have six parts, ending with lines 15–16, but I

take it to have five parts, ending with line 14. One can test one's own sense of the rhythms and relations of the story by reading the two one after the other. The claim made by the right-hand analysis is that the story is informed by relations, hence rhythms of three and five, that the sequence makes sense so understood, and that onsets and outcomes in those patterns fit.

Bright

- [I] [Amekyaram Village]
 [A] [The Two Sisters]
 (1) 1–3
 (2) 4–5
 (3) 6–7
 [B] [Coyote]
 (4) 8–9
 (5) 10–11
 (6) 12
 (7) 13
 (8) 14
 (9) 15–16
 [C] [The Sisters and Coyote]
 (10) 17–18
 (11) 19–21

 (12) 22–24
 (13) 25–26
 [D] [The Sisters]
 (14) 27
 (15) 28–31
 (16) 32–34
 (17) 35–36
 (18) 37
 (19) 38
 [E] [The Sisters and Coyote]
 (20) 39
 (21) 40–41

Hymes

- [I] [Coyote discovers the hidden salmon]
 [i] [Two sisters hide the salmon]
 (a) 1–3
 (b) 4–7

 [ii] [Coyote pretends to have salmon]
 (A) (a) 8–9
 (b) 10–11
 (c) 12
 (d) 13
 (e) 14

 (B) (a) 15–16
 (b) 17–18
 (c) 19–20
 (C) (a) 21
 (b) 22–24
 (c) 25–26
 [iii] [The sisters reveal the salmon]
 (A) (a) 27
 (b) 28–31
 (c) 32–34
 (B) (a) 35–36
 (b) 37
 (c) 38

 (C) (a) 39
 (b) 40
 (c) 41

- (22) 42–43
 (23) 44–45
 (24) 46–48
 (25) 49–54
- (26) 55
- [II] [In the mountains]
 [A] [The sisters and Coyote]
 (27) 56–58
 (28) 59
 (29) 60–63
 (30) 64–65
 (31) 66–67
- [B] [Coyote]
 (32) 68–70
 (33) 71–72
 (34) 73–74
 (35) 75–76
- [C] [The Sisters]
 (36) 77–78
 (37) 79–81
 (38) 82–83
 (39) 84
- [III] [On the Ridge]
 [A] [The Sisters]
 (40) 85
 (41) 86–87
 (42) 88–92
- (43) 93–94
 (44) 95–99
- (45) 100–103
- (46) 104
- [B] [Epilogue]
 (47) 105–108
 (48) 109–111
- [II] [Acorns and salmon for all]
 [i] [Coyote gets them to take him for acorns]
 {(a) 42–43
 (b) 44–45}
 {(c) 46–48
 (d) 49–50}
 {(e) 51–52
 (f) 53–54}
- [ii] [He scatters the acorns]
 {(a) 55
 (b) 56–58}
 {(c) 59
 (d) 60–63}
 {(e) 64–65
 (f) 66–67}
- [iii] [He frees the salmon]
 {(a) 68–70
 (b) 71–72}
 {(c) 73
 (d) 74}
 {(e) 75
 (f) 76}
- [III] [Transformation and renewal]
 [i] [The sisters accept the world to come]
 (a) 77–78
 (b) 79–84
- [ii] [One returns for her knife]
 (a) 85
 (b) 86–87
 (c) 88–92
- [iii] [Her sister and their dog become quartz]
 (a) 93–94
 (b) 95–97
 (c) 98–99
- [iv] [Witnessing the Jump Dance, she becomes quartz]
 (a) 100
 (b) 101–103
 (c) 104
- [v] [The salmon knife]
 (chiasmus)
 (a) 105–106
 (b) 107–108
 (b') 109–110
 (a') 111

Analytic Notes

This analysis incorporates, in quotation marks, notes to the text in Bright, 1984:147. It also explains analytical choices and provides comment on the point and interpretation of the story.

The analysis is organized in sequence by the lines in question, but two sections involve extended discussion of particles that have consequences for the shape of the story as a whole: *yakún* (lines 1–7), and *taʔittam* (line 27), together with *virí*. At

line 41 a third discussion focuses further on the implications of the two different kinds of shape found in the story.

Where the Karok text differs in choice of phonemic transcription between Bright, 1957, and Bright, 1984, I follow the latter (e.g., line 44). In 1957 (p. 204) sentence 27 reads *yakú nu: tá nu? iffikar*, whereas in 1984 (p. 141) verse 23 reads *yakún nu: tánu? iffikar*

LINES 1–7. Bright identified these lines as three verses. At first I did also. The first three lines identify actors and a location, a common way of beginning a story. The next two begin with a pair of particles recurrent in the story, equivalent to ‘now then’, a kind of marker also found in other western North American languages (e.g., Wishram-Wasco Chinook, Santiam Kalapuya).

The third verse poses a problem for my approach. My experience has been that a single turn at talk may have internal organization, but it still counts as a single unit in a sequence of verses. Why are lines 6–7 a separate verse? The organization of the story as a whole leads one to expect sequences of three or five, but expectation can only be hypothesis. The answer depends on the status of the particle that begins the lines, *yukún* (elsewhere *yakún*). (For *yakún/yukún* see Bright, 1957:363, #917; 400, #1657.1. The variation in the first vowel is unexplained.)

Let me set forth the reasoning that at first persuaded me that lines 6–7 and 84, although in each case part of a single turn at talk, should be considered distinct as verses, because of the initial particle. The initial *yukún* ‘see’, seems semantically strong, analogous to a marker, such as the marker *qúštiaxa* ‘behold’ in Wishram Chinook. That it can be the initial marker of a verse is clear in line 100; there it begins scene [iv] of Act III. An occurrence in line 84 is parallel to that in line 6. In terms of their own content and what follows, lines 77–84 must constitute a distinct scene. If *yakún* (in line 84) does not mark a verse, then the first scene of act III has just two verses, the second verse a turn at talk, both marked by the initial pair *kári xás*. If *yakún* marks a verse, then the scene has three verses. In this case the third and final verse begins with *yakún* within a turn at talk. (As discussed at lines 82–83, it is possible that the scene in fact has five verses (including that beginning with *yakún*.)

All this might seem conclusive, but there is a fourth occurrence that has a different context. The first scene of Act II would make sense with five turns at talk: the two sisters, Coyote, the two sisters, Coyote, the two sisters. In this view, *yakún* in line 44 is simply the second part of a single turn at talk. The overall organization of the act, however, suggests a different interpretation. Its three scenes are the center of the story, so far as acorns, salmon, and Coyote are concerned. Scenes two (he scatters the acorns) and three (he frees the salmon) are specially marked in organization. Instead of three or five verses, each has three pairs of verses. (Three pairs of verses are frequent in languages to the north, such as Kalapuya and Chinookan). A five-verse first scene would stand apart. Of course such a scene is perfectly possible. After all, it is the provision of acorns and salmon that counts. But if *yakún* is taken as having

the same role here that it has elsewhere, then the first scene has three pairs of verses, as do the second and third scenes. The second act is marked in the same way throughout.

There is more to be said about these three occurrences of *yukún*, *yakún*. I noticed later that they are structurally equivalent in relation to the organization of the text as a whole. Each occurs in the first scene of its act, one of the three acts of the myth. Each has to do with a focus of the act: hidden salmon, going to gather acorns, the end of the myth age and the coming of the Indian people who will inherit the benefits that the myth recounts. Each is involved in setting the stage for such a focus.

Notice that these three occurrences are all spoken by the two sisters. There is a fourth occurrence in line 100, spoken not by them but by the narrator in her own voice. It echoes and confirms the third occurrence spoken by the sisters. In effect, there is prophecy (by the sisters) and confirmation (by the narrator). Things have come to be as they are because of what has happened in myths, indeed in this myth itself.

When this analysis was presented to the Smithsonian seminar in 1985, Ives Goddard, Smithsonian Institution, challenged taking *yakún* as a marker, asking if there were a consistent criterion. The challenge led me to notice the parallelism in structural position and the role of the occurrences. But evidence from several other narrative traditions in western North America now leads me to another view. *Yakún* does have significance, as an expressive or rhetorical device, consistently used. But there is reason to think that it does not structurally distinguish verses. That the first scenes of Acts I and III have two verses, not three or five, is not a failure of patterning, but patterning with a special role.

In the first scene in both acts I and III the two women are speaking, asserting authority and announcing a decision.

- I [i] Nobody will eat salmon.
See, we’ve hidden it, that salmon.
II [i] Look, he’s spoiled it, that Coyote.
So let’s go, all right, let us be transformed.
See, another (kind of) person will come to exist.

Even numbers and women deciding co-occur. This connection between women and two- or four-part patterning has been found in the Karok myth Phoebe Maddux told to Harrington (Hymes, 1997) and in several other western North American traditions that otherwise use relations of three and five (Hymes, 2000).

It makes special sense, I think, in this case. A woman, telling the story, has the women in the story themselves foretell that there will be a decisive change, and it is their knife that will be used with spring salmon, so long as that world continues to be renewed.

LINE 3. “*?ame-kyá-ra-m*, literally ‘salmon-making place’, is at Ike’s Falls, on the west side of the Klamath River, not far below the confluence with the Salmon River. As the concluding verses of this text indicate, it was the site of the Jump Dance, one of the sacred ceremonies of Karok world-renewal” (Bright, 1984:147, note 2).

LINE 5. The particle *vúra* has an emphatic force, but it is not equivalent to any single English word. I show its presence by !.

LINE 7. Bright gave a separate line to extraposed nouns, such as 'that salmon'. (The object in such a case is already marked pronominally in the verb: 'we've hidden it'.) This is a fine insight, fitting the character of the language and enhancing the verbal effect.

LINE 15. The two presentations diverge again at line 15. Bright put lines 15–16 in the same scene as preceding lines because Coyote continues to be the actor in question. I begin a new unit (stanza B) with line 15 because it expresses a significant change of location: Coyote arrives at the house of the sisters and sits down in the corner. Perhaps a more thoroughgoing use of the criterion of change of locale of action would have led Bright to the same finding. (After all, Coyote is just as present to the sisters in their house here as he is in line 41, which was kept by Bright within a scene that also involved the sisters.) The example is one of several in which criteria for verses interact with criteria for larger units.

LINE 20. I end the verse with line 20 because it ends the quoted speech. Also, line 21 begins with the (single) particle marker *xás*. Note that Coyote travels upriver in many myths. When only his own gain is in mind, he is usually a comic buffoon. When, as here, the welfare of people to come as well as himself is in mind, he usually succeeds.

LINE 26. "Alder bark is red, the same color as salmon flesh" (Bright, 1957:205, note 39, 1984:147, note 3).

LINE 27. Here as elsewhere the present translation may be painfully literal and invariant, but that is in order to preserve the evidence of structure and emphasis (and sometimes of variation). Part of the literalness has to do with invariant rendering of a particle that marks organization, *taʔittam*. The following is a discussion of its role.

Bright usually translated *taʔittam* 'so'. I render it 'already', partly to distinguish it from *viri*, which is also translated 'so'. 'So' seems to fit *viri* best because *viri* commonly occurs where outcomes are indicated. Note the stanza-ending parallel in the culminating lines 66–67 and 75–76, when acorns and salmon are freed. *Viri* indeed is clustered toward the end of the story (lines 66, 75, 82, 93, 95, 105).

Taʔittam, on the other hand, has some association with onsets. The connections that emerge between role and meaning, and with position in the story, strengthen my conviction that the configuration arrived at by this approach has validity and can make a contribution to lexicography as well as to narrative. Following indications given by Bright in his grammar and lexicon, I translate it 'already'

Bright (1984:138) always took a line containing *taʔittam* and the anterior tense suffix *-he-n* as the beginning of a scene (lines 27, 39, 56, 85 herein). In the present analysis, two such lines also begin scenes (27, 85), but two do not. *Taʔittam* begins a stanza in line 39 and begins a verse in line 56, the second verse in its scene (and stanza). (Both Bright and I also have one oc-

currence of the particle without the tense suffix beginning a verse (35)).

In lines 27, 35, and 39, *taʔittam* forms a pattern marking three successive stanzas of a scene (I (iii) ABC). The first and third stanzas have the anterior suffix *-he-n*, the second stanza does not. Unity of marking parts of a stanza seems to outweigh the absence of the suffix.

As to line 56 not beginning a scene: line 55 cannot be the last line of the preceding scene. Travel to a new location, as in 55, regularly begins a scene, and the lines that precede line 55 are internally quite coherent without it. Line 55 is clearly the start of a new sequence. Line 56 is not the first but the second verse of its scene, despite having *taʔittam* and *-he-n*. It may be that the pattern of grouping verses in three pairs, which runs through the act, gives *taʔittam* some initial force here. The first two verses of the scene (lines 55, 56) may work together here as an initial group. That finding in turn supports the finding that the stanza has such grouping.

LINE 35. In Bright (1984) the prefix *pa-* was omitted before *musvirik*, evidently by oversight. Compare Bright, 1957:204, sentence 20.

LINE 41. I recognize a new verse because the actors change from the two sisters to Coyote, there is separate action, and the line begins with Coyote's name (rather than a pronoun). (A proper name sometimes has such force in other traditions). This verse has an important place in the configuration of the story as a whole.

I have said that when the goal of the action is taken into account, the apparent unity of segments changes. Line 41 is a case in point. It is the second line of the second verse in a series of seven in Bright (verse (21) in Scene E). In the present analysis it is the culminating third verse in the culminating third stanza of a scene that completes an act (I(iii)C).

The point of the story, after all, in keeping with analogous stories among many Native Americans of the Pacific coast, is that two women are keeping the salmon (and here, acorns) that the people of the future will need, and that Coyote causes these foods to be released and become available. Coyote knows at a distance that the two sisters hide the salmon and announces his intention to correct the situation at his first appearance in the story. In this version, he must first find out where they hide the salmon. He disarms them by seeming to have salmon of his own. When the story tells that the sisters release salmon within their dwelling, the point and culmination of the scene comes, not simply when they eat but in 'Coyote is watching that!'. Now that the secret is out, something can be done.

That point seems the culmination not only of a scene but of the whole first arc of the story: the salmon have been hidden, but Coyote finds out where they are.

LINES 49–54. Bright gave one verse (his (25)) to three turns at talk. They are distinguished herein as (d, e, f).

LINE 52. 'the acorns' because the verb stem *-θav* uniquely implies that it is acorns that are knocked down (Bright, 1957:389, #1435). The other word in the line, *minik*, is a particle expressing reassurance, often translated by Bright as 'all

right' (not 'there', as in his 1982 and 1984 articles). Assurance of course is what Coyote wants to convey at this point, and 'for you' seems to fit that purpose in this line.

LINES 62–63. Bright put the four directions on one line, but there seems to be internal structure: two directions on hills, then two directions on rivers. (The sequence itself forms a chiasmus: up, down, down, up). Notice the pairing of uphill, downriver in lines 86, 87, and the pairing of downriver, upriver in lines 75, 76. In Bright's edition (his verses 41 and 35) as well as in mine, the members of both pairs each have a separate line in these cases.

LINE 74. I distinguish this line as a verse because it begins with the particle marker *xás* 'then'. Bright included lines 73 and 74 in his verse 34. There is a couplet effect in the parallel endings, *-rišuk* 'out' (Bright, 1957:101, §758.18), of the two lines (concealed in line 73 by morphophonemics, *r > n* and gemination of *š*). This effect is part of the 3×2 organization of the verses in the scene. The stem in line 74, *-ívyi-h-*, has the connotation 'to arrive', and in one construction the connotation 'to arrive home' (Bright, 1957:356, #800); indeed, the salmon are arriving where they should be.

LINE 76. I distinguish this line as a verse because it begins with the particle sequence *káru va· kúθ* 'and that for', parallel to *viri va· kúθ*, 'so that for', which clearly begins a verse in preceding line 75. Bright included lines 75 and 76 in his verse 35. The initial parallel is part of the 3×2 organization of the verses in the scene.

LINES 82–83. Bright distinguished these lines as a separate verse, probably because 82 begins with the particle *viri*, which elsewhere in the text is initial to verses. Those verses, however, all occur outside quoted speech (lines 66, 75, 93, 95, 105). In Bright's edition the result of counting 82–83 as a verse is that the section (his scene C, my scene [i] of Act III) has four verses. Counting *viri* as beginning a verse would have the same result for me as I agree with Bright that the next and last line of the section, 84, is a verse, despite occurring within quoted speech, because of the status of *yakún* (see discussion of *yakún* in regard to lines 1–7).

A stanza of four verses contravenes the expectation of relations of three or five. Moreover, the stanza (=scene) in question is like the opening stanza of the story, which has three verses, one introductory and giving location, one beginning with *kári xás* and starting quoted speech, and a third beginning with *yakún*. That suggests taking this sequence as also having three verses, one introductory that begins with *kári xás* and gives location, one that begins with *kári xás* and starts quoted speech, and a third that begins with *yakún*. I have shown the stanza this way.

It remains possible that further analysis of Karok narratives would lead one to interpret the stanza as having five verses. Such an analysis would find parallels to justify treating line 82, and line 83, each as a verse. It would recognize a parallelism between the two lines, taken as

'so + let's go' : 'all-right, let we-be-transformed'

The parallelism would be based on taking 'all-right' (*čémni*), not as the end of line 82, as printed by Bright, but as the beginning of line 83. *Čémni* is related to *čími* (cf. its line initial use in line 91), used in anticipative meaning, generally with imperative and future forms (Bright, 1957:330), and the future form is the last word of line 83.

Bright (1957:348, #660) associated the verb *ipke·viš* 'be transformed' with the resultative function of *-iš* (Bright, 1957:97). Such a result, framed in an optative sense ('let's'), is semantically future. Perhaps the verb itself has historically a connection with the future postfix *-aviš/-e-š* (Bright, 1957:65).

The result of such an interpretation of lines 82 and 83 would be a stanza of five verses.

LINES 98–99. I distinguish a verse here because of the initial particle *xás* and the parallel with the preceding verse. Each has to do with turning into quartz.

LINE 100. "I. e., the human race" (Bright, 1984:147, note 4).

LINES 101–103. I distinguish a verse here because the lines initiate an action of a single actor, the remaining sister, and *yánava* 'behold' is initial in the middle of these three lines, just as it is in the middle of the three-line verse of 95–97. The following line (104) is clearly a new verse, beginning with 'now then', and the preceding line (100) is apparently a remark addressed to the audience.

LINE 104. "Three quartz rocks can still be seen on the ridge: two large ones are the sisters, and a smaller one is their dog" (Bright, 1984:147, note 5).

LINES 105–111. These lines were retranslated and aligned to bring out the internal structure. Note the recurrence of 'while the world' (105–106, 111), enclosing 'just...that knife' and 'that's what...salmon' (107–108, 109–110). In addition, 'long' links the first two units (105, 107), and 'they' links the three following units. Nicely interwoven. Overall, chiasmus seems appropriate for a close that evokes seasonal recurrence and renewal.

Note

1. Seminar on Discourse, 22 Nov 1985, Department of Anthropology, Smithsonian Institution, Washington, D.C.

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The Distribution and Habits of the Ringed Seal and Central Eskimo Settlement Patterns

David Damas

Research on hunting societies has established that settlement patterns of these groups, including their distribution over large areas and their annual cycles of fission and fusion, frequently are affected by the availability of resources and by the practices they adopt to exploit them. For the Central Eskimo¹ of the Canadian Arctic, the ringed seal exerts important influences both on the distribution of the total population and on aggregations of people.

The ringed seal (*Phoca hispida*) reaches an adult length of about 155 cm (4.4 ft) and can attain weights of up to 90 kg (200 lbs), although fat reserves vary seasonally and weights vary accordingly (McLaren, 1958a:14). They feed on small planktonic animals and arctic cod. In late winter females prepare birth lairs in deep snow drifts near shore, especially along tide rifts where rafting of ice occurs.

In his pioneering ethnography *The Central Eskimo*, Franz Boas (1888) laid the groundwork for this study regarding the associations among conditions of sea ice, locations of seals, and human populations.

There are only a few districts where the proximity of open water favors walrus hunting during the winter, and all of these have neighboring floes on which seals may be hunted with the harpoon....As to the remainder the Eskimo live altogether independent of the open water during the winter.

Generally speaking, two conditions are required for winter settlements, viz, the existence of an extensive floe and smooth ice. (Boas, 1888:461)

Regarding characteristics of settlement over the year, Boas (1888:461) wrote: "The natives who lived in large settlements during the winter are spread over the whole country in order that everyone may have a better chance of traveling over his own hunting ground."

In this passage, Boas referred to hunting seals on smooth ice through breathing holes as being the main occupation of the winter season. Ringed seals were also hunted in some locales in the spring when they basked in the sun beside holes then thawed open, in the summer from skin boats, and by several other methods.

Biological Studies and the Distribution of Seal and Human Populations

Although Boas's statement provides a starting point for discussion, animal biologists have broadened our knowledge of the habits of the ringed seal. Their studies also lend insight into the aboriginal distribution of Central Eskimo populations. For example, in a series of papers, Ian McLaren (1958a, 1958b, 1961) developed a formula for estimating the populations of seals for the east-central Arctic. He found that birth lairs occurred in greatest frequency in regions of complex coastlines, where stable ice and sufficient snow cover existed. Furthermore, these places were regions where greater numbers of adult seals, in particular, were to be found. In applying his formula, I found a striking correspondence between regions of Central Eskimo populations and occurrences of complex coastlines in the east-central Arctic (Figure 1). Conversely, habitual Inuit residence is absent in regions of relatively simple or convex coastlines.² In the range of the Iglulik Eskimo tribe, the correspondence is very close. Aboriginally, the Igluliks were divided into three regions of habitation, separated by linear distances of roughly 400–480 km (250–300 mi) between each. These were Repulse Bay-Lyon Inlet at the base of Melville Peninsula, the Iglulik region itself at the north end of Foxe Basin, and the Admiralty Inlet-Pond Inlet region in northern Baffin Island (Mathiassen, 1928:23–36). According to McLaren's formula, the southern section of Melville Peninsula and the area around Iglulik Island and northern Foxe Basin should have sizeable ringed-seal populations. He estimated a population of 30,000 animals for the former region and 47,000 animals for the latter region, but he estimated a population of only 8700 animals for the long stretch of relatively smooth coastline in between (McLaren, 1958b:32–33). Indeed, although this section of coast was frequently traveled, Iglulik Eskimo settlement there was characterized by short-term residence only (Mathiassen, 1928:26). Some locales along this coast were favored from time to time because of the availability of walrus.

The third main region of Iglulik Eskimo habitation was reached by traveling overland to the north end of Baffin Island. The long smooth coast from just west of Fury and Hecla Strait to the northwest cape of Baffin Island historically has been devoid of human habitation. The area comprising the entire north

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FIGURE 1.—Central Eskimo utilization of the ringed seal.

and east coasts of Baffin Island, which included the range of the northern groups of Igluliks and also a large part of Baffinland Eskimo territory, has a complex coastline (McLaren, 1958b:32–33). Although these coasts did not exhibit a continuously inhabited stretch, there were no large gaps such as existed between the Iglulik centers of habitation. The south coast of the island is characterized by a central region of great complexity, with less complex coasts toward the extreme east and extreme west. Human population there, however, was more evenly distributed. According to McLaren (1958a:74), migration of immature seals from the coasts of greatest complexity made possible reasonably high numbers of seals along adjoining stretches of simpler shorelines. Similar migration into Cumberland Sound, where high tides interfere with establishment of birthing lairs, has also been noted (Smith, 1973:50).

The west coast of Hudson Bay presents a variation from the expected relationship between apparently favorable sealing conditions and the distribution of human populations. The southern coast from Churchill to Eskimo Point and the northern section along both shores of Roes Welcome Sound,³ which have smooth coastlines, show correspondence between expectable scant seal numbers and lack of human occupation (McLaren, 1958b:30–33). The stretch of complex shoreline in between, however, did have evidence of some winter habitation at times. Glover (1969) and Burch (1977) indicated that in the period after about 1800 and until the southward movement of Aivilingmiut from Repulse Bay, in 1860 (Robinson, 1973; Ross, 1975), the region was devoid of winter habitation.

Even though his formula for estimating the distribution of ringed seals shows strong correspondence to the nature of

coastlines, McLaren (1961:168) cautioned that "there are complications in other areas (including the ice-locked central Arctic) which have not yet been resolved." Unfortunately, as far as I have been able to determine, this statement from 1961 is still the case at the time of this writing.

There are, however, studies from the western extremes of the vast west-central area, including regions that were part of the aboriginal range of the Copper Eskimo. These studies were based on spring aerial surveys of open breathing holes and of basking seals in 1971–1974 and 1983–1984 (Smith, 1987) and in 1980 (Alliston and McLaren, 1961). The studies revealed migrations into Prince Albert Sound and Coronation Gulf from Amundsen Gulf to the west. Smith (1987:8) thought that the main migration into Prince Albert Sound came in late summer and early autumn, while Alliston and McLaren (1961:17) opined that migration occurred in late spring. The latter also found that in western Coronation Gulf high winter populations could be judged by the number of breathing holes. Smith (1987:11) thought that the number of basking seals in spring "was not representative" of the winter populations. Studies that have attempted to estimate populations of seals from numbers of breathing holes show rather wide variations from region to region and from study to study (Smith, 1973; Finley, 1979; Smith and Hammill, 1981; Hammill and Smith, 1990).

In assessing the general conditions in western Coronation Gulf and the sounds and gulfs of western Victoria Island, Alliston and McLaren (1961:29) concluded that seal populations in the survey region of 1980 "probably fell in the 'moderate' to 'high' range when compared to other areas in the Canadian high arctic" and that densities found in the Prince Albert Sound region exceeded those estimated for southeastern Baffin Island. An abundance of seals in part of the area surveyed was affirmed by Rasmussen (1932:75), who, regarding his visit in February 1924, wrote the following:

Sealing during the past month has been splendidly successful, the greatest we had ever experienced, as many seals being caught in one single day as would have taken a whole month at a similar camp among the Seal Eskimos further east. It was not because the people here were more skilful, but that the current-split ice in Dolphin and Union Strait seemed to be a place of assembly for seals from east and west.

To emphasize that there must be regional variations in sealing within the west-central regions, Rasmussen's (1931:153) earlier report from the "Seal Eskimos," the Netsilingmiut, described a village where only 150 seals had been caught all winter by 12 hunters, which had to feed 31 people and about 30 dogs.

The apparent relative abundance of seals at the western margins of the west-central Arctic may have been due in part to immigrations from open water, but if one applies the criterion of coastline complexity to the region as a whole, estimates of seal populations are confounded. There are both smooth coasts, as on the southern shore of Victoria Island, and much more complex ones as, for instance, along the shores of Queen Maud Gulf. Yet human population was more or less evenly distributed. Copper Eskimo could be encountered almost anywhere in

their wanderings, except that they would almost always be found on the sea ice in winter.

In order to examine more closely the role of the ringed seal in the economy and the settlement patterns of the Central Eskimo as a whole it is necessary to take into account aboriginal economic activities and movements of people over annual cycles.

The Role of the Ringed Seal in Yearly Cycles

Boas (1888:424–439) described cycles of annual economic activity and settlement in the period predating intensive whaler contact for the Cumberland Sound-Davis Strait region.⁶ He noted that from December until about the middle of March, the hunting of seals at breathing holes, which was carried out from encampments on the sea ice, occurred almost everywhere. During the latter half of March, large villages broke up for a period of hunting newborn seals at the birthing lairs, although the settlements remained on the sea ice. Hunting of basking seals began in April on the southern side of Cumberland Sound, but it began somewhat later on the northern side. Some groups instead began to move inland toward Nettilling Lake, where they hunted caribou until December. A more common practice was to extend surface sealing until sometime in July, when the ice began to break up. After that, some people moved to the heads of fiords for the caribou hunt, and some used skin boats to pursue sea mammals during the open-water season. Late autumn was apparently a period of unstable ice, when pursuit of sea mammals was curtailed.

Whale hunts were carried out either along floe edges, as on Davis Strait, or from skin boats. There was a potential of collecting large stores of meat and blubber from these animals, but "it is not probable, however, that a sufficient number of whales were ever caught to support the entire population during the whole of winter" (Boas, 1888:440). The cycle of activities for Davis Strait differed somewhat from that of Cumberland Sound because bears that robbed seal birth lairs along Davis Strait became the chief quarry during March and April (Boas, 1888:439).

The American explorer Charles F. Hall, who visited the region just to the south of Cumberland Sound in 1860–1862, noted settlement in the bays at the outer end of Hall Peninsula. In visiting one of them in February 1861, he observed the following:

Nearly all the inhabitants of both villages had gone away to Frobisher Bay where they hope more success would attend their exertions to procure food. Indeed, I understand that not less than a hundred were together in one place and doing well. (Hall, 1864:325)

This statement seems to confirm McLaren and Smith's studies that indicate seals would be found in greater numbers in the inner parts of fiords than at their mouths or elsewhere. Also, there appears to be a suggestion, at least, that breathing-hole sealing is in some way best practiced under conditions of large gatherings of people, and thus, of hunters. This is discussed further under "Seals and Aboriginal Settlement," below.

There is very little information available regarding the aboriginal hunting and settlement practices of the people of the south shore of Baffin Island, although "the ringed seal has been the single most important resource" in that region as for Baffin Island in general (Kemp, 1984:467).

For the regions of the Iglulik Eskimo, we have the first contact accounts of Parry (1824) and Lyon (1824). On 1 February 1822, while their ships were frozen in at Winter Island near the mouth of Lyon Inlet, a group of 64 local Inuit (Aivilingmiut) set up camp nearby. These people remained together, hunting seals at the breathing holes and walrus at the floe edge, until the beginning of April. They then split into two groups, one of which moved farther from the floe edge and lived, apparently exclusively, on seals (Lyon, 1824:116–118).

Boas (1888:445–450) drew together material from early accounts of the Aivilingmiut that stressed living on stores for most of the winter, although for the winter of 1867–1868, sealing began in early January (Hall, 1879:371). Mathiassen (1928:24–25) thought that more commonly these people moved onto the sea ice at the end of January or in early February (consistent with Parry and Lyon) for hunting at the breathing holes. The young seals were hunted later in the winter. He wrote, "when the weather became warmer (May–June) they pitched their tents on points and islands in the bay and hunted the seals which basked in the sun until the ice broke up at the beginning of August" (Mathiassen, 1928:24–25). After that, kayak hunting was carried out from various points of land "at the end of August or early September, the young men went inland while the older men continued the sea hunt as long as there was open water" (Mathiassen, 1928:24–25). When the sea froze, these men joined the younger men in the caribou hunt. These hunts lasted well into the autumn, after which the people lived on stores until the winter sealing began again.

When Parry and Lyon reached Iglulik Island, in July 1822, they found 120 people encamped there (Lyon, 1824:146). In September, after the explorers returned, they found people settled there in winter quarters (Lyon, 1824:149–150; Parry, 1824:279–280). In mid-December people began to move out onto the sea ice, where two camps were eventually established. One was established near the floe edge for walrus hunting, and the other was located some distance away, where the Inuit were devoted to breathing-hole sealing. Some people remained at the shore through most of the winter. In April some moved to the mainland, where walrus were supposed to be especially abundant along the shore (Lyon, 1824:251–252; Parry, 1824:424–425).

In his account of the traditional cycle of the Iglulingmiut, Mathiassen (1928:29–33) did not describe a long period of living from stores but rather autumn aggregations at places where winter clothing was sewn. After that, "about the new year they assembled at the two winter settlements...and hunted the walrus from the ice edge and seals at the breathing holes" (Mathiassen, 1928:30). The spring and summer activities and places

of residence apparently were identical with those described above for the Aivilingmiut.

In the Pond Inlet region, although Boas (1888) indicated that there were places where conditions allowed walrus hunting in winter, the Inuit incorporated narwhal hunting at the floe edge as a supplement to sealing in winter (McClintock, 1859:157; Mathiassen, 1928:34).

Caribou Eskimo relied for the most part on the animal for which they are named, but in the spring, before the end of May, about one quarter of the Caribou Eskimo travelled to the coast to hunt seals (Birket-Smith, 1929:36, 125). In July "sea mammals are pursued with watercraft" (Arima, 1984:453).

The Netsilik area was first visited by Sir John Ross (1835), whose party encountered people in Lord Mayor Bay on 9 January 1830 when a group of 31 Netsilik men approached the overwintering expedition ship *Victory*. Later, members of the Ross party visited their village, which probably housed over 100 people. Seal hunting was observed only at the breathing holes, but the village split into two sections in March, ostensibly to exploit fresh sealing grounds (Ross, 1835:171–172). Fishing "was commenced remarkably early" by the Netsiliks (Boas, 1888:455), with evidence from both sides of Boothia Peninsula pointing to large gatherings at Netsilik Lake, apparently both in the autumn and in the spring (Ross, 1835:228). Both McClintock (1859), who visited the region in 1859, and Amundsen (1908), whose ship *Gjoa* spent the years 1903–1904 and 1904–1905 at King William Island, reported large aggregations of sealers during the winter months. Rasmussen (1931:160) played down the role of spring surface sealing for the Netsilik. He found people moving inland early in spring either to fish or to intercept the migrating herds of caribou that entered the country. Apparently there were subregional differences, however, because Balikci (1964:41–43) reported surface sealing as well as sealing in the water of open holes in spring. Also, a special method of spring hunting was practiced at Thom Bay, where seals were captured through both natural and artificial holes at a tide crack along which seals migrated in late spring (Rasmussen, 1931: 160–161; Damas, 1965).

For Copper Eskimo regions, sealing commenced about the beginning of December, except in eastern parts of the area, where normally successful caribou hunting delayed the move to the sealing areas about a month (Rasmussen, 1932:77). For the western part of the tribal area, Stefansson (1913:169–170) developed a model of winter movements based on seal-hunting practices. A circle of 8 km (5 mi) in radius was the normal hunting range. After about a month, when such a circle was hunted out, the snow-house camp was moved about 16 km (10 mi), and so on, throughout the sealing season, which lasted until mid May. Jenness (1922:120) gave a picture of the movements of Copper Eskimo during the winter of 1915–1916 that corresponds in most respects with the model of Stefansson. A group of 33 families (perhaps 100 people or more) camped at one place from December to early February. After two more monthly moves as a unit, in early April the group split into sev-

eral smaller groups. This description differs from that of the Stefansson model in that the first two months of winter were spent at one place. This occurrence, as well as the splitting of the village later in this season, can be probably traced to conditions of daylight:

Spring with its mild, sunny weather brings longer and pleasanter days for hunting. Seals, although not more plentiful, perhaps, are more easily discovered. ...Migrations take place more frequently...and the men hunt nearer home, staying away eight or nine hours a day instead of four or five as in winter. (Jenness, 1922:119)

Rasmussen's (1931:78–85) enquiries and mine (Damas, 1969a:121–122) indicate that much the same picture emerges for eastern elements of Copper Eskimo, with the various hunting groups coalescing during the winter sealing period which, as farther east, lasted until about mid-May.

Two ecological anomalies in the Copper Eskimo area were noted by Stefansson. One is that only a few of the older men knew of the method of stalking seals on the ice (Stefansson, 1919:297). I have cited the work of animal biologists, however, that shows that numbers of seals were counted basking on ice in the western part of Copper Eskimo country. Another anomaly was that "environment...does not explain why they never hunted seals from kayaks during the summer months" (Stefansson, 1951:3). It could be argued that Copper Eskimo could more profitably hunt the caribou in the late spring and summer, but the hand-to-mouth existence of some elements of the tribe (Jenness, 1922:123–124, 127–142) during those seasons challenged the advantage of omitting sealing at those times.

Seals and Aboriginal Settlement

In reviewing the above descriptions of the effects of the habits and methods of hunting the ringed seal upon the settlement patterns of the Central Eskimo for aboriginal periods, a number of points can be stressed. (1) For the east-central Arctic where biological data are available, the correlation between regions of Inuit habitation and those of expectable large populations of seals is striking. (2) In certain east-central locales, hunting of large sea mammals obscured the role of the ringed seal and induced groups to split up for part of the winter season; however, because these regions had favorable sealing conditions, the seal hunt was carried out over a greater part of the year's economic and settlement cycle. (3) The development of a variety of seal-hunting techniques within a region created access to that animal over the major part of each year and thereby influenced the location of settlements. (4) Large winter gatherings were persistently associated with the period when breathing-hole sealing was carried out in most Central Eskimo regions. (5) With failure to develop the full inventory of seal hunting methods, west-central settlement was influenced by sealing for shorter periods of each year than was settlement farther east. During the winter and early spring, however, the Inuit were completely dependent on breathing-hole sealing and encampment on the sea ice at great distance from open water.

The above statements need some elaboration. First, with regard to hunting larger sea mammals, most hunting techniques, especially those for walrus and large whales, entailed considerable danger. Both Lyon (1824:114) and Hall (1864:264–265) noted incidents in which men were caught on drifting ice while hunting walrus along the floe in winter. Much of the summer hunting of larger sea mammals was carried out from kayaks, and this frail craft was very vulnerable to animal attacks. There is also the fact that seal hunting continued even when ample stores of walrus fat were available because of the superiority of seal fat as fuel in the stone lamps (Damas, 1960–1961).

Regarding the size of settlements associated with the various sealing methods, both Balikci (1964:17) and the present author (Damas, 1969b:51) have posed an almost deterministic linkage based on the notion that the more breathing holes that are attended, the greater the chances of success in the hunt. Consequently, there would be an advantage to large numbers of hunters in this pursuit, implying larger aggregations of people during the breathing-hole sealing season. On the basis of the above discussion, this hypothesis may have to be modified according to the actual numbers of seals in a particular locale, and the ratio between numbers of seals and numbers of breathing holes. Also, in a largely reconstructive study like this one it is impossible to judge the degree of motivation for these associated gatherings that came from the social and ceremonial events that accompanied them.

On the basis of analyzing census data of the Fifth Thule Expedition (modified by information gathered in the field), Damas (1969a:122) arrived at an average figure of about 100 individuals for winter groupings of Iglulik, Copper, and Netsilik tribes.⁵ This figure probably represents the maximum number of people that could be fed from the normal range of these winter hunts. To establish a minimum number of hunters for effective breathing-hole sealing is more difficult both on the basis of actual reports from the breathing-hole phases of yearly cycles and because of variations in local numbers and of availability of seals.

Culture Contact, The Ringed Seal, and Settlement Changes

There have been three main phases of culture contact in the Central Arctic. The first of these, the period of contact with commercial whalers, had its beginning on Baffin Island before the middle of the nineteenth century. In such places as Cumberland Sound, introduction of firearms and wooden boats enhanced procurement of game (Boas, 1888:466–467) and made the pursuit of larger sea mammals safer and more productive. Caribou could also be hunted more easily. The chief change in sealing methods was the addition of shooting seals at current holes in the ice and at floe edges (Boas, 1888:480, 498). With increased game production, settlements became more dispersed and permanently established over the year's cycle. Aggregations on the sea ice for sealing largely faded. Whaling stations also drew people for much of the year (Boas, 1888: 467–468).

After 1860 similar improvements in economy and changes in sealing methods occurred in the northwestern Hudson Bay region, but more substantial alterations in settlement also took place. Numbers of Aivilingmiut gathered around overwintering whaling vessels in the Fullerton Harbour-Depot Island region, well to the south of their normal range. Also, some Netsilik were drawn from their traditional hunting grounds to the northwestern Hudson Bay region. Winter hunting began to include floe-edge sealing and walrus hunting as well as interior pursuit of both caribou and musk oxen (Robinson, 1973; Ross, 1975).

Only the east-central Arctic was directly involved in the activities of commercial whalers, but the entire central Arctic was drawn into the second phase of contact, which involved the trade in arctic foxes. Between 1910 and 1923, trading posts, with very few exceptions, were established throughout the area. In general this was an era of improved subsistence and altered settlement practices. For eastern and southern Baffin Island there was a continuation of the changes brought on by the whalers, but the benefits of introduced technology were spread more widely with the establishment of a chain of posts by several trading firms. The practice of floe-edge and current-hole sealing increased and the now shore-based settlements were dispersed and seasonally more stable. In this period traders discouraged the sort of aggregations around their posts that had existed around whaling stations.⁶

Indeed, for the central Arctic as a whole, the hunting-trapping base camp set on land at sites of large caches became a typical settlement type (Damas, 1988:116). For both Netsilik and Copper Eskimo regions, the adoption of sealing in summer from boats contributed to the enhanced cache-oriented base camp. Other parts of the associated complex of factors were improved takes of caribou through the use of rifles (in areas where large herds persisted) and large catches of fish through the use of nets. In Rasmussen Basin and in the Queen Maud-Coronation Gulf regions, however, Copper Eskimo continued to move to the sea ice to form breathing-hole sealing villages. During this period, these moves began later in winter or in early spring when supplies were exhausted (Damas, 1988:112).

The third stage of culture contact began in the mid 1950s and was almost fully established by the late 1960s. This was the ingathering of people into large centralized settlements of mixed ethnic composition. Despite the influx of cash income, subsistence hunting of both land and sea animals continued. By this time, additional methods of seal hunting had been introduced, including the use of set guns or hooks at breathing holes and the use of nets. While the bulk of populations remained in the large centers for much of the year, some men made excursions (more and more with snowmobiles) for caribou and to visit traps, or hunted seals nearby. In summer, shore-based tent encampments were sites of seal hunting from boats. A boom in sealskin prices began in 1962, bringing a new or expanded source of income to the Inuit and enhancing the value of seal hunting (Wenzel, 1991). In 1978 an anti-sealing movement brought about a sharp decline in trade prices of the skins. After

a brief resurgence of the trade in the early 1980s, a virtual demise of the industry was brought on by the boycott on importation of sealskins by the Council of the European Economic Community in 1983. This decision was renewed in 1985 (Wenzel, 1991:1) and again in 1989 (Wenzel, 1991:179); however, subsistence hunting of seals still continued in many places in the Canadian Arctic.

Cultural Ecology versus Environmental Determinism

I have examined the relationship between the distributions and habits of the ringed seal and Central Eskimo settlement. Both the overall dispersal of populations and the seasonal cycles of subsistence activities and settlement have been argued as supporting such relationships until the attractions of the new centralized communities provided more powerful incentives for aggregation. I have indicated that regions of habitation and likely occurrence of good sealing grounds coincided to a great degree in the east-central Arctic and that populations were evenly spread in the west central area. There were, however, unexploited regions of game potential lying outside the central Arctic as conceived herein. During the 1920s, 1930s, and 1940s the Hudson's Bay Company moved native people to places where the fur trade could be expanded. The relocations of people to Clyde River, which is within the normal range of Baffinland Eskimos, and to Somerset Island, which is outside the normal habitation of Central Eskimo tribes, are two examples⁷ (Jenness, 1964). Later, in the 1950s, native people were moved to Ellesmere Island (Grise Fiord) and to Cornwallis Island (Resolute) by the Canadian government. As in the earlier moves, the colonies were established at places that were considered to be underexploited for fur and game. Although the later moves have involved problems (Tester and Kulchyski, 1994), successful subsistence hunting, especially of the ringed seal, has sustained the relocated people (Bissett, 1968; Freeman, 1984). Distance from kindred has presented difficulties in these cases and may well have been a strong factor in accounting for lack of human occupation of the High Arctic in the ethnographic era.

Other contact-inspired moves have been made on more voluntary bases, such as the aforementioned movement of Aivilingmiut and Netsiliks on the west shore of Hudson Bay.⁸ Also, the opening of the Keewatin interior for occupation has been attributed to the disappearance of the Chipewyan, largely due to an epidemic (Smith and Burch, 1979).

The frequent occurrence of large aggregations of people in winter sealing villages was not the only example of large aggregations during annual cycles of settlement. The period just preceding the winter sealing, when Inuit usually lived on stores, is a case in point. Certain activities that were usually carried out from dispersed groups could have been undertaken from larger gatherings. These include surface sealing and summer sea-mammal hunting. Periodic gatherings are congenial to the creation and reaffirmation of kinship and to voluntary asso-

ciations among the Central Eskimo, as is the case among other hunting peoples. But dispersal has the advantage of easing social tensions in such societies. In the case of the Central Eskimo, blood feuds, witchcraft, the narrow scope of kinship, and the almost total lack of political organization served to foster interpersonal strife. If students of hunting societies are tempted to place too much emphasis on economic factors, it would be well for them to take note of the following considerations:

Groups are forced into this or that kind of aggregation by their size, their technology, the suitability of the environment for subsistence and settlement, or the need for self-protection from enemies. Yet these are not the only determinants. ... It would seem that groups simply choose a locale congenial to them, that is, congenial with regard to the relationships they desire to maintain in all or in certain spheres of activity. I suggest that this desire is not further reducible. (Nadel, 1951:156)

Notes

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1. I follow herein designations set forth for "tribe" and for "Central Eskimo" in Damas (1984a:3, 1984b:391, respectively). I include Melville Peninsula, Baffin Island, and the west coast of Hudson Bay in "east-central Arctic." For "west-central Arctic" I refer to the aboriginal ranges of the Netsilik and Copper Eskimo (see Figure 1).
2. An exception occurs in Amundsen Gulf, where birth lairs have been found on shifting but relatively stable ice some distance from land (Smith and Stirling, 1975).
3. An exception is the usually inhabited deep indentation of Wager Bay.
4. Indeed, there is some difficulty in finding an aboriginal picture for Cumberland Sound from Boas's account, which includes references to "shooting seals," and settlement had been altered at the time of his visit in 1883–1884 (Boas, 1888:426, 467–468).
5. I accounted for satellite camps in the Iglulik region in terms of good walrus locations, but some camps relied mainly on seals. Other studies of the ringed seal have been made in the Barrow Strait (Finley, 1979; Hammill and Smith, 1990), which lies beyond the normal aboriginal range of Central Eskimo groups treated herein and is within range of the floe edge for the movements of seals.
6. Edward Fitzgerald to W.W. Cory, 10 Apr 1925, RG85 volume 1069, file 251(1), National Archives of Canada, Ottawa, Ontario, Canada.
7. For Clyde River, see A. Brabant to Edward Fitzgerald, 22 Dec 1922; Ralph Parsons to Fur Trade Commissioner, 8 Jan 1922, R.G. 2/4/F87, Hudson's Bay Company Archives, Winnipeg, Manitoba, Canada. For Somerset Island, see Jenness, 1964:59–64.
8. This movement inland has been attributed to acquisition of guns, fish hooks, and nets (Glover, 1969), and to overpopulation of the coast (Burch, 1977). Another interpretation is that this coast is unsuitable for breathing-hole sealing because its shallowness causes "freezing to the bottom even a good way from the shore" (Birket-Smith, 1929:126–127).

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Species Transformations in Northern Mexico: Explorations in Rarámuri Zoology

William L. Merrill

In nova fert animus mutatas dicere formas corpora...
Ovid, *Metamorphoseon*, book 1, 1–2, XXXX

Because of Bill Sturtevant's interest in the ethnobotany of the Indians of the southeastern United States, I had the opportunity—in the summer of 1972—to complete a research project under his direction on a Southeastern Indian ceremonial beverage and emetic called “black drink.”¹ Over the course of that summer, Bill shared his unparalleled knowledge of American Indian ethnobotany and ethnology with me. He also loaned me his copy of *Purity and Danger*, Mary Douglas's path-breaking study of the relationship between the social construction of reality and cultural practices. In this work, Douglas (1970) explored, among many other topics, the linkages between the classificatory status of certain animals and the special treatment afforded them in diverse cultural settings, proposing, in the best-known example, that the ancient Hebrews had prohibited some kinds of animals as food because these animals were classificatory anomalies.

Douglas's views on such matters took on added significance for me in 1977, soon after I began research in the Rarámuri (Tarahumara) Indian community of Rejogochi, located in the rugged Sierra Madre Mountains of northern Mexico.² One morning in July of that year, I was following Mauricio Aquíchi, a Rarámuri man in his mid-twenties, up a pine- and madrone-covered slope just behind his house in Rejogochi. I always enjoyed spending time with Mauricio because he usually assumed that I knew little or nothing about anything. As we reached a level spot on the trail, he called me over to a small pool of rainwater that had collected in a depression in the volcanic rock and, with some delight, pointed out a multitude of tadpoles wiggling about. He explained that these tadpoles would soon grow legs, lose their tails, and emerge from the water as frogs and asked me if they did the same in my country. I replied that they did. He then asked if squirrels in my country change into snakes. Taken aback, I said, “No,” and asked for details. Scrunching up his shoulders and flattening his arms

against his body, he described the process whereby two species of squirrels, upon reaching old age, would gradually lose their fur and legs, become more elongated, and emerge as two different kinds of snakes.

Intrigued by this view of zoological possibilities, my wife, Cecilia Troop, and I began compiling information on Rarámuri ideas about animals. Through both formal interviews and informal conversations with a number of people in Rejogochi, we worked out the basic structure of their zoological taxonomy and recorded a rich corpus of ethological and ecological knowledge along with a number of stories in which animals figured as the protagonists.³ In the process, we discovered that the Rarámuri people in this community agreed on eight different sets of species transformations that Western zoologists do not acknowledge as valid (Table 1).

These transformations are of three types. The first involves the maturation of what the Rarámuri identify as immature forms into adult forms, in this case, the maturation of salamanders into pocket gophers. The second type of transformation—in which small rodents change into bats, large fish change into otters, and squirrels change into snakes—takes place when the animals that transform reach old age. The Rarámuri consider these maturation and old-age transformations to be inevitable, but they say that the third type of transformation occurs only when certain domesticated animals (goats, pigs, and house cats) spend extended periods in the wilds beyond areas of human occupation, where they transform into their wild equivalents (deer, peccaries, and ring-tailed cats, respectively).⁴

The Rarámuri state that these eight species transformations are similar to metamorphoses that are readily observable in nature—for example, the metamorphosis of tadpoles into frogs or caterpillars into butterflies—and they do not have separate verbs or categories to distinguish between them. They believe that, as in the case of the observable metamorphoses, the species transformations occur in only one direction and are irreversible. They also view the morphological changes entailed by these species transformations to be no more radical, and in

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TABLE 1.—Rarámuri species transformations.

	Maturation	
salamanders (<i>rotébi</i>)	→	pocket gophers (<i>ripósi</i>)
	Old Age	
small rodents (<i>chikúri, rokirike, rori, sotóchi, chichimó, chiká</i>)	→	bats (<i>so'pichí</i>)
large fish (<i>kuchú, musí, sikachi</i>)	→	otters (<i>bahuri</i>)
red tree squirrels (<i>chimori sitákame, alias chimori láchame</i>)	→	large constrictors (<i>bahomáwari</i>)
rock squirrels (<i>chipawí</i>)	→	large rattlesnakes (<i>sayáwi</i>)
	Domesticated to Wild	
goats (<i>chibá</i>)	→	deer (<i>chomari</i>)
pigs (<i>kochine</i>)	→	peccaries (<i>kabilin</i>)
house cats (<i>misi</i>)	→	ring-tailed cats (<i>rikamúchi</i>)

some cases less radical, than those involved in the metamorphoses. They point, however, to one major difference between the species transformations and the observable metamorphoses: all the animals involved in the transformations “breed true,” that is, they produce offspring. There is one exception. The Rarámuri believe that salamanders mature into pocket gophers without reproducing, comparable in this regard to tadpoles and caterpillars, which are immature, nonreproducing forms of frogs and butterflies. They recognize that adult pocket gophers produce their own offspring, which they distinguish from salamanders, but, as in the case of the other species transformations, they claim that individual animals that are the result of transformations are identical to those that mature from the natural offspring of members of their species.

The postulation of such species transformations probably can be found in many other societies around the world, but the ethnographic record is uneven. The most detailed information, and the only systematic efforts to explain the phenomenon, come from research among the Kalam and Rofaifo of highland New Guinea and the Nuaulu of nearby Seram, most conducted during the 1960s and 1970s, the heyday of ethnobiological research in anthropology (e.g., Bulmer, 1967, 1968, 1979; Ellen, 1972, 1993; Bulmer and Menzies, 1973; Dwyer, 1976a, 1976b). These proposed transformations have piqued the interest of researchers primarily because “they often relate to common creatures and contrast sharply with empirical biological knowledge which is otherwise extremely accurate” (Ellen, 1993:163; cf. Bulmer and Menzies, 1973:101; Dwyer, 1976a:189, 198).

In attempting to account for these beliefs, scholars have addressed two fundamental questions: Why do the people in question postulate species transformations that Western zoologists do not recognize? and Why do they involve these particu-

lar animals rather than others in these transformations? Below I offer responses to these questions in regard to the Rarámuri examples of species transformations, evaluating in the process the extent to which explanations formulated to account for species transformations in other cultural settings can be applied to the Rarámuri case and vice versa. I argue that the postulation of such species transformations is intended in all instances to establish an affinity among the species involved but that the motivations for doing so vary widely from one society to another, primarily reflecting differences in the ways in which the members of these societies approach classifying the animal world. I propose that the Rarámuri employ species transformations as a device to explain why certain animals display features that set them apart from most other members of the general taxonomic categories with which they are most closely associated. I note, however, that at least one animal that appears to be taxonomically “singular,” the *ropogópali*, an unidentified lizard, is not involved in such transformations, and I offer my perspective on why this particular exception exists. I also suggest more generally that, because such bodies of knowledge are dynamic systems in the process of being developed through individual intellectual activity, it is unrealistic to expect that specific phenomena within them (like species transformations) can be exhaustively explained at any particular point in time.

I then discuss some interconnections between the Rarámuri's zoological knowledge and other areas of their thought, proposing that they apply their concrete, empirical knowledge about animals to support fundamental but ultimately nonverifiable propositions about the nature of their universe. I conclude by considering the characteristics of the metaphysical systems within which the postulation of such species transformations might be expected to occur.

Rarámuri Species Transformations

The Rarámuri of Rejogochi employ three verbs to convey the meaning ‘to transform’: *nahítama*, *so'pétama*, and *nirema*. I found that people tended to use *nirema* to refer to any sort of change in condition or form, whereas they usually restricted *nahítama* and *so'pétama* to refer specifically to transformations of one kind of being into another. When discussing such transformations, they tended to employ *nahítama* more frequently than *so'pétama* and to employ both more frequently than *nirema*. Otherwise I discovered no differences among these verbs.⁵ The Rarámuri's use all three to denote the contemporary transformation of one kind of animal into another and to denote as well the transformation of humans into animals, which took place in the ancient past and can also occur today, but only after death.⁶ Here I focus on the three types of contemporary animal transformations that the Rarámuri propose, returning to the human-to-animal transformations near the end of the essay.⁷

The first transformation type—maturation—includes only one example that Western zoologists do not recognize: the mat-

uration of salamanders into pocket gophers.⁸ The Rarámuris report that the salamander develops from eggs in the water, and then, when it has grown legs, it emerges from the water and burrows underground, where they often encounter it while hoeing their maize (this “intermediate” stage is labeled by some as *ruʔrusi*). There it grows fur and gradually develops into a pocket gopher. No one in Rejogochi was entirely sure what animal is the mother of the salamander. All agreed that it is not the pocket gopher, and those who hazarded a guess suggested a frog, although they could not explain where this frog comes from if all salamanders change into pocket gophers.

The second type of transformation occurs only when the animals that undergo transformation reach old age. Four sets of animals are involved in these transformations. The first set consists of mice, rats, shrews, chipmunks, and small ground squirrels, all of which grow wings and become bats when they grow old. A number of species of bats are found in the Rejogochi area, but the Rarámuris include all of them under the single term *soʔpichi*. They explain that they differ in appearance because they derive from different animals.

The second set of animals involved in old-age transformations is composed of fish that, when old, move under a stream bank and grow fur, emerging as otters. Three kinds of large fish are mentioned by most people, all of which live in the principal streams where otters are found. Of these, the catfish (*musí*) is most often named because of its “whiskers.”

The third and fourth sets of old-age transformations involve squirrels that transform into snakes when they reach old age. The red tree squirrel hides itself inside a hole in a tree where it loses its fur and limbs and a few weeks later slithers out as a large constrictor. The rock squirrel, which climbs bushes but not trees, transforms into the largest rattlesnake of the Rarámuri area in the underground burrows where it lives.

Some people claim that the animals that undergo transformation at old age can decide when but not whether they will transform; if they delay too long, the transformation will occur anyway. When one of these animals transforms, the resultant animal is believed to be at a slightly earlier stage in its life cycle than the animal from which it derived. Such transformations thus involve rejuvenation and replace, or at least delay, death for the original animals, but the Rarámuris disagree whether this consequence is desirable. Some assume that these animals are pleased to be able to postpone death momentarily, whereas others express sympathy for them, commenting that, as in the case of human beings, it is good to die when one is old.⁹

In the third type of contemporary transformations, three kinds of domesticated animals are believed to change into wild animals if they abandon the Rarámuris’ settlements for the surrounding forests and mountains: goats transform into deer, pigs into peccaries, and house cats into ring-tailed cats.¹⁰ These transformations can occur at any point in the life cycle of these domesticated animals but only if they remain in the wild long enough to transform, usually two to three weeks. If they are there for shorter periods, they will not transform, nor will any

wild animals that the Rarámuris capture and take to their homes change into their domesticated equivalents. By the same token, domesticated animals that have undergone transformation will not revert to their former selves if they return to the human sphere.

When I asked people in Rejogochi how they knew that these eight sets of species transformations actually occur, all responded that they had learned of them from their elders or from acquaintances. Familiarity with metamorphoses that occur in nature reinforces confidence in the reliability of this transmitted knowledge, as does the fact that a few people claim to have seen rock squirrels in the process of changing into rattlesnakes (cf. Lumholtz, 1902:309). Although most said that they have never seen such transitional forms, they did not regard this absence of firsthand experience of the transformations as sufficient to undermine their belief in their reality. They pointed out that these transformations are difficult to observe because they take place outside the human realm—in the deep forest, under river banks, below the ground, or high in trees—and that once a transformation is complete, evidence of the derivative species disappears. Some people also speculated that an animal might not transform if it realized it was being watched by humans.

Explaining Species Transformations

The species transformations postulated by the Rarámuris are quite similar in form to those proposed by the Kalam and Rofaifo of highland New Guinea and the Nuaulu of Seram, Indonesia, but the kinds of animals involved are for the most part quite different. Among the last three societies, these animals include roundworms, flatworms, earthworms, beetle larvae, fruit flies, butterflies, crabs, eels, snakes, lizards, sea turtles, terrapins, frogs, birds of paradise, bats, rats, marsupial cats, pigmy possums, ring-tailed possums, cuscuses, and bandicoots. Researchers have concluded that some of these transformations are postulated because people lack information on the complete life cycles of the animals they involve in these transformations and do not realize that the animals that are the end products of the proposed transformations produce their own offspring. In the case of most of these transformations, however, the people who maintain that they occur recognize that each of the linked species also breeds true. Because the transformation of one reproducing species into another does not occur in nature, the postulation of these species transformations cannot be attributed to a mistaken extrapolation from observation but must be motivated by something else (Ellen, 1993:166). I consider only these “motivated” transformations here.

The Kalam propose six motivated species transformations, all of which involve rodents and marsupials (Table 2). Bulmer (1968) and Bulmer and Menzies (1973) described in detail the similarities between the linked species to indicate why the Kalam might postulate these transformations. They suggested that the transformations possibly are based on a confusion between the linked species caused by the existence of atypical ex-

TABLE 2.—Kalam, Rofaifo, and Nuaulu species transformations.

	Kalam	
shrew-like marsupials (<i>as aln</i>)	→	ring-tailed possums (<i>kmn skoyd, kmn ymdn</i>)
shrew-like marsupials (<i>as aln</i>)	→	“native cat” (<i>kmn swatg</i>)
prehensile-tailed rats (<i>as ymgenm</i>)	→	ring-tailed possums (<i>kmn skoyd</i>)
bush rats (<i>as mwg</i>)	→	giant rats (<i>kmn abpen</i>)
long-snouted rats (<i>as sjan</i>)	→	giant rats (<i>kmn mosak wlm-ket</i>)
ring-tailed possums (<i>kmn wcm</i>)	→	cuscuses (<i>kmn atwak, kmn maygot</i>)
	Rofaifo	
small rats (<i>sogolobawe</i>) (unmarked)	→	small bandicoots (<i>warena hawa</i>)
large rats (<i>soka</i>) (unmarked)	→	large bandicoots (<i>warena ekiriva</i>)
prehensile-tailed rats (<i>hiongo heriwe</i>)	→	ring-tailed possums (<i>mi noi</i>)
prehensile-tailed rats (<i>hiongo anduaba</i>)	→	cuscuses (<i>igana</i>)
pygmy possums (<i>anumuna songi</i>)	→	ring-tailed possums (<i>mi</i>)
ring-tailed possums (<i>heufa, honingi heufa</i>)	→	cuscuses (<i>duana eniona, duana mula</i>)
small snakes (<i>sorafainya</i>)	→	amphibious lizards (<i>leme</i>)
	Nuaulu	
fruit fly larvae (<i>mumne</i>)	→	intestinal nematodes (<i>susue</i>)
intestinal nematodes (<i>sohane</i>)	→	tapeworms (<i>nikonake</i>)
frog taxon no. 1 (<i>notu anae</i>)	→	various frog species (<i>notu</i>)
various frog species (<i>notu</i>)	→	frog taxon no. 1 (<i>notu anae</i>)
various frog species (<i>notu</i>)	→	frog taxon no. 3 (<i>kako</i>)
frog taxon no. 4 (<i>poro-poro</i>)	→	frog taxon no. 5 (<i>inararai</i>)
water monitor lizards (<i>puo</i>)	→	geckos (<i>imasasae ai ukune</i>)
snakes (<i>tekene</i>)	→	agamid (or dragon) lizards (<i>kasa 'un</i>)
reticulated pythons (<i>teke patona</i>)	→	skinks, lizards, centipedes (<i>nopa ina, poso, niniane</i>)
sea turtles (<i>emu</i>)	→	amboinan box terrapins (<i>peku</i>)
hermit crabs (<i>kumake</i>)	→	coconut crabs (<i>katanopu sipu-sipu</i>)

amples of each. Alternatively, they proposed that, by linking animals that are subject to different dietary restrictions, the Kalam may be able to manipulate these restrictions through

“taxonomic casuistry” (Bulmer and Menzies, 1973:104). They expressed, however, their dissatisfaction with these explanations and concluded that the postulation of these transforma-

tions possibly is motivated by beliefs about which they were unaware (Bulmer and Menzies, 1973:104).

The majority of the species transformations proposed by the Rofaifo also involve rodents and marsupials (Table 2).¹¹ In his analysis of the Rofaifo's transformations, Dwyer (1976a) sharply criticized the explanations offered by Bulmer and Menzies for the transformations proposed by the Kalam. He noted that these transformations could not be motivated simply by the existence of similarities among the species involved because such similarities can be found among many other species. He also rejected the suggestion that these transformations emerged as a mechanism for dealing with dietary restrictions, arguing that such exploitation of these transformations presupposes the existence of the beliefs in them and thus cannot be used to explain these beliefs.

Dwyer attempted to explain the Rofaifo's postulated transformations by isolating certain trends that the transformations all display to varying degrees. Most significant to his analysis is the discovery that the animals that undergo transformation tend to be more abundant but less culturally significant than those that are the end result of transformation. He proposed that the Rofaifo postulate these transformations to resolve what might be called the "hunter's paradox," the fact that rare but culturally valued species continue to exist despite being "so constantly and capably culled by men" (Dwyer, 1976a:203). Yet, acknowledging that this hypothesis cannot account for all the transformations, Dwyer (1976a:203) resorted to a teleological argument, attributing the transformations as a whole to the human preoccupation with order. In the end, he failed to explain why the Rofaifo have involved these particular animals in transformations or to address the problem of why the Kalam and Rofaifo should propose transformations among many of the same kinds of animals.

In his impressive study of animal classification among the Nuauulu of the island of Seram, located to the west of New Guinea, Ellen (1993) evaluated the explanations of species transformations offered by Bulmer, Menzies, and Dwyer and found them wanting. He concurred with Dwyer's critique of Bulmer and Menzies but also characterized Dwyer's suggestion that transformations are postulated in part to resolve the hunter's paradox as a local, ad hoc explanation, not applicable to the Nuauulu (Ellen, 1993:163–171).

Although there is some overlap in the faunas of Seram and highland New Guinea (Ellen, 1993:14–15), the animals that the Nuauulu involve in species transformations are quite different and include no rodents or marsupials (Table 2). Ellen suggested that the Nuauulu postulate these transformations to explain the existence of similarities between certain kinds of animals and, more importantly, to establish linkages between animals that are peripheral to the taxonomic scheme and those that fit comfortably within it. The latter, called "focal taxa," are believed to transform into, and in a sense give rise to, the peripheral taxa so that "assertions of inter-species ontogeny may locally serve to

confirm convenient classificatory relationships of otherwise ambiguous and marginal creatures" (Ellen, 1993:171).

This explanation apparently accounts for most of the species transformations proposed by the Nuauulu, but there are some animals that seem to fit the conditions for inclusion in transformations that are not involved in them. For example, two species of land crabs exist in the Nuauulu area but only one is included in a transformation (from the hermit crab) (Ellen, 1993:165, 272). Although Ellen did not explain why such animals are not involved in transformations, his ideas have great merit because they suggest that the postulation of species transformations is closely tied to processes of classification. Most of the species transformations proposed by the Nuauulu involve animals that share a number of morphological features: worms to worms, crabs to crabs, frogs to frogs, lizards to geckos, snakes to lizards, and turtles to terrapins. By linking these animals through transformations, the Nuauulu reinforce the connections, based on other criteria, that exist between them.

Bulmer, Menzies, and Dwyer did not indicate if the animals that the Kalam and Rofaifo include in transformations are focal or peripheral taxa, but because these transformations link similar kinds of animals—primarily rodents and marsupials—Ellen's perspective probably is applicable to them as well. Both the Kalam and Rofaifo organize smaller rodents and marsupials into one higher-level category and larger rodents and marsupials into another, but they do not propose a more encompassing category that would include all rodents and marsupials.¹² In every case but one, the transformations they propose among rodents and marsupials involve small rodents or marsupials changing into large rodents or marsupials. The exception, which is found in both societies, is the transformation of one large marsupial (a ring-tailed possum) into another (a cuscus). These transformations thus bridge the taxonomic division separating large and small mammals and establish linkages between rodents and marsupials. The result is a strengthening of the ties among these mammals in the absence of a higher level taxonomic category that would consolidate them.¹³ As such, these transformations apparently play an organizational role within the Kalam and Rofaifo animal classification schemes comparable to that proposed by Ellen for the species transformations postulated by the Nuauulu.

With some adjustments, this explanation is also relevant to understanding the domesticated-to-wild transformations proposed by the Rarámuris because these transformations involve animals that are morphologically similar. Most of the maturation and old age transformations, however, link quite different kinds of animals (e.g., salamanders and pocket gophers, fish and otters, squirrels and snakes) and thus appear to require a different kind of explanation. As the point of departure for developing an explanation of these transformations, I evaluate the hypothesis, suggested by the work of Douglas (1957, 1970), that these animals are selected for such special treatment because they are classificatory anomalies.¹⁴

Rarámuri Animal Classification

CLASSIFICATORY ANOMALIES

To determine whether the animals involved in these species transformations are anomalous first requires a clarification of the concept “classificatory anomaly.” Douglas (1970:50) defined an anomaly as “an element which does not fit a given set or series,” and noted the affinity, in practical terms, between anomaly and ambiguity, the latter defined as “a character of statements capable of two interpretations.” Scholars who have focused their attention on folk biological taxonomies have arrived at a similar view in their formulation of the concept of “aberrant taxa” (Berlin et al., 1974:26, 30–31). Like all taxonomies, a folk biological taxonomy consists of a hierarchical organization of categories, or taxa. In their ideal model of such taxonomies, ethnobiologists propose five principal levels: unique beginner (an example from English is “animal”), life form (“mammal”), generic (“dog”), specific (“schnauzer”), and varietal (“giant schnauzer”) (Hunn, 1976, 1977:41–75). In terms of this model there are two kinds of aberrant taxa: unaffiliated taxa, which are generic taxa that are not incorporated into any of the life-form taxa, and ambiguous taxa, which are generic taxa that exhibit characteristics of two or more life-form classes. Because it is formulated in relation to taxonomic structures, the ethnobiologists’ concept of aberrant taxa is more precise and operational than Douglas’s concept of anomaly. Herein I define classificatory anomalies as these two kinds of aberrant taxa.

RARÁMURI ZOOLOGICAL TAXONOMY¹⁵

At the unique beginner level, the Rarámuri have a term—*namúti*—that means ‘animal(s)’, but often they use this same term in a more specific sense to denote ‘livestock’ and in a more general sense to mean ‘thing(s)’. Because this noun is polysemous, it is important to mention other evidence that supports the conclusion that the Rarámuri maintain a concept that closely corresponds to the English concept of “animal.”

To begin with, they incorporate the various animal species that they recognize into the category *ariwéame*, which literally means ‘soul-possessing’ and can be more freely translated as ‘living things’; the term *ahákame* also denotes ‘living things’. This category includes, in addition to animals, human beings (*rarámuri*)¹⁶ and plants (*aʷwiame*, literally ‘things that sprout’). They also employ some terms primarily in reference to animals, for example, *owira* ‘male’, *bamirá* ‘female’, and *nasawima*, a verb denoting sexual intercourse among animals. When these terms are used in reference to humans, they have somewhat vulgar connotations. Another, more tenuous bit of evidence for the conceptual reality of this category is the fact that Rarámuri people who speak Spanish employ the Spanish term for animal without making mistakes, whereas they misuse other terms in Spanish for which there are no comparable terms in the Rarámuri language, for example, some kinship and color terms.

Immediately below the level of “animal” are seven life-form classes. Four of these life-form classes are labeled: ‘birds’ (*chuluwi*), ‘fish’ (*rochí*), ‘snakes’ (*sinói*), and ‘lizards’ (*rochá*). The remaining three life-form classes are unlabeled (marked with “< >”): <mammals>, <amphibians>, and <invertebrates>.¹⁷ The Rarámuri incorporate all the animals with which they are familiar into at least one of these seven life-form classes.¹⁸ Thus, there are no unaffiliated taxa in the Rarámuri zoological taxonomy but there are three examples of ambiguous taxa. The first is the bat, which the Rarámuri usually classify as a ‘bird’, but they recognize the morphological features that it shares with mammals, especially its fur and teeth. The second is the otter, which they tend to think of as a <mammal>, but they often classify it with ‘fish’ because of its extraordinary swimming ability. The third is an unidentified lizard called *ropogópali*, which they classify as both a ‘snake’ and a ‘lizard’.¹⁹ The *ropogópali* shares many morphological features with lizards, including legs, but the Rarámuri consider it to be more closely related to snakes than to lizards. They comment that its tail is longer than those of other lizards and, more importantly, they believe that, unlike any other lizard in their area, it is venomous, defending itself by spitting its saliva, which causes the skin to swell.²⁰

Understanding Rarámuri Species Transformations

Of the 23 animals (i.e., named taxa) included in the eight sets of species transformations proposed by the Rarámuri, only two—the otter and bat—fit the definition of classificatory anomaly, and a third anomaly—the *ropogópali*—is not involved in a transformation. The Rarámuri’s postulation of these transformations thus cannot be explained as a way of dealing with such anomalies.

An alternative hypothesis is suggested by Hunn’s (1979) reanalysis of the dietary prohibitions of the ancient Hebrews. Hunn (1979:113–114; cf. Ellen, 1993:183) argued that the animals prohibited as food by the ancient Hebrews stand out because they display “empirically infrequent trait complexes,” the most famous example being the pig, which, unlike most cloven-hoofed mammals, does not chew the cud. He referred to such animals as “singular,” but because the zoological taxonomic scheme of the ancient Hebrews has never been reconstructed, he did not consider whether these singular animals were also taxonomic anomalies in the sense of being aberrant taxa.

If Hunn’s notion of singularity is applied to the Rarámuri case, all the animals that are the end products of the maturation and old age transformations are, from the Rarámuri’s perspective, singular. The bat is singular with respect to birds because it has fur and teeth, and it is singular with respect to mammals because it flies. The mammalian features of the otter set it apart from fish, and both it and the pocket gopher are singular among mammals because their typical domains—aquatic for the otter and subterranean for the pocket gopher—differ from the aboveground habitat of the other thirty-plus taxa included in

the <mammal> life-form class. Similarly, the large rattlesnake and large constrictor are singular because they are the only snakes considered edible by everyone.²¹

These examples allow a clarification of the relationship between the concepts of singularity and taxonomic aberrancy. By definition, taxonomic aberrancy exists only in terms of a particular taxonomic scheme, whereas singularity in Hunn's sense is a feature of the biological world, which exists independently of any particular taxonomic scheme. Obviously the recognition of singularity and the significance attached to it are culturally conditioned, but the features that render an animal singular may be irrelevant to the criteria used to classify it within a taxonomic system. In this regard, it is crucial to distinguish between singularity and uniqueness. All named taxa are unique in some way or they would not be named. For an animal to be singular, it must be unique with respect to features that characterize all or most of the other members of the class or classes with which it is most closely associated.

From this perspective, it can be proposed that all aberrant taxa will be singular but not all singular animals will be taxonomically aberrant. In other words, all animals that are regarded as members of two or more categories or that are not affiliated with any other categories will be singular, but the singularity of a particular animal may not result in its being incorporated into two or more separate categories or placed in an unaffiliated category. In the Rarámuri case, the bat and otter are singular and also taxonomically ambiguous because the features that set them apart from one life-form class are typical of another. In contrast, the pocket gopher and snakes are singular but are not taxonomically ambiguous because the pocket gopher's habitat and the edibility of the two snakes are not defining features of any other life-form classes.

On the basis of this analysis, we can conclude that the Rarámuri propose that these five animals are the end results of transformations in order to explain why they display features that make them singular with respect to their life-form classes. By this logic, they should also propose that *ropogópali* is the end result of a transformation, either deriving from a snake to explain why a 'lizard' is venomous or, more likely, deriving from a lizard to explain why a 'snake' has legs. That the Rarámuri fail to incorporate the *ropogópali* into a transformation is puzzling, and I can offer only ad hoc suggestions to account for this failure. Perhaps the Rarámuri do not involve the *ropogópali* in a transformation because, except for its legs, it is quite similar to snakes and is believed to be venomous, a highly salient although not universal feature of snakes that the Rarámuri regard as one of the principal characteristics distinguishing snakes from lizards. Its presumed venomness and its morphological similarities to snakes perhaps render the fact that it has legs sufficiently insignificant to the Rarámuri that they feel that it does not warrant explanation. An alternative suggestion, which I find more satisfactory, is that the *ropogópali* is a prime candidate for inclusion in

transformations that might be proposed in the future. I develop this idea in more detail below.

In contrast to the animals that are proposed as the end products of the maturation and old-age transformations, none of the animals that are said to transform appears to be singular. This conclusion is supported by the fact that, in the case of the bat and otter, several different animals are proposed to transform into them. Such multiple origins suggest that the choice of which animals will undergo transformation is not significant as long as these animals display the same features that make the end products of transformation singular. Nonetheless, secondary considerations render certain animals more likely candidates for transformation than others. Small rodents are more similar to bats than are other mammals, and the fish said to transform into otters are the largest ones in the area and live in the larger streams where otters are found; the catfish's barbels also echo the otter's whiskers. Similarly, the salamander is appropriate as an immature pocket gopher because the habitats of these animals overlap and because the Rarámuri assume that the salamander is a larval stage, more like tadpoles than like other mature amphibians.

Because edibility is a characteristic of so many animals, it is not clear why squirrels should be the animals selected to transform into snakes. The Rarámuri's rationalization for this choice is that squirrels have elongated bodies, their meat is highly regarded as food, and their domains overlap with those of the constrictor and the large rattlesnake: the constrictor climbs trees like a squirrel, whereas the rock squirrel never does, living among rocks and in caves like a rattlesnake.²² These snakes also prey on these squirrels, but they eat many other animals as well. Nonetheless, it would appear that lizards would be more appropriate than squirrels to transform into these snakes because some lizards are edible and they are much more similar to snakes than are squirrels. Perhaps the choice of squirrels over lizards in this case reflects an attempt to maintain a clear distinction between the closely related life-form classes of 'lizards' and 'snakes', a distinction that would be blurred if lizards were said to transform into snakes. The fact that the Rarámuri classify the *ropogópali* as a snake without proposing a transformation to explain why it looks like a lizard suggests that some sort of classificatory boundary maintenance is at work here, in addition to whatever survival value classifying all poisonous reptiles in a single category might have.²³ This suggestion, like that offered to account for why the Rarámuri do not involve the *ropogópali* in a transformation, is ad hoc. It is impossible, however, to evaluate it further because no other life-form classes within the Rarámuri's zoological taxonomic system are as closely related to one another as 'snakes' and 'lizards'.

In the case of the bat and otter—the only taxa involved in transformations that are linked to two separate life-form classes—an examination of the animals that are said to transform into them provides some insight into the life-form class with which the Rarámuri consider each of them to be more

closely associated. The transformation of small rodents into bats suggests that the Rarámuris think of bats primarily as birds rather than as mammals. If they regarded the bats as mammals, they would have used transformation to explain why a mammal can fly, presumably by proposing that it derives from one or more species of birds, such as the barn swallow, which they consider to be very similar to bats in dietary preferences, time of activity, and style of flight. In contrast, they appear to regard otters more as mammals than as fish because the transformation of fish into otters explains why a mammal is such an adept swimmer, not why a fish has mammalian features.

The only animals involved in transformations that remain to be discussed are goats, pigs, and house cats, which change into deer, peccaries, and ring-tailed cats, respectively, if they move to the wilds (*kawichi*) to live. None of these animals is singular with respect to the defining criteria of the life-form class <mammals>. The domesticated animals that undergo transformations, however, share two features that, when taken together, distinguish them from all other mammals: they all have close equivalents among wild mammals and all were introduced to the region during the Spanish colonial period.

I suggest that the Rarámuris involve these introduced animals in transformations today because this is the manner in which they dealt intellectually with these animals when they first encountered them 400 years ago. This historical perspective differs from that of the contemporary Rarámuris themselves, who do not consider any animals to be introductions, but it does account for why only certain animals are included in these transformations.²⁴ Of the other domesticated animals that the Rarámuris own today, those that were introduced—horses, mules, burros, cattle, sheep, and chickens—do not have wild equivalents, and dogs, which do have a wild equivalent in the coyote (*basachi*), were not introduced. Turkeys are also indigenous to the New World. The Rarámuris occasionally capture wild turkeys and bring them to their homes to tame, and they also acquire domesticated turkeys through purchase or trade (cf. Pennington, 1963:85–86). They acknowledge the possibility that domesticated turkeys can escape to the wild, but they do not propose that they transform into wild turkeys, probably because they consider both wild and domesticated turkeys to be the same “kind” of bird, which they label with a single term, *chiwí*.

Rarámuri Species Transformations in Broader Context

The animals that are the end results of maturation and old age transformations stand out primarily because the life-form classes with which they are associated—<mammals>, ‘birds’, ‘fish’, and ‘snakes’—are homogeneous in the sense that their members share a number of morphological, ecological, or behavioral features (Hunn, 1977:48–50). In this regard, these life-form classes contrast with more heterogeneous ones, like <amphibians> and <invertebrates>, none of the members of which are proposed as the end products of transformations. Yet the

ropogópali is not involved in a species transformation even though it is singular with respect to two homogenous life-form classes: ‘snakes’ (because it has legs) and ‘lizards’ (because it is believed to be venomous). I suggested that the Rarámuris do not involve the *ropogópali* in a transformation because it shares so many characteristics with snakes, including the highly salient feature of being venomous, and also possibly because proposing that the *ropogópali* derives through transformation from either a snake or a lizard would obscure the boundary between these two life-form classes. Nonetheless, it is equally plausible to regard the *ropogópali* as a “transformation-in-waiting,” the most likely candidate for inclusion in a transformation at some later time.

These two perspectives on the *ropogópali* are not mutually exclusive. The first attempts to account for how the Rarámuris have, in the present, addressed the taxonomic dilemma presented by the *ropogópali*, whereas the second suggests how they might address this problem and develop their thinking on the topic in the future. Like all bodies of knowledge, Rarámuri ideas about animals are not static or finished constructions that can be explained exhaustively at any given point in time. Instead they are works-in-progress, subject to revision and elaboration through the intellectual activities of human actors, who develop their ideas in terms of their experiences of the world according to culturally specific principles of knowledge creation and organization as well as more general (and presumably universal) mental processes.

The dynamism of such knowledge is exemplified by the fact that some people in Rejogochi suggested a few species transformations in addition to the widely accepted transformations discussed above, for example, that rabbits transform into a small variety of rattlesnake and other fish transform into otters. Many also proposed some transformations of humans into animals in the ancient past that other people did not (see below). It was evident from my conversations with these people that they had thought a great deal about these transformations, had reflected on the principles governing their postulation, and had selected animals to be involved in additional transformations on the basis of this understanding.²⁵ Presumably they will continue to do so.

The alternative perspectives on the *ropogópali* that I have suggested are based on the assumption that, although many aspects or areas of knowledge lend themselves to synchronic analysis, others can only be understood diachronically. This assumption also underlies my proposal that the Rarámuris dealt intellectually with certain domesticated mammals introduced during the Spanish colonial period by linking them through transformations to the indigenous animals that they most closely resembled. This explanation resembles Ellen’s (1993:171) proposal that the Nuauulu postulate species transformations primarily to create linkages between peripheral and focal taxa. The historical record indicates, however, that the Rarámuris rapidly incorporated Old World domesticated mammals into their economies and presumably into their zoological

taxonomy as well (Merrill, 1988:44–45, 1993),²⁶ and today these animals are in no way regarded as peripheral or singular. The possibility of their transformation into wild animals exists today not as a way of resolving their persisting marginality but as one of many markers of the dichotomy between the human and nonhuman domains of the world and a commentary on the implications of moving between them.²⁷

The contemporary Rarámuris link focal and peripheral taxa simply by noting that they are similar and by incorporating both into more inclusive categories. For example, they regard horned lizards (*wikókere*) as distinct from other lizards, but they also say that they are ‘relatives’ (*nahirémaga*; singular: *rihimára*) and include horned lizards in the life-form category ‘lizards’ (*rochá*). The Rofaifo adopt this same approach (Dwyer, 1976b, 1979), but, together with the Nuauulu and Kalam, they also employ species transformations to establish connections among similar animals that are separated taxonomically.²⁸

The Rarámuris differ most significantly from the Nuauulu, Kalam, and Rofaifo in their use of transformation as an explanatory device. The Nuauulu, Kalam, and Rofaifo invoke transformations to explain similarities between peripheral and focal taxa, whereas the Rarámuris propose transformations—at least maturation and old age transformations—to explain why certain singular taxa are different from the other members of the life-form classes with which they are most closely associated. Unlike the Rarámuris, the Nuauulu, Kalam, and possibly the Rofaifo tend to explain the existence of such singular taxa primarily through myth (Ellen, 1972, 1993:184; Bulmer, 1967, 1968; Dwyer, 1979:17).²⁹

Apart from the role that these species transformations play in the Rarámuris’ approach to organizing the animal world, many are linked to important cosmological concepts or moral values. The otter, for example, is criticized for being a cannibal because it eats its ‘relatives’, the fish. Such ideological significance is also evident in many of the transformations of humans into animals that the Rarámuris propose. I recorded over 20 of these human-to-animal transformations, which are of two types: those that occurred in the ancient past and those that occur today, after death. In both cases, these transformations are associated with a time (the beginning of the present world) or a sphere (the world of the dead) within which form is regarded as more fluid.

The ancient human-to-animal transformations that most people mention are those reported in well-known stories or that are suggested by the fact that the name of an animal coincides with a kinship term. For example, because *ochikare* means both ‘grasshopper’ and ‘paternal grandfather’ people assume that in the ancient past a human being, who was a paternal grandfather, transformed into a grasshopper. More variation is encountered where such transformations are believed to have occurred simply because the behavior of an animal is perceived to be similar to that of humans. In some of these transformations, humans changed into animals while they were alive, in others they were reincarnated as animals. The Rarámuris often at-

tribute the origin of these animals to such transformations, but they also maintain that all animals as well as humans were created by God or by the Devil near the beginning of the world. Animals seen as industrious and of benefit (or at least harmless) to humans are believed to have been created, like the Rarámuris and other Indians, by God. Those that threaten humans or that display features interpreted as moral defects are assumed to have been created, like non-Indians, by the Devil (Merrill, 1988:73–78).

The contemporary transformation of humans into animals is believed to occur only after a person dies and is regarded as a punishment for a person’s misdeeds. People who commit incest or who ally themselves with peyote during their lives or who die while lying on an animal skin usually are said to transform into a special kind of coyote, called ‘short-tailed coyote’ (*basachi póchi*), the human origin of which is revealed not only by its short tail but by its skill at entering residential areas undetected to steal livestock.³⁰ When the requisite series of death rituals are performed for these people, the animals into which they transform die and their souls are released. This destiny contrasts with an alternative fate believed by some to befall incestuous people. One or more souls of such people transform into a special kind of moth, called ‘soul moth’ or ‘ancestor moth’ (*nakarówili ariwá* or *nakarówili anayáwari*), which God sends from heaven back to earth to die in the fires of the living, to which these moths are irresistibly attracted.³¹

This interpretation of the behavior of these moths reveals an important epistemological dimension to the Rarámuris’ ideas about animals. They frequently regard such ideas as providing empirical confirmation of basic propositions about the nature of the universe and their place within it, propositions that are nonverifiable and nonfalsifiable (Rappaport, 1979:117, 209–210). Such confirmation is particularly evident in the accounts that the Rarámuris relate to explain distinctive characteristics of certain animals.³²

The Rarámuris have a story that might be titled “Why the Burro Has a Short Tail,” in which the Rarámuris’ chief deities sent a flood to destroy the world that preceded the present one. All the different kinds of animals in the world assembled on top of a cylindrical-shaped mountain to escape the boiling waters of the flood, but the burro arrived just as the mountain began to rise to heaven and, because of its tardiness, about half of its long tail was burned off.

In addition to providing an entertaining explanation for why the burro’s tail is shorter than those of the horse and the mule, this story proposes that the shortness of its tail substantiates an event—the destruction of the previous world—that no living person witnessed. The assumption that the current world is but the most recent in a series of worlds is linked to fundamental ideas in Rarámuri philosophy and religion about the cyclical nature of time, the magnitude of the power of their deities, and the implications for their continued survival if they fail to fulfill their ritual obligations towards these deities. These ideas in turn structure and motivate much of Rarámuri social action.

Such accounts, found throughout the world, have always struck me as much too elaborate to have been developed simply to explain the special features of certain animals, but their complexity is comprehensible if they are understood as exploiting these features to confirm basic assumptions about the universe.

In a broader cosmological perspective, the Rarámuris regard transformations as part of the natural course of things and as an important aspect of the development of the current state of affairs. At the same time, they tend to associate all transformations—ancient and modern, humans-to-animals and animals-to-animals—with the Devil, and some maintain that the transformation of humans into animals in the ancient past was possible only because the people involved had not yet been baptized. Today the members of those Rarámuri communities who have accepted an affiliation with the Roman Catholic Church refer to themselves as ‘baptized Rarámuri’ (*Rarámuri pagótame*) and employ baptism as a symbol of and metaphor for order, regarding a nonbaptized state and the Rarámuris who have historically rejected such an affiliation as epitomizing disorder (Merrill, 1988:76–77, 1993). Their views on these matters echo the Spanish colonial perspective that conversion to Christianity and incorporation into the colonial system were prerequisites for the creation of order in indigenous New World societies. That the contemporary baptized Rarámuris should maintain these alternative and rather contradictory views on transformation reveals how thoroughly intertwined indigenous and colonialist ideas have become in their thinking.

Conclusions

Proposing that one kind of animal transforms into another is an indirect but vivid way of stating that the two are closely connected. The postulation of such transformations thus should not occur randomly within a zoological classification system but only where people want to suggest or emphasize a special affinity between certain animals. Within this restricted range of appropriate applicability, species transformations can be used as a conceptual tool to accomplish many different things.

If my analysis is correct, the Rarámuris employ species transformations primarily to account for why certain animals should deviate from patterns that characterize the higher-level taxonomic classes with which they are most closely associated. As such, the postulation of species transformations serves as an adjunct to taxonomic classification, reinforcing taxonomic boundaries by explaining (or explaining away) challenges to them (cf. Douglas, 1970). In contrast, the Nuaulu, and probably the Kalam and Rofaifo as well, use species transformations to establish horizontal linkages between taxa where a taxonomic scheme of hierarchically ordered sets of increasingly inclusive classes is poorly developed. Here species transformations appear to play a role that is more organizational than explanatory, serving as an alternative rather than an adjunct to taxonomic classification.

Given the diversity of the biological world, peripheral or singular taxa probably will be found in the biological classifications schemes of all human societies, but species transformations apparently are encountered only in some. Such cross-cultural variation presumably reflects, among many other things, differences in approaches to organizing the biological world and in the extent to which peripheral or singular taxa are regarded as problematical. Alternative solutions to the same problem also exist. The Rarámuris deal with singular taxa by involving them in species transformations, whereas the members of other societies explain their singularity in myths or compartmentalize them through dietary prohibitions or other ritual restrictions. Others simply ignore them. These variations in turn reflect differences in their relationships to the biological world and in their fundamental attitudes toward order and disorder.

Throughout the centuries, people around the world have entertained the idea that one kind of being can transform into another, but most have relegated this notion to the realm of fiction or folklore. In such cases, we can say that they have a concept of species transformation but not the belief that species transformations actually occur as part of an ongoing biological reality (Ellen, 1993:166). Only where people assume that the world they experience during their waking lives represents only one dimension of reality, that appearances can be deceiving, and that the universe as a whole is characterized more by fluidity than by stasis is a belief in the reality of species transformations likely to be found.

Involving animals in species transformations is just one of the many ways in which people in different societies have afforded animals special treatment. Animals figure prominently in myths, provide apt metaphors and the weapons of verbal abuse, and serve as markers of social distinctions. The usefulness of animals as intellectual tools has led Claude Lévi-Strauss and others to observe that animals are good to think, good to eat, and good to prohibit (Lévi-Strauss, 1963, 1966; Leach, 1964; Tambiah, 1969; Bulmer, 1979). To this litany I would add that animals are also fun to think. People everywhere derive great pleasure from watching, talking, and thinking about animals; the Rarámuris are no exception. They regard animals as interesting, at times puzzling, and more often than not humorous, and they consider the possibility of the transformation of one kind of animal into another as an intriguing aspect of the animal world. As a result, their postulation of species transformations should not be reduced to a mechanism that only enhances order within their understanding of the animal world. Indeed, by proposing these transformations, the Rarámuris not only explain the singularity of certain animals but simultaneously render them even more singular, involving them in a process that sets them apart from all other animals.

A society's ideas about animals often provide a privileged window through which to view the basic values of its members and their understandings of their society, their history, and their cosmos (Willis, 1974). Unfortunately, over the past two de-

cedes anthropological interest in studying such ideas has waned. A dedicated but small group of scholars has continued such research, but most anthropologists have shifted their attention to other topics. Like many other disciplines, anthropology tends to focus intensely but briefly on one topic or theory, soon abandoning it to move on to the next. One result, probably inevitable, is an uneven ethnographic record. Another, more regrettable consequence is the growing expanses of theoretical issues only partially explored. The prospect that anthropology will adopt a more systematic, sustained approach anytime soon appears remote, but the discipline will continue to contribute to a more profound understanding of humanity as long as its practitioners maintain a firm commitment to the standards of good scholarship. This is the lesson that Bill Sturtevant taught me in 1972 and one that he has applied unflinchingly in his own work.

Notes

I prepared the original version of this essay in 1979 as an oral presentation delivered to the Department of Anthropology, Smithsonian Institution, during the course of my interview for the position I currently hold. Michael Brown, Richard Ford, Charles Hudson, Raymond Kelly, and Cecilia Troop read the written version of this presentation and offered suggestions for improving it. Jonathan Amith, Michael Brown, Don Burgess, Roy Ellen, Ives Goddard, Eugene Hunn, and Cecilia Troop did the same for the revised version. Their comments have been of invaluable assistance to me in preparing the final version. I am also grateful to Cecilia Troop for working on this project with me and to the residents of the Rejogochi area, especially Candelario Martínez, Agustín Durán, Chonel Pérez, and Moreno Sahuárare, for sharing their knowledge of animals with us. I also thank Don Burgess, Miguel Carrillo, and Ventura Orozco for providing information on Rarámuri zoology from other communities; George Zug for his guidance through the herpetological literature; and Ana Silva for her assistance in organizing our fieldnotes. Finally, I would like to acknowledge the University of Michigan, the National Institute of Mental Health, the Wenner-Gren Foundation for Anthropological Research, and the Smithsonian Institution for providing the financial support that allowed me to undertake this research.

1. I began this project in 1971 as part of my senior honors thesis at the University of North Carolina under the direction of Frederick McEvoy and Richard Yarnell. I developed the section on black drink in collaboration with Charles Hudson (Cable et al., 1971; Hudson, 1979; Merrill, 1979), who was instrumental in my receiving a Smithsonian internship and who introduced me to the concept of classificatory anomalies through his own work on Southeastern Indian ethnology (see Hudson, 1976:139–149, 318, 1978).
2. For information on the Rarámuri of Rejogochi, see Merrill, 1988; Hard and Merrill, 1992; and Graham, 1994.
3. Ellen (1993:22–34, 93–125) provided a well-reasoned overview of the methodological problems associated with documenting and analyzing zoological classification systems and the debates that have surrounded them. In our case, we began by recording comments about animals made by different people and then inquired in more detail about specific animals during both informal conversations and open-ended interviews. When we had compiled what appeared to be an exhaustive list of Rarámuri animal names—over 170 terms in all—we copied each of them onto an index card. We then asked two men—both in their twenties, literate, and bilingual in Rarámuri and Spanish—to organize these cards into groups. Their responses provided the basis for the model of Rarámuri zoological taxonomy that I present herein. We attempted the same exercise with a man in his fifties, a nonliterate, monolingual speaker of Rarámuri. It was very difficult for him to keep track of so many cards without the benefit of literacy, so we dropped the card-sorting exercise and simply discussed relationships among animals and groups of animals with him. We also

talked about the local fauna with lifelong, non-Indian residents of the area to learn Spanish common names and to compare their ideas about animals with those of the Rarámuri. Because I did not originally envision that my project would involve ethnobiological research, I did not secure a permit from the Mexican government to collect zoological specimens and thus cannot provide scientific identifications of the species involved. The obvious limitations of our ethnozoological research are made more apparent when our research is compared to the thorough studies completed elsewhere (e.g., Hunn, 1979; Taylor, 1990; Ellen, 1993).

4. Rarámuri in other communities propose some of these same species transformations: fish into otters, small rodents into bats, salamanders into pocket gophers, and rock squirrels into snakes (Lumholtz, 1902:309; Thord-Gray, 1955:420, 518–524, 961, 1034; Brambila [1980]: entries for *bahuri* and *so'pechi*; Burgess, 1985:97, 119, 142). In an account recorded by Burgess (1985:142), rattlesnakes are said to transform into rock squirrels rather than the reverse. Lumholtz (1902:436) indicated that around the turn of the century the Rarámuri's neighbors, the Northern Tepehuan, also proposed the transformation of catfish into otters, one kind of squirrel into a bat, another kind of squirrel into a parrot, rock squirrels into serpents, and certain insect larvae into doves.

5. Rarámuri people who live to the west, in the area known as the Tarahumara Baja, rely primarily on the verb form *gayéna* 'to transform, become, arrive' when discussing these animal transformations. They tend to use the form *nahita* when the entity in question is in the process of transforming and to use *so'péta* when it is about to do so; *so'péta* is especially associated with old age transformations. When referring to readily observable metamorphoses, like that of tadpoles into frogs, some people prefer to use the form *ochéera* 'to grow, mature, age' instead of *gayéna* (Don Burgess, pers. comm., 14 Nov 1997). I did not encounter these usages in Rejogochi.

6. During the Spanish colonial period, missionaries occasionally reported that Rarámuri "witches" could change into animals at will (e.g., Tardá and Guadalaxara, 1676:362). Although this idea is found in many societies, it is impossible to determine whether its attribution to the colonial Rarámuri is accurate or is simply another example of missionary efforts to discredit Rarámuri ritual specialists. Today the Rarámuri of Rejogochi deny that sorcerers (*sukuriáme*) can change into animals, although they firmly believe that sorcerers exist and that certain kinds of animals, particularly large birds called *oromá*, identified with shooting stars, assist them in their malevolent work (Merrill, 1988:75).

7. The Rarámuri of Rejogochi propose no cases of animals transforming into humans, but stories of such transformations, as well as transformations of humans into animals, are found in other Rarámuri communities (Mares Trias, 1975; Burgess, 1985). I encountered only one example of an animal-to-animal transformation taking place in the ancient past: one man proposed that a dog changed into a hare in order to account for why the hare has a black tail. Similarly, the only animal that is believed to undergo reincarnation today is the dog, which is said by many people to come back as a "furry" brown caterpillar. Another kind of transformation, involving humans only, occurs when a man or woman goes to a special mountain and touches articles associated with the opposite sex that are found there. Thereafter, the person switches sex (in both genitalia and orientation) each month. There are two such mountains in the Rejogochi area, which people say came into existence in the ancient past when God decapitated a Rarámuri man. The man's head became one of these mountains, his stomach the other.

8. Many people in Rejogochi also believe that the germs of maize kernels sometimes transform into moths (Merrill, 1988:92). This belief arises from the fact that the moths deposit their eggs in maize kernels and the larvae consume the germs. Taylor (1990:73–74) noted a few examples of the transformation of plants into animals proposed by the Tobelo of Halmahera Island, Indonesia.

9. The association between old age and transformation is also encountered in other areas of Rarámuri thought. For example, the Rarámuri state that the world itself ages and that when it becomes old, the normal state of affairs will be reversed: livestock will eat humans, objects will come to life and attack their owners, and so forth. The old world will then be replaced by a new one, at which point the possibilities for transformation will increase dramatically.

10. When I asked if sheep transform into wild animals, one man speculated that if they did they would become bears because both animals are “woolly.” This transformation is not proposed by anyone, probably because sheep and bears share few other characteristics and because the Rarámuris regard sheep as the epitome of dumb animals, entirely dependent on humans for their survival. They assume that sheep would not survive long enough in the wild to undergo transformation.

11. Both the Kalam and Rofaifo propose the transformation of females and immature males of various species of birds of paradise into adult males of the same or different species. The Kalam also propose a sequential transformation involving certain kinds of worms, snakes, and eels (Bulmer, 1968; Dwyer, 1976a). Because these transformations are based on a misunderstanding of the reproductive cycles of the animals involved, I do not consider them here. I also have not included in Table 2 two sequential transformations that the Rofaifo propose, although they recognize or believe that most of the animals involved breed true: (1) beetle larvae (*foua*) → butterflies and/or moths (*ongombila*) → bats (*litimbi*); and (2) very small rats (*sogolobawe hunahimi*) → small rats (*sogolobawe*) → medium-sized rats (*soka-sogolobawe*) → large rats (*soka*) → largest large rats (*soka hendea*) → giant rats (*fuema lolamba*) → largest giant rats (*fuema angaia*).

12. The Kalam classify large marsupials and rodents in the category *kmm*, which they distinguish from small marsupials and rodents, classified along with frogs in the category *as*. They maintain separate categories for dogs, pigs, and house and garden rats (Bulmer and Menzies, 1973). Similarly, the Rofaifo classify small rodents and marsupials as *hunembe* and classify larger mammals as *hefa*, a class that includes pigs and dogs and, in an extended sense, also casowaries (flightless birds) and eels (Dwyer, 1976b).

13. According to Dwyer (1979:16–17), the Rofaifo are much more inclined than are the Kalam to postulate higher-level categories, but they do not organize mammals into a single category.

14. Douglas's ideas have stimulated a number of studies of the significance of classificatory anomalies in diverse cultural settings, most of which have suggested that her original perspective should be revised (Tambiah, 1969; Willis, 1974; Sperber, 1975; Hunn, 1979; Ellen, 1993:179–184).

15. Taxonomic classification based on set inclusion is one of three procedures that the Rarámuris use to organize the animal world. They also create chains of linked pairs of taxa connected horizontally on the basis of isolated features they share, for example, color, or an aquatic habitat. These features vary from one dyad to another, for example, a bird and a snake might be linked because both are red and the same snake might be linked to a specific insect because both are associated with water. Because any feature can be used to link two taxa, and this chaining procedure in and of itself generates no higher-level categories, neither unaffiliated nor ambiguous taxa are associated with it. Another process involves organizing zoological taxa into categories that crosscut the entire taxonomic classification scheme. These categories tend to be quite general (e.g., edible versus nonedible), and each set of them exhausts the universe of possibilities. Because this cross-cutting procedure is applied to the entire set of zoological taxa, there are by definition no unaffiliated taxa, but some taxa can fall between the categories, for example, animals that are eaten by some people but considered inedible by most.

16. The term *rarámuri* has meanings on several different levels of increasing specificity: ‘human beings’ as opposed to ‘non-humans’, ‘Indians’ as opposed to ‘non-Indians’, ‘the Rarámuri proper’ as opposed to the members of other Indian societies, and ‘men’ as opposed to ‘women’ (Merrill, 1988:78).

17. The names I assign to these unlabeled classes represent what I consider to be the English terms that best describe the animals included in each of them by the Rarámuri men who completed the card-sorting exercise described in note 3. The psychological reality of such covert categories has been thoroughly debated (see Taylor (1990:42–51) and Ellen (1993:93–125) for summaries). I am confident that, with the exception of the ambiguous taxa discussed in the text, the Rarámuris think of the animals included in each of the unlabeled life-form classes <mammals> and <amphibians> as a set, more closely related to one another than to the members of other life-form classes, and they point to features that distinguish the members of each of these two unlabeled classes from those of all other life-form classes. In the case of <mammals>, these features are pri-

marily the fact that all have fur and are quadrupeds. In the case of <amphibians>, which includes frogs, toads, and their larval forms, they emphasize the morphological similarities of the adult forms and their unusual reproductive cycle. In contrast, the unlabeled life-form class <invertebrates> may exist only as a by-product of the card-sorting exercise, emerging as a catch-all category for several lower-level classes and unaffiliated taxa that did not fit into any of the other life-form classes.

18. The Rarámuris regard the horned toad (*wikókere*) and the earthworm (*sa?i*) to be somewhat atypical of their life-form classes (‘lizards’ and <invertebrates>, respectively), but they apparently do not consider these animals sufficiently distinct as to require their placement in separate, unaffiliated categories. Water monsters (*walúluwi*) and rainbows (*konomi*), both regarded as extremely dangerous to humans, are classified as ‘snakes’ (*sinói*), even though they are said to be able to shift between serpent and human forms (Lumholtz, 1902:310; Bennett and Zingg, 1935:324–325; Burgess, 1985:103, 133, 140; Merrill, 1988:73–74).

19. It is possible that the term *chuluwi* is better glossed as ‘birds plus bats’, a grouping found in other parts of the world (Bulmer, 1967:7; Dwyer, 1976a:188; Bulmer, 1979:61; Kesby, 1979:43), and the term *sinói* glossed as ‘venomous reptiles and snakes’. Expanding the semantic scope of these life-form labels would incorporate the bat and *ropogópali* more comfortably into these classes, but these animals would remain ambiguous because both are associated with two life-form classes.

20. During a visit in May of 1997 to the capital city of Chihuahua, I asked Ventura Orozco, a Rarámuri man living there, if he knew about a lizard that spits venom. He immediately gave the name *pakókke* (a form related to *ropogópali*) and, on the basis of photographs in *The Audubon Society Field Guide to North American Reptiles and Amphibians*, identified it as the Arizona alligator lizard (*Gerrhonotus kingi*) (Behler and King, 1979, pl. 446 and p. 541; some herpetologists classify *G. kingi* (or *G. kingii*) as *Elgaria kingi* Gray (Flores-Villela, 1993:22, 67)). I did not have the opportunity to check this identification with the residents of Rejogochi, but Mr. Orozco is originally from the Rarámuri community of Samachique, located about 20 km south of Rejogochi. His identification is supported by information provided to me, also in 1997, by Don Burgess, a linguist. Mr. Burgess told me that Rarámuri people who live farther west also report the existence of a lizard that spits venom (which they call *gu?ógali*, *ko?ógali*, or variants of these terms), and that a Rarámuri woman from this area showed him open sores on her body that she said had been caused by this lizard. He has seen this lizard and noted that it closely resembles the Arizona alligator lizard illustrated in *A Field Guide to Western Reptiles and Amphibians* (Stebbins, 1966, pl. 25). He suggested that this lizard may excrete venom from its skin rather than spitting it.

Non-Indians in the vicinity of Rejogochi concur with the Rarámuris that the *ropogópali* spits venom. They refer to it as *escupión* (‘spitter’), or *escorpión*, the latter term used elsewhere to designate the scorpion, which is known in local Spanish as *alacrán* and in Rarámuri as *ma?chiri*. Other published sources on the Rarámuris also report spitting, venomous lizards, called *guapahócare* and *kojáwari* (or *kujáwari*); a third spitting lizard, called *ropogókuri*, is said to be harmless (Brambila [1980]; Wheeler, 1993:47). Thord-Gray (1955:220, 972) identified, probably incorrectly, the “ro-pa-go-go-ri” as the horned toad and the “ko-hawa-ri” as the scorpion (in this case, possibly confusing the local Spanish common name for the “spitting” lizard with the term for “scorpion”).

Herpetologists recognize only two venomous lizards in the world, neither of which spits its venom: the Gila monster and the Mexican beaded lizard. Both are members of the same genus (*Heloderma*) and are reported from northern Mexico but outside the Rarámuri area (Bogert and Martín del Campo, 1956; McCranie and Wilson, 1987; George Zug, pers. comm., 23 Jan 1997). Although none are known to be venomous, alligator lizards display several snake-like features, including elongation and lateral undulation (Kelly Zamudio, pers. comm., 11 Dec 1997). Assuming that the identification of the *ropogópali* as an alligator lizard is correct, such features may account for the Rarámuris' association of it with snakes.

21. Among the Rarámuris, taxonomic status is unrelated to dietary prohibitions. Instead of taxonomic singularity motivating dietary prohibitions, here edibility renders these snakes singular. One man claimed that, in addition to the

sayáwi and *bahomáwari*, two other snakes are edible: a small rattlesnake (*chopésini*) and a constrictor (*rinólowi*), which is similar in length but is thinner than the *bahomáwari*, with quite distinct markings. Other people say that the small rattlesnake is not eaten because it has little meat and that the meat of the *rinólowi* cannot be eaten because doing so will make a person sick. This same man differed from everyone else I consulted by reporting that the larger constrictor (*bahomáwari*) is not involved in transformations but that the two rattlesnakes (*sayáwi* and *chopésini*) both change into rock squirrels rather than only the larger rattlesnake deriving from the rock squirrel. The *bahomáwari* and *rinólowi* are the only two constrictors found in the Rejogochi area. In the canyon country to the southwest, the Rarámuris keep a constrictor called *niwi* in their fields and near springs to kill squirrels and other pests that raid their fields (Miguel Carrillo, pers. comm., 26 Apr 1995).

22. The Rarámuris have two named taxa of tree squirrels, the labels for which can be translated as 'red tree squirrel' (*chimori sitákame* or *chimori láchame*) and 'gray tree squirrel' (*chimori siyóname*). Some people also report a third taxon, the 'white tree squirrel' (*chimori rosákame*), which they closely associate with the 'gray tree squirrel' and which perhaps is a subspecies of it. Only 'red tree squirrels' are said to transform into the large constrictors, probably because they tend to be found in the same areas as the constrictor. 'Gray tree squirrels' are said to live only at higher elevations.

23. The life-form label *sinói* 'snake(s)' is also used metaphorically to refer to insects that sting.

24. Of course, they recognize that the new breeds of livestock they acquire are introductions, but these recent breeds are not new kinds of animals. From the Rarámuri perspective, all the different kinds of domesticated animals were created by God (or in the case of the pig, by the Devil) in the ancient past and were acquired directly or indirectly by their ancestors from these deities.

25. I should note that many other people seemed to regard the species transformations as isolated, natural facts or curiosities rather than exemplifications of principles that could be extended to other animals. I did not, however, encounter any Rarámuris who rejected the possibility or reality of species transformations entirely, although I imagine some do. Such individual variation in approaches to knowledge creation, as well as differences in the content of belief, presumably is found in all human societies.

26. Unlike the members of many other New World societies, the Rarámuris did not assign names to Old World domesticated animals derived from those of similar indigenous animals. The majority of their names for these Old World introductions are adapted from Spanish. Exceptions include *totori*, 'chicken(s)', from the Nahuatl *tōtol-in* (Karttunen, 1992:248), and *bo?wá*, 'sheep', a Rarámuri term that probably originally meant 'fur' and 'feathers' and presumably was extended to include sheep. It is interesting that the Rarámuris of Rejogochi use the term *kabilin* (from Spanish *jabalí*) for peccaries, even though these animals are indigenous to the Rarámuri area. Pennington (1963:102) gave the term *gowi* as the Rarámuri name for these wild pigs. The

Western, or Baja, Tarahumara designate domesticated pigs as *gowi* and wild pigs as *gowi gusigame* 'pig(s) of the forests or wilds' (Don Burgess, pers. comm., 21 Sep 1997).

27. For a discussion of Rarámuri views on the relationship between the human and nonhuman realms, see Merrill, 1988:72.

28. The Rofaifo appear much more inclined to postulate higher-level categories than are either the Kalam or the Nuaulu. It could be suggested that the reported absence of higher-level covert categories among the Nuaulu and Kalam reflects the reluctance of researchers to accept the existence of unlabeled categories. Ellen (1993:93–125), however, provided a detailed explanation of why he believed such categories do not exist to any great extent within the Nuaulu animal classification scheme, and the data presented by Bulmer and his colleagues for the Kalam support his argument (Bulmer, 1967; Bulmer and Menzies, 1973; Bulmer et al., 1975; cf. Dwyer, 1979, and Dwyer and Hyndman, 1983).

29. It would be expected that zoological classification schemes that include few higher-level categories would be associated with few singular taxa and with a tendency to leave "unusual" animals in separate, unaffiliated categories. Contrary evidence comes from Dwyer (1979:16–17), who reported that the Rofaifo animal classification system includes a number of higher-level classes but that "striking anomalies are few." Unfortunately, he provided no detailed picture of their zoological taxonomic system as a whole, and, although he stated that such anomalies "may be rationalized in terms of economic or ritualistic criteria" (Dwyer, 1979:17), he failed to describe what these anomalies or rationalizations might be. There is no evidence that the Rofaifo deal with such anomalies by involving them in transformations (Dwyer, 1976a).

30. Such people are also reported to transform into other kinds of animals, usually carnivorous mammals like foxes and skunks, but sometimes large birds, like owls (*rutúkare*) or turkey buzzards (*wilú*) (Merrill, 1988:158–159). The Rarámuris explicitly associate owls with death, but such symbolism is not associated with turkey buzzards, despite their status as scavengers. Except for grasshoppers and the moths known as soul or ancestor moths, all the animals into which humans are believed to reincarnate or to have transformed into in the ancient past are either mammals or birds. This fact suggests that the Rarámuris, like the members of societies in many other parts of the world, regard mammals and birds as the kinds of animals most closely associated with humans.

31. Some people believe that the souls of all people transform into these moths, not as a punishment for incest but as the final stage in a series of reincarnations (Merrill, 1988:113–114). The term *anayáwari* also means 'ancient people' and sometimes is used to denote very old living people.

32. Examples of stories about animals collected in other Rarámuri communities can be found in Lumholtz, 1902:296–310; Thord-Gray, 1955:517–524; Hilton, 1969; Burgess, 1970, 1978, 1985; Mares Trías, 1975; López Batista, 1980; and Wheeler, 1993. Some information about the fauna of the Sierra Tarahumara from the perspective of non-Indian residents is found in Boudreau, 1975.

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Quenching Homologous Thirsts

Sidney W. Mintz

Although his major scholarly effort has been focused upon the native peoples of the Americas, William Sturtevant has ranged widely in his writing. His pioneering paper on the black drink (1979), his well-known study of root crops (1969), and his wonderfully concise essay on Taino agriculture (1961) attest to an enduring interest in ingestible substances. One would not describe Sturtevant as an anthropologist of food, but his interest in processes connected with the production of edible materials may justify the inclusion, in a work in his honor, of a paper on two “modern” drinks.

In “Black Drink and Other Caffeine-Containing Beverages among Non-Indians,” Sturtevant illuminated the manner in which beverages can slip in and out of a structurally “indigenous” position as well as jockey for primacy in a commercial market. The drinks described below are at a large remove from indigenous America, and they differ in many ways from each other; however, a brief comparison of the histories of their use might serve to substantiate some of Sturtevant’s arguments (see Sturtevant, 1979).

Tea and Coca-Cola are primarily associated with different countries, they are consumed in quite different ways, and their histories differ radically. One is a generic beverage base; the other has a trademark and could be said to have been invented. Yet as the world grew smaller, the histories of these beverages became intertwined. Along the routes to their eventual intersection, one discerns certain parallels. These may be called “structural” (even if one has reservations about some of the things that word is taken to mean) because they suggest that like forces may exploit certain inherent or situational similarities to achieve like results. Partly because of their intrinsic natures, partly because of the purposes for which they were used, and partly because of the context in which their uses grew, in important ways these two drinks are more alike than different. In looking at them, however—in studying the ways in which they differ as well as the ways in which they seem to accord with any postulated universal features of our humanity—it is useful to keep in mind the dividing line between their common

and their distinctive features. Along that line one describes the making of human history.

The history of liquids for drinking is of course far longer than the human career, but their history as labeled products is extremely short. As Sturtevant pointed out, Western societies learn to drink most novel liquids first as medicines or tonics and only later as beverages. Some of these liquids, of course—*ipecac*, cod liver oil—will probably never stop being medicines. Others, including many so-called “teas”—*valerian*, *camomile*, and the black drink, *cassina*—continue to be quasimedical (Sturtevant, 1979). Yet others have largely lost their medicinal character to become beverages. Precious medicines, of course—drugs to counteract acquired immunity deficiency syndrome or baldness—often begin by being available only to the wealthy. As wonder drugs have their uses extended over time, they usually become cheaper and more available, thus repeating an old and familiar pattern.

Tea

One index of how the world has changed is the downward movement that typified the history of substances within European social systems, beginning in the seventeenth century. Some of what are today among the most important liquid foods in the West acquired that importance only when they changed from being the luxuries of European royalty to being the everyday beverages of the masses. I wrote (Mintz, 1985) about this transition with the seventeenth century’s three new beverages, coffee, tea, and chocolate, in mind. In particular, I was interested in the ways these beverages became common and popular in England, Scotland, and Wales. The British people had long been drinkers of ale and beer as well as milk, drinks that would by no means be completely supplanted by the new ones. But in seventeenth- and eighteenth-century Britain, keeping a cow became more difficult because of where people lived and the work they had to do; milk lost ground as a daily beverage. Over time, ale also became less a home product and more something to be purchased at alehouses. The gradual and partial elimination of milk and of home-brewed ale was related to the increased difficulty working people faced in finding time to produce them and in securing what was needed—fodder in one case, hops and malt in the other. The latter difficulty was aggra-

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vated both by urban living and, even more, by the taxes imposed upon hops and malt.

Although a decline in milk-drinking and ale-making marked the eighteenth century in urban (and even rural) England, coffee, tea, and chocolate did not become common drinks until the last quarter of the seventeenth century. Before about 1670 or 1680, they were consumed only in exiguous quantities, mainly by royalty and the privileged in English society. All these drinks had been consumed without sweetening in their indigenous contexts—coffee in the Middle East and Africa, tea in Asia, and chocolate in the New World.¹ In Europe, however, it became the custom to add sweetening to all of them, usually in the form of molasses or low-quality granular sugar. No one has so far been able to specify the time or place that these practices began, but Smith (1992) proposed that sugar with tea took hold widely in Britain around 1695 or 1700. In some parts of Europe, different substances, such as cardamom seeds or lemon peel, were added to coffee, but these were probably culturally specific customs that diffused from certain locales, along with the use of coffee itself. In other places, such as Spain, cinnamon was sometimes added to chocolate, and this may have been where sugar was first added to chocolate. (These usages were innovations, whereas the use of vanilla with chocolate had indigenous origins.) In England, lemon or milk came to be added to tea, together with sugar. Of all the many varied additions to the three new drinks, sugar was certainly the most widespread. Because all three beverages are bitter, Europeans probably found drinking them without a sweetener disagreeable. Probably not long after such beverages were introduced, sugar began to be added to them, perhaps as part of the transition from medicinal to beverage status. All were served hot, and all contain caffeine or a caffeine-like drug. Of the three, only chocolate has any food value. Sweetening made them more attractive to European palates and also added substantial calories to all of them.

In the mid-nineteenth century, the partial shift away from short beer or ale and toward tea preceded many other changes in mass diet in Britain, such as the rising consumption of store-brought bread to replace bread baked at home. Increasing urbanization and the participation of women in the work force outside the home meant that, to meet the time schedules of the mills, they had to leave food for school-age children and to prepare dinner more quickly at the end of the work day. But in the case of ale, the immediate impulse for the shift toward other drinks came from the imposition of substantial taxes on malt by the state, such that home brewing was taken out of the economic reach of laboring families. In the eighteenth and nineteenth centuries, tea and bread together came to make up one entire meal each day for many British workers (Burnett, 1966). Easy to prepare, heavy with calories, hot and appetizing, tea taken with bread heavily spread with jam became a favorite, especially for women and children. In my own work, I have pointed out that meat tended to be monopolized by the husband (Mintz, 1985:144). If there were intrafamilial figures on tea

and sugar consumption for working-class Britain in the eighteenth and nineteenth centuries, they would probably indicate that women and children consumed them more than men did and that men were getting most of the meat.

Tea's success was swift. By 1700 it was immensely popular in England; soon, even the Scots and the Irish had become sworn consumers of it. In his pioneering studies of rural life, the Scottish cleric David Davies argued persuasively that the rise in tea consumption was in response to the increased cost of small beer or milk and was not simply a preference for tea. "Tea (with bread)," he wrote, "furnishes one meal for a whole family every day, at no greater expense than about one shilling a week, at an average. If any body will point out an article that is cheaper and better, I will venture to answer for the poor in general, that they will be thankful for the discovery" (Davies, 1795:37). Davies was indignant that the well-off would criticize the poor for drinking tea. "After all," he wrote,

it appears a very strange thing, that the common people of any European nation should be obliged to use, as part of their daily diet, two articles imported from opposite sides of the earth. But if high taxes, in consequence of expensive wars, and the changes which time insensibly makes in the circumstances of countries, have debarred the poorer inhabitants of this kingdom the use of such things as are the natural products of the soil, and forced them to recur to those of foreign growth; surely this is not their fault. (Davies, 1795:39)

The Scottish historian David MacPherson wrote that the reduction in duties on tea in 1784, combined with the higher taxes on malt, opened the way to an even greater expansion of tea consumption:

Tea has become an economical substitute to the middle and lower classes of society for malt liquor, the price of which renders it impossible for them to produce the quantity sufficient for them as their only drink. . . . We are so situated in our commercial and financial system, that tea brought from the eastern extremity of the world, and sugar brought from the West Indies and both loaded with the expense of freight and insurance. . . . compose a drink cheaper than beer. (MacPherson, 1812:132)

Not everyone was charmed by the rapid spread of tea-drinking, particularly among the laboring poor. In addition to reformers' complaints that the poor should use their pennies for better things, tea was held responsible for illness among the needy. But at least some of the critics recognized that the working classes had little choice. Dr. Lettsom, an English physician and amateur botanist writing at the very end of the eighteenth century, quoted a doctor in Leeds who believed excessive tea-drinking caused new mothers to lose their milk and who argued that tea was addictive in nature:

The difficulty with which animal food is procured by the lower ranks of society, in quantity sufficient for daily nutriment, has led many of them to substitute, in the place of more wholesome provisions, a cheap infusion of this foreign vegetable, whose grateful flavour (and perhaps narcotic quality, which it possesses in a small degree in common with most ever-greens) is found to create an appetite for itself. (Lettsom, 1799:98)

Today, a radical transformation of the basic foods of ordinary people might not surprise us, but in the seventeenth century it was probably happening for the first time in world history.

Tea became even more important in the nineteenth century, when its cultivation—innovated, organized, and managed by the British—was established in India. This obviated the need to deal with the emperor of China, where tea cultivation was native and its commerce managed by bureaucratic intermediaries. Because the Royal East India Company controlled the tea trade with China, it also destroyed the company's monopoly on the tea trade:

Its early adventures in the Far East brought it to China, whose tea was destined later to furnish the means of governing India. ...During the hey-day of its prosperity John Company [the East India Company] maintained a monopoly of the tea trade with China, controlled the supply, limited the quantity imported to England, and thus fixed the price. It constituted not only the world's greatest tea monopoly but also the source of inspiration for the first English propaganda on behalf of a beverage. It was so powerful that it precipitated a dietetic revolution in England, changing the British people from a nation of potential coffee drinkers to a nation of tea drinkers, and all within the space of a few years. (Ukers, 1935:67)

In Britain, tea, whatever its nutritive deficiencies, increased labor productivity. It also yielded great profit to capital and to the state. But not all such products are benign, even in the short term. In the case of imperial China, for instance, the "need" of the Chinese people for opium hardly explains its importation there.

Opium was supplied to the Chinese by British "pushers." In the early nineteenth century, before tea production was shifted to India, the emperor of China and his bureaucracy struggled violently but in vain against British insistence upon smuggling into the country great quantities of opium, which was being produced in India. A.S. Thelwall, in *The Iniquities of the Opium Trade with China* (1839), wrote that His Majesty's Government was thriving on an evil commerce. Although recognized, this was an evil none would try to prevent:

But public men knew of the evil and in most cases deplored it. Their difficulty was that they did not see how the Government of India could manage without the revenue that it derived from opium or how the tea trade (from which the home Government derived vast revenues) could be carried on unless tea were paid for by opium. (Waley, 1958:89)

Thus a modern European government supported the production of a dangerous drug in one of its own colonies to export to an archaic non-Western state, against the will of its rulers, in order to pay for its importations of another, much less dangerous drug. Because of China's strenuous but futile attempts to stem British opium smuggling, the British navy attacked Chinese forts and China was forced to cede Hong Kong and trading rights in Chinese cities to Britain. We are never told why the British imported tea, rather than opium, to sell to their own working class.

Tea's eventual and long-term success in competition with the other stimulant beverages in Britain owed at least in part to the re-siting of its production within the Empire. But that success is in some ways less interesting than how a costly luxury of the wealthiest class could be transformed into an absolutely essential food of the poorest. Tea not only became the beverage of the ordinary person, it also became the very essence of hospi-

tality in the British home, an exportable symbol as central as cricket or the Union Jack, and more profitable. The precocious democratization of tea consumption is a leitmotif in the study of the evolution of capitalism itself.

Tea gained its popularity because it could descend through the social system to enter into the food habits of masses of ordinary people. I believe it did so particularly in connection with meals eaten outside the home. I think tea and sugar were first consumed by laboring people in association with their work; these were interval or snack foods, linked to the need for brief, specific moments of rest and ingestion at labor.

Although it was amid the demanding rhythms of modern labor that people probably learned to consume these stimulant beverages, they were not consumed only at work, nor did they end up being consumed primarily as work foods. This was not simple emulation or imitation of others. What such new foods supplied physiologically was quite different from what they supplied to the privileged classes, and the daily rituals with which they became associated were also different.

The frequent drinking of strong, heavily sweetened tea developed in the late seventeenth century and matured in the eighteenth century. As late as 1700, a majority of English people had not yet tasted sweetened tea. By 1800, probably every English person alive had tasted it and wanted more of it. From about the middle of the nineteenth century onward, if not earlier, tea became the household drink. Soon it became a daily marker of time, as coffee is for many Europeans and for most North and South Americans. It was already solidly established as the daily work drink in Britain.

The initial downward diffusion of tea in the eighteenth century had helped to displace home-brewed ale and short beer. In the late nineteenth century and thereafter tea became competitive with alcoholic beverages, particularly gin but also whiskey and rum, through the British temperance movement.

Before the 1820s, the temperance movement had been directed specifically against the "ardent liquors": whiskey, rum, and gin. Temperance thinking held that partial abstinence, at least from hard liquor, was a necessary first step on the road to temperance. But as the nineteenth century wore on, the feeling grew among temperance reformers that all alcohol, not just hard liquor, would have to be proscribed. The term "teetotalism," used to mean complete abstinence, was coined by a British worker in the early nineteenth century. The word "teetotal," meaning complete or absolute, had a long history predating this innovation, but once the new meaning had been imparted to it, "teetotal" became the property of the temperance movement. (It was not connected with the word "tea," although such an association has sometimes been incorrectly attributed to it.) As complete abstinence grew, the role of nonalcoholic drinks in the politics of temperance grew with it.

In the three new tropical drinks, the temperance movement found useful weapons in its struggle (Harrison, 1971). But the importance of alcoholic beverages for British workers, and the ensuing fight against them, makes sense only if one sees the so-

cial context within which such drinks were consumed. During the first half of the nineteenth century, the houses of urban English workers were small, uncomfortable, and often wretched. Domestic poverty and meanness of abode, when added to the nearly complete lack of locales of entertainment, meant that for most workers, bars were the only social centers and meeting places they had. This doubtless enhanced the consumption of alcoholic beverages.

The nineteenth-century temperance movement found that the nonalcoholic drinks, such as soda, lemonade, and soda flavored with ginger, were attractive alternatives to alcohol. The temperance reformers, however, were more active in encouraging the consumption of tea and the other hot beverages rather than seltzer or soda pop (Harrison, 1971). This was not a matter of taste: barkeepers encouraged their clientele to mix soft drinks with whisky, so such drinks facilitated the consumption of alcohol, rather than supplanting it. Hence the reformers took an adversarial position toward the bars as such, plumping in favor of cafes, which, before the nineteenth century, were frequented more by middle-class persons than by laboring people. The cafes served tea and coffee and light refectations rather than seltzer and soda and alcohol. They were places to meet, converse, and read while drinking and eating. The character of many of them changed, partly as a consequence of a new clientele.

When the temperance movement was still concentrating on hard liquor consumption, leading Quaker families, such as the Whitbreads, the Barclays, and the Lucases, had been involved in the promotion and sale of beer, a drink viewed by the temperance reformers at that point as a preferable alternative to rum, whisky, or gin. But in the nineteenth century, other Quakers, such as the Tuke, Mennell, and Horniman families, became leaders in the creation of tea import and retailing outlets, promoting tea against all alcoholic drinks, including beer. The Quakers were the first to create the "tea meeting," an institution both religious and secular, where tea was consumed regularly as a sort of interval drink in the religious service. Chocolate (cocoa), as well as tea, was also promoted by important Quaker commercial families, including the Cadburys, the Frys, and the Rowntrees.

In the construction of these events as part of a movement against alcohol, one sees the emergence of new patterns of consumption, consonant with the industrial work day. The ideal worker would renounce alcohol, consuming instead these other drinks, often in the company of his family. The tea garden, like the tea meeting, was an institution that provided a context for consumption, as well as a product to consume. A writer of the mid-nineteenth century suggested that the calendar of proletarian social life was affected by tea when he wrote of "Sunday afternoon tea, the most formidable appliance of the matchmaker of working-class society, and a really powerful means of promoting courtship and matrimony among the working classes" (Wright, 1867:236). By drinking nonalcoholic beverages in a familial context, it was thought, the worker saved his family from ruin. He also got to the factory sober, early Mon-

day morning. Sobriety was healthful, moral, and economical; it was also in the service of the society and its leaders.

The habit of combining meals with hot beverages, taken in restaurants where alcoholic beverages were not sold, became stabilized just at the middle of the nineteenth century. In Scotland, in Dundee and Glasgow, and soon enough in England, in Manchester and London, workers began to buy their luncheons at midday and to consume them with coffee, tea, or chocolate. Later, industrial canteens, selling hot beverages at low prices, became institutionalized within large mills and factory complexes.

Even before 1870 the temperance movement had concerned itself not only with the consumption of alcohol but also with the consumption of other foods and with the social circumstances of ingestion. The growing awareness, earlier in the century, that the laboring classes had no places outside the bars in which to meet and enjoy themselves was a sociological insight of the first order. Tea gardens, important particularly on weekends and holidays, were hence a significant innovation. The provision of venues for ritual and familial gatherings among the laboring classes transformed to some degree the ways in which the consumption of alcoholic and nonalcoholic beverages took place; they also helped to alter the eating habits of ordinary people. The temperance movement in England had important consequences for British drinking habits. At the least, it helped to enrich the participation of English workers in a public social life, and as part of this process, it deeply influenced the consumption of various nonalcoholic beverages, such as tea, and the circumstances of their consumption.

Tea was a new beverage in Britain in 1660, but by the mid-eighteenth century it had become a truly national beverage. Legal imports of barely nine metric tons in 1700 had grown to more than 9000 metric tons by 1800 (Mintz, 1985:113). After 1850, tea increasingly was drunk. One might say that from that time on, at least, drinking tea embodied Englishness. Indeed, the production, sale, and consumption of tea played a certain part in international politics, in connection with the pacification and rule of India, in the wresting of control of Hong Kong and various commercial privileges from the Chinese, in the provision of markets for West Indian molasses and sugar, in national taxation policy, and otherwise. In its emergence as a marker or symbol of British culture, tea also played some part in the temperance movement. Teetotaling was invented in connection with tea. Religious meetings were conducted with, and around, the drinking of tea, and it seems certain that tea played a part in the forging of a sober labor force for British industry.

In approximately two centuries, the "oriental vegetable" had been changed from an exotic medicinal of the court into a daily necessity of the poorest people. It did so together with other changes underway in Britain, and it was pressed into service for objectives that probably would never have occurred to persons in the ancient tea-drinking cultures of China or Japan. What tea was had become different; what tea meant had become even more different.

Coca-Cola

No matter from what perspective—except for the fact that it, too, is a beverage—Coca-Cola gives an impression radically different from that given by tea. One was a product of the seventeenth century, brewed from the leaves of a single plant; the other is a product of the end of the nineteenth century, composed of diverse ingredients. One is usually consumed hot, the other is usually consumed cold. One is marked, often dramatically, by the ritual of its preparation; the other is sold ready to be drunk as is, anywhere, anytime, by anybody. One is an ancient drink, emphatically foreign and exotic; the other is “indigenous” and modern, a concocted beverage with its own inventor (even if most of its ingredients were at least known, and reputedly medicinal). Yet there are basic ways in which these drinks are alike.

During the first half-century of its existence, Coca-Cola was a regional drink, consumed principally in the South. It was not well known in other regions of the United States and was even less known in the rest of the world,² with one major exception. Special efforts were made by the Coca-Cola Company to diffuse Coca-Cola to Cuba soon after it fell under United States control in 1898–1899. At least two Coca-Cola bottling plants were built in Cuba (one was built principally to supply the United States naval base at Guantánamo). Coca-Cola was made part of the active Protestant missionization undertaken in Cuba by Warren Candler, a Methodist bishop who was the brother of Asa, the head of the Coca-Cola Company (Louis and Yazijian, 1980:29–30).³

In contrast, tea had been spread across the globe, largely by the British, centuries before the invention of Coca-Cola. In its world spread, tea was a beverage linked to the dried leaves of a single specific plant. Tea leaves compacted in the form of bricks traveled well and could be stored for long periods, and some European regions, such as Imperial Russia, had been importing tea for centuries. There were tea companies and tea brands, as well as varieties of tea. In these ways, tea was a product quite different from Coca-Cola. Like coffee and chocolate but unlike sugar and Coca-Cola, it was not standardized for a one-product market.

Like tea, Coca-Cola began as a medicinal. Its inventor, John Styth Pemberton, a pharmacist, had wanted to perfect a tonic—his word—rather than a beverage. What he did on his first try was to copy a French beverage called “Vin Mariani,” which was sold as a medicine in France and elsewhere. Vin Mariani was a brutally simple drink: a pedestrian Bordeaux red wine, heavily dosed with extract of coca leaves. Its consumption produced a mood both tranquil and euphoric. Pemberton named his first such invention “French Wine Coca: Stimulant and Ideal Brain Tonic.” Its basic chemical nature resembled that of Vin Mariani. But Pemberton could already foresee Prohibition in the United States, and he was prescient enough to gamble on creating a new product that would provide the effects of Vin Mariani but without the alcohol. This

latter criterion constituted an important aspect of the eventual success of Coca-Cola, although Pemberton could not have forecast in precisely what way. The formula for Coca-Cola (both “classic” and “new”) is proprietary, but we do know that Pemberton modified his “French Wine Coca” in order to produce Coca-Cola. He eliminated the wine and added caffeine, cola extract, and vegetable oils. For a couple of decades after Coca-Cola was perfected, the cocaine was also retained, in the form of fresh-brewed coca leaves. As frequently happens with the inventors of new products, Pemberton had to improvise to get things started. The first Coca-Cola was distributed to retailers in the form of syrup, packaged in old beer bottles. In the soda fountain-pharmacies that sold Coke, the custom emerged of mixing a soup spoon of syrup with water. This was taken as a remedy by customers who were suffering from headaches (especially those caused by overindulgence in spirituous liquors). In 1886, the year of its creation, Coca-Cola was first mixed with soda water; this was the mixture that then became standardized as a drink, rather than as a syrup, which one might add to water. Its medicinal, curative nature changed from medicine to medicine/beverage.

Pemberton soon had to sell his enterprise. His Coca-Cola ended up eventually in the hands of a man named Asa Candler. In contrast to Pemberton, Candler definitely was a businessman. The total price of Pemberton’s enterprise, including his vats and his little stock of substances, was slightly more than 2000 dollars. From such beginnings came today’s Coca-Cola.

Candler suffered from headaches and dyspepsia and was apparently very happy to become the sole proprietor of a remedy for both maladies. Always a fiercely religious man and a fervent member of the Methodist Church, Candler was also a lifelong teetotaler, and he was unusually astute in the ways of commerce. He clung to the definition of Coca-Cola as a medicine, and he was strait-laced about how its virtues were touted. Under his guidance, even the publicity for Coca-Cola was monitored. During the first half-century, advertising for the drink stressed its purity and its nonalcoholic nature. For a lengthy period under Candler’s leadership, Coca-Cola was advertised exclusively in religious magazines. Candler clearly thought that he was doing God’s work with this product.⁴

Yet at the same time—and, as it were, informally—Coca-Cola acquired a reputation as a mixer, and its fame in this regard was diffused even more widely than the drink itself. In some parts of the United States, Coca-Cola also acquired a reputation as a contraceptive douche. One suspects that what had begun as a medicinal consolation to middle-aged over-indulgers was now becoming a drink of youth and adolescence. The company under Candler, however, never advertised the drink’s fame as a mixer.

In the British social setting, tea and alcohol were counterposed. Although alcohol can be added to tea, I have noted that this is clearly glossed as medicinal in British usage. But in the American social setting, Coca-Cola and alcohol were not

counterposed. Coca-Cola's taste is so powerful that many young women—and young men—got their first introduction to alcohol through Coca-Cola. Particularly convenient was the fact that alcohol could be poured into a half-empty bottle of Coca-Cola—at dances, for instance—and disingenuously carried about. Coca-Cola was also much touted in the subculture of adolescence in the 1930s for its usefulness, once spiked, for seduction.

As a company jingle promised, apparently things did go better with Coke. The Coca-Cola Company profited from Coca-Cola's nonalcoholic virtue even while it was becoming a popular alcoholic mixer among many of its users. Before Prohibition became federal law, Candler's ideal temperance beverage turned out to be an ideal mixer, especially in the dry South, where local moonshiners marketed various poisons as whisky. After the enactment of Prohibition, in 1919, what had become custom in the South spread more widely. To some extent, the transformation of Coca-Cola into a national drink hinged upon its use as a mixer during Prohibition. From this vantage point, it is possible to see that Coca-Cola was a more adaptable drink than tea. Tea can be mixed with other substances, including alcohol, but it has never been very successful as a mixer, except medicinally, and then only to a limited degree. How Coca-Cola became a social drink when combined with alcohol is only part of its later history. From an antidote for the consequences of overindulgence, Coca-Cola changed not only into a mixer but also into a drink of the work day.

Coca-Cola was vigorously promoted as the South became less rural and agrarian, more urban and industrial, and less isolated and overwhelmingly dry. After World War I, southern industry grew at a rate and on a scale that might allow one to compare it superficially to western Europe's Industrial Revolution, a century and a half or so earlier. The rising intensity of mill work in the South was marked by changes in the length and tempo of the work day and by sharp alterations in the prescribed times for respite. These changes were all the more dramatic for occurring as often in small towns and open country, where the mills sprang up, as in large cities.

Most of the workers came from agrarian backgrounds. In North Carolina, "At 12:00 the whistle sounded, and Gastonia County's twenty five thousand mill workers left to go to the villages to eat a hot meal. At 12:55 it was time to start again, the whistle said" (Hodges, 1986:29). Living by the clock was something Southerners, too, had to learn about. Rhyne (1930:12) wrote also of mill laborers in North Carolina:

Promptly at 12 "dinner" is ready and on the table in the kitchen. After hurriedly washing his hands, perhaps occasionally bothering to comb his hair, the mill worker sits down to his noon-day meal which he finishes with as much dispatch as does the college boy on his commons—perhaps fifteen minutes. Then by 12:30 o'clock he is ready to start back to "the mill." At 12:30 a second whistle blows, repeated sometimes at 12:45, to warn the worker that either in 25 or in 20 minutes he must be on the job again. If he gets to the mill a few minutes ahead of time he goes to the inevitable drink stand and drinks his coca-cola, although he often partakes of this refreshment in the middle of morning or after-

noon working periods. A stand in the mill may supply him, although in most mills he is free to go out to a shop across the street.

The easy availability of liquid refreshment—nonalcoholic liquids only—was an "urbanizing" feature of the southern factory village. The laborers neither brought nor fixed their drinks; they bought them. Almost exclusively they drank Coca-Cola. Carlton (1982:175), describing factory conditions in South Carolina in the period 1880–1920, stated that the generally accepted workday began at 4:30 a.m. and ended at 9:30 p.m. Twelve hours belonged to the mill; the other five and a half were mainly for eating but were a wasteful use of time in the view of the mill owners. "Efficiency experts" worked to speed things up after World War I. Workers could eat faster if they didn't go home at noon and could eat faster yet if the foods they ate were available right in the shop:

Mills encouraged that habit by operating "dope carts" that sold cold drinks and sandwiches. "They had what they called 'dope wagons,' Mack Duncan explained. "People used to call Coca-Colas 'dope'." (Hall et al., 1987:209)

The usefulness of Coca-Cola as a workday drink was enhanced by the concerted movement across the South to make alcohol illegal. Thanks to the efforts of Southern religious leaders, the temperance movement had significantly changed the social situation. As early as 1907, for instance, of the 994 counties in the former Confederate states, 825 were dry (Louis and Yazijian, 1980:26). That there was some relationship between the temperance movement and the expansion of Coca-Cola seems certain. Candler was not only a teetotaler, he was also a fierce exponent of Coca-Cola as brain tonic, refresher, and medicine designed for clean living. Tea, which came to be known in the United Kingdom as "the beverage that cheers but does not inebriate," found its match in what was soon being referred to as "the holy water of the South" (Louis and Yazijian, 1980:26).

Pemberton, the inventor of Coca-Cola, had perfected his nonalcoholic formula while retaining the coca leaves, thus adapting his beverage to the times. At the end of the nineteenth century, however, leaders of the antinarcotic movement in the United States, including a number of physicians, pushed for the classification of cocaine as a narcotic. Candler, the creator of Coca-Cola's image, reacted violently; in 1892 he declared that he would give up Coca-Cola if it could ever be shown that cocaine was addictive (Louis and Yazijian, 1980:33). But as pressure against the unregulated distribution of cocaine mounted, Coca-Cola quietly took the cocaine out of its syrup, replacing it with spent coca leaves. Federal narcotics regulations (1914–1922) did not prohibit the importation of coca leaves for medicinal purposes. The Coca-Cola Company still (AD 1990) purchases the spent leaves from the Stepan Pharmaceutical Company to be used in its syrup (Louis and Yazijian, 1980:35).

Under Candler's direction, Coca-Cola continued to be advertised heavily in religious journals: "Coca-Cola revives and sustains"; "full of vim, vigor and go"; "refreshes the weary, bright-

ens the intellect"; "the drink that quenches the heart's desire." Other claims, as assertively cheery as they were absolutely vague, were also made. The main thrust was that Coca-Cola was "good for you"—good for the spirit as well as the body. Above all, such advertisements imparted a sense of inner cleanliness. A therapeutic undercurrent typified the messages. The appeals, at least during the early decades, were always astringently innocent of any sensual overtones, stressing instead brightness, purity, and friendship. The analogous message in today's advertising appears to be that for vaginal douches, such as Massengill's. Continuity with the past, yet appropriate for today's youth: "with it," but traditional.

The line against suggestive ads of any kind was held to sternly by the Coca-Cola Company, at least until the end of World War II. But the wartime ads did stress Coke's virtues for persons in the armed services. Coke expanded greatly between the wars, but only with World War II did Coke become a truly national drink.

Soon after the Japanese attack on Pearl Harbor, in 1941, General George Catlett Marshall invited all of his field commanders and general officers to request the construction of Coca-Cola bottling plants so that the product could be produced at the front. By his letter Marshall endowed Coca-Cola with the same status in the wartime economy as that occupied by food and munitions (Watters, 1978:164; Louis and Yazijian, 1980:56–57; Pendergrast, 1993:203–204). It is quite unclear why he did so, but Marshall was a southerner; his estimate of the popularity of the beverage may have been influenced at least in part by his own origins and experiences. The Army thus officially spared Coca-Cola from sugar rationing at a time when sugar was—as is always so in war—highly prized. In all, 64 Coca-Cola plants were established in allied theaters of war, including the Pacific theater, Australia, North Africa, and elsewhere. The Coca-Cola Company was asked by the armed forces to supply technicians to run the production; 148 such technicians were sent by the company, and three were killed in theaters of war during World War II.

War stimulates symbol production. In the case of Coca-Cola, such production was enhanced by advertising, connecting the beverage to the war effort and to the simple virtues of fighting men and women. The drink acquired its national stature as American forces occupied more and more remote portions of the earth's surface, for Coke had been enabled to travel with them. Its traditional public associations with health, virtue, and nonalcoholism were in good measure relinquished as Coke became the drink of the United States armed services. Although the war years were full of stories of small southern villages where thirsty GIs were charged extortionate prices for Coke, it was Coke that they drank. The men and women under arms were truly national: Native Americans, patriotic young Nisei out of the relocation camps, black Americans in segregated units—in short, some of nearly everybody—ended up in uniform. As they learned so much else in the service, so they

learned to be drinkers of Coke. War remakes diet. But the significance of Coke went much farther. The first bottle to come ashore during the Allied landing at Anzio, for instance, was shared by 19 GIs. And it was not unusual to find in the letters that servicemen wrote home the assertion that they were fighting for the right to drink Coca-Cola (Louis and Yazijian, 1980:59). It was in this manner that Coke became a national symbol among the warrior youth of the 1940s generation. That so many members of the professional officer corps and of the noncommissioned officer ranks were southern—a legacy from an earlier war and its divided spoils—may also have played a role in the story.

In the last 50 years, Coca-Cola has changed from a national to an international drink. Its worldwide expansion has accompanied the spread of American influence and power; to some degree the Coca-Cola Corporation has been a stalking-horse for American culture. Today's advertisements confirm Coke's popularity while increasing it further. The oddly shaped bottle,⁵ once critical to the identification of Coke as Coke, is no longer important; the very name "Coke" has supplanted the bottle associated with it. The association between Coke and a particular generation—the GIs of the 1940s—has been deliberately downplayed, as has Coke's association with specific times of day, specific foods and, most of all, with the United States. Yet drinking a Coke has become a powerful way of validating a symbolic identification with American might and influence. In recent decades, the Coca-Cola Company has become a multi-commodity corporation and (together with its arch rival, Pepsi) one of the 50 largest corporations in the United States. It may have all begun with a mixer for bad booze and seduction, or a stimulant to help one endure the long hours of drudgery in southern mills, but that is not the way it has stayed. Arguably, Coke is now the best-known symbol of the United States in the world; its company is one of the most powerful private economic organisms in that world. Some of the most important figures in American public life won their spurs selling soft drinks—and before then, hard drinks. When American Revolution patriot John Adams contended that molasses (from which rum is made) was "an essential ingredient in American independence," he was referring to the political consequences of the mother country's policy of taxation without representation (Mintz, 1985:256). That latter-day patriots may view Coca-Cola as an essential ingredient in American power world-wide today resonates a bit oddly with Adams's view. Soda pop is big business—indeed, it is about as big as business gets.

Conclusions

This paper offers its readers a comparison. It may appear that the comparison is between two drinks, and, on the face of it, that is correct. But if one were to seek a broader way to describe this comparison, it might be called systadial (Childe, 1946:250). I do not use this word in its geological or archaeo-

logical sense, but to refer to the same social process occurring in different places and at different times. This essay is not so much a comparison of two drinks as it is a comparison of two places and two historical moments or periods. Or as I would prefer to suggest, it really deals with one stage—but unfolding at two different times, and in two different places. That we deal with two drinks is relevant, of course. Anthropologists care about substances: their origins, uses, contexts, and meanings. But beyond the drinks themselves, the backdrop matters. These drinks figured in the histories of two societies in situations in which they were modernizing themselves by becoming industrial. In both instances—although widely separated in time as well as by space—large numbers of people were becoming habituated to factory schedules, and their eating habits were changing accordingly. And in both instances, drunkenness was a problem with which industry had to cope. In both instances, beverages of a certain sort turned out to serve simultaneously as “proletarian hunger-killers” (Mintz, 1966) and as antidotes of a kind for alcohol. As hunger-killers, these drinks served to punctuate the day, to mark intervals between exertions, and to enhance repasts. Much as a cup of tea could turn a cold meal into a hot one (Burnett, 1966) for the British laborers, so Coca-Cola could turn bread and cheese, or a hamburger sandwich, into a modest feast. It is scarcely a coincidence that both of these beverages packed a double wallop: powerful stimulants, charged with plenty of calorie-heavy sucrose. But coincidence or no, laborers in two different places, at two different times, found that these two different interval beverages provided solid comfort.

It seems to me that systadial comparisons of this sort serve more than one purpose. They often enhance one’s understanding of the individual time periods, limned as each period is by the shadow of the other. But of course comparison also reveals differences between the periods—and these turn out to be differences of a different kind. By this I mean that the original comparison, which may seem so outlandish at the start, turns out to be useful because the structural-functional similarities between the things compared expose in turn other, more important differences between them.

At the outset, it may be difficult to see what could possibly be learned from comparing eighteenth-century England with the early twentieth-century United States South in any regard; and even less from comparing two such different beverages. But if it turns out that the comparison does, in fact, bring something into clearer focus, then the similarities between these beverages reveal—by contrast—a new sort of difference. For purposes of this comparison, for instance, it is not particularly important that one beverage is usually served hot, the other usually served cold. It is not even important that one is a compound, whereas the other is made from a single plant. Nor is it important—for this sort of comparison—that the time periods

are different. More important is that tea became a national drink, first as the exclusive luxury of the wealthy, and was supported from the outset by the policies of an imperial government, made from products of faraway lands and, soon enough, colonies. In contrast, Coca-Cola began as a local hangover remedy for ordinary people and only acquired national status with the help of good friends in high places, in time of war. Or, again, it is more important that at the outset tea was controlled by a single distributor—different (although perhaps not too different) from what has happened with Coca-Cola in much more recent times.

Throughout the so-called Third World today, much of it consisting of former British colonies, the half century since the Second World War has witnessed a continuous expansion of Coca-Cola. In many parts of the world, people whose families drank tea for 250 years have been learning to drink Coca-Cola. Yet nowhere are masses of Coca-Cola drinkers now learning to drink tea. When we anthropologists argue that human beings are the only symbol-making and symbol-using creatures, we stand on secure ground. But we must also learn to explain, better than we have, under what circumstances symbols are created for us by others who profit from our capacity to employ symbols. Symbol creation can itself be an important source of power. If, nearly everywhere in the world, Coke is supplanting tea, is that because Coke tastes inherently better? Because it is cheaper? Because it is healthier? Because things go better with Coke? And if none of those is the right answer, what is the answer—if not the power that Coke connotes, and the cultivated need people have to identify with that power?

Notes

1. Chocolate was drunk by the indigenous peoples of highland Mexico mixed with hot dried peppers (*Capsicum* spp.), annatto (*Bixa orellana* L.), and aniseed. There are also descriptions of it mixed with honey, but this does not appear to have been important. Coe and Coe (1996:89), for example, described chili as a “universally popular” addition to chocolate throughout Mesoamerica but listed “honeyed chocolate” as only one of nine sorts of indigenous chocolate drink.

2. Pendergrast (1993) thought that I underestimated Coca-Cola’s importance nationally and internationally during the pre-World War II period.

3. I have never seen any historical account of the drink called “Cuba libre” (“Free Cuba”), but I believe that this name for the rum and Coca-Cola drink originated at the time of the introduction of Coca-Cola to that country, soon after the United States occupation.

4. This was not a trivial matter. When the western Coca-Cola Bottling Company of Chicago produced a free—and unauthorized—tray with the legend “Wherever ginger ale, seltzer or soda is good, Coca-Cola is better,” graced by a young seminude female in classic European portrait style, the parent company, horrified, disavowed the tray and stopped its distribution. They turned the item into one of the rarest and costliest collectibles in American advertising art.

5. There is some humor in the fact that the bottle designer, instructed to create a container resembling the kola nut, mistakenly designed one resembling a cocoa bean pod. No matter; it turned out to be one of the serendipitous coups of modern advertising.

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