

Jan W. Brown

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THE MAN AND HIS WORKS

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(Author's note: I would like to express my sincere appreciation to the following people for their invaluable information on James Ford's life: Clifford Evans, James B. Griffin, William G. Haag, Betty J. Meggers, Philip Phillips, Robert S. Neitzel, Clarence H. Webb, Gordon R. Willey, and Stephen Williams.)

Edited by: Drexel A. Peterson, Jr.
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James Alfred Ford had an inauspicious beginning for a man who was to make such an impressive contribution to American archaeology. He was born to a railroad worker and his wife in the small town of Water Valley, Mississippi on February 12, 1911. Ford was proud of his humble beginnings. He often used to state that he was just an overgrown country boy (Meggers and Evans 1968: 1262). His father died shortly after Ford's birth, but under the powerful guidance of his mother, Ford's simple beginning was accompanied by a strong puritanical background stressing morality, honesty, hard work, and productivity. This tradition followed Ford throughout his life and was undoubtedly responsible for much of the conservatism in his later works. At the same time, his analytical qualities enabled him to move off on unexplored paths. Robert S. Neitzel, who perhaps knew Ford best, described him as "a very complex character that looked simple on the outside, strong, durable, quiet, even shy, but with a complicated brain going on all the time"(Neitzel, personal communication). The physical and mental intensity of the man seem to have affected all that came in contact with him. "The power of Ford's personality was overwhelming. One remembers the 6'4" Lincolnesque frame, the wide, slightly hunched shoulders, the deep-set, intense eyes. Clearly there was a quality of the messianic about him."(Willey 1969: 66)

As one scans through Ford's voluminous publications, it is difficult to contain the impression that the man was a machine, yet it is too easy to lose sight of Ford in his works. His friends knew and loved him best for his non-academic activities:

He was certainly fun to be with of an evening if one learned soon enough not to try to keep up with his prodigious capacity to take on a fifth of bourbon and remain coherent while all around him (save Neitzel) became much the worse for wear. For Jim, a necessity was good strong coffee taken immediately upon rising, this seemed to clear any lingering cobwebs, and Jim was ready for a full day while others stepped about gingerly hoping not to separate their swollen heads from their less-than-tranquil bodies (Williams nd: 5).

Ford was also a great storyteller and had a strong inclination for practical jokes. Most of the latter activities took place at the American Museum. One of the most publicized jokes was when he and Junius Bird dressed up in a bear skin one winter's night, claws and all, and paraded around the village of Sleepy Hollow, New York. For the next few days the entire village was up-in-arms over the giant animal loose in the area (Webb 1968: 143). Ford seems to have been versatile in all activities, yet he could not understand those who were not. "Ford was a man capable of doing anything, from mapping to repairing vehicles and designing and building whatever was needed, including his own home on Old Sleepy Hollow Road in Pleasantville, New York, and he had little patience with those who lacked those skills"(Evans 1968: 1164).

Impatience and frustration with others who did not stand up to his own rigid code was a foible which Ford was never able to transcend. Much of the disappointment and pessimism he felt in the latter half of his career resulted from this weakness in his character. However, as an archaeologist and a methodologist, he has been surpassed by few. His contributions to Arctic and South American archaeology have been enormous, yet the Southeast owes the most to the labors of this man. Excellently stated by Stephen Williams, "You couldn't work in the Southeast without walking in Jim's giant footsteps." (personal communication)

Editor's Note:

This publication on the life and works of one of the most significant figures in all of American archaeology was submitted to the editor last year and was approved for publication by the officers of the Conference. Subsequently, at our 1977 meeting, the issuance of this work as a separate publication of the Conference at additional cost was approved by the membership.

It is appropriate as a preamble to this study of Ford's life and works to restate the tribute paid to him by the Southeastern Archaeological Conference in 1967. From the "Editor's Note" to the Proceedings of the 24th Southeastern Archaeological Conference published as Bulletin 8 of the Conference (1968):

During the business meeting the following resolution was read, unanimously passed and honored with standing acclaim:

BE IT RESOLVED THAT it is most fitting and proper that on this the occasion of the 24th meeting of the Southeastern Archaeological Conference and the 30th anniversary of the founding of said organization that the membership here assembled in Macon, Georgia, the 11th day of November, send warmest greetings to James Alfred Ford, one of its original founders, and most beloved member who unavoidably is prevented from partaking in this year's Conference, its annual deliberations as well as its extra-mural activities.

BE IT RECORDED THAT the Conference sets down and makes it a matter of permanent record its deep feelings of indebtedness to the significant contributions that Jim Ford has made to southeast archaeology over a period of more than forty years with his abundant publications and his vigorous prosecution of new and challenging ideas. What he has done in the past and what he is doing now on the Formative cultures of the New World will stand as a monument to the strong will and high courage of the archaeologist whose giant footsteps every archaeologist now working in the southeast must follow, whether in total agreement or not. This is a measure of Jim Ford's lasting contribution which we honor with this resolution.

Stephen Williams
William Haag
Resolutions Committee

... A motion was made to dedicate the Proceedings of the Twenty-Fourth Southeastern Conference to James A. Ford. This was also unanimously approved by the members assembled.

--Drexel A. Peterson, Jr.
Editor, 1978

Beginnings

Jim Ford's interest in archaeology and his plans to make a career of it must have occurred very early in his life, as he entered the profession immediately after high school. In 1927, at the age of sixteen, he and Moreau B. Chambers received jobs with the Mississippi Department of Archives and History to dig sites for the purpose of collecting "Indian relics" for the state (Willey 1969: 62). Shortly after this episode, Ford met a young Mississippian by the name of Henry B. Collins who had recently been employed by the Smithsonian Institution. Ford and Chambers worked with Collins at the Deasonville site in Yazoo County, Mississippi in 1927 and apparently impressed the latter so much that he invited them to work with him in Alaska in 1930. In later years, Ford admitted that his explanation and understanding of archaeological seriation was largely conceived and based upon his experiences on St. Lawrence Island with Collins (Willey 1969: 62).

In 1931, Chambers returned to St. Lawrence Island while Ford moved further north to Point Barrow (Evans 1968: 1162). Most archaeological work in this area was confined to a one-month period as late break-up and early freeze-up prevented water traffic. However, Ford missed the boat home and was forced to take advantage of a longer field season (Griffin, personal communication). His activities went beyond those usually attributed to archaeologists, ranging from taking finger and footprints of Eskimos for racial studies (Ford 1932a, 1932b) to participating in the annual reindeer round-up. For the latter job he received meat and a supply of skins for winter clothing (Webb 1968: 136). Ford also had the opportunity to take part in a whale hunt. For his efforts in the capture and killing of a bowhead whale, he was awarded

the skull (it is now on display at the National Museum in Seattle).

Jim must have made quite an impression on the Eskimos of Point Barrow, not only for his size and hardiness, for which he became a "legend" amongst the local inhabitants (Griffin, personal communication), but also for his mental ingenuity. The conversion of a Model T Ford chassis into a primitive snowmobile (Ford 1932a) must have created quite a stir in that part of the north. With the melting of the snow, Ford raced with time to collect as much data as possible. He excavated five sites in the vicinity of Point Barrow, discovering and describing burials dating to the Old Bering Sea Culture (Ford 1932c).

Ford returned from Alaska in 1932 and continued his education at Mississippi College in the town of Clinton where he majored in physics. His strong scientific foundation was in part due to this experience (Evans 1968: 1162). In 1933, he was awarded a National Research Council grant to conduct archaeological investigations in Mississippi and Louisiana (Willey 1969: 62). His first work was at the Peck Village site (Webb 1968: 136), which resulted in the publication, "Ceramic Decoration Sequence at an Old Indian Village Site Near Sicily Island, Louisiana"(1935a). Excavations at this site were not extensive as the midden was very shallow. Most of the pottery described and illustrated dated to the Marksville and Coles Creek complexes. Ford also presented a classification system in this report which warrants mentioning. He arbitrarily assigned a number to particular decorations which he felt were most significant, i. e. types. Those sherds which had the same major decoration, yet varied somewhat in the less (emphasis mine) significant designs, were given the same number but different letters. For example,

there were seven varieties of Type 5 (a-g).

Ford also spent some time working with Frank M. Setzler in 1933 at the Marksville site in east-central Louisiana (Willey 1969: 63). As a result of this work, he published a short article entitled "Mound Builders Were Pit Dwellers" in which he reported the discovery of a semi-subterranean house similar to Southwestern forms (1934). In 1933 and 1934, Ford worked with Arthur R. Kelly at Macon, Georgia where one of the first and largest Federal relief projects was sponsored (Willey 1969: 63). Immediately after this work, the Park Service of the State of Georgia hired him to investigate some "tabby" ruins near Brunswick. The state was in the process of converting these ruins into a state park as they believed it to be the site of the 1680 Spanish Mission of Santo Domingo de Telago. Ford's job was to prove it (Webb 1968: 137). Instead, Ford proved beyond the shadow of a doubt that the "tabby" ruins were a sugar mill dating to around the 1820's (Ford 1937). Needless to say, Georgia was not pleased, but Ford knew he was right and made no attempt to hedge his results.

Ford was accepted to the School of Geology at Louisiana State University in 1934 and worked as both a student and a research associate under Fred Kniffen. At the same time, he continued with his Lower Mississippi Valley studies. In 1935, with the publication, "An Introduction to Louisiana Archaeology"(1935b), Ford took his first stance on the definition of culture. Following Kroeber's superorganic, he believed that culture was merely carried by people. At the same time, he saw that it was responsive to diffusion and changes from within the culture.

The archaeological survey of Mississippi and Louisiana was virtually "completed" by 1935. Along with the Peck Village site excavations (Ford 1935a) described earlier, Ford also published an "Outline of Louisiana and Mississippi Pottery Horizons"(1935c). The chronology of the Lower Mississippi Valley, consisting of three horizons was set up on the basis of ceramic classification--Horizon I - Marksville; Horizon II - Deasonville and Coles Creek; Horizon III - Natchez, Tunica, Caddo, and Choctaw. He admitted that these horizons were just of a general nature as much of the material had not been typed. The material itself was of no importance except as a means to an end in discovering the history and the manner of living of the Indians. Following the above article was "Archaeological Methods Applicable to Louisiana"(Ford 1936a) which stressed that, with the aid of surface collections and correlations of sites with old natural levees and deltas, the time sequence of the Lower Mississippi Valley could be secured. This particular method had been suggested by M. W. Walker in 1932 at a conference in Birmingham (Griffin, personal communication).

In 1936, Analysis of Indian Village Site Collections From Louisiana and Mississippi, Ford's first full-length book, came out. This monumental work, resulting from nine years of work, laid the foundation for Lower Mississippi Valley archaeology. Much was added to the chronology in later years, but very little was removed. In this work, Ford presented the means by which he hoped to reconstruct culture history. His method of attack was to measure time in terms of cultural changes--the seriation of surface-collected pottery--starting at the historic period and working back in time. On the basis of the defined periods, various sites were to be excavated to

give a better understanding of each time level, hopefully to subdivide the time scale even more. This report was concerned only with the first half of this procedure (Ford 1936b: 6). Ford saw this only as a beginning, for without the firm control of chronology systematic archaeology in the Southeast was an impossibility. He was also aware of the limitations and the tentativeness of this approach:

It must be pointed out that a study of this type, with so many unavoidably subjective elements, is full of pitfalls and possibilities of error. And, even if the student is able to avoid the many tempting false trails, the result of his best efforts is still merely a theory (1936b: 13).

The classification system employed in this report had changed markedly from the one he had presented in the previous year (Ford 1935a). Ford introduced an extremely complex numerical system for classifying pottery. There were three parts to each type. The first number represented the motif (plan of the decoration); the second indicated the decorative elements (means used to express the motif); and the last number referred to the specific application (manner of using the decorative elements to form the motifs). A typical type was "11;81;14", yet they often became as complex as "31; $\frac{23}{101/102}$ 1/2"(Ford 1936b: 17, 23). The curious thing about this system was that it actually worked. Ford understood it and used it to produce results, yet few ever made the attempt to comprehend it. It was far too complex and unwieldy.

Ford received his B. A. from LSU in 1936 and continued there with his graduate studies (Willey 1969: 63), remaining as a research associate until 1946. With the completion of his degree requirements, Ford returned to the Arctic with Collins to conduct a survey from Cape Prince of Wales to Point

Barrow. A skin boat was his only means of locomotion in this investigation (Evans 1968: 1162, 1163). Ford returned to the Southeast in 1937. He was hired to work on the restoration of the "earth lodge" at Macon, Georgia; and it was on this project that he met Gordon Willey, a man whom he was to have considerable dealings with in the future. Shortly after this date, Ford was accepted into the graduate program at the University of Michigan. From James B. Griffin he was to obtain a comprehensive knowledge of Eastern archaeology, and from Leslie A. White he was to learn cultural evolution (Willey 1969: 63).

Binomial Classification and Typological Theory

Archaeology in the Southeast rapidly progressed in the 1930's. The great amount of work and the resulting collections were also rapidly getting out of control. Each archaeologist, unaware of the work of others, had his own typological system. Chaos was imminent. The need for a uniform nomological system was apparent, and Griffin and Ford were the two principal agents in bringing it about. In 1937-1938 they called the First Southeastern Conference which was to be held at the Ceramic Repository for the Eastern United States in the Museum of Anthropology at Michigan. The purpose of this meeting was to deal with ceramic classifications (Ford and Griffin 1937). It was at this conference that Neitzel first met Ford. He described the encounter as follows:

Up to that time I don't think anyone knew anything about the Lower Mississippi Valley, and then this big tall glowering fellow got up there and put on some slides of pottery which he had systematized into a logical three-way classification of Marksville, Coles Creek, and Historic Periods. At the end

of the talk some old guy at the meeting got up and said, "Young man, I've sat here and listened to your exposition and I was wondering, do you have any idea of what you're talking about? (Neitzel, personal communication).

There seems to be some question as to when this particular even, or one like it, actually occurred, as revealed in Griffin's response to the above statement:

There was no "old guy" in Ann Arbor. A man in Milwaukee at the SAA and Central States AAA meeting asked Jim if he would please speak up. Jim's reply was, "I will not, it's your business to listen," and Ford continued to mumble. He was not a good speaker, and I think he knew it and stayed away from teaching. (Griffin, personal communication)

The actual events of the First Southeastern Conference were of course not as important as the fact that this first major meeting of the "minds" established the Binomial Classification System (Ford and Griffin 1938) and, in turn, the foundation for Southeastern archaeology (Griffin 1976).

Most of Ford's time at Michigan seems to have been preoccupied with ceramic classifications. As a result of the above conference, and under the close tutelage of James B. Griffin and Carl E. Guthe (Griffin, personal communication), Ford arrived at a typological system which was to remain with him for the rest of his life. He presented his ideas in his Masters Thesis, "An Examination of Some Theories and Methods of Ceramic Analysis" (1938a), a work which unfortunately has never been published. In this volume, Ford set up two steps for ceramic analysis: 1) the archaeologist must do a detailed examination of all the specimens to determine their characteristics, and 2) group the specimens into types on the basis of what he, the archaeologist, feels are the most significant features. The type, according to Ford, was the smallest unit into which pottery could be segregated

for measuring variations due to cultural forces (1938a: 13). Whereas most archaeologists at this time were grouping types together for purposes of ordering and presenting material, Ford maintained that the structure must show the degree of genetic relationships. A group of several types implies they were derived from a common ancestor (1938a: 30). With the discovery of types and their genetic relationships, series, wares, and family trees could be set up which would indicate the change and migration of change through time (1938a: 84). Ford did not lose sight of what these ceramic families were supposed to accomplish though:

The final step of these studies are the translation of ceramic history into the history of cultural spread and development and the articulation of cultural history with the history of the physical groups of man (1938a: 86).

Ford was aware that types were not species and that associated features defined as types often appeared in other combinations. Rather, he saw the type as a mean about which a certain body of material clustered. He agreed with Gladwin and Gladwin (1930), who set up their types in the Southwest on the basis of surface finish and decoration, that types should be formulated from a group of mutually exclusive features. The requirements for types were that: 1) the specimens had in common a number of features which readily distinguished them from other types, and 2) the types had areal or temporal significance (Ford 1938a: 24, 25).

Ford realized that determining what were and what were not significant characteristics in forming types was very subjective, as there were an infinite number of observable features (1938a: 13), yet he felt there was a way in which the archaeologist could approach objectivity. Many archaeologists

at this time were not forming types until the material was completely classified. The smallest units of the classification hence became identified with the significant types. Ford maintained it was necessary to formulate types as soon as the sorting procedure began. This could be done objectively if, and only if, the collection was from a large area rather than a few sites and the archaeologist was familiar with the material of the area before sorting so that he could select the "significant idea groups":

In the final analysis they (archaeologists) are not particularly concerned with the history of each village that may have been occupied within an area. For these reasons selections of types should be based on a knowledge of a comprehensive area, rather than from a series of sites treated as though each were a unique and separate entity. . . .

All of this means that from a wide and comprehensive knowledge within a particular area, the investigator must deductively select the significant idea groups. These groups must be demonstrated by numerous examples of association of specific features. It now becomes possible and desirable to describe new collections in terms of the recognized types which appear in it.
(1938a: 80, 83)

One can easily see the avenues for subjectivity in Ford's proposal. One wonders how the "significant idea groups" are to be selected and also how the archaeologist must proceed when coming into a large area where no archaeology has been completed in the past. At what point in the analysis of his surface collections would he realize that he had found the "significant idea groups" and was hence capable of formulating types?

One other point was brought out in Ford's thesis which bears mentioning, and that is the search for general principles, an obvious manifestation of White's teachings:

The search for general principles which may be expected to underlie the phenomena has been ignored.

Actually, of course, neither induction nor deduction can reach valid conclusions in the discovery of general principles. It is a matter of the combination and balance of the two approaches. The present emphasis on induction is undoubtedly holding up the progress of anthropology toward its ultimate goals, but it is the result of a very common cultural phenomenon. Largely, it seems to be a reaction from the too facile theories of the last century (1938a: 5).

Ford realized that neither deduction nor induction alone could lead to the discovery of general principles, but that they may be attained only by a balance of the two. I had come to a similar conclusion after the reading and rethinking of many recent theoretical contributions in archaeology, yet it was somewhat surprising to see it in print in 1938. Unfortunately, this important work never went to press.

WPA Days

With the completion of his graduate work at the University of Michigan, Ford hurried back to the Lower Mississippi Valley. In 1938, he submitted an article to American Antiquity entitled "A Chronological Method Applicable to the Southeast" explaining, in short, the main methodological concepts already introduced in his earlier work--the use of surface collections in setting up ceramic and chronological complexes and the further employment of stratigraphic excavations to make the chronology more precise (Ford 1936b). Ford was aware that the ceramic complexes were not indicative of separate cultures:

However, it cannot be accepted that these ceramic complexes will represent different cultures or culture phases. It is

entirely possible that two cultures may have used the same pottery, or at different times a culture may have changed its pottery types. What the method attempts to do is to use ceramic decoration, probably the most flexible of the remaining cultural features, as "type fossils" to distinguish the passage of time (Ford 1938b: 263).

Almost immediately Ford received the opportunity to put the second part of his program into operation. The Works Progress Administration provided him with funds and crew to use as he pleased. In 1938, work commenced at the Crooks site and was followed soon after by the Greenhouse excavations. Work continued until 1940 (Willey 1969: 63). The latter operation was one of the largest archaeological projects in North America up to this time consisting of a crew of fifty workmen, two foremen, a timekeeper, six clerks and technicians, and two experienced archaeologists--Robert S. Neitzel and Edwin Doran. The laboratory crew was composed of thirty-two people working under the direction of Gordon R. Willey. The last unit of the project, headed by Andrew C. Albrecht, was engaged in historical research of the Lower Mississippi Valley at the time of contact in order to tie the history and the prehistory together (Ford 1939).

The Crooks Site, A Marksville Period Burial Mound in La Salle Parish, Louisiana, co-authored by Ford and Willey, was published in 1940. Two mounds were excavated at this site providing information on mound stratigraphy and the interment of burials. Along with the Greenhouse site, Crooks served to fill in some of the gaps in the chronological scale. Two more periods, Tchefuncte and Troyville, were added to the three horizons (now periods) presented earlier by Ford (1936b). The work at the Crooks site also served to arouse Ford's interest in the origin of the northern

Hopewellian, a subject which had earlier stimulated the research of Henry B. Collins, Frank Setzler, and W. W. Walker (Griffin, personal communication). It was obvious that the Crooks and the Marksville sites were related to each other and to Hopewell. Crooks also had affinities with Tchefuncte sites in southern Louisiana, which were at this time dated to the bottom of the time scale (Ford and Quimby 1945). Ford concluded from this that Crooks was earlier than Marksville--now proven to be correct--and that the Hopewell culture and distribution started in the Lower Mississippi Valley and spread north¹(Ford and Willey 1940: 138, 141).

The WPA excavations did exactly what Ford predicted they would. The already existing time sequence was validated by the stratigraphy, the chronological gaps were filled in (Willey 1969: 64), and a tighter control of the range and variation of the ceramic types and complexes was attained (Ford and Willey 1939a, 1939b).

Even though this was his ambition, Ford was never one for composing broad regional syntheses. He made one single attempt in 1941 with Willey entitled "An Interpretation of the Prehistory of the Eastern United States, which was based on their earlier work in the Lower Mississippi Valley. It is unfortunate that Ford's first attempt was his last, as this article is considered by some to be "one of the finest interpretive syntheses in American archaeology"(Webb 1968: 39). The junior co-author of the paper continued to employ its format in his most recent text on North American archaeology (Willey 1966).

¹The Marksville derivative may have been the result of a migration from the Illinois Hopewell region (Toth 1966), but its origins and cultural affinities are still largely hypothetical (Griffin, personal communication).

Jim Ford entered Columbia University in 1940 as a doctoral candidate. His area of concentration was still at this time the Southeast. In the previous year he, Philip Phillips, and James B. Griffin made plans to do an extensive survey of the Lower Mississippi Valley. The objectives of this project were to search for the origins of Middle Mississippi culture, to determine the Hopewell-Marksville connections, and to examine in detail the shift from Hopewell into Middle Mississippi (Phillips, Ford, and Griffin 1951: 39, 40). In 1940 and 1941, the program was largely designed to make surface collections and perform limited test excavations. All three authors participated in this work. Ford increasingly developed seriation as a major archaeological tool and was able to put into effect the correlation of sites with river channels, a procedure he had reported upon several years earlier. The survey was continued primarily by Phillips in 1946 and 1947, and the publication appeared in 1951, Archaeological Survey in the Lower Mississippi Alluvial Valley, 1940-1947 (Phillips, Ford, and Griffin 1951).

South America

While at Columbia University, Ford decided to broaden his horizons by moving away from the Lower Mississippi Valley. In 1941, he joined Wendell C. Bennett in an archaeological survey of the Cauca Valley, Colombia (Willey 1969: 64). This was Ford's first introduction to South American archaeology. The results of the survey were published by Ford in 1944, Excavations in the Vicinity of Cali, Colombia. In this volume Ford presented a description of all the sites discovered and those which were excavated,

providing illustrations of the various shaft tombs in the area and their associated pottery (Ford 1944: 13). On the basis of the latter, he divided the region into three complexes (1944: 71). Ford found it extremely difficult to put his typological techniques into operation in Colombia. He obtained significantly large samples from thirty-five surface collections, yet 95% of the sherds were heavily sand-tempered fragments from large globular ollas possessing no distinguishing features and totally defying separation into definite groups. Though the material defied chronological analysis, it might have been useful in the delimitation of activity areas. Unfortunately, Ford either did not consider other applications such as this or else felt they were of little significance.

As with most people, a gap occurred in Ford's career in the early 1940's due to World War II, but with its termination he immediately resumed his work. In the same year that he passed his Ph.D. exams (1946), he received a curatorial position at the American Museum of Natural History, a position he was to hold for eighteen years (Willey 1969: 64). In 1946, his archaeological experience in South America still largely unsatisfied, he joined an expedition which was to operate in the Virú Valley of Peru. The team consisted of seven professional archaeologists including Gordon R. Willey, two ethnologists, a geographer, and a native crew (Ford 1954a).

Ford and Willey were to work in very close association on this project. While Willey was involved in recording settlement patterns, including tracing the development of land utilization, irrigation, community pattern, and architecture, Ford's job was to provide the relative dating of the sites used in Willey's analysis (Ford and Willey 1949: 6). The time scale was to

be formulated on the basis of the excavations of Duncan Strong, Clifford Evans, and Donald Collier. The gaps in the scale, which turned out to be few, were to be filled in by seriation of the surface collections made by Ford. As stated above, the second aspect of his work was to gather and classify collections from the sites examined by Willey and to supply the latter with relative dates (1949: 32). Ford apparently approached his work with enthusiasm:

In this study, which was connected with my own settlement pattern studies, we worked closely. Since then, I have often thought that my settlement sample might not have been so heavily weighted on the side of high rocky-crag fortifications if Jim had not been urging me on in the exploratory spirit, to rise to these mountain-climbing challenges (Willey 1969: 64).

Surface collections had never been employed in Peru prior to Ford's appearance. The chronology of the Virú Valley had formerly been set up by Max Uhle and A. L. Kroeber on the basis of grave lot comparisons (Ford and Willey 1949: 16, 17). However, Ford found the existing chronology to be of little use in this study, as the beautiful painted ceramics from the North Coast of Peru, so often seen in museums, averaged only about 1% in the occupation refuse. Also, as the utilitarian wares differed from the burial offerings, it was impossible to correlate cemeteries with nearby buildings (1949: 32). In the analysis of the excavated material, Ford began to be able to distinguish plain types on the basis of differences in paste, composition, surface finish, hardness, thickness, firing, and decoration (when present), and noticed that these types were constantly appearing in the same order in the strata. The Virú Valley chronological system was thus formulated almost entirely on the basis of plain sherds (1949: 41).

In his earlier work, Ford had relatively little interest in undecorated ceramics. In the Peck Village site excavations they were not even considered in the analysis (1935a: 7), and in the overall survey of Louisiana and Mississippi in the early 1930's, only those sherds which would yield information on vessel decoration, shape, temper, or appendages were saved (Ford 1936b: 11). The formulation of plain pottery types had a tremendous effect on Andean archaeology, where previously plain sherds had either been thrown away or separated on the basis of gross distinctions (Evans 1968: 1164).

... as a result of his methodology, which is known all over Latin America as "el metodo Ford," some sound sequences have been developed in areas that remained before without sequences and with the people believing that nothing could be done with their plain wares or chipped stone artifacts....
(Meggers and Evans, personal communication)

James A. Ford and Albert C. Spaulding

The 1950's started out rather quiet for Ford. The volume he shared with Phillips and Griffin came out in 1951, and in the same year the long-awaited Greenhouse report (Ford 1951) reached the press. The latter volume, though rather sketchy, is still the only published report on a Coles Creek mound. In 1952, Ford composed a popular account of the prehistory of the Lower Mississippi Valley including an illustration of the use of seriation for Scientific American, "Mound Builders of the Mississippi" (Ford 1952a). He also submitted to the Encyclopedia Americana an article entitled "Mound Builders and Mounds" which dealt with a description of burial mounds throughout the world and the temple mounds of America (Ford 1952b).

In 1953, he wrote a short article summarizing the various projects occurring in the Western Hemisphere (Ford 1953), and this was followed in the subsequent year by "The History of a Peruvian Valley"(Ford 1954a), a synopsis of the previous work in the Virú Valley of Peru.

Ford's life was not meant to be blessed by tranquility however.

In 1952, he published "Measurements of Some Prehistoric Design Developments in the Southeastern States"(Ford 1952c), a paper which was originally supposed to be a chapter in the 1951 volume (Phillips, Ford, and Griffin 1951) except that "Griffin couldn't stomach it"(Williams, personal communication). Griffin comments that he,

...objected to it on the grounds that the conclusions were not valid in terms of Ford's fixation on everything spreading East, West, and North from the Red River mouth. I had had enough with the misplacing of many sites in the seriation graph and the ignoring of stratigraphy and intrusions in pits in these graphs. (Griffin, personal communication)

This report had two main objectives: 1) to correlate and align the ceramic chronologies of different areas, and 2) to study the changes in the pottery, indicative of cultural processes, by tracing and measuring (quantitatively and qualitatively) the evolving types through time and space (Ford 1952c: 318, 319). Ford selected and studied nine chronologies in this work.

Albert C. Spaulding challenged the above article by pointing out another approach to dealing with the data, thus placing Ford on the firing line (Spaulding 1953a). Ford was a firm believer in gettings discussed, though not necessarily settled, before ideas were released to the public (Neitzel, personal communication). He felt that the literature was confusing enough without personal tirades flowing freely through the publications

(Ford 1952d), yet he apparently did not adhere to his own advice in the ensuing "battles" with Spaulding. Ford published a review of "Spaulding's Review of Ford" in the American Anthropologist in 1954, arguing that Spaulding, just doing synchronic studies of sites, believed they would be connected by "logical truths". Ford had his doubts that a connecting device for a study of this sort would ever be found: "I am somewhat more uncertain than Spaulding that nature has provided us with a world filled with packaged facts and truths that may be discovered and digested like Easter eggs hidden on a lawn"(Ford 1954b). The accumulation of facts without the guide of cultural theory could only result in chance contributions to the solution of cultural problems (Ford 1954b).

Ford also attacked Spaulding's "Statistical Techniques for the Discovery of Artifact Types"(Spaulding 1953b) in a review sent to American Antiquity (Ford 1954c). He believed that Spaulding's procedure for setting up types from a single village site collection was slightly naive--a view he had expressed earlier (Ford 1938a)--as only time and place could be dealt with in a study of this sort. He gave the example of Woodland pottery to demonstrate that, with culture, there is no break in time and space. Culture is dynamic and so must also be formulated types. Ford was cognizant of the problems involved in a dynamic typological system. The difficulty existed in forming fixed boundaries for the type, as the range of features of the type definition could not be fully illustrated on graphs. Much depended upon the capability and consistency of the archaeologist (Phillips, Ford, and Griffin 1951: 66, 68; Ford 1952c: 322).

To make sure he got his point across, Ford presented again his conception of the type in "On the Concept of Types: The Type Concept Revisited" (Ford 1954d), which was essentially a continued attack of Spaulding's method. In this article Ford agreed that order will be found by doing synchronic ceramic studies, but this would not provide information on historical processes (1954d: 43). Ford felt that static studies could be fruitful but only after the chronology was securely controlled (Ford 1952b), but "the ethnographic view of a culture resembles a snapshot taken in the middle of a race for it is a static view of a very fluid process"(1954d: 51). To prove his point, Ford created the mythical Gamma Gamma culture and excellently demonstrated how the norms (types) of this culture changed over time and space (1954d: 47, 50). He did realize, however, that, in order to have historical significance, types had to have a limited (emphasis mine) range in time and space (1954d: 43).

Ford was convinced that types did not exist in culture, nor could they ever be discovered by competent methodologies (1954d: 42). He was not at all concerned with the meanings the various decorations had in the minds of the makers. He saw this as sheer guesswork and of no concern to students of cultural forces (Ford 1938a: 21, 84). Ford felt that the type concept that he used could best be applied to the reconstruction of culture history in time and space. However, he was well aware that this purpose was not the ultimate goal of the archaeologist:

This tool (type concept) is designed for the reconstruction of culture history in time and space. This is the beginning and not the end of the archaeologist's responsibility. After culture history has been outlined various other methods of

classification become possible and may be designed to measure different facets of the culture history (Ford 1954d: 52, 53).

It was of extreme importance to Ford to establish precise chronology first. Only after this has been accomplished should methods such as those presented by Spaulding be applied.

The Ford-Spaulding disputes tapered off after 1954, yet in their prime the "battles" often attained high levels of ferocity. A statement by Spaulding serves as an appropriate epilogue for this heated section:

This glimpse into Ford's personal demonology reveals a nearly half-witted Middlewesterner whose archaeological activity consists of an aimless and endless pigeon-holing of artifacts in compartments tagged Heavy, Light, Hard, Soft, Big, Little, and so on, varied by an occasional excursion into the meaningless exercise of writing the names of sites under such headings as Focus and Aspect. (Spaulding 1954).

Return to the Lower Mississippi Valley

Upon the completion and subsequent publication of Ford's work in Peru, he began to become interested in new developments occurring in the Lower Mississippi Valley. The Poverty Point site in Louisiana had always been somewhat of an anomaly, as great quantities of material were discovered there which lacked affinities with other areas. In the late 1940's artifacts similar to those at Poverty Point were beginning to be found at various sites throughout the Mississippi Valley. The most famous of these was the Jaketown site near Belzoni, Mississippi. A bulldozer cut at this site, revealing a thick, rich midden deposit containing Poverty Point material immediately below the ceramic levels, served to bring Ford, Phillips, and William G. Haag to the scene. Excavations began in 1951, and the joint publication was released in 1955 (Ford, Phillips, and

Haag 1955). Ford had a somewhat different role at this site. While Phillips worked on the ceramic classification, Ford carried out the excavations. He was also responsible for writing the section on the recent geography of the site (1955: 5). It was somewhat reassuring that Phillips' classification reaffirmed, rather than changed, the ceramic chronology presented by Ford in 1951 (1955: 151).

Ford next went to the Poverty Point site itself where he worked with Clarence H. Webb for three field seasons--1952, 1953, and 1955. He also found some time in 1953 to make a trip to Alaska. While at Poverty Point, Ford's mechanical ingenuity was constantly at work. He tinkered with a bulldozer, adjusting a backhoe blade to cut trenches by taking thin slices. He created a mechanical dirt sifter attached to a gasoline engine, a description of which was published by him and Junius Bird in American Antiquity (1956). He even developed an artifact-gathering machine which would loosen the soil, gather it, and pass it through a sifter. Unfortunately, a description of this invention never reached the presses¹(Webb 1968: 141)

Three articles and a monograph were the results of the work at Poverty Point. "Additional Notes on the Poverty Point Site in Northern Louisiana"(Ford 1954e) briefly summarized the work completed by Webb and Ford and included the aerial photo discovery of six octagonal concentric

¹Editor's note: the machine was, however, built and even tried on sites near Helena, Arkansas. Its success was limited, and the machine was left for storage, never to be reclaimed, at the museum at the Chucalissa site in Memphis (now the C. H. Nash Museum of Memphis State University). Drawings and even parts of the machine, converted into other machines, are preserved there (Gerald Smith, personal communication).

earthworks, the proof of the artificiality of the Poverty Point mound, its unusual bird-like shape, and the securing of a geophysically derived date of 1500 B. C., corresponding to the radiocarbon date of 2350 \pm 80 B. P. for the Jaketown site. A brief article appeared in Science (Ford 1955a) in the following year as well as a popular account in Natural History entitled "The Puzzle of Poverty Point"(Ford 1955b). The final volume, presented in 1956 by Ford and Webb--Poverty Point, A Late Archaic Site in Louisiana--was a detailed account of their 1954 article discussing and describing the ten test trenches placed in what they thought were octagonal ridges, the mound excavations, and the resulting collections.

Ford always realized that the professional archaeologist had a responsibility to the public. Almost every major work he presented to the academic community was accompanied by a popular work for the layman. Even in the popular accounts he still maintained a high professional and intellectual caliber. One example of this was "Early Man in America" (Ford 1957), a short two-page article which summarized the prehistory of America from 8000 B. C. to A. D. 1500, running through the four stages of Folsomoid, Archaic, Burial Mound, and Temple Mound.

Ford became involved in a number of various projects in the late 1950's and early 1960's, ranging from investigating early man sites in the Texas Panhandle to participating in a teaching symposium in archaeological field methods at the Universidad del Atlantico in Baranquilla, Colombia (Willey 1969: 65). By publishing in 1959 Eskimo Prehistory in the Vicinity of Point Barrow, Alaska, he even did some mopping up work. Ford labored in this area in the early 1930's, yet the United States National Museum did

not have funds available to have him work on the collection at that time. In 1952, he had the material and field notes sent to the American Museum where he worked on them intermittently for seven years (Ford 1959: 16, 17). Had this work appeared in the 1930's it would have had a profound effect on Arctic archaeology.

In 1958, Ford was asked to take over the excavations at the Menard site in Arkansas. The results of this work appeared in the Menard Site: The Quapaw Village of Osotouy on the Arkansas River published in 1961. In the late 1950's Ford made plans for another survey in the Lower Mississippi Valley, the purpose being to discover preceramic sites. The discovery of Dalton points on the same river channels in southeast Missouri and in the Cache Lowland in Illinois had stimulated Ford's interest (Williams, personal communication). Geological changes in the valley floor could by then be traced back 13,000 years, and so he had strong hopes of correlating sites with old river channels (Ford 1961a). The Dalton Project, the result of this stimulation, was started in 1960 and continued until 1963 chiefly under the direction of Alden Redfield (Redfield nd).

Ford was also active in Peru in 1958 and 1963 (Neitzel, personal communication) conducting archaeological surveys and excavations in the valleys of Chira, Piura, and Lambayeque and developing ceramic chronologies for each of these regions (Evans 1968: 1165). These commitments did not prevent him from tackling other work, as he was also busy excavating burial mounds near Helena, Arkansas in 1960 and 1961 (Willey 1969: 65). A short report appeared in the Arkansas Archaeological Society Newsletter (Ford 1961b) describing progress in this work at Helena Crossing, and

the full volume appeared in 1963, Hopewell Culture Burial Mounds Near Helena, Arkansas. This report was Ford's last work published by the American Museum.

It is obvious that Ford was extremely restless at this point in his life. He constantly jumped from one project to another, from one continent to another; and his reports which came out at this time (Ford 1961d and 1963) greatly suffered from this lack of attention. It seems as if Ford had been looking for something, a challenge perhaps, which would sustain his interest. His restlessness is indicative that these projects were not the answer.

The Type-Variety Threat

Philip Phillips, working in the Yazoo Basin, Mississippi from 1949 to 1955, started to derive a new typological system on the basis of his work. The applicability of the Type-Variety approach did not hit the literature with any great force until Phillips' monumental work (1970), yet its potential was making its way around the academic circles in the late 1950's. Ford, the initiator of so many new approaches, was one of the most avid opponents of the Type-Variety system. Perhaps because the Binomial Classification had had such a hard birth, he was reluctant to change it. Ford's oppositional excuse was that it would confuse the literature. From his personal experience (1936b) he feared a system which, on the surface, looked as though it might have complicated the situation rather than clarifying it. Quoting Gladwin (1936: 259) Ford stated:

This does not mean that the finer distinctions will not be made as knowledge increases; nor that such distinctions would not warrant publication when they could be shown to indicate cultural or chronological relationship. It does mean that no method is as important as the result which it is designed to achieve, and that it is possible to defeat both the purpose and the method by too great elaboration of non-essential details (Ford 1938a: 28).

Ford responded to the Type-Variety system in a short article printed in American Antiquity, "In Favor of Simple Typology"(Ford 1961c). His main criticism of this new approach was that varieties were now being used as types had been in the past with only the terminology altered. The type had just been elevated to a higher level, possessing a number of varieties below it. As long as he could talk about a type being early, middle, or late, it seemed unnecessary to him to bombard the literature with a host of new names. Ford felt that this was a waste of time and therefore a threat to his code of productiveness (Neitzel, personal communication). His view on the subject was excellently presented in a discussion he had with Stephen Williams, "I don't know why you and Phil (Phillips) spend so much time with these varieties, when it can only be interesting to about five or six people in the whole universe"(Williams, personal communication). Neitzel was prone to agree with Ford. His recent work at the Fatherland site in Mississippi served to decrease the number of types compared to his earlier research (Neitzel 1965), yet the order in which they meshed and were arranged had not changed significantly. He felt he could still make the old system meaningful to both himself and others (Neitzel, personal communication).

Ironically, it should be mentioned that the typological system Ford presented in the Peck Village site report, consisting of a number for each type and letters for each variety (Ford 1935a), was not far removed from the Type-Variety system. However, Ford had had a discouraging experience with a complex typological system (1936b), and this apparently closed his mind to seeing any value in the latter approach. This was unfortunate because ceramic varieties not only better demonstrate the genetic relationships so earnestly hoped for by Ford, but they also do much for alleviating the problem of the dynamic type as they provide a much greater control over time and space.

The American Formative

The early 1960's were not the best of times in Ford's life. Not only was the Binomial Classification being challenged, but so also were his cultural theoretical foundations. From his work at the Universidad del Atlantico in Colombia, Ford prepared a short manual explaining the use and development of the concept of seriation. This was published in 1962 in Spanish and English, "A Quantitative Method for Deriving Cultural Chronology." Morris E. Opler (1963) and Robert Ascher (1963) each wrote a scathing review of this work, attacking Ford because he regarded "human-kind as a passive instrument through which an impersonal evolutionary process realizes itself" (Opler 1963: 900). Admittedly Ford followed Kroeber's superorganic, but to suggest that he was unaware of the manner in which culture was passed was to ignore his earlier publications:

"... culture may be briefly defined as a stream of ideas, that passes from

individual to individual by means of symbolic action, verbal instruction, or imitation"(Ford and Willey 1949: 38).

In examining cultural history, which was what this manual was intended for, it was the superorganic not the free will of some individuals which had to be studied (Ford 1964). As Ford would often say, "A human being is just a flea on the back of a dog"(Williams, personal communication).

Ford, the wounds still healing from an earlier tussle with Spaulding, was hesitant to enter into the arena of conflict again, not because he feared defeat, but because he empathized with the public. He presented the comical "code duello" in his article, "A Whimper from a Pink Granite Tower":

If vigorous attacks, charges and counter thrusts are to continue to enliven the pages of our professional journals, a set of ground rules, comparable to the code duello, should be developed to make the process more efficient and the oneupmanship rewards of the winner more apparent and enjoyable. First, advance copies should be sent the attackee, either by mail or through the intermediary of a mutual acquaintance, leaving time for fomulation of a reply. Replies to replies could honorably be exchanged and only after the spilling of this preliminary gore should the contending parties arise before and converge on a field of honor such as the American Anthropologist. There, accompanied by seconds and a capable chirurgeon, they should be allowed one shot each and in the same issue. Think what the innocent public would be spared in the way of confusion--if nothing more (Ford 1964: 399).

Ford continued to become more depressed and disappointed with his lot in life. He felt that everything had been completed in the Lower Mississippi Valley, and so he stopped going to the Southeastern Conferences (Williams, personal communication). He also became very dissatisfied with the American Museum at this time. Ford, a firm believer in "publish or perish", became frustrated and angered at the lack of productiveness around him (Neitzel, personal communication). When one considers that

such industrious archaeologists as Junius Bird and Gordon Ekholm were employed at the American Museum, one can well imagine what Ford's standards were like. It is ironical that the two greatest honors Ford received in his life--elected President of the Society for American Archaeology for 1963-1964 (Willey 1969: 65) and awarded the Spinden Medal for noteworthy, controversial concepts (Haag 1968: 32)--were to come when he was the most depressed.

As a result of his contact with Betty J. Meggers and Clifford Evans, Ford's attitude began to change somewhat. They were interested in inter and intra-continental contact (Meggers and Evans 1957, 1962, and 1965), a subject which kindled Ford's imagination. "Up to then he was complaining all the time that he hadn't had a productive idea in so long that he was afraid he would never have another one"(Neitzel, personal communication). Williams felt that he also may have had some role in Ford's new lease on life:

At one time, back in the early 1960's, I was examining and comparing the Marksville pottery to some Issaquena village pots when I noticed that some of the rim designs were just vertical or diagonal lines, rather than cross-hatched. I mentioned this to Ford who, having classified the collection many years before, apparently had forgotten about this. Jim started thinking, bringing to mind similar designs on rims of fiber-tempered sherds and on steatite vessels found at Poverty Point. Immediately he came up with the sequence that Florida fiber-tempered pottery developed into Poverty Point, which hence developed into the cross-hatched rim of the Marksville Period. Later this design moved down the rim. He then thanked me for the idea, credit which I neither deserved nor wanted (Williams, personal communication).

The response to the above stimulations was an archaeological survey on the Veracruz coast of Mexico, where Ford hoped to find clues for

Mesoamerican-Southeastern diffusion (Willey 1969: 65). His untimely sickness cut short the Veracruz project. It was Ford's last project for the American Museum before taking up residence as the Curator of Archaeology at the Florida State Museum in 1964 (Haag 1968: 32). The results of this work combined with his continual contact with Meggers and Evans and his growing familiarity of the ceramics of Georgia and Florida led to the conception of the "American Formative".

Ford first presented this theory in "Early Formative Cultures in Georgia and Florida," an article which demonstrated Ford's conviction that the fiber-tempered ceramics of the Florida and Georgia coasts had their origin in Colombia (Ford 1966). In this same year, Ford called a conference in Gainesville, Florida to argue out some of the problems in this recently conceived theory (Willey 1969: 65). Apparently the conference had only positive effects on Ford, as three years later his famous (possibly infamous) volume was released, A Comparison of Formative Cultures in the Americas: Diffusion or the Psychic Unity of Man? (Ford 1969). In this work Ford proposed that almost all the major cultural changes in the Americas resulted from migration and/or diffusion from the more advanced regions within the Western Hemisphere and ultimately from transpacific contacts with Asia. Williams felt that this work was entirely antithetical to Ford's previous writings, as searching for events and diffusion was adverse to the basic theory of cultural evolution (Williams, personal communication). Williams was correct in respect to White's cultural evolution, but the latter was not the only individual responsible for the development of Ford's mental processes. Kroeber was also of extreme importance to

Ford's understanding of culture, and the idea of diffusion was prevalent at the beginning and throughout his career (Ford 1935b; 1952b; 1961b: 14).

The various experiences of the 1960's fused with his earlier interest in the subject and led to the daring proposals of his last two publications. Ford did not crusade his theory though: "Jim never held any big candle over the thinking he was doing. He merely suggested that this was good subject matter for about thirty Ph.D. theses in his outline, if they could follow it from there. He was just blazing the trail"(Neitzel, personal communication).

Ford's final gift created a theoretical rift in American archaeology. Meggers and Evans, as expected, overwhelmingly accepted his thesis. James B. Griffin opposed the ideas from the beginning but realized he could not convince Ford differently (Griffin, personal communication). Gordon R. Willey felt much of what Ford proposed will one day be accepted (Willey 1969: 67). Stephen Williams, observing that all the recent papers at the Southeastern Conferences take off from Ford's work as fact, felt that Ford did a terrible disservice to American archaeology in this particular work (Williams, personal communication). Whatever the consequences of this scheme, it promises to enliven the archaeological literature for quite a few years. The man who conceived it, controversial throughout his life, loved and hated, but always respected, unfortunately did not live to defend it. James Alfred Ford died on February 25, 1968 at the age of fifty-seven.

Conclusion

Jim Ford will probably be most remembered for his methodological contributions. The development of his particular brand of archaeological seriation did much to integrate the vast amount of accumulated archaeological data. Critics have often pointed out that things do not change as smoothly as depicted in Ford's battleship curves, yet few would argue that this method did not provide a solid chronological foundation. Ford's greatest gift was supplying to the East what the Southwest had twenty years earlier--"the nerve of history"(Willey 1969: 66, 67). The cultural sequence of the Lower Mississippi Valley, based on ceramic analysis and formulated during the WPA days, has stood the double test of time and correlation across the Southeast (Webb 1968: 139).

Ford was often reproached for his constant emphasis on pottery instead of other aspects of culture, but his answer to this criticism was simply, "Go ahead, fine, but let's not forget that the abundance and wide presence of potsherds offers us a means of cross-regional comparisons that will link the entire East"(Willey 1969: 66). Ford wanted to go beyond typological formulations though. His main desire was to show the flow of history as wide and as deep as possible. A great ambiguity underlaid Ford's entire academic career. As a cultural evolutionist he wanted to express archaeology and anthropology scientifically by quantitative and empirical means; and, with his firm belief in the superorganic, he saw people as the carriers, rather than the causative agents, of cultural change. Yet at the same time, he never went beyond the simple percentage formulations in his computations although more advanced statistics were available.

Perhaps Ford was more interested in the content and the qualitative aspects of culture than he would care to admit. "Like Twain, Ford was a romantic who scoffed bitterly at romanticism, proclaiming the practical"(Willey 1969: 68).

Ford's career was marked by extreme productiveness. He had decided early to devote his life to research rather than teaching (Neitzel, personal communication). Undoubtedly this had much to do with his resourcefulness, yet there was more involved than this. Few could compete with the quantity and quality he put forth, thereby setting a standard for all archaeologists. Very few of his projects were left undone. Even his earliest work in Alaska eventually reached the press. The Marksville site always haunted Ford. Though it was Setzler's responsibility, Ford always felt guilty that this report was never published. The drive and selfless gift of himself to his chosen profession will be remembered and valued. The scene on his deathbed speaks better for this than I:

Jim was bedridden for the last year of his life. Most of his work was done from the bed, his secretary constantly doing dictation. Whoever was around would hold up charts on racks so he could view them from the bed.

The last time I (Neitzel) saw Jim, I had gone to pack and send off the plates to the Smithsonian for publication. This was the last thing which had to be done on his book. Jim was just a ghost of himself. He was almost a travesty of the big husky man he had been. There was no meat left on him at all. He said, "Well . . . I've got to have something else to do now." He didn't want to talk about dying or anything like that. He knew as well as anybody how much time he had and all that. He said, "Now we can get down to business. It won't be quite as expansive as this thing we just finished, but you have to be my legman."

I said, "Okay, what do you want to do?"

He said, "That's right--the Marksville report." He died about two weeks later (Neitzel, personal communication).

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