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AN INDIVIDUAL'S PARTICIPATION IN AMERICAN ARCHAEOLOGY, 1928–1985

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My participation in anthropology and archaeology did not come about as the result of a revelation or as a revolutionary departure from the ideas often acquired during one's early education in what was then called "grammar school," followed by four years of high school. I was attracted to history and geography at an early age in Atchison, Kansas, and in Denver, Colorado, but did poorly in arithmetic and continued to find any mathematics a difficult task. When we moved to Denver in 1912 our home was in easy walking distance of the Denver Museum of Natural History. Whether stimulated by the southwestern archaeological exhibits there or not, I remember writing an English theme in fifth grade on the "Cliff-dwellers."

My family moved to Oak Park, Illinois, in 1914, where I finished grammar school and high school. In those days the Field Museum was in one of the World's Columbian Exposition buildings in Jackson Park on the south side of Chicago. It is now the Rosenwald Museum. I made several trips on the elevated train (the "El") to see the museum. In high school I read a great deal, almost stumbling on books on evolution, *The Golden Bough,* European archaeology, and was also impressed by an admittedly fictionalized account of cultural evolution, *Wanderers* by Knut Hamsum (23). My selection of the University of Chicago was hardly the result of carefully weighing its advantages against those of other places. Compulsory military training was still in effect at the University of Illinois and Northwestern. It was available at Chicago but was not compulsory. Many colleges required modern language credits, but Chicago was content with the three years of Latin I managed to complete in four years. Furthermore, I could travel by "El" an hour and a half each way and live at home, which was a major factor as far as my parents were concerned.

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In October 1923 I matriculated at the University of Chicago, embarking on a three-year program in business administration, then a three-year study of law. The first year was reasonably successful with courses in English, geography, history, and so on, but during the first quarter of my sophomore year my performance in accounting, the Manager's Administration of Finance, and economics caused me to shift to a social science major in the liberal arts college. The latter program was a potpourri of English history; Europe before, during, and after World War I; geography; ancient, medieval, and modern philosophy; political science; sociology and introductory anthropology; and a general course in ethnology from Fay-Cooper Cole. I was attracted both to Cole and to the breadth of view anthropology gave on human origins, cultural development, the many varieties of human culture within varied environments, and the many interpretations that had been developed on the relationship of people to the supernatural.

My father's illness was a factor in my embarking on a training program to become a junior executive in Standard Oil of Indiana, for I had no taste for selling securities as many of the 1927 graduates did. For a year and a half I manned service stations on the west side of Chicago, Cicero, and Oak Park. That experience plus some eight years of summer jobs produced dissatisfaction with business ethics. In the summer of 1928, Fred Eggan and I had a meeting with Fay-Cooper Cole about enrolling for graduate work in anthropology, and we were told that there were more positions open than PhDs to fill them. So in October 1928 I happily enrolled to become a member of a community of scholars in which there was little friction. It took me some years to learn how university competitiveness and the administrative bureaucracy worked.

Having taken only two courses in anthropology, I had a lot of catching up to do. The instructors were Cole, Edward Sapir, and Robert Redfield, who was taken into the department in the fall of 1928. I was startled when Sapir approached me to ask if I would be interested in studying linguistics with him, and I declined as gracefully as I could. I had had only a high school botany course, and to go into physical anthropology would mean spending considerable course time acquiring a biology background. While ethnography was interesting, it did not appeal to me as something I wanted to pursue. I was intrigued with archaeological fieldwork after the 1929 summer excavation of Parker Heights Mound north of Quincy, Illinois, and with the second summer of fieldwork in Fulton County, Illinois. I also found there was much more work to it than I had been led to believe in Cole's one-quarter course on field method with smatterings of Old and New World Archaeology.

In Redfield's course on Middle America, I had enjoyed preparing a class presentation on Maya burial practices, so when Redfield became my master's thesis advisor I set out to "test" Kroeber's proposal that burial practices were an affect-laden pattern and were slow to change. I picked the western half of the

Northeast Woodlands Culture area and collected information on the mortuary customs from historic accounts and prehistoric practices. The data I gathered did not confirm Kroeber's interpretation.

The course in which I did my best work was Cole's "The Peoples of Malaysia," and my class presentation topic was on the pygmy and negrito populations. Fortunately, there were not only the monographs but also the Field Museum collections to draw from, and Cole allowed me to talk in class for four days. He had mentioned that the Philippines were a promising area to do fieldwork in archaeology and I indicated my willingness to go, but H. Otley Beyer, of the University of the Philippines, did not, as far as I know, answer Cole's letter.

Some of the students with whom I associated were Fred Eggan, Richard Morgan, William Krogman, George Neumann, Frank Setzler, Paul Nesbitt, William Gilbert, J. Gilbert McAllister, Alfred Bowers, and later in the mid-1930s, John Bennett. Charlotte Gower and Paul Martin had been graduate student assistants to Cole and graded papers for my undergraduate courses. In the summer of 1930, Cole supervised the excavation in Fulton County conducted by Chicago students and a group supported by the Laboratory of Anthropology field training program. This latter group included Dorothy Cross from Pennsylvania, Rita Hahn from Columbia, J. O. Brew from Dartmouth and headed for Harvard, and Harold Driver from California. Thorne Deuel was the field boss.

The University of Chicago program in archaeology was fostered by Cole, beginning in 1925, as a visible demonstration of one of the benefits of anthropology and something which would appeal to some wealthy donors in the Chicago area. He also cultivated capable amateurs in the state such as George Langford and the Dixons in Fulton County. On the basis of the archaeological information that was obtained during the previous five years, Cole, in one of the Sunday morning discussion sessions in the 1930 season, proposed a chronological sequence for the area running from Black Sand and Red Ocher, the early Woodland complexes, through Hopewell to Late Woodland, and then Mississippian as represented at the Dixon mound and other sites. This sequence was applicable for the area from St. Louis north to Wisconsin and was a major development, paralleling a comparable potential sequence in Ohio interpreted in terms of the Northern Illinois findings. Short papers by a number of authors were published in the Transactions of the Illinois Academy of Science in the early 1930s (8, 26, 33) and one in the Illinois Blue Book by Kelly & Cole (25) entitled "Rediscovering Illinois."

In the summer of 1931 I was hired to direct excavations at Athens, Pennsylvania, for the Tioga Point Museum under a grant from the National Research Council. There was a strong local belief that the site of Carontouan, an Andaste Iroquoian village said to have been visited by one of Champlain's followers during A.D. 1615 from Canada, was located on Spanish Hill, an erosion remnant located between the Chemung and Susquehanna rivers near the New York-Pennsylvania state line. There were stories of bushel baskets of projectile points and other artifacts from the surface. I was not, however, told of the extensive testing there by a party under Warren Moorehead and Alanson Skinner in 1916, working for the Robert S. Peabody Foundation, that had failed to find evidence of Indian occupation. There was a low earthen "wall" around the brow of the hill, and when our surface collecting and a few exploratory pits failed to produce, we ran five or six trenches across the wall. These showed multiple stages of construction, leading to the interpretation that instead of having been constructed for an Indian fort, it was gradually built up by generations of farmers to prevent soil erosion. At other locations in the vicinity we excavated Second and Third Period Algonkian and Andaste Iroquoian material. On the Thatcher farm south of Athens we stripped rather large areas of top soil in order to reveal sub-plow zone features. In one of our excavation trenches we partly uncovered a large mammal, which was a bit puzzling until we found out that the farmer's recently deceased bull had been buried in one of Skinner's excavations. My report on the excavations was submitted in 1932 to the Director of the Museum but was never published, primarily, I think, because it ran counter to her cherished convictions. Recent attempts to have the report published were frustrated by failure of the museum personnel in 1931–32 to catalog the collections we obtained and documented, so that now the only published statement is a brief resume (7).

As a result of this excavation experience, I was selected to excavate and survey in the Susquehanna Valley near Wilkes-Barre in 1932. Richard Morgan was to do the same in Somerset County and William Ritchie in the west branch of the Susquehanna. We met in Harrisburg and were shown our equipment and cars by Donald Cadzow on the Tuesday morning after Decoration Day, but by noon we were informed that Governor Gifford Pinchot had vetoed the appropriation bill. By that time there was no possibility of joining other excavation programs. That summer I prepared a report on the Parker Heights excavation which, alas, was also never published. Some three years ago a copy of that report was turned over to Northwestern University to be included as an appendix to work they have recently done in the Mississippi Valley south of Quincy.

William T. Corlett, a Cleveland physician, had prepared a manuscript on "The Medicine Man of the American Indian and His Cultural Background" (5). He met rebuffs from publishers, and Paul Martin at the Field Museum was asked to revise it extensively. As a result, I was recruited to help and spent considerable time reading and writing, and produced necessary changes in the chapters on the Plains, the Northeast Woodland, and the Southern Woodland. For the latter area I made considerable use of the Field Museum Library and of the manuscript collections in the Ayer Library. J. Eric Thompson rewrote the Middle American section, Donald Collier did the Northwest Coast and perhaps the Arctic, while Martin did the Southwest chapter. We were paid for our work, but I was still startled when the book was finally published to find only Paul Martin's name mentioned as having aided its preparation. The rest of us turned out to be unmentioned ghostwriters.

There were a number of job opportunities which might have led to a PhD thesis topic. Sapir nominated me for a Bishop Museum Fellowship in Hawaii, but it went to Edwin G. Burrows. I tried to go with J. Alden Mason on his first trip to Piedras Negras, but instead he took Linton Satterthwaite. I also wrote to Ephraim Speiser, in order to go to the Near East, but his choice was Dorothy Cross, and I understood that. Then in late 1932 I was supported by Cole for a fellowship in Aboriginal North American Ceramics at the University of Michigan, administered by the Museum of Anthropology and supported by Eli Lilly through the Indiana Historical Society. I had met Carl Guthe and his secretary at a number of meetings. I was chosen and arrived in mid-February 1933. I learned the Fellowship was for three years, that I was to take graduate courses in geology, geography, history and a few in anthropology, write a thesis, and would receive a PhD in February 1936. The Fellowship stipend was one of the best in the country, $1000 on a 12-month basis, plus $200.00 for travel expenses.

My fellowship support was provided to aid in identifying ceramic complexes in the eastern United States, with special emphasis on those from the Ohio Valley and surrounding areas. This was to be done by studying the collections that Guthe had obtained since 1928 in the Ceramic Repository for the Eastern United States and by visiting and analyzing museum and private collections. I read extensively the pertinent publications and knew something of the then recognized achaeological cultures. On a late spring Sunday afternoon in 1933, I was escorting a young lady to a concert on the campus when, in front of the library, I suddenly realized that a study of the Oneota material in the Upper Mississippi Valley, Fort Ancient in the Ohio Valley, and Iroquoian in New York and Ontario might result in the recognition of an Upper Mississippi Phase of Mississippian in the terminology of the Midwestern taxonomic method. I excused myself, hurried back to the Museum, and worked until midnight. That was initially to be my thesis topic. I began with Fort Ancient, and Guthe arranged a loan from the American Museum of Natural History of the pottery from Fox Farm collected by Harlan I. Smith in north central Kentucky (32). In working with this material, functional forms such as salt pans, bowls, and jars were easily recognized, and these vessel shapes, along with other ceramic features such as tempering materials, surface treatment, handle forms, and animal effigy heads, made grouping them into ceramic types a relatively easy task. I remember showing my grouping or types to Guthe in the summer of

1933, and saying that we would be able to have pottery types in the East comparable to those in the Southwest. He thought I should write that idea down before I forgot it!

It was apparent early that my original thesis topic would take too long to complete, and I was often diverted to preparing reports on pottery submitted from many locations. A large consignment of pottery from the Norris Dam area in northeastern Tennessee, and one from the Wheeler Dam in northwestern Alabama were received in 1934. The Norris Dam material formed the basis for my PhD thesis in February 1936 (9), and the Wheeler Basin report was completed late in 1936 (10). Following completion of the thesis and a most beneficial marriage to Ruby Fletcher on February 14, 1936, in the University of Chicago Chapel, we pursued additional Fort Ancient collections at the University of Kentucky, the National Museum of Natural History, the American Museum of Natural History, and the Peabody Museum at Harvard. We were diverted for a week to Macon Plateau, where Arthur Kelley was struggling with a wide variety of ceramics from the extensive excavations in Bibb County, Georgia. A number of days were spent in Harrisburg, Pennsylvania, preparing a statement on the Susquehannock material from southern Pennsylvania, and we had a short visit at New Haven, where we saw Cornelius Osgood and his wife, Leslie Spier, Peter Murdock, and met Ben Rouse and Froelich Rainey for the first time. In New York and Cambridge, I saw large collections from New York Iroquoian sites which had never been studied and published.

My manuscript on Fort Ancient was completed in 1938–1939 and turned over to Carl Guthe for reading in early 1939. It reposed on his desk for a year, during which he had turned my writing style to his on only some 20 pages. He then agreed that editing should be left to the Museum editor, and the report was finally ready for distribution in early January, 1944 (11), five years after its essential completion. Another major study was begun in 1936 on the emerging recognition of local cultural sequences from the Plains to the east coast, and from the Gulf north into Canada so that a broad sequential cultural development could be identified over the whole area. This was discussed with students and colleagues in Ann Arbor, at archaeological meetings in a number of locations over a five-year time span, with a final formal presentation at the Andover meeting of the AAA in December, 1941, for a symposium on "Man in the Northeast." It was not published until 1946 (12), so I was able to update and annotate it during the intervening war years.

One of the highlights of the late 1930s was a meeting held in my office in Ann Arbor in the Museum of Anthropology at which a format for the typological identification and description of Southeastern and Eastern pottery was hammered together, using a binomial identification for the pottery type (18). In spite of Guthe's ten-year attempt to establish a major center for ceramic study in the Museum of Anthropology, he agreed only to welcome the 16 participants.

There was clear irritation visible on his part, as well as by some archaeologists in Washington and by W. S. Webb concerning this project. With the further refinement of the type variety system, it has become an effective guide to the eventual delineation of social groups within a small geographic region and within a relatively short time period. Probably the best formulations have been in Phillip's work on the Lower Yazoo Basin (28) and by Toth on the Marksville occupations of Mississippi and Louisiana (35).

In 1940–1941 I was involved with Jim Ford and Philip Phillips in a survey of part of eastern Arkansas and northwest Mississippi to identify the ceramic complexes in those areas before the appearance of the Mississippian complexes. This aim was successful in spite of a painfully low budget. By the time the publication appeared its scope had broadened to include presentations of the physiographic divisions of the area we surveyed, the importance of Mississippi River former courses as an aide to site chronology, a discussion of ethnohistory of the area, the settlement patterns through time and contemporary regional differences, a section on seriation of the pottery, and stratigraphic test results, as well as the presentation of the ceramic typology. The summary and conclusions were not based on our work alone but on integrated data from a broad area of the east. It was a fruitful collaboration even if some lemons appeared in the publication (30).

In early 1944 Carl Guthe resigned from his position as Director of the Museum of Anthropology and Director of the University Museum, and became Director of the New York State Museum. Guthe had been led to believe that the president of the university would support Guthe's recommendation that the head of the Museums Exhibit program should be removed from that office because of incompetence. The president did not do so, and in addition, refused to make a counteroffer to the one that Guthe had received from the New York State Museum. Guthe's recommendation to the president that I should succeed him as Director of the Museum of Anthropology was ignored. For a time requisitions and all other business of the Museum of Anthropology were handled by a budget clerk in the business office. I grew disenchanted with carrying requisitions to be signed across campus and was not provided with a messenger boy's uniform. The regents of the university had become dissatisfied with President Alexander G. Ruthven, and one of them was promoting the appointment of Froelich Rainey as Director of the Museum of Anthropology, while Ruthven was interested in appointing Leslie Spier as both Chairman of the Department of Anthropology and Director of the Museum of Anthropology. It was a stalemate, and I began to act as though I had the responsibility of some of the administrative chores of the Museum. The unfortunate state of affairs at the Museum was brought to the attention of the chairman of the department of botany, Harley H. Bartlett, who had been instrumental in the appointment of Hayward Kenniston as the new Dean of the Literary College. The Dean in his contacts with anthropologists learned that Leslie White had the best reputation of the department members, so he was appointed chairman of the department in 1945, and I was finally approved by the regents as Director of the Museum early in 1946. During one of the regents' meetings in 1945, Ray Baker, the University reporter for the Ann Arbor News, was in the Museum, and I expressed feeling puzzled by their failure to act. His reply was, "Look, Griffin, if Pendergast (the Kansas City Democratic boss) was on this campus, he would lose his pants in two weeks."

The reasons for the above discussion are to give a brief version of the activities involved in a quite minor affair in a university. It is an example that is certainly paralleled many times in universities and other complex bureaucracies. Mature and older professional anthropologists will recognize it as the normal functioning of such an institution. Younger, perhaps more naive, professionals should be aware of some of the problems they are likely to meet in the advancement or hindrance of their careers. Also, it is a recognition of the unforeseen circumstances which led to my appointment. My career would certainly have been much different from 1946 to 1976 if I had been rejected as Director, or if Guthe had not left the University of Michigan.

A major difficulty of being the Director in the late 1940s and early 1950s was that the Museum of Anthropology had very little support from the higher officials allocating university funds. There was no money in the Museum budget for fieldwork, and the current expense amount was at an absurdly low level. Salaries for the Museum staff, on a 12-month basis, were about half those for comparable people on the teaching staff of the college, who were on a 9-month basis. Volney Jones and I were allowed to teach courses in the department in the summer of 1944 and the academic year 1944–1945 without teaching titles, even though one member of the department staff opposed our continuing teaching and being given professorial titles. My spot in the staff budget was filled by the appointment of Albert C. Spaulding, and he was a fine asset until his departure in 1959–1962 to become the second Director of the Anthropology Program of the National Science Foundation.

My attendance at a lecture by Dr. Pablo Martinez del Rio in 1945 and our discussions the next day developed into support from the graduate school to spend six months in Mexico in 1946. I was to collaborate with Eduardo Noguera, the director of the Museum of Anthropology in Mexico City, on a study of the prehistoric connections between Mexican cultures and those of the eastern United States. I had attended the 1943 Mesa Redondo meetings in Mexico City on the connections between Mexico and the Southwest and southeastern United States. My immediate aim was to study the collections from the Valley of Mexico area in the museum in Mexico City, but I soon learned that substantial or even representative collections from the years of work by Mexican archaeologists were either not preserved or not available. As a result, I made collections from Archaic to Aztec sites in the Valley, in the western Cholula area, Tula, and in the state of Mexico. I also examined material from Monte Alban that Alfonso Caso and Ignacio Bernal were studying in a former monastery in southern Mexico City. This served to stir my interest in seeing that magnificent complex and the number of large ruins that were in the Valley of Oaxaca, many of which were occupied at the same times as Monte Alban. This opportunity to see a substantial number of the major highland Mexican population centers and to observe the considerable number of ancillary "contemporary" towns and hamlets gave me a comparative base for the cultural developments in the Southwest and eastern United States. In no way did the United States sites have the development or complexity of those in Mexico.

From my collections and observations I felt that the application of the binomial pottery typology in use in the United States might be an improvement on what had been done earlier in Mexico. Noguera declined to participate, but Martinez del Rio allowed me to work with the collections from Tlateloco in Mexico City. With the addition of material from Chalco, called Aztec I in the then current typology, given to me by Antonieta Espejo, and with her translation into Spanish, two papers were eventually published (21). I associated with many of the Mexican archaeologists, among whom was Miguel Covarrubias. He was interested in examining possible connections with the southeastern United States, but the only one that really stimulated him was on a photographic print I had with me of an engraved shell from Spiro in eastern Oklahoma (29) with individuals paddling a dugout; a portrayal similar to a mural painting on the Temple of the Warriors at Chichen Itza. Ignacio Marquina, Director of the National Institute of Anthropology and History, offered me the opportunity to direct excavations at one of the Tlaxcallan sites, and while I was honored by the offer, I did not feel that my temperament could adapt to a long exposure to Mexican archaeological culture. Among other handicaps, I was not able to even swear effectively in Spanish.

Noguera and I had many congenial hours together, and I went with him on quite a few excursions to Valley of Mexico sites for his course on Mexican ceramics and stratigraphy. We never did, however, get down to serious work together on the program that I had hoped would materialize. The six months' study in Mexico considerably improved my course in Mexican prehistory. I returned with a fairly impressive representative collection of pottery that I used in my teaching; it has also been an aid to Jeffrey Parsons, first in his thesis work and subsequently in his Meso-American course and training of graduate students. It also provided a base on which to build in my subsequent papers on Mexican-Eastern United States interaction. My confidence in such interaction or exchange has over the years been eroded to the point where in my last interpretation I could recognize only the spread of Mexican domesticated plants into the Southeast, some probably from northeast Mexico during the Late Archaic, 4000 to 1000 B.C., and corn arriving during the first millenium after Christ and probably from the Southwest (19). On subsequent trips to Mexico in the 1950s and 1960s, I was able to visit sites in eastern and northern Vera Cruz including Tajin, see some of the materials that Garcia Payon had excavated at the Formative Period site at El Trapiche, and see Chichen Itza and Mayapan in northern Yucatan and Uxmal and Kabah in the Puuc area. In 1955, I was also able to drive north from Mexico City along the highway on the east side of the Sierra Madre Occidental to Santa Fe. We stopped at the impressive site of La Quemada and obtained ^{14}C samples which supported the view that its T-shaped doorways, masonry construction, and some other features suggestive of the Southwest were probably indicative of interconnection between the two areas.

In the summer of 1946, Fred Eggan and I prepared an outline of a festschrift to honor Fay-Cooper Cole on his retirement in 1947 as chairman of the Department of Anthropology at the University of Chicago. Responsibility for editing the volume was turned over to me. If I had known the problems that would arise during the ensuing six years I would not have undertaken the task. The 28 chapters and an appendix on C^{14} dates for the eastern United States were a testimonial to Cole's leadership in establishing the University of Chicago as a preeminent trainer of archaeologists. The first chapter was received in April 1947 and the last one in April 1950. In the late spring of 1950 the complete manuscript was shipped to the University of Chicago Press. The Wenner-Gren Foundation and the Lichstern Fund of the department of anthropology provided substantial support. The contract with the Press provided that any royalties would be turned over to support archaeological research by the department of anthropology, and the editor at the Press for the volume agreed that archaeology would be spelled that way.

In the summer of 1950 I was asked by Robert Redfield and Sol Tax to come to Chicago and have lunch with them at the Quadrangle Club. I was told that the projected cost of the volume was $10,000 and that it was much too long. Redfield asked, "Jimmy, what would you do if you had $10,000? Would you expend it on this volume, or use it for research?" My reply indicated that I thought the volume was the result of years of archaeological research that was worth publishing by individuals who had the best knowledge of their subjects and that it was a valuable summation. I was astounded at their attitude. It was finally agreed that the volume size should be cut one-third and that several chapters should be returned to the authors. However, some individuals were out of the country or otherwise could not be contacted, so the task fell to me. I acquired the mailing lists of many of the state and regional archaeological societies for a prepublication reduced-price flyer. I read proof in July 1952 in Tucson and found to my consternation that the key word in the volume had been changed to archeology. The Press had also changed the contract and eliminated any royalties. By the time the volume appeared the cost was covered by an 11% return by subscribers, and by the subventions. The volume has had five impressions, and had sold 6513 copies as of 1978 and is now permanently out of stock (13). It is my interpretation that the sales represent a vote of 6513 in favor of publishing the Cole volume to two against it.

In the latter half of the mid-1940s W. F. Libby and his associates were developing radiocarbon dating (34). With the assured support of the Viking Foundation for Anthropological Research, the American Anthropological Association Executive Board appointed the Committee on Radioactive Carbon[14] with Frederick Johnson as Chairman, Froelich Rainey, and Donald Collier in February 1948, and in March 1948 enlarged by the addition of Richard F. Flint representing the Geological Society of America (24). I was asked by the Committee to obtain samples of Adena and Hopewell and related material. The results of these first assessments were surprising for they indicated that contrary to archaeological data, Adena was later than Hopewell, Tchefuncte was later than Marksville and that Marksville in the Lower Mississippi Valley, which should be essentially contemporary with Hopewell, was substantially later. Some archaeologists did not believe those results could be correct, including me, while others did (24). On the other hand, I was in error in my estimate of the chronological age of the time periods for Adena-Tchefuncte and Hopewell-Marksville. In the summer of 1949, with support from the Michigan Memorial Phoenix Fund, H. R. Crane of the department of physics began the construction of a radiocarbon counter, and an interdisciplinary committee was appointed by the graduate school to do the curatorial work on the specimens submitted and I was appointed chairman. Professor Crane constructed his counter and within a year it was in operation. I believe we were the second laboratory to produce dates. We functioned for 20 years until the pressure of other duties, inadequate financial support, and other factors caused the highly successful project to be abandoned. We issued reports on our results, first in *Science* and then in the journal *Radiocarbon*, with a total number of somewhat over 2000 dates. The curatorial responsibility of the Museum of Anthropology for the submitted samples provided financial support for quite a few graduate students in archaeology. The dates as they were obtained were of interest to staff and students and provided interesting discussion sessions.

The location of the University of Michigan made knowledge of the glaciation episodes of considerable importance so that with the formation of a Quaternary studies seminar of glacial geologists, botanists, paleontologists, and archaeologists, graduate students in archaeology were encouraged to enroll from the 1950s to the 1980s, and many of them did so. It was not only an advantage to them as an introduction to these supplementary sciences, but it also allowed them to understand something of the problems inherent in those fields.

One proposal that was prominent in the mid-years of the century was that the Eastern United States Woodland "pattern" had been introduced into North America from Northeast Asia (27). This concept, along with the more established derivation of the earliest human New World populations from that area, prompted me to obtain support from the Wenner-Gren Foundation for a study of northern Eurasian collections in Europe in 1953–1954. I particularly wanted to work with Russian and Siberian materials and applied to the Soviet Embassy in Washington for a visa in early 1953. My study of some of the Cape Denbigh ceramics for J. Louis Giddings had rekindled my interest (14), for it seemed possible to be somewhat more precise regarding the possible connections than was the case in the 1930s and 1940s. Needless to say, the Russian visa application was not approved. Nevertheless, some progress was made with the aid of Karl Jettmar in Vienna in 1954 in obtaining some comprehension of the southwestern Siberian prehistoric pottery. In addition, I had been intrigued with Grahame Clark's study of the mesolithic in the North Sea area (4). Visits to Norway, Sweden, Denmark, and England made me increasingly aware of the similarities of the environment in the North Sea-Baltic areas to that of the Great Lakes. Both areas had been glaciated, undergone subsidence and uplift with resultant changes in land-water relationships. Both had gone through a long period of reforestation, and of changes in fauna, with an important effect on the cultural adaptation of populations who moved to the north as the land became habitable. One of the results of this first European trip was the preparation of a manuscript of 50 or more pages on prehistoric connections between Siberia and North America for the International Congress of Prehistoric and Protohistoric Sciences in Hamburg in 1958. It was, of course, much too long, and I asked Grahame Clark if he would consider it for the Proceedings of the Prehistoric Society in England. The manuscript was returned about a year later with a rejection letter, and I submitted a shorter version to *Science* which was accepted (15).

I had been placed on the Permanent Council of the International Union of Prehistoric and Protohistoric Sciences as one of four United States representatives at the reorganization meeting of that body in Paris after World War II. This was probably done by Pedro Bosch Gimpera, whom I had met at the 1943 Mesa Redondo in Mexico City and again in 1946. The Permanent Council met every four years, and I was successful in obtaining support for frequent attendence. In Prague in 1962, where the Congress of the International Union was held, the other three United States representatives nominated me for the eight-man Executive Committee. To my, and their, surprise I was elected, and reelected for a second term, so that I attended most of the biennial meetings until I became an Honorary Member at the Congress in Nice in 1975. These European contacts were of considerable value in initiating the study in Poland of Saraunas Milisauskas and his later excavation program near Cracow, the introduction of Gregory Johnson to European prehistory, and the excavation program of Martin Wobst near Derventa in Yugoslavia.

In 1961, having noticed the approval of a cultural exchange between the United States and the Soviet Union, I applied to the American Council of Learned Societies to study the prehistoric collections in Yakutsk which had been gathered by A. P. Okladnikov from the Lena River, as well as other materials from northeast Siberia and the Lake Baikal-Angara river area in Irkutsk. I received word in April that others had been selected before me. However, within a month or so, another letter informed me that one of the five original selectees was unable to go. Later I learned that the ACLS had been asked by Soviet authorities if they really wanted to send one of the original group they had approved. Whoever it was did not go. I planned to attend the Sixth International Quaternary Congress in Warsaw, then Fred Matson's Ceramics and Man Conference at Burg Wartenstein in Austria, return to Warsaw, and take a train to Moscow on September 15, 1961. I immediately applied to the Soviet Embassy in Washington for a three-month visa, intending to return to Ann Arbor for Christmas. The visa had not been delivered when I sailed on the S. S. Flanders for France. I was told to visit the Soviet Embassy in Warsaw, which I did before and after my trip to Austria, but the visa did not appear. The American Embassy in Warsaw phoned our embassy in Moscow a number of times to determine what was holding up my visa. One of the Russian replies forwarded to Warsaw was "If Griffin is the man he says he is, he will get his visa." By mid-October the Cultural Affairs officer at the Warsaw office asked if there was anywhere else I wanted to go in Europe. So I retreated to West Germany, then north to Copenhagen, Stockholm, and finally to Helsinki, for somewhere along the way I was informed the visa would be at the Russian Embassy there. I spent about a week in Helsinki at the National Museum going over some of the early "Neolithic" pottery from Finland, which is very similar to that from the adjoining Russian area. Finally, a visa appeared in late October, and I was permitted to enter the Soviet Union and arrived in Moscow. I appeared at a formal gathering of the staff of the Institute of Ethnology, for they were my official host while I was in the Soviet Union. I was asked what I wanted to do even though my program had of course been approved by them. I was told that everything I wanted to see was in Moscow or Leningrad and that there was nothing in Yakutsk. Besides, I was told, it was very cold in Yakutsk. When it was suggested that an American ought to be able to stay there a few weeks if Russians lived there through the long winter, the inference was ignored. The real reasons were primarily "political," for major Soviet aboveground tests were taking place in northwest Siberia on Novaya Zemlya and no intruders were allowed to enter Siberia. Some of the reasons were also economic, because of the cost to the Institute of paying my way and that of an interpreter. However, during my three-month stay I did manage to see and work with a considerable amount of material from southcentral and northeastern Siberia. Some part of this work was later published (17).

A major conclusion was that while the earliest pottery in Alaska indeed derived from northeast Asia, about 1000 B.C. as the Norton Complex in Alaska, and then spread east to the mouth of the McKenzie River and south in Alaska, the tradition of pottery manufacture in the Eastern Woodlands or northern Plains did not come from Asia. A sidelight of this research trip is that if anyone reading this statement thinks an American is not presented with opportunities to stray from the paths of the righteous, I can testify that at least one was so opportuned. Both in Warsaw and Moscow, there were phone calls from young women which seemed to represent individual initiative but were so patently part of programmed professional patriots that it was diverting to engage in a bit of banter with the unseen callers. When it is understood that there were no telephone directories in Moscow in 1961, connivance was the only way the calls could have been made.

A program was put together in 1959 to correlate the gradual changes during the Holocene in the Great Lakes area, and particularly in Michigan, with prehistoric cultural changes. Initially the professional directorate was to be Griffin, George Quimby, and Albert Spaulding. However, Quimby was not able to participate because of his obligations to the Field Museum, and Spaulding took a leave of absence from the Museum in 1959 to become the head of the anthropology program of the National Science Foundation. With help from a great many people in the state, able graduate students, and adequate funding from a number of sources, we were able to support three field parties, in the Saginaw basin, on Bois Blanc at the Straits of Mackinaw, and at the Norton Mound Group in Grand Rapids. This was certainly the most extensive excavation program conducted by an institution in the state and perhaps still is. Major excavation reports were published, and the additional studies stimulated by the investigation made the period from 1960 to the early 1970s a rewarding one.

In the late 1950s I attended a lecture on campus by Erling Dorff, who had been captain of the University of Chicago swimming team (1924–1925) when I was a sophomore on the team. He was at the time professor of paleontology at Princeton and he talked on paleoclimates. The last segment of his presentation discussed briefly the Little Ice Age of about A.D. 1300–1500s in northwestern Europe. This set me off on my venture into identifying what North American prehistoric cultural changes might have been at least partly influenced by climatic change. As I associated with professionals in the various fields whose data were vital to identifying the effect of climatic variation on flora, soil changes, erosion, prehistoric agriculture, and other factors, I became aware of the complexity of each of these areas, and realized that our chronological controls in any of them were not adequate at the time to identify specific climatic or weather patterns that could have produced demonstrable cultural change. General trends of increasing warmth, or dryness lasting for millenia or even hundreds of years, inevitably contained fairly long and shorter periods when the trends were muted by variations. By 1963 I had decided that firm results from this approach were not likely to be reached in the near future, with the possible exception of the Southwest, where tree ring chronology provided a much tighter temporal framework than is now possible in most other areas. Since then considerable progress has been made with better excavation techniques in marginal vegetation areas and better identification and interpretational skills in the natural sciences, including the reconstruction of paleoclimates.

Another excavation program launched in the mid-1960s was in southeast Missouri on sites of the Powers phase. I became aware of this Mississippian complex in 1966 through James E. Price, a "native" of the area, when he was an undergraduate at the University of Missouri. A visit to the area in late July of 1967, with Price as a guide, convinced me that it was an unusual opportunity for investigation of a functioning prehistoric society. Powers Fort was the major site, with platform mounds, a palisade, plaza, and indications of houses. Within relatively short distances there were contemporary villages which from surface indications appeared to be members of the same polity. The locations of house structures were easily identified. Furthermore, there did not seem to be evidence of earlier or later occupations, and the surface debris and what was available from some clandestine excavations gave presumptive evidence of considerable interaction between near neighboring groups within a quite short period of time. In some 30 years of viewing sites in the east, I had never seen an area with such potential. Sitting in his vehicle parked at the Snodgrass site, Jim and I roughed out a proposal to the National Science Foundation to support an excavation program with Jim as the field director. A revised request was prepared and submitted to the National Science Foundation, and Price began his graduate studies at Ann Arbor. At that time random sampling was the golden key to unlocking the door to many archaeological questions, and some number of the graduate students, when we initiated the field program, were insistent that random sampling should be the procedure followed. However, we already knew where an adequate number of sites were located, their dimensions, the locations of house structures, and that the surface finds demonstrated rough contemporaneity. At that time there had not been a complete excavation of a Mississippian village or small town. Many sites had been sampled by stratigraphic pits, and a few houses in towns or villages, platform mounds, and cemeteries had been excavated. I thought it was more important to deviate from a then popular acceptable archaeological approach, and so the Snodgrass site of some 90 houses, with a barely discernible palisade wall, inner compound, and short-lived occupancy was almost completely excavated. Its "sister" village, the Turner site, was approximately three-fourths excavated, while the Powers

Fort and a few other villages had a few houses excavated to give additional guarantees of structural and artifactual similarities (31).

Again, a large number of students were able to participate in the pleasures and drudgery of field work, and some were able later to take charge of their own excavations and improve upon what was done in the Powers phase project. One of our strategies was to open large areas of the Snodgrass site, using power machinery to remove the plow-zone in order to reveal more accurately the house structures and also the areas between structures. We also were able to identify activity areas within houses, the essential homogeneity of projectile point forms, variation in vessel features and forms, and their normal location within houses. It was determined that the occupation of the site was short-lived on the basis of the physical evidence, and ^{14}C dates of some 29 samples from four sites on tree branches, while displaying a disturbing length of time, nevertheless clustered around A.D. 1300. A number of subsequent excavations in the Mississippi Valley adopted some of the procedures, modified by their own unique situations and funding. While this effort at complete exposure of subsurface features was not new, it was the first time that many archaeologists in the Mississippi Valley had witnessed it in operation.

The most stimulating excavation program in my recent experience has been, and still is, that of the Federal Highway I-270 project along the east side of the Mississippi flood plain opposite St. Louis (2). It began with an intensive survey of the then proposed right-of-way in 1975. In 1977 the University of Illinois at Urbana-Champaign signed an agreement as prime contractor, with Charles J. Bareis as the general director, and I use the adjective "general" advisedly. Several other institutions in the state were allotted subcontracts. Many of the professional archaeologists of the program had extensive experience in the American Bottom Mississippi flood plain and were also familiar with previous archaeological work within 500 miles, more or less, of the area of their investigations. The central Cahokia site, where most previous investigations has been conducted, has been recognized as the premier Mississippian period population concentration for almost 200 years. Excavations there, while contributing significantly to the attempt to understand the growth and development of the society, still suffered from the results of rebuilding and living activities so that separation of the sequent societies was difficult.

Very little was known about the populations outside the central area and how they might be related to the central area. The presence of Early and Middle Woodlands societies was known primarily from surface material, as well as the even earlier Archaic populations. Systematic studies of the subsistence base, the reasons for site locations, and many other pertinent delineations were not understood. In the published summary report (2), it is stated that almost 100 sites were excavated in the flood plain and the borrow pit locations on the adjoining bluffs. Many of these were not known to professional archaeologists before, especially those covered with alluvial and colluvial deposits. For the first time the geomorphology was studied, correlated with the periods of prehistoric occupation, and the temporal period was determined. Paleobotanical materials testified to the initial dependence upon local plant foods from the riverine environment in the Late Archaic populations and the gradual changes as domesticated plants were introduced, with native North American seed plants becoming domesticated. Tobacco was added in the first half of the first millenium after Christ and maize in the latter half. While there were clear indications of population growth from the third millenium B.C. to the introduction of maize, it is not until that domesticate arrives that the population expands and societal complexity, public works, and other Mississippian features make their appearance. Zooarchaeology studies were comparable in sophistication to those in paleobotany, and bioanthropology studies yielded interesting results on morbidity, diseases, trauma, and the influence of cultural practices on hard-tissue morphology.

Gradual changes in most artifact forms, house construction, village settlement pattern, and pit construction are seen to have evolved slowly, at varying speeds, in an almost unbroken sequence from A.D. 300 to A.D. 1450. It is certainly the most extensive continuing program in a concentrated area in eastern archaeology and has produced the most important results in regard to societal adaptation and structure. As this is written, nine final reports have been published by the University of Illinois Press with some eleven in progress on the excavations. There are other facets that will need to be studied and integrated into the available information from the central site and the interaction of American Bottom prehistoric populations over a large area of the Mississippi Valley. Much more can be learned by the expenditure of over six million dollars than by the comparativcly paltry sums available in the earlier days. My participation in this program was miniscule as an advisor and commentator, and I am most grateful in my retired, but not retiring, years to have contributed in a small way to the major results it has produced.

For a volume of essays in honor of Emerson Greenman, Curator of the Great Lakes division of the Museum of Anthropology, I prepared a paper on "Hopewell and the Dark Black Glass" (16), which provided distributional data, assessment of the probably short time period in which obsidian appeared in the Midwest, differences in utilization between Ohio Hopewell sites and those of the Upper Mississippi Valley, the probable source, and if Yellowstone Park was the source, suggestions on how obsidian might have reached the Midwest. It was an antidote to the somewhat more casually constructed presentation of the diffusion or interaction sphere studies then in vogue. Shortly thereafter I became aware of the potential of neutron activation studies and enlisted Adon Gordus of the chemistry department to do the analyses. I gathered source material from 44 locations in the western United States, 4 in Meso-America, 2 from South America, 2 from Alaska, and others from Europe, the Near East, Iceland, Tenerife, and the Ascension Islands. I included some of the latter group to forestall suggestions that prehistoric immigrants or explorers from the Old World brought the obsidian with them. Sixty-three samples from 30 sites in five states and Ontario were analyzed, and all of them indicated Yellowstone Park as the source (22). What had been a topic of speculation for over a hundred years was thus placed in a much more secure framework. Unfortunately, how the obsidian arrived in the Midwest and its dispersal over the areas is still much less certain. This study apparently had an impact, for a number of papers have since appeared documenting the source areas for galena found in Late Archaic to Mississippian times (36, 37). The sources of Middle Woodland silver and copper have also been identified by their minute amounts of chemical elements (3, 6). It is much better to have such materials accurately pinned down to nonlocal sources because it is then possible to postulate how the raw material traveled. Unfortunately, the techniques of identification usually require expensive equipment and the time of specialists so that most such investigations are short lived.

In a recent manuscript I gave a brief resume of some of the concepts followed at the time of my introduction to archaeological research and for some time thereafter. The statement is quoted below:

> One of the major approaches to interpreting cultural activities was the recognition of culture areas which had a strong emphasis of the effect of environment on the development of material culture traits. This was based on museum studies in the last quarter of the nineteenth and the early twentieth centuries. Along with the identification of the area center went the identification of marginal tribes within and between areas. Trait associations were perceived as an important factor. Since many traits were assumed to have originated in or near the culture area center and spread outward from there, the age area hypothesis came into use. This may be regarded as at the base of the thinking which postulated that the Mississippian phenomenon originated in the central Mississippi Valley and spread outward from there. This is only one instance where anthropological theory led archaeologists astray. It was essentially a timeless framework applied to both ethnology and archaeology or when a temporal sequence was proposed it was not very accurate. The emphases on material culture traits in framing the culture area may be regarded as a forerunner of the University of California Trait Element lists and the Midwest Taxonomic Method.
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> Diffusion was the means by which cultural elements spread. It was accomplished in some instances by borrowing from neighboring groups. A whole cycle of processes was required for the spread of the horse from the Southwest to the north and east and resulted in the several varieties of innovations developed by tribes or tribal groups. It accounted for the spread of maize and other agricultural crops. Trade and exchange were regarded as mechanisms for diffusion. Migration was a form of diffusion. Conquest and colonization resulted in the diffusion of new ideas and behavior.
>
> The psychic unity of man accounted for discovery and invention as similar needs produced similar results. There still lingered remnants of cultural evolutionary schemes of the nineteenth century, and the cultural evolution revival with energy as the engine which produced cultural change and development. Variations were convergent development and parallelism where historical connection could not be demonstrated. The direct historical

approach was utilized over much of the east as well as other parts of the country for many years where early historical records of tribal locations allowed it. Tribal traditions were extensively employed to explain separation of peoples who belonged to the same linguistic stock, and was partly responsible for the interpretation of the movement to the northeast of the Iroquois from a homeland in the central Mississippi Valley. Many archaeologists called for intensification of efforts to obtain a stratigraphic developmental sequence of culture in localized regions.

One of the major difficulties was a lack of adequate funds to explore and delineate village site remains. The long emphasis on mound exploration was giving way however to the excavation of village sites but these were not adequate to recover more than a small proportion of the settlement. Distributional and classificatory studies were pursued, ethnobotanical and faunal identifications were done wherever possible. It was an exciting period in the thirties as old interpretations were tested for their validity, particularly against the increasing evidence of prehistoric cultural depth and gradual change. The arrival of man in North America was pushed back to a period contemporary with extinct fauna at the Pleistocene-Holocene border (20).

Many facets of archaeology have changed a great deal in the last 30 to 40 years, as is true in many other areas of American culture. Much of this must be related to the growth and expansion of the economy and the accompanying population expansion. The research activities that accompanied the increase in students interested in higher education occurred in all fields of science. Existing universities and colleges expanded, and major new schools were established, in order to accommodate the need for instructors and research directors. Where one adequately trained individual was available in the 1930s, hundreds were pursuing careers by the 1970s. Many new developments took place in mammalogy, conchology, paleobotany, palynology, physiography, statistics, and dating techniques to name but a few of them. New theoretical models were proposed in the rapidly expanding fields of Anthropology, some of which were valuable for archaeological research, and some were applied which were either not applicable or could have been used with more caution.

The establishment of the Viking Fund, which later became the Wenner-Gren Foundation for Anthropological Research, provided a source for funds in many subdisciplines and to my own work in archaeology. It was followed about a decade later by the National Science Foundation, followed by the National Council for the Humanities. These federally funded sources enabled research activities to be conducted on a scale not normally possible earlier. Their proposal reviews and panel approval of projects raised the level of research activities in archaeology as well as in other fields. These funding sources enabled American archaeologists to operate in Latin America, Europe, Africa, and Asia in unprecedented numbers. This not only enabled American techniques and strategies to be introduced in many areas but also enabled Americans to incorporate new ideas into the American scene. It enabled American students to become familiar with a large number of areas, problems, and personnel while earlier they had relatively few sources written by little known individuals to broaden their knowledge of prehistory. Similar structural and economic growth was taking place in many countries. This cross-fertilization, dare I say diffusion, or interaction should produce more international similarities in the pursuit of archaeology than was the case in the past.

There has been a burgeoning of archaeological state and local societies in the United States, accompanied by a growth of regional organizations and conferences and national organizations such as the Society for Historical Archaeology, the Society of Archaeological Sciences, and so on. Each has its journals and a remarkable amount of information published, which means that an individual who tries to keep abreast of new programs and data over a large area is in a hopeless position. At one time I had read and knew much of what had been written on Eastern woodlands archaeology, somewhat less so on the Plains, and the Southwest. I had some knowledge of European prehistory and of Mexico.

The impact of federal legislation requiring surveys to determine if locations of highway construction, river and coastline changes, building construction, and pipeline routes would destroy sites has provided extraordinary funding and swollen the number of individuals identified as archaeologists to unprecedented levels. Some of those programs have produced admirable published reports, others are of lesser quality, and some have been disasters for a variety of reasons. Reports by the hundreds have been prepared and turned in to federal, state, and local agencies and private firms; many of these are languishing in files and are known to very few archaeologists. If the stated aims of archaeology are to be served adequately, it is vitally important that interpretation of prehistoric contemporaneous cultural systems be based on the most complete knowledge of the areal extent, density, and functional varieties that is possible. This is in contrast to the point of view recently expressed that "... it has seemed important to work towards filling in the gaps on maps or chronological charts. But except for remote corners and unusual circumstances, the need for descriptive inventories of the remains of human activity during successive epochs is long past" (1). The author then proceeds to identify other important goals and decry the purely descriptive approach. However, the eastern United States still is not adequately known and cannot provide definitive data to answer many legitimate questions of prehistoric research. I suspect that in most areas of the world a similar situation exists. This does not mean that interpretive studies of social organization, ideology, or adaptive strategies should not be pursued if they are regarded as studies which will inevitably have a relatively short life span.

One of my major objectives for most of my academically active career was to establish the Museum of Anthropology at the University of Michigan as a significant unit of the instructional and research activity of the university. In order to do this it was felt that the museum should be a separate administrative entity with its own budget and director. In addition, the staff should increase in size, in wider research interests, in additional space for offices for staff and students, and more space for research collections. The museum staff should be integrated on a half-time academic year basis with the department of anthropology so that its course offerings could provide a prehistoric background for the areal interests of social anthropologists in the department. In addition, the museum should provide a center for graduate students in archaeology to work on collections, their own research, and to be able to interact with each other and the professional staff in an agreeable cooperative social environment. At least some of these objectives have been accomplished. In spite of administrative personnel changes and shifts in attitudes toward the functions of research museums, persistent and reasonably polite presentation eventually established the museum as a significant program. The department's instructional interests were a great help in the staff additions from 1966 to 1974, particularly the efforts of William Schorger to add Near Eastern archaeologists.

An important part of a museum's responsibilities is to have a publication program as an outlet for staff research production, that of students, and of manuscripts of "friends" of the museum. The Museum of Anthropology had the *Occasional Contributions* series, which was established by Carl Guthe in 1932 and published by the University of Michigan Press. In the late 1940s a new director of the University Press was appointed, and publication of that series was abandoned. Shortly thereafter, a request came to me to publish a short manuscript by Frances Densmore that had been supported by a former governor of Michigan, Chase S. Osborn. As a result, the *Anthropological Paper* series was established in 1949, and by 1975 60 numbers had appeared. Later a *Memoir and Technical Report* series was begun, and seven Memoirs and three Technical Reports were issued by 1975. Almost all of the funds for these were obtained from outside sources and from income from sales.

There have been many areas of archaeological research in which I have not actively participated, not for lack of interest in them, but rather for lack of training, adequate skills, sufficient motivation, or because of more pressing duties. Various well- and ill-meaning criticisms have reached my eyes and ears over the years and will probably continue in the future. I am reminded of a presentation given by Alfred Kroeber to anthropologists at the University of Chicago in the mid-1930s which I happened to attend on one of my trips from Ann Arbor to my parents' home in Oak Park. After Kroeber's talk there were a number of questions, and then Radcliffe-Brown took Kroeber to task for his interests in cultural history when he could be doing much more meaningful studies such as those Radcliffe-Brown did. He asked Kroeber "Why do you continue to do hypothetical reconstructions of cultural history?" Kroeber's reply was, "Because I enjoy it. Do you mind?"

Along the path I have followed I have met and enjoyed association with a great many individuals and institutions. From most of these I have directly benefited and I trust such contacts have been of some benefit to them. It has been a long journey with many milestones and not too many millstones.

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