Introduction

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The creation of the National Research Council (NRC) in 1916 reflected a growing concern that the United States was ill-prepared to enter a war into which it was inexorably being pulled. The council's express purpose was to assist the National Academy of Sciences (NAS), which had been signed into existence by President Abraham Lincoln in 1863, in advancing the cause of knowledge and advising the federal government on matters of science and technology. From its inception, the NAS had undertaken a wide variety of studies for different branches of government, but by the second decade of the twentieth century it was obvious that the body was too small to deal effectively with the exponential growth of science and technology taking place not only in the United States but also in Europe and Russia. Members of the NAS, including the outspoken astrophysicist George E. Hale, who served as the organization's foreign secretary, saw this scientific and technological explosion as a potential threat to the security of the United States. At Hale's instigation, members urged President Woodrow Wilson to create a body that could broaden the scope of the NAS and coordinate efforts among government, industrial, and educational organizations to strengthen not only national defense but the security of American industry as well (Cochrane 1978; Hale 1916, 1919). Hale was made the first chairman of the newly created council, which drew its membership from universities, private research institutions, and various branches of government. After the war, the NRC was made a permanent body when President Wilson signed Executive Order No. 2859 on May 11, 1918.

This was how Vernon L. Kellogg, permanent secretary of the NRC, saw the charter of the organization:

> The council is neither a large operating scientific laboratory nor a repository of large funds to be given away to scattered scientific workers or institutions. It is rather an organization which, while clearly recognizing the unique value of individual work, hopes especially to help bring together scattered work and workers and to assist in coordinating in some measure scientific attack in America on large problems in any and all lines of scientific activity, especially, perhaps, on those problems which depend for successful solution on the cooperation of several or many workers and laboratories, either within the realms of a single science or representing different realms in which various parts of a single problem may lie. It particularly intends not to duplicate or in the slightest degree to interfere with work already under way; to such work it only hopes to offer encouragement and support where needed and possible to be given. It hopes to help maintain the morale of devoted isolated investigators and to stimulate renewed effort among groups willing but halted by obstacles. (NRC 1921:6)

Until 1943 the NRC was divided into two broad sections, one concerned with relationships with the government and other bodies, and the second representing specific scientific disciplines. Each section was subdivided into divisions, with the membership composed of representatives of scientific societies and various government departments. One division on the scientific side of the house was the Division of Anthropology and Psychology, which during its lifetime oversaw the creation of 55 committees, each charged with specific tasks dictated by members of the division's executive board. As one might expect given the diversity of subject matter subsumed under the broad rubric of anthropology and psychology, the committees were diverse in terms of purpose. For example, a Committee on Accurate Publicity for Anthropology was established in 1928, a Committee on Pelvic Structure in 1926, a Committee on Psychology of Highway in 1922, and a Committee on Vestibular Research in 1921. There was even a proposal in 1919 to create a Committee on Morality, but that idea was soon abandoned. For modern students attending their first American Anthropological Association meeting and feeling that the discipline has lost its focus, it might be comforting to know that things were not completely different in the 1920s.

The Committee on State Archaeological Surveys

One of the first committees created within the Division of Anthropology and Psychology was the Committee on State Archaeological Surveys (CSAS) in 1920. Clark Wissler (Figure 1), curator of anthropology at the American Museum of Natural History in New York City and chairman of the Division of Anthropology and Psychology, reported on the formation of the committee:

> A committee was appointed to encourage and assist the several States in the organization of State archaeological surveys similar to the surveys conducted by the States of Ohio, New York, and Wisconsin. The chairman of this committee is R. B. Dixon, of Harvard University. The plan contemplates the coordination of all the agencies within those States, enlisting the cooperation of local students and interested citizens so that an effective appeal may be made to the various State legislatures for special appropriations for these surveys. (NRC 1921:53)

Roland B. Dixon (Figure 2), a hybrid ethnologist-archaeologist who was on the faculty at Harvard and was curator of ethnology at the Peabody Museum (Harvard), and who had served as president of the American Anthropological Association in 1913, was joined on the CSAS by Berthold Laufer, an expert on Chinese art and material culture who was on the staff of the Field Museum of Natural History in Chicago, and by C. E. Seashore, a neuropsychologist at the University of Iowa and the man who would succeed Wissler as chairman of the Division of Anthropology and Psychology. Subcommittees were created in four states—Illinois, Indiana, Iowa, and Missouri (Indiana Academy of Science 1921:79; NRC 1921:54). In July 1921 Dixon resigned as chairman—apparently the "correspondence involved was distasteful to him"[1]—and the CSAS was reorganized, with Wissler, having completed his term as chairman of the Division of Anthropology and Psychology, serving as committee chairman and Dixon, Laufer, Frederick W. Hodge of the Museum of the American Indian (Heye Foundation) in New York City, and Amos W. Butler of Indianapolis serving as members. Hodge was an ethnologist-archaeologist who before assuming the directorship of the Museum of the American Indian had worked at the Bureau of American Ethnology, where he was appointed ethnologist-in-charge in 1910 (Lonergan 1991:294). Butler was an ornithologist of considerable reputation, having founded the Indiana

Academy of Science in 1885, and at the time of his appointment was serving as secretary of the State Board of Charities in his home state. The committee was expanded to 7 members a year later and, to obtain better geographic coverage, to 11 members in 1924.

One might well ask why such an important entity as the NRC was involved with state archaeological surveys when there were other, seemingly more important, scientific and technological issues facing postwar America—serious issues of national welfare and defense, not the examination of pelvic structure or the psychology of highway. The answer, we think, lies in the credentials and political acumen of several key anthropologists involved with the NRC from the start. Dixon was influential from his post at Harvard, having served not only as president of the American Anthropological Association but as a trainer of a generation of archaeologists and ethnologists. Wissler was a powerful force in Americanist archaeology and ethnology from his dual positions as curator of anthropology at the American Museum of Natural History in New York City and later as professor of anthropology at Yale.[2] Working under Wissler at the museum were some of the leading figures in southwestern anthropology—Leslie Spier and Nels Nelson, for example—and Wissler was friends with Alfred L. Kroeber, the most influential anthropologist in the western half of the United States from his position at the University of California and the person who on July 1, 1921, assumed the vice chairmanship of the Division of Anthropology and Psychology. As division chairman, Wissler had the respect of the discipline and could guide the unit's direction, and one of the first things he did was to create the CSAS.

One impetus for forming the committee was the destruction of archaeological sites that was occurring with increasing frequency across the eastern United States, much of it the result of indiscriminate fieldwork by amateur societies. Making matters worse was the absence of any baseline data against which to judge the magnitude of destruction. In other words, site surveys had never been conducted in most states, and hence there was no way to gauge the percentage of sites being destroyed. A few states, such as Ohio, New York, and Wisconsin, had conducted state surveys, and Wissler was determined to see similar surveys established in other states.[3] The best means of accomplishing that objective was through an arm of the Division of Anthropology and Psychology, which, following Kellogg's vision, would act as both an organizing body and a clearing house for information.

The CSAS's decision to focus first on Illinois, Indiana, Iowa, and Mis-

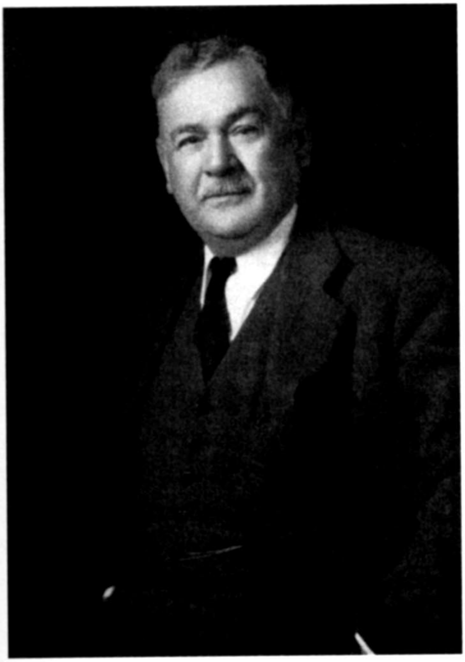

Figure 1. Clark Wissler, longtime ethnologist with the American Museum of Natural History and the first chairman of the National Research Council's Division of Anthropology and Psychology, ca. 1940. (Reproduced by permission of the Society for American Antiquity from *American Antiquity* 13, no. 3 [1948])

Figure 2. Roland B. Dixon, longtime member of the Peabody Museum (Harvard) staff and faculty member in the Harvard anthropology department, ca. 1910. (From Coon and Andrews 1943; reprinted courtesy Peabody Museum, Harvard University)

souri was not entirely accidental. All four states had strong statewide support for science and history as well as active historical and scientific societies. The decision to include Indiana was certainly no surprise given that Wissler grew up there and had received all his degrees, including one in law, from the University of Indiana (Guthe 1940). Carl Guthe of the University of Michigan, who assumed the chairmanship of the CSAS in 1927, reported a year later at the International Congress of Americanists meeting in New York that

> Before the end of 1920, interest had been awakened in Illinois and Indiana. A discussion of the Illinois project constituted a part of the meeting of Section H [Anthropology] of the American Association for the Advancement of Science at Chicago in December of that year. A few days prior to this, the plans for a similar project for Indiana had been presented to the Indiana Academy of Science and the Indiana Historical Conference, both of which organizations appointed committees to further the work. During 1921, W. K. Moorehead began his excavations at the great Cahokia mound group in East St. Louis, Illinois, working in cooperation with the University of Illinois, and in Indiana the State Historical Commission and the State Department of Conservation, through the State Geologist, jointly developed a survey of the State by counties, recording all facts of an archaeological nature obtained either by field parties or questionnaires. (Guthe 1930b:52)[4]

With respect to the composition of the four state committees, Wissler (1922:233) reported soon after their formation that

> In Indiana the State Academy of Sciences and the Historical Society appointed a State committee to cooperate, viz., Dr. Frank B. Wynn, Dr. Stanley Coulter, Judge R. W. McBride; for Illinois and Iowa similar State committees; Illinois, Dr. Berthold Laufer, Dr. Otto L. Schmidt, Dr. Charles L. Owen; Iowa, Prof. B. F. Shambaugh, Dr. E. R. Harlan, E. K. Putnam. The Missouri survey was initiated by the Anthropological Society of St. Louis and is under the direction of the following committee representing a number of societies and institutions: Dr. R. J. Terry, Leslie Dana, B. M. Duggar, R. A. Holland, George S. Mepham, Dr. H. M. Whelpley, J. M. Wulfing, Dr. C. H. Danforth. Satisfactory progress has been made in each of these States. The Indiana Survey is by the State under the direction of the State Geologist. In Iowa the work has begun under a grant from the [State Historical Society of Iowa]; in Missouri under a fund raised by the

above-mentioned committee. As the results of all these surveys will be published, the outlook is stimulating.

Anatomy of a State Committee

For a perspective on the goals and methods of the state organizations that were brought about under the CSAS, we focus on the Anthropological Society of St. Louis, which in many ways was typical of the kinds of organizations that the committee was attempting to assist. Like societies in some of the other states, it grew out of an amalgam of earlier organizations, or more precisely, out of a recombination of members from different societies, some of which had long histories. The earliest scientific society in Missouri was the Academy of Science of St. Louis, which was formed in March 1856. Fifteen members—seven medical doctors, three lawyers, and five professors—attended the first meeting, and the constitution and bylaws they adopted spelled out the objectives of the fledgling society:

> Section 1. It shall have for its object the promotion of Science: it shall embrace Zoology, Botany, Geology, Mineralogy, Palaeontology, Ethnology (especially that of the Aboriginal Tribes of North America), Chemistry, Physics, Mathematics, Meteorology, and Comparative Anatomy and Physiology.
>
> Sec. 2. It shall furthermore be the object of this Academy to collect and treasure Specimens illustrative of the various departments of Science above enumerated; to procure a Library of works relating to the same, with the Instruments necessary to facilitate their study, and to procure original Papers on them.
>
> Sec. 3. It shall also be the object of this Academy to establish correspondence with scientific men, both in America and other parts of the world. (cited in O'Brien 1996:42)

The Academy of Science of St. Louis was small, but it was anything but dormant. In terms of topics that were pursued by the members, there was little in the realm of science that did not fall under the academy's purview. Understandably, topics that fell broadly under the rubric of natural history, including ethnology, archaeology, and paleontology, enjoyed keen interest. From the beginning, the academy reached out to eastern societies and institutions, perhaps as a means of gaining recognition but probably also because of an insatiable thirst for knowledge on the part of its highly educated members. The academy had established two types of membership: associate and corresponding. The former was for members living in St. Louis County and who were thus able to attend meetings, and the latter was for persons living elsewhere. It is obvious from examining the *Journal of Proceedings*[5] for the first month and a half of the society's existence that members were interested in adding to the corresponding membership some of the most well-known names in science —men such as Joseph Henry, first secretary of the Smithsonian Institution; Ferdinand V. Hayden, geologist with the U.S. Geological Survey; and Joseph Leidy of Philadelphia, arguably the top vertebrate paleontologist of the time.

The Academy of Science of St. Louis was active in archaeological fieldwork through the late 1800s (O'Brien 1996), but by the turn of the century it had been eclipsed in prominence by the Missouri Historical Society, which had been founded in 1866, and within a few more years by the St. Louis Society of the Archaeological Institute of America (AIA), which was organized in 1906. From its beginning, the historical society maintained an active interest in prehistory and as early as 1880 proposed a statewide survey of known archaeological sites (Broadhead 1880). The society was also intensely interested in obtaining collections of artifacts from Missouri sites, as remarks made by Frank Hilder in 1880 made clear:

> [Hilder] spoke to the disjointed efforts made to collect relics of the people who once dwelt in these lands. It was certainly most discreditable that one had to resort to the Smithsonian Institut[ion], the Peabody Museum, and the Blackmore Museum in Salisbury, England, to find proper collections of our prehistoric remains. He hoped to see the time when St. Louis would possess a collection in which the ancient history of the race can be studied.
>
> The spirit in which the work had been begun by the Historical Society gave promise that it would be the agency to bring together a collection which would not only rival, but surpass, any similar archaeological and historical collection.[6]

The society wasted no time in following up on Hilder's plea, as is evidenced by an advertisement placed in the January 23, 1881, edition of the *Missouri Republican* (cited in Trubowitz 1993): "The Society particularly wishes to procure archaeological specimens, popularly known as Indian curiosities or stones, flint arrow and spear heads, chisels, discoidal stones, stone axes, pottery from mounds, etc., and will be thankful for every object of this class." The historical society was extremely successful in acquiring various collections; an inventory made in 1903 showed that the organization had at least 11,000 artifacts in storage and almost 14,000 on display in 42 cases (Trubowitz 1993:3).

The other St. Louis organization that was gaining prestige in the early twentieth century was the local affiliate of the AIA known as the St. Louis Society. More than 100 people attended the organizational meeting on February 8, 1906. The AIA had been formed in 1879 with the goals of "promoting and directing archaeological investigation and research, —by the sending out of expeditions for special investigation, by aiding the efforts of independent explorers, by publication of reports of the results of the expeditions which the Institute may undertake or promote, and by any other means which may from time to time appear desirable" (AIA 1880:6).

One activity sponsored by the St. Louis Society was Gerard Fowke's (1910) survey and excavation of sites in southeastern and central Missouri. Fowke was a peripatetic journeyman connected at times with the Bureau of American Ethnology (O'Brien 1996), but apparently it was an unsalaried connection. The research proposal that was drawn up in advance of Fowke's work in Missouri has a modern ring to it (Pool 1989): records of the November 1906 meeting state that "it was unanimously agreed from the outset that, in the archaeological investigations that were proposed, the object should be scientific results, whether negative or positive, rather than in the making of large finds of relics; and that a district should be selected and worked systematically, regardless of whether the finds were great or small, so that the archaeological record might be complete."[7] Interestingly, it was the national organization, the AIA, and not the local chapter that transmitted the final report of the work to the Bureau of American Ethnology for publication in its *Bulletin* (Fowke 1910).

To note that Fowke aligned himself with the St. Louis chapter of the AIA does not do justice to an important episode in the history of Missouri archaeology, because the relationship that apparently developed between Fowke and the chapter was much more productive than is evident on the surface. In many respects the relationship between Fowke and the professional and the nonprofessional chapter members foreshadowed what was to come several decades later with the founding of the Missouri Archaeological Society in 1935, and it was the kind of relationship that Wissler hoped to foster through the CSAS. To understand the relationship between Fowke and the St. Louis chapter of the AIA requires a brief discussion of a group with the bizarre name of the "Knockers" that was formed by members of three local St. Louis organizations: the AIA, the Academy of Science of St. Louis, and the Missouri Historical Society. The spark of this group was a father-and-son combination—David I. Bushnell, Sr. and Jr.—the latter of whom had begun to make a name for himself in archaeology. Never formally trained as an anthropologist, Bushnell, Jr., worked as an assistant for the University of California in the 1890s, for the Peabody Museum (Harvard) between 1901 and 1904 (e.g., Bushnell 1904), and as an employee of the Bureau of American Ethnology beginning in 1907.

David Browman (1978:2) suggests, and we think he is correct, that it was the younger Bushnell's emerging prominence in the field and his ties to institutions such as the Peabody Museum that acted as a magnet in attracting other professional archaeologists such as Fowke to St. Louis. And it wasn't only Fowke who began keeping company with the Knockers. Browman notes that leading archaeologists of the day—men such as Earl Morris, who went on to have a distinguished career as a southwestern archaeologist with the Carnegie Foundation, and Edgar Hewett, a southwesternist and Mayanist who in 1906 became director of American research with the AIA—regularly turned up at various meetings of the Knockers.

By at least the early 1920s, another group, the Anthropological Society of St. Louis, had been formed. It was organized "chiefly by members of the Medical School in Washington University" and had the purpose of "bringing together all the institutions in St. Louis interested in historical and archaeological work."[8] This was the group identified by the Committee on State Archaeological Surveys as its contact point in Missouri—a status reflected in the organization's listing in the "Notes on State Archaeological Surveys" (Wissler 1922). At least two members of the Knockers—George S. Mepham and J. M. Wulfing—are listed as members of the committee, along with Dr. Henry M. Whelpley, a pharmacist, and Dr. R. J. Terry and Dr. Charles H. Danforth, both of whom were on the staff of the Washington University School of Medicine. Whelpley, an avid artifact collector (Blake and Houser 1978), was one of the driving forces behind the local chapter of the AIA and had long advocated a statewide survey of Missouri (Pool 1989). Thus the group that was put together to represent the state to the CSAS might have been short on professional archaeological expertise, but few states had a more respected body to coordinate such a survey than Missouri.

Wissler, as he would do with other state groups, offered the St. Louis group advice on what the goals of a statewide survey should be. In a long letter to Danforth, secretary of the St. Louis society, Wissler noted that

> As I see the problem in such State surveys, there are, in the main, two alternatives: First, to project a rapid comprehensive survey of the State to be carried out in a year or two and to be supported by a specific appropriation. Second, the inauguration of a modest program under the auspices of a society or some existing State agency which can be counted upon to continue the work of the survey indefinitely.
>
> To my way of thinking, the second is preferable. For one thing, an appropriation of sufficient magnitude for quick comprehensive survey is not likely to materialize. On the other hand, if fortune did favor us and the appropriation were made by the State, we could not look forward with confidence to a continuation of the work in the future.[9]

Wissler also made sure Danforth understood that support for the state survey "must necessarily be found within the State concerned. No direct financial support from the outside can be expected." Apparently Wissler's warning did not dampen the St. Louis group's enthusiasm, because Danforth soon wrote him back, noting that "Several hundred dollars are already in sight and it is proposed to make an immediate (next spring) survey of St. Louis County and, if possible, inspire simultaneous surveys of several other favorable counties."[10] Despite this enthusiasm, there is no evidence that the planned surveys were ever carried out, and by 1928 the Anthropological Society of St. Louis was listed as "inactive."[11]

Annual Reports of the State Committees

Yearly reports of state surveys appeared in the *American Anthropologist* from 1922 to 1934, at which point that journal decided to stop carrying them. The reports then moved to the newly created journal *American Antiquity.*[12] Some of the later summaries also appeared in various volumes of the *Pan American Union Bulletin.* As Guthe (1930b:56) pointed out, the initial survey summaries were so well received that

> the Committee several years later sought and secured the cooperation of those institutions which were not State agencies, but were likewise engaged in archaeological research. Their cordial response made possible the expansion of these summaries to record nearly all of the archaeological fieldwork in North America. That this aspect of the Committee's activities has been a popular one is evidenced by the growth of the summaries. That for the year 1921 contains reports from thirteen State agencies [Wissler 1922], that for the year 1924, the first of the expanded summaries, gives reports from eleven State agencies and eight other institutions [Kidder 1925], that

for 1927 reports on the work of nineteen State agencies, and thirteen other institutions [Guthe 1928]. Today the Committee conducts a correspondence with representatives of fifty-one institutions.

Summaries were prepared by contributing correspondents from whichever state organization was sponsoring the work. Twelve states—Alabama, Arizona, California, Colorado, Illinois, Indiana, Kansas, Nebraska, New York, Ohio, Tennessee, and Wisconsin—plus New England were represented in the first set of summaries, which were for work carried out in 1921. If one were to read only what was published, it might sound as if the CSAS was making significant inroads into establishing statewide surveys across the Midwest and East, but this was not the case. For example, although Nebraska and Kansas submitted summaries for 1921, Wissler made it clear in a letter to C. E. Seashore, who had succeeded Wissler as chairman of the Division of Anthropology and Psychology, that all was not well in those states: "The situation in Nebraska is such as to render inadvisable any effort to launch a survey. The organizations that should be interested in the project are not working in harmony, chiefly because of the questionable scientific character of some of the men. . . . There is considerable interest in the subject in Kansas, but no live leadership at present."[13]

As might be expected given the long history of work in those states, the 1921 summaries from New York, Ohio, and Wisconsin were the lengthiest. Ohio's mounds had long figured prominently in Americanist archaeology, receiving in-depth treatment early in the nineteenth century, especially through the work of Ephraim G. Squier and Edwin H. Davis (1848), and Frederic Ward Putnam (e.g., 1887), and continuing through the opening decades of the twentieth century with the work of Warren K. Moorehead (1892a, 1892b, 1897, 1899, 1922) of Phillips Academy in Andover, Massachusetts, and Henry C. Shetrone (1920) and William C. Mills (1906, 1907) of the Ohio State Archaeological and Historical Society. Likewise, the mounds and petroglyphs of Wisconsin had been well documented through the work of Theodore H. Lewis (e.g., 1883) and others.

The most interesting of the reports published in 1922 is the one on New York, written by Arthur C. Parker (Figure 3) of the New York State Museum and the man who would become, in 1935, the first president of the Society for American Archaeology. Parker's paternal great-grandfather was a Seneca, and Parker himself had been president of the Society of American Indians in 1914–1915. The New York survey began in 1905, and the

Figure 3. Arthur C. Parker, director of archaeology at the New York State Museum and first president of the Society for American Archaeology, ca. 1950. (Reproduced by permission of the Society for American Antiquity from *American Antiquity* 21, no. 3 [1956])

office of the Archaeologist of the State Museum was created a year later, with Parker as the director. We say the New York report is the most interesting of the 13 that were published that year for several reasons. First, Parker made the objectives of the survey clear. Although the operation was referred to as a "survey," the term was not used in exactly the same way as it is today. Whereas modern archaeologists tend to think of site survey as an analytical exercise in its own right, prehistorians in the early part of the twentieth century saw it almost solely as an immediate prelude to excavation. Granted, in some states preservation efforts grew out of surveys—Mills made this clear in his 1921 summary report on Ohio, as Charles E. Brown did in his report on Wisconsin—but it seems undeniable that many statewide surveys were little more than prospecting exercises.

This rationale is apparent in Parker's three-part plan for New York: "The Museum began by exploring and excavating important sites without regard to culture. If a site seemed of special interest and likely to yield information and artifacts it received the attention of the season" (Wissler 1922:239). The CSAS had faced this problem early on, as Guthe (1930b:55) later noted: "The first problem before the Committee was that of defining what constituted an archaeological survey. It is immediately apparent that a survey consists of exploration and excavation." But there was more to it than that, and Guthe (1930b:55) summarized what had been the position of the committee from the start:

> Since it is always advisable to take a careful inventory of the assets of a given project before paying special attention to a detailed aspect of it, and since the scientific excavation of an archaeological site requires at least the supervision of a technically trained man, the Committee has always recommended, upon the inauguration of a survey, that emphasis be placed on a somewhat detailed exploration of the State, covering a period of several years, if necessary, before a program of excavation is undertaken.
>
> The most patent aspect of such an exploratory survey is a compilation and description of the many kinds of archaeological sites found in the State. . . . With this must be coupled an examination of the literature of the subject. . . . Moreover, each State contains a number of amateurs, who have become interested through the discovery of "Indian relics" in their immediate vicinity. The director of the State survey must not overlook the latent possibilities of these enthusiasts, but must become acquainted with them, enlist their support, and record and evaluate the material found in their private collections.

It was a good plan, but by the end of the decade it was becoming obvious to the CSAS that the real interest of nonprofessionals lay in excavation—a phenomenon not uncommon in modern times. Had this not been the case, there probably would have been no need for the NRC-sponsored Conference on Midwestern Archaeology in 1929.

Returning to Parker's summary of activities in New York, the second reason for our interest in the report resides in his third paragraph, where he laid out a suspected chronological ordering of what he termed "occupations" (Wissler 1922:239–240):

> As a result of this work the survey has determined the general localities and the chief characteristics of several occupations. The latest is the historic Iroquois in central and western New York and the Algonquian along the coast. Using these as datum we have been able to chart the successive oc-

cupations of the several areas within the State. By general areas, these are broadly as follows. (In reading these lists note that the higher the number the earlier the date.)

Western New York

1. Historic Iroquois (Seneca), tributary Algonquian peoples
2. Seneca and others who followed the Erie and Neutral
3. Erie, Neutral, Seneca, Iroquoian indeterminate
4. Algonquian, various tribes
5. Earth-work builders with pottery between Algonquian and Iroquoian
6. Mound-Builder-like sites
7. Algonquian (?)
8. Early Algonquian (?)
9. Indeterminate

Central New York (south to the Pennsylvania line)

1. Historic Cayuga, Onondaga, and Oneida
2. Andaste in the south along the Susquehanna and tributaries
3. Algonquian about the Finger Lakes
4. Mound-Builder-like
5. Algonquian
6. Early Algonquian
7. Algonquian (?)
8. Eskimoan (?)
9. "Red Paint" (?)
10. Indeterminate

Northern New York and Mohawk Valley

1. Iroquoian (in Jefferson County, early Onondaga)
2. Algonquian
3. Early Algonquian
4. "Red Paint" (?)
5. Eskimoan (?)
6. Indeterminate

(Contemporaneous with 3, in the Mohawk Valley there were "stone grave" people.)

Southern New York and Coast

1. Algonquian tribes
2. Iroquoian influence
3. Pre-Colonial Algonquian (Iroquoian traces)

4. Early Algonquian, certain Eskimo-like traces (?)
5. Indeterminate

Questions of chronology were not new in 1921, although it had not been that long since Americanist archaeologists began to think seriously about how to measure the passage of time in anything more than crude fashion. William Henry Holmes and his colleagues at the Bureau of American Ethnology had finally succeeded in demonstrating that purported evidence of glacial-age humans in North America was suspect (e.g., Holmes 1892, 1893a, 1893b, 1897; Hrdlička 1907, 1918), and for several years the notion of a relatively shallow time depth to the archaeological record was more or less axiomatic (Meltzer 1983, 1985). When Parker penned his summary of occupations in New York, the landmark chronological work undertaken in the Southwest by Nels Nelson (1916), A. V. Kidder (1916; Kidder and Kidder 1917), A. L. Kroeber (1916a, 1916b), and Leslie Spier (1917) was no more than five years old (Lyman et al. 1997; O'Brien and Lyman 1999a). This work had little effect on efforts in the East, although some archaeologists working there (e.g., Mills 1907) were beginning to wonder if there was more time depth to the archaeological record than had previously been proposed. What makes Parker's scheme so remarkable is that it implies *considerable* time depth to the archaeological record of New York.

Do not be misled into thinking that the perceived shallowness to the archaeological record in the East means that archaeologists were not interested in marking the passage of time. Simply because there was no incontrovertible evidence of glacial-age humans in the East did not imply that there was *no* time depth, and eastern archaeologists were interested in measuring whatever time depth there was. They knew very well the law of superposition—that artifacts at the bottom of a stratigraphic sequence were deposited before those on top—and they often used that positioning as a proxy for age differences among sets of artifacts recovered from different vertical positions (Lyman and O'Brien 1999). The problem was in figuring out how *much* older one set of artifacts was than another. For example, H. C. Mercer, a Harvard-trained curator of archaeology at the University of Pennsylvania Museum, excavated a trench through a mound adjacent to the Delaware River in New Jersey and upon making some postfacto stratigraphic observations concluded that the mound was stratified and contained the remains of "two village sites, set one upon the other,—an upper and a lower" (Mercer 1897:72). Mercer knew the superposed "village sites" were different in age, but he lamented that "the upper site might have been inhabited one or five hundred years after the lower was overwhelmed. If, therefore, we sought for inference as to the relative age of the two sites, we could only hope to find it in a comparison of the relics discovered. Realizing this, the depth, position, and association of all the specimens found, and particularly their occurrence above or below the lines of stratification, was carefully noted" (Mercer 1897:74). As we point out later, this early interest in stratigraphic relations preceded the so-called stratigraphic revolution in the Southwest (e.g., Browman and Givens 1996) by some 20 years and can be traced back to Frederic Ward Putnam, second director of the Peabody Museum (Harvard), and the training he provided students and staff.

Carl E. Guthe: The Quintessential Committee Chairman

By the summer of 1922, local committees in several states were so well run that the national committee discontinued its official connection with them (Guthe 1930b:53). At the same time, several midwestern states were asking for support, and in 1923 Wissler's committee began developing a plan for surveys in the Mississippi Valley, which led to the circulation of a pamphlet on suggestions regarding the aims and methods of statewide surveys (Wissler et al. 1923; see Appendix 1). Costs associated with the production of the pamphlet were subsidized by the State Historical Society of Iowa.[14] Interestingly, the secondary message of the pamphlet was that all work should be done by or under the supervision of professionally trained individuals, a foreshadowing of the tone of the 1929 Conference on Midwestern Archaeology.

Wissler retired as chairman in July 1924 and was replaced by A. V. Kidder, a Harvard-trained archaeologist working in the Southwest under the aegis of Phillips Academy in Andover, Massachusetts. The CSAS was again enlarged and consisted of holdovers Wissler, Roland B. Dixon, Frederick W. Hodge, Amos W. Butler, Marshall Saville, Charles E. Brown, and Peter A. Brannon, and new members W. C. Mills of the Ohio State Archaeological and Historical Society, Henry M. Whelpley of the Anthropological Society of St. Louis, and Charles R. Keyes of the State Historical Society of Iowa. Guthe (1930b:53–54) noted that during Kidder's service as chairman, the CSAS "continued to extend its contacts, particularly in the southern and western portions of the country and its function as an advisory board was thereby strengthened and expanded."[15] Under both Wissler's and Kidder's chairmanship the committee continued to hold formal and informal meetings—for example, at the American Association for the Advancement of Science meeting in Cincinnati in 1923 and at the Central Section (later the Central States Branch) of the American Anthropological Association meeting in Columbus, Ohio, in 1926—but it was at the 1927 Central Section meeting in Chicago that the CSAS took its boldest step to date, proposing that the NRC establish a cooperative laboratory for the study of pottery from the eastern United States.[16] The University of Michigan offered to maintain the laboratory in its Museum of Anthropology,[17] and it became known officially as the Ceramic Repository for the Eastern United States, with "Eastern" referring to anything east of the Rocky Mountains.

Kidder resigned his position as chairman of the CSAS in the fall of 1927, and the person who succeeded him was the same man who had responsibility for the daily operation of the Michigan repository, Carl E. Guthe. Under Guthe's chairmanship, which lasted until 1937, the committee stabilized and became the organizing force for which it had been designed. In fact, despite the rotating nature of NRC committee membership, the makeup of the CSAS was remarkably stable from 1924 on, with Brannon, Brown, Butler, and Keyes serving until the CSAS was abolished in 1937.

Guthe received his Ph.D at Harvard in 1917 and worked with another Harvard graduate, Frederick H. Sterns, in Nebraska in 1915 and then with Kidder at Pecos Pueblo, New Mexico, from 1916 until 1921 (Figure 4). He became associate director of the Pecos project in 1917. From 1920 to 1922 he also worked as a research associate for the Carnegie Institution's Tayasal, Guatemala, project that was directed by Sylvanus G. Morley (Griffin 1976b). Guthe joined the staff of the University of Michigan in 1922 and became associate director of the newly created Museum of Anthropology, eventually assuming the directorship in 1929. Given both his position and training, as well as his enthusiasm, Guthe was a natural choice to head the CSAS.

Guthe went to great lengths to increase the effectiveness of the committee in its relations with nonprofessionals, even taking an extended trip in the summer of 1928 to visit coordinating offices in 15 states in the Mississippi Valley. He later recalled, "Impressed by the attitudes and accomplishments of these earnest amateurs, I felt they deserved to be helped rather than censured" (Guthe 1967:434). Perhaps, but he did not mince words in the report that summarized what he found during his trip. For example, with respect to Arkansas he stated that archaeology there was

Figure 4. A. V. Kidder (*center*) with Alfred M. Tozzer (*left*) and Carl E. Guthe, Kidder's assistant at Pecos Pueblo, New Mexico, 1916. (From Woodbury 1973; photo courtesy Columbia University Press)

"the hobby of [Samuel C.] Dellinger, a biologist at the State University who has seen fit to leave our letters unanswered. . . . The 'Arkansas Museum of Natural History and Antiquities' is a newly formed group, with a big paper organization. The situation here is pathetic because of the well-intentioned but blissfully ignorant enthusiasm of the promoters. A quantity of extremely obvious frauds have been purchased by them."[18] Amusingly, in Mississippi he found "[t]wo inadequately trained young men . . . conducting excavations" for the director of the Mississippi Department of Archives and History. Those two "inadequately trained young men" were Moreau B. Chambers and James A. Ford (O'Brien and Lyman 1998, 1999b).

Despite the decade-long effort of the committee to foster cooperation among various state organizations and to channel local energies into less commercially motivated activities, the outlook was still bleak in 1929, as Guthe (1967:435) recalled almost four decades later:

> In 1929 . . . archaeological explorations were under way in about half of the states of the Union, many of them carried out by lay students of the subject. The lack of communication between groups was enormous. State political boundaries served as corral fences, preventing archaeologists in one state from communicating with their colleagues in adjacent and neigh-

> boring states. Nor were the channels of communication between the professional and the serious-minded laymen as broad and open as they should have been.
>
> The professionals were outspoken in their condemnation of Indian-relic collectors and dealers who destroyed irreplaceable archaeological evidence. . . . Equally objectionable, because of the resulting destruction of evidence, were the activities of well-intentioned amateurs who did not understand the dangers of careless excavation and neglected to keep adequate records.

The only possible solution to the problem resided where it had for the previous decade: "the cultivation and friendly education of another type of amateur," namely, the "[s]erious-minded, thoughtful collectors, [who,] intrigued by the conditions and associations under which the relics were found, sought information on their origins and functions by consulting libraries, fellow collectors, and, when possible, professional archaeologists" (Guthe 1967:435). By 1929 this approach had paid dividends but certainly not big ones. How could the CSAS change the situation? The answer, it seemed, was to hold a large conference and attack the issue head on. Not simply a conference such as had been held at the annual meetings of the American Anthropological Association and the American Association for the Advancement of Science—those were attended only by professionals—but a large gathering of both amateurs and professionals, where the former could listen to recommendations offered by the latter, and the latter could listen to the concerns of the former.

This is how Knight Dunlap, chairman of the Division of Anthropology and Psychology from 1927 to 1929, pitched the conference to Edmund Day, director of the Laura Spelman Rockefeller Memorial, the foundation Dunlap approached for funding to offset the estimated $3,000–4,500 needed to host such a meeting:

> The Conference on American Archaeology seems to be the most important thing to be done for the anthropologists at the present time. Some of the mid-western states are "sold" on the idea of comparative work, and realize that institutions working, or wishing to work on their mounds, etc., do not wish to "rob" them, or to interfere with "States Rights." Other states are still on the defensive. It is believed that in this Conference the officials of states already favorable would help with the other states. . . . The most favorable place in which to call this Conference, seems at present to be Indianapolis.[19]

The Conference on Midwestern Archaeology

Given the model nature of the statewide survey of Indiana, its capital was a logical venue for such a meeting, but Indianapolis was passed over in favor of St. Louis. Fifty-three people, including 9 of the 11 members of the CSAS, attended the two-day conference, which was held at the Hotel Coronado on May 17–18, 1929. Among them were Dunlap; Henry S. Caulfield, governor of Missouri; W. E. Freeland, majority leader in the Missouri House of Representatives; G. R. Throop, chancellor of Washington University; Thomas M. Knapp, chancellor of St. Louis University; John C. Futrall, president of the University of Arkansas;[20] Rufus Dawes, president of the Chicago World's Fair Centennial Celebration; and William J. Cooper, U.S. commissioner of education. Those on the professional-anthropological side included Matthew W. Stirling, chief of the Bureau of American Ethnology; Fay-Cooper Cole of the University of Chicago, chairman of the Division of Anthropology and Psychology from 1929 to 1930; William S. Webb, head of the newly created Department of Anthropology and Archaeology at the University of Kentucky; Frans Blom, director of the Department of Middle American Research at Tulane University; J. Alden Mason of the University of Pennsylvania; and Clark Wissler and Nels Nelson of the American Museum of Natural History. Several state archaeologists and geologists also attended the meeting, including Calvin S. Brown, an archaeologist with the Mississippi Geological Survey, and M. M. Leighton, chief of the Illinois State Geological Survey.

Three fairly high-profile amateurs also attended—Don F. Dickson of Lewiston, Illinois; Harry J. Lemley of Hope, Arkansas; and Jay L. B. Taylor of Pineville, Missouri. Dickson had earned a reputation as a preservationist by erecting a structure over human skeletons he unearthed on his property in Fulton County, Illinois (Harn 1980), and Lemley was a collector of Caddoan artifacts, although he had contact with professionals throughout part of his life (O'Brien and Lyman 1998) and would go on to publish articles on his excavations (e.g., Lemley 1936; Lemley and Dickinson 1937). Taylor had assisted Warren K. Moorehead in his excavations at Cahokia, located across the Mississippi River from St. Louis in Collinsville, Illinois (Moorehead 1929a), and he knew Wissler and Nelson very well—in the case of Nelson all too well, for it was Nelson (1928) who in a very clear and concise argument shredded Taylor's (1921a, 1921b) claims of authenticity of a bone with an engraving of a mastodon that

Taylor had ostensibly found in a cave in southwestern Missouri (O'Brien 1996).

The conference consisted of three parts: (1) an open meeting of the CSAS on Friday morning, followed by a trip to Cahokia mounds guided by Moorehead and an evening lecture by Henry C. Shetrone, director of the Ohio State Museum; (2) the main conference on Saturday morning and afternoon; and (3) Saturday evening dinner and presentations, which were broadcast on radio station KMOX. The conference proceedings reveal the striking disparity of topics that were addressed. As one might expect, given the ink that had been spilled up to that point, numerous presenters, from Governor Caulfield on down, spoke of preserving archaeological sites for the future. There were polemical statements on the need for preservation, which is not unexpected given the political nature of the meeting and the fact that some of the presentations were being broadcast to the public, but there also were presentations that dealt with specific advantages that accrued from preservation, such as increased tourism. Several presenters excoriated vandals and relic hunters for the catastrophic damage done to an irreplaceable resource—exactly the problem the committee had been working for a decade to solve, with little visible success. Arthur C. Parker made persuasive arguments in this direction in his paper, "The Value to the State of Archaeological Surveys." He laid out four reasons for surveying and preserving archaeological remains:

> 1. *Archaeology explains the prehistory of the state.*—The recoveries from ancient sites constitute visual exhibits of the people who occupied the state before the coming of a population of European origin. . . .
> 2. *Archaeological remains constitute a vast reservoir of valuable knowledge.*—Judged by every moral standard the state is bound to conserve and protect its resources. The aboriginal sites within each state constitute unique and fundamental sources of archaeological facts, highly valued by the scientific world. . . .
> 3. *Archaeological remains are monumental exhibits.*—The marking of prehistoric Indian sites and their protection from promiscuous digging would not only attract the attention of the sight-seeing public, but would stimulate the investigation by scientists. . . .
> 4. *Archaeological collections are exhibits of lasting worth.*—Wherever archaeological collections have been made by trained students of prehistory the resulting exhibits and publications describing them have constituted genuine contributions to knowledge. (Parker 1929:33–34)

Parker railed against unskilled collectors and the effect they were having on the archaeological record:

> The relic-hunter digs only to destroy and his recoveries are often abortive things with undetermined parentage. . . . Whether the relic-hunter will continue to ruin the field, or whether state-supported agencies shall preserve the field and draw from it the information that an enlightened age demands, depends very largely upon the citizens of each state; but it depends most of all upon how thoroughly archaeologists who understand the importance of their quest are able to present it to the public. Archaeology must advertise and it must seek thereby to stimulate such a desire to know more of prehistory that support will follow. (Parker 1929:37–38)

It was one thing to say that states should take control of preserving their archaeological resources, but there was a catch, and Parker knew it: *Which* organization within a state was best suited to carry out a survey and to spearhead preservation efforts? Parker (1929:34) pointed out that it "matters little what institution or agency promotes the survey so long as its operating force is composed of trained archaeologists familiar with the problems to be met or capable of meeting these problems when they occur." To him the ideal institution, "other things being equal, is a state museum, for then there will be a centralized repository for the specimens, and at least a certain amount of clerical and professional help." He then noted—an understatement if there ever was one—that "A specially constituted commission cooperating with local groups may have difficulty in meeting the problem of distributing the recoveries, especially when it has invited the aid of numerous local historical and scientific societies" (Parker 1929:34–35). In other words, if a loose amalgam of persons constitutes the committee, how are they going to maintain control of the artifacts that result from field exercises, especially when their field crews consist of collectors? Even when a solid organization such as a state museum acts as the coordinating body, local organizations and municipalities will want to maintain control over artifacts, and, as Parker noted, the organizing body is going to have to educate them about the dangers in doing so. Modern readers may be struck by how little things have changed since 1929 when it comes to civic pride and private ownership of artifacts.

Although the topic of preservation and statewide committees dominated the Conference on Midwestern Archaeology, close reading of the proceedings turns up a few passages that give us some idea of the state of archaeological method in 1929 and that were preludes to topics that dominated much of the NRC-sponsored conference in Birmingham, Alabama, in 1932. Three papers and a prepared set of remarks on one of those papers furnish useful examples.

Emerson F. Greenman on Artifact Classification

The title of the paper by Emerson F. Greenman, curator of archaeology at the Ohio State Archaeological and Historical Society, was "A Form for Collection Inventories." Greenman received his Ph.D. from the University of Michigan in 1927 and had worked in Guthe's Museum of Anthropology; thus he was no novice when it came to artifact analysis. He began his paper by noting that "In view of the increasing activity in state archaeological survey work, some attempt should be made to bring about uniformity in the use of terms, and in the methods of describing archaeological objects, in order that the work done in one state may be compared with that in adjoining states. . . . Distributions [of artifacts] common to more than one state can only be worked out by the use of a uniform terminology" (Greenman 1929:82–83).

The classification system Greenman (1929:83–84) proposed was fairly rigorous and obviously had been well thought out. The system revolved around the identification of types, which Greenman defined as "the frequent linking together of a number of features on the same specimen" (83). He identified 11 projectile-point characteristics useful for defining types and 21 other characteristics as providing a means of narrowing the type definitions. He also listed four other sets of characteristics—those related to the overall shape of a projectile point—which were used as initial sorting criteria. Thus a point could have wide, shallow notches, or it could be angular- or side-notched or wide-stemmed. Greenman even devised a shorthand notation for his system; in our example just cited, such a specimen would be listed as an A/A, 5, 13. In several respects the system exhibited characteristics of some modern approaches to classification, including paradigmatic classification (Dunnell 1971; O'Brien and Lyman 2000).

Classification theoretically serves two functions—to structure observations so that they can be explained and to provide a set of terminological conventions that allows communication. In the United States, early classification systems were developed solely as a way to enhance communication between researchers who had multiple specimens they wanted to describe (see Dunnell's [1986:156–159] discussion of Rau [1876] and Wilson

[1899]). The "Report of the Committee on Archeological Nomenclature" (Wright et al. 1909), which was commissioned early in the twentieth century by the American Anthropological Association, exemplified this kind of approach. Since the intent of the persons devising the classification schemes was to standardize terminology, most systems were based on readily perceived differences and similarities among specimens. This meant that form received the greatest attention. However, despite the best efforts of the classifiers, form and function were often conflated. Certainly this was the case with the system devised by the Committee on Archaeological Nomenclature, headed by Charles Peabody of Phillips Academy. Despite the statement that "it has been the particular aim of the Committee to avoid or to get rid of those classes and names that are based on uses assumed but not universally proved for certain specimens" (Wright et al. 1909:114), many of the committee's unit names—such as vessels, knives, and projectile points—have functional connotations in English.

Piles of more or less similar-looking specimens that late-nineteenth- and early-twentieth-century classifiers were forever creating lacked any archaeological meaning: "In an effort to make categorization more systematic and scientific, these early workers had arbitrarily focused on formal criteria that lacked any archaeological or ethnographic rationale" (Dunnell 1986:159). Further, variation in artifact form within each pile—and to some extent between piles—of specimens had no perceived explanatory value and was simply conceived of as noise resulting from different levels of skill in manufacturing or from raw-material quality.

Greenman's scheme was different because he emphasized the identification of variation and established a concise set of criteria to be used in the identification. Using precise language, Greenman (1929:84) explained the rationale behind his classification system: "It is the intentional forms whose distributions are significant, and for that reason stress is laid upon the *types*." In other words, the classification system was developed to create groups that had spatial (and perhaps temporal) meaning; haphazard or idiosyncratic classification couldn't produce such groups. Greenman obviously believed that types were reflections of what the original makers of the projectile points had in mind—hence his use of the term "intentional forms." The epistemological significance of types would be an issue with which Americanist archaeologists would wrestle for decades after Greenman presented his system (e.g., Ford 1954a, 1954b, 1954c; Spaulding 1953, 1954a, 1954b).

Frederick W. Hodge and Warren K. Moorehead on Cultural Complexity

The paper by Frederick W. Hodge (Figure 5), which was read by Roland B. Dixon, was titled "The Importance of Systematic and Accurate Methods in Archaeological Investigation." It was a primer on select topics in archaeological method, including analytical uses to which certain artifacts can be put. For example, Hodge (1929:20) pointed out that pottery was "the most important means of cultural determination" available to the archaeologist. It was "the master-key, above everything else made by primitive man, to the determination of multiple occupancy through stratification, and by its usual fragile character it commonly did not find its way very far from the place of manufacture. It stands to reason therefore that it is of the greatest importance" (Hodge 1929:21). Being a product of the late nineteenth century and the stagelike evolutionism of Edward B. Tylor (1871), Lewis Henry Morgan (1877), and others, Hodge (1929:21) went on to note that "Not all Indians made pottery, to be sure, for some were low indeed in the culture scale, subsisting on the products afforded by a not too prodigal nature and making little in the way of utilitarian, ceremonial, or esthetic objects that have survived to the present time."

Warren K. Moorehead picked up on the notion of cultural complexity in his paper, noting that in the eastern United States there existed a large territory "in which mound art . . . is rather highly developed. Surrounding it in the greater area, mounds and their contents indicate less complex cultures" (Moorehead 1929b:74–75). Moorehead, whose view of archaeology was greatly colored by his work on Ohio mounds (Moorehead 1892a, 1892b, 1897) and by his ongoing work at Cahokia (Moorehead 1927, 1929a; see Kelly 2000), developed a 19-point scale for measuring the culture status of mound-building peoples. The "famous Hopewell culture of the lower Scioto valley [Ohio]" received 13 points, and "the high Etowah culture of north Georgia and of the Tennessee-Cumberland valleys of Tennessee" received 11 points. Fort Ancient—a term originally coined by W. C. Mills (1906) to refer to non-Hopewellian culture in southern Ohio and surrounding regions—was lower still. To Moorehead (1929b:75), Fort Ancient meant "neither high mound builder art nor yet an exceeding low status but might be roughly compared with the term middle class, commonly employed to differentiate the bulk of individuals from those who are extremely well to do or very poor." Illinois Hopewell

Figure 5. Frederick W. Hodge, at various times ethnologist-in-charge at the Bureau of American Ethnology, employee of the Museum of the American Indian, and director of the Southwest Museum in Los Angeles, ca. 1935. (Photo courtesy National Anthropological Archives, Smithsonian Institution)

groups fared less favorably, receiving eight points, but they outscored groups in southern Georgia and Florida, which received only four or five points, despite the fact that "there are an enormous number of shell mounds, platforms for houses or temples, and indications of a very heavy and industrious population" (Moorehead 1929b:75).

Moorehead (1929b:74) admitted that there was "overlap" between the "distinct mound builder cultures" and that archaeologists "have gone entirely too far in extending the boundaries of certain of these cultures." Related to this problem was the origin of the various mound-building groups, and in his paper Moorehead focused specifically on the southern-Ohio Hopewell. We bring up this topic because in the early 1930s it would consume the attention of several archaeologists working in the lower Mississippi River valley, in particular Frank M. Setzler and James A. Ford.

Moorehead, never one to pass up an opportunity to engage in fanciful flights of fantasy, believed that southern-Ohio Hopewell peoples originated in eastern Iowa and at some point migrated eastward. On reaching the Scioto River valley, "where conditions were extremely favorable for their development, they remained, became sedentary, and attained the culmination of their wonderful development" (Moorehead 1929b:77). He indicated that trade items found at Ohio Hopewell sites were evidence that the Hopewellians had a knowledge of the South, but his objection to a southern point of origin of Hopewell was that the Ohio mounds did not contain the kind of ceramic art that was so prominent in the South. Moorehead was apparently unfamiliar with Gerard Fowke's (1928) excavations at the Marksville site in Avoyelles Parish, Louisiana, where he recovered several vessels similar in form and design to vessels from Ohio Hopewell sites. Ironically, Fowke himself failed to note the similarities, even though he had spent considerable time working in Ohio. The similarities, however, would not be lost on Setzler, who in 1933 began a reexamination of the Marksville site. In assessing the resemblances between vessels from Marksville and those from southern Ohio, Setzler (1933a, 1933b) came down decidedly on a south-to-north migration of Hopewell peoples. Setzler would have more to say about this at the Indianapolis conference in 1935. Ford, Setzler's field assistant at Marksville, would have *much* more to say on the subject two decades later (Ford et al. 1955; Ford and Webb 1956).

Matthew W. Stirling and Historical Continuity

In our opinion, the most interesting remarks made at the St. Louis meeting were not in a prepared paper but in comments made by Matthew Stirling (Figure 6) in his discussion of Hodge's presentation. Stirling received his undergraduate degree from the University of California in 1920 and his master's degree from George Washington University in 1922. He joined the U.S. National Museum in 1921, and in 1928 was named chief of the Bureau of American Ethnology. We focus specifically on two points he made, each of which symbolizes where Americanist archaeology was headed in the late 1920s. First, Stirling (1929a:28) noted that "One cannot be a competent archaeologist without ethnological training. Archaeology is not merely a matter of digging and careful observation, but it requires an ability to interpret these observations accurately." This sentiment was not something that Stirling alone felt but rather was an implicit notion that had been present from the earliest days of Americanist archaeology.

Figure 6. Matthew W. Stirling alongside Olmec colossal head 4 at La Venta, Mexico, ca. 1940.

In the United States, degrees were not granted in archaeology but in anthropology—a phenomenon that holds true today. Any professional archaeologist in attendance at St. Louis would probably have agreed with Stirling's remarks, having spent several years taking courses in general ethnology as well as courses focused on the ethnology of particular groups or regions. As we discuss elsewhere (e.g., Lyman et al. 1997; O'Brien and Lyman 1998, 1999c), much of what passed as archaeological theory during the culture-history period was grounded in ethnological theory. Thus the archaeological record was viewed in ethnological terms, and it became commonplace to equate such things as artifact assemblages with particular "cultures."

The second point Stirling made was related to the first, and it concerned the tracking of ethnohistorically known groups back in time. Stirling (1929a:25) saw two extremes in archaeology: "On the one hand is the tying up of archaeological research with the historical period concerning which we have definite information, and on the other hand the projecting of it backwards to that period of which we may be able definitely to say that there was no human occupancy of this continent." There was, however, a means of linking these two extremes, and Stirling (1929a:25) laid it out in clear terms for his audience:

> It is possible to determine rather definitely the dates of the introduction of certain types of articles of European manufacture which may have been found in an archaeological site. We know when and where certain varieties of trade beads were made; we know rather definitely the period during which certain smoking pipes were manufactured and introduced as trade articles among the Indians, and there are innumerable other examples of the same sort which may aid greatly in giving us something definite from which to project backwards a chronological sequence.

Why, Stirling asked, should an archaeologist be depressed upon discovering a silver ornament or a string of glass beads alongside articles of native origin? To the contrary, "There is no justification for such a reaction, and in most instances the archaeologist should feel rather a sense of elation. Where an association of this sort is discovered it becomes possible by a process of overlapping to carry a native culture throughout its successive stages of development well back into the prehistoric period" (Stirling 1929a:25).

Stirling was advocating what his Smithsonian colleague Waldo Wedel (1938) would refer to a decade later as the *direct historical approach*. No one can legitimately argue with the logic of the approach, which was not new in the 1930s but, as we discuss in more detail in the next section, had been the strategy adopted in the 1880s by John Wesley Powell and Cyrus Thomas (1894) for the Division of Mound Exploration in its quest to destroy the myth that a race of people separate from Native Americans had constructed the thousands of mounds evident across the eastern United States: First, document similarities in cultural materials between those evident from ethnographic and ethnohistorical research and those evident archaeologically. Second, assume similar materials are temporally and phyletically related and construct a continuous thread, or cultural lineage, from the past to the present (Lyman and O'Brien 2000; O'Brien and Lyman 1999a). Roland B. Dixon (1913) had espoused just such a strategy in his presidential address to the American Anthropological Association in 1913.

In "An Introduction to Nebraska Archeology," Bureau of American Ethnology archaeologist William Duncan Strong (1935; see also Strong

1936), who received his Ph.D. under A. L. Kroeber at the University of California in 1926, noted the importance of the direct historical approach:

> It is the firm belief of the author that the possibilities of historic archeology in North America are not fully realized by the majority of anthropologists at the present time. . . . It seems surprising, therefore, that even today there are archaeologists more interested in segregating obscure early cultures of unknown periods and affiliations than they are in determining the historic cultures and sequences represented in the regions to be worked. Obviously, in such work the historic cultures need not be an end in themselves, but they do seem to represent the threads that give most promise of untangling the complex skein of prehistory. (Strong 1935:296)

There are two critical aspects of the direct historical approach. First, it provides "a fixed datum point to which sequences may be tied" (Steward 1942:337); that is, it provides a chronological anchor in the historical period to which archaeological materials of otherwise unknown relative age can be linked. Second, the more similar prehistoric materials are to the historically documented materials, the more recent they are; conversely, materials that are less similar to historically documented materials come from further back in time. Thus the direct historical approach demands the study of homologous similarity, a point generally unrecognized at the time (Lyman and O'Brien 1997, 2000). Without a chronological anchor, sequences cannot be established, and assemblages of artifacts have the unsavory characteristic of floating in time and thus being of minimal utility in determining the development of historically documented cultures. This is the point Stirling was making in his comments on Hodge's paper, and it was the same point made by Neil Judd, curator of archaeology in the U.S. National Museum, in a paper published in the *American Anthropologist* that same year. Judd (1929) lamented that archaeologists knew little about the late prehistoric remains of more than 200 historically known tribes and noted that a "relative chronology for each culture area is one of the surpassing needs of archaeology in the United States today" (Judd 1929:418). As we will see, the NRC-sponsored Conference on Southern Pre-History addressed this issue head on.

The Bureau of American Ethnology

In closing the St. Louis meeting, Fay-Cooper Cole (1929:112) expressed the feeling that "we will all leave here, much more assured of the future of archaeology than when we came here two days ago." There may have been some reason for such optimism, but it is apparent that the field was still plagued with difficulties. Nowhere was this more apparent than in the Southeast, where state and local institutions were for the most part working in an intellectual vacuum. Compared to the Midwest and Northeast, there was a dearth of trained archaeologists, which meant there was little or no hope of introducing current methodological advances to the amateur societies that seemed to crop up everywhere. This situation did not escape the notice of professional archaeologists working in other regions, and it was the major reason the Conference on Southern Pre-History was held in Birmingham, Alabama, late in 1932. Although the CSAS was instrumental in organizing that conference, we need to look a little deeper at the few professionals who were working in the Southeast just prior to that meeting to determine what their influence on the field was. Stirling's closing comments at the conference in St. Louis provide a starting point for examining that topic. After mentioning the myriad issues that participants had addressed, Stirling (1929b:109–112) added,

> there is one topic on which I might profitably add a few words, and that is something concerning the history and the nature of the institutions which I represent: The Smithsonian Institution and the Bureau of American Ethnology, which is a part of that great institution. . . .
>
> The Bureau of American Ethnology at the present time has, among its duties, not only the pursuit of field work in various parts of the country, but it has also become, in a way, a court of appeal for the population throughout the country who are interested in matters pertaining to anthropology. . . .
>
> There is probably no organization in the country that has published as many pages or as many volumes dealing with the American Indian and with the subject of Archaeology as has our Bureau. . . . We stand ready to assist at any time, to the best of our ability, any of you who are interested or professionally engaged in the study of archaeology.

This assistance showed up in a significant way in Birmingham just a few days before Christmas 1932. Although the conference was attended by professional archaeologists from a number of institutions, it was personnel from the Bureau of American Ethnology and its sister institution, the U.S. National Museum—individuals who, as Stirling put it, stood "ready to assist at any time"—who had by far the most impact on the group.

The Bureau of American Ethnology was founded in 1879, and its involvement in the Southeast dates to the formation of the Division of

Mound Exploration within the bureau in 1881 and the mandate that bureau director John Wesley Powell received from Congress to decisively answer the question of which group or groups constructed the thousands of earthen mounds so evident across the eastern United States. By the time he was appointed to head the Division of Mound Exploration, Cyrus Thomas, like most other prehistorians, was convinced of the equation of the mound builders with the American Indians. For example, in 1884 he asked and then answered the question, "'Who were the mound-builders?' We answer unhesitatingly, Indians—the ancestors of some, perhaps of several of the tribes of modern or historic times" (Thomas 1884:90). Thomas published the "Report on the Mound Explorations of the Bureau of Ethnology" in the *Twelfth Annual Report of the Bureau of Ethnology, 1890–1891* (Thomas 1894), and in it he discussed in detail the mound explorations carried out by members of his crews as they worked their way over two dozen eastern states, including all the southern states, with the exception of Texas and Virginia.

Thomas continually referred to historical records of the sixteenth through eighteenth centuries, where it was documented that the post-Columbian Indians were sufficiently "culturally advanced" (being sedentary agriculturists) to have built the mounds. In some cases Indians had actually been observed building them. Documenting typological similarity of artifacts from the historical and prehistoric periods (e.g., Holmes 1886, 1903) merely completed the evolutionary, ethnic, and cultural linkages on which the direct historical approach was founded (Meltzer and Dunnell 1992; O'Brien and Lyman 1999c). Thomas noted that there was no logical reason to suspect that the mound builders were of Mexican origin or that later Indian groups had pushed the mound builders south into Mexico. In other words, the archaeological record demonstrated to Thomas's satisfaction that a high degree of cultural continuity had existed for an untold number of millennia and that such threads of continuity showed no major disruptions. Undoubtedly, change had occurred—that much was indicated by the myriad forms of earthworks recorded and the different kinds of artifacts found within them—but such change was an orderly, continuous progression as opposed to a punctuated, disruptive progression of cultural epochs such as was evident in the European Paleolithic-Neolithic sequence (Lyman and O'Brien 1999; Meltzer 1983, 1985). To Thomas, continuity had ruled throughout human tenure in the East, and it is clear that he favored tribal differences to explain the immense variation evident in the archaeological record.

Jesse Jennings (1974:39) once noted that the publication of Thomas's report could be thought of as "marking the birth of modern American archeology," although as we have noted elsewhere (O'Brien and Lyman 1999c) we consider this to be an overstatement. Prior to the founding of the Division of Mound Exploration in 1881, archaeology was primarily an antiquarian activity, meaning that interest centered on artifacts and earthen monuments themselves rather than on using such things as a means to other ends. The work summarized by Thomas (1894) was superior in many ways to what had come before, primarily because he demanded rigor in how materials and information were gathered (Smith 1990), but it was not particularly revolutionary. Further, to use 1894, the date of publication of Thomas's final report, as marking the birth of modern American archaeology overlooks the excellent work done by Harvard's Frederic Ward Putnam and those he trained.

The case can be made that it was through Putnam's example (e.g., Putnam 1887), and certainly through the training he provided, that Americanist archaeologists began excavating stratigraphically and keeping track of artifacts by stratum. By the time the so-called stratigraphic revolution (Browman and Givens 1996; Willey and Sabloff 1993) occurred in New Mexico a decade and a half into the twentieth century (e.g., Nelson 1916, Kidder 1916), those trained or influenced by Putnam—Henry Mercer and Charles Peabody, for example—had been digging stratigraphically in the East since the late 1800s (Browman 2000; Lyman and O'Brien 1999; O'Brien and Lyman 2000). Call him what you will—the "Father of American Anthropology" (Phillips 1973), the "father of American archaeology" (Dexter 1966), or the "professionalizer of American archaeology" (Mark 1980; Willey and Sabloff 1993)—Putnam played as large a role in the birth and subsequent growth of Americanist archaeology as Cyrus Thomas did.[21]

With the death of the mound-builder myth in the closing decade of the nineteenth century, Bureau of American Ethnology archaeologists turned their attention to other matters, some of which had been of considerable concern to them for some time. The one that has received the lion's share of attention from historians of archaeology (e.g., Meltzer 1983, 1985) was the great debate over the antiquity of humans in North America. Southeastern prehistorians, with rare exceptions, did not figure into this debate, but they were active nonetheless, and their activities did not go unnoticed. Over time, both the Bureau of American Ethnology and the National Museum began turning their attention to the Southeast as their interest became piqued by what prehistorians were uncovering there. One such individual was Clarence B. Moore, yet another Harvard-trained prehistorian, who spent a quarter of a century, from roughly 1892 to 1917, exploring mounds along the major waterways of the southern states, in the process excavating several thousand skeletons and recovering countless ceramic vessels and other artifacts. Although his work was not sponsored by a federal agency, it would be important background material for research by later archaeologists. He underwrote not only the costs of his projects but also the expense of producing 20 reports dealing with the excavations, which appeared in the *Journal of the Academy of Natural Sciences of Philadelphia*. The reports are rather sketchy, but the accompanying field photographs and artifact illustrations are excellent. Moore's work (e.g., 1892, 1894, 1896, 1902, 1905, 1907, 1908, 1909, 1910, 1911, 1912, 1913), and especially the artifacts it produced, spurred a resurgence of interest in the Southeast, especially by small state organizations and regional museums—precisely the groups at which Matthew Stirling and the Bureau of American Ethnology took aim in the early 1930s.

The Conference on Southern Pre-History

It was into these intellectually rather shallow waters of southeastern archaeology that the CSAS waded in 1932 when it hosted its second regional meeting designed to facilitate communication among archaeologists.[22] Organizers, again led by Carl Guthe, were careful not to give the impression that a group of outsiders, all from the North, was telling southerners not only how to do archaeology but also how to organize a meeting. Neil Judd expressed this concern to Guthe in a letter written in September 1932: "As you well know, the South is most conservative and sectional in its attitude; in general it resents northern advice and aid however altruistic" (cited in Lyon 1996:54).

The three-day conference, which was, as Jon Gibson (1982:258) pointed out, "without doubt one of the most influential professional meetings ever held on Southeastern archaeology," convened at the Hotel Tutwiler in Birmingham, Alabama, on December 18, 1932. The report that was issued after the meeting[23] carried the text of the papers presented, along with comments made by session chairmen. The report makes it obvious that Guthe took Judd's concern seriously when he drew up the program, because although the major papers were by nationally recognized archaeologists and anthropologists from northern institutions—in addition to Guthe, Judd, Wissler, Cole, and Moorehead, presenters were Ralph Linton of the University of Wisconsin and John R. Swanton, Matthew W. Stirling, and William Duncan Strong of the Bureau of American Ethnology—their papers were interspersed among summaries of the archaeological records of individual states, presented for the most part by southern prehistorians familiar with those records. Peter A. Brannon of the Alabama Anthropological Society and long-term member of the CSAS[24] chaired the session "Recent Field Work in Southern Archaeology," in which Samuel C. Dellinger of the University of Arkansas spoke on Arkansas, Walter B. Jones of the Alabama Museum of Natural History spoke on Moundville cultures, Charles K. Peacock of the East Tennessee Archaeological Society spoke on Tennessee, and James E. Pearce of the University of Texas spoke on eastern Texas. In addition, Winslow Walker of the Bureau of American Ethnology spoke on Louisiana, and Henry B. Collins of the National Museum spoke on Mississippi.

Although it carried no byline, the short introduction to the conference volume was authored by Guthe. In it he stated the purpose of the conference:

> The Conference on Southern Pre-History . . . was called for the purposes of reviewing the available information on the pre-history of the southeastern states, discussing the best methods of approach to archaeology in this region, and to its general problems, and the developing of closer cooperation through the personal contacts of the members of the conference. During the past few years, the interest in Indian pre-history of the lower Mississippi Valley and the southern Atlantic states has been increasing steadily, and a number of institutions have undertaken research work in this field. Developments from studies of the same period in the northern part of the Mississippi Valley and from work on certain Southwestern problems indicate that as the knowledge of the pre-historic cultures of the southeast increases, the problems of the neighboring areas will be more clearly understood. It was for the purpose of fostering more rapid increase of this knowledge that this conference of experts in the study of pre-history from all over the United States was called to meet with interested students of the South. (Guthe 1932b:1)

Guthe selected his words carefully because he was really saying that nowhere in the Southeast were approaches that were routinely employed in the Southwest being incorporated into fieldwork and analysis. Part of the problem lay in the attraction the Southwest had long held for prehistorians—archaeological brainpower had been drained into that region at the expense of other regions (O'Brien and Lyman 1999c)—and part of it lay in the fact that southern universities were not producing students trained in archaeology. In states such as Alabama the majority of work was undertaken by museums, often in conjunction with local archaeological societies. In other states, amateur-based societies were left to their own devices. In some cases the quality of work was credible for the time period, but in others it was deplorable.

In language a bit stronger than Guthe's, Collins, who was then assistant curator in the ethnology division of the National Museum, summed up the state of affairs in the Southeast. He was speaking specifically of one state, but his remarks were applicable to the region as a whole: "Although Mississippi is rich in aboriginal remains and a considerable number of these have been investigated, it cannot be said that the work has clarified to any great extent the archaeological problems involved. The early investigators, in accordance with the unfortunate tendency of the time, too often proceeded on the assumption that the accumulation of specimens was an end in itself rather than a means toward the elucidation of archaeological problems" (Collins 1932:38).

Ensuring that everyone was on the same page meant that the regional experts—the ones actually doing much of the work in the Southeast—either had to be trained in proper procedure or, failing that, had to be made aware of what proper procedure was. To that end, the last day of the conference was dedicated to three topics—"exploration and excavation," "laboratory and museum work," and "comparative research and publication"—with the morning devoted to presentations by Cole, Judd, and Wissler and the afternoon to discussions led by Moorehead, Strong, and Webb, who was soon to head much of the federal-relief archaeology that took place in the South (Griffin 1974; Haag 1985; Lyon 1996). The sessions were geared toward imparting information on the proper methods of excavating a site, of analyzing artifacts, of preserving those artifacts, and of presenting the results of the work. These were critical topics to members of the Committee on State Archaeological Surveys, as evidenced by their publishing the suggestions on field methods early in the history of the committee (Wissler et al. 1923). The publication by the committee of a second pamphlet on field methods (CSAS 1930; see Appendix 2) took place only two years before the Conference on Southern Pre-History.

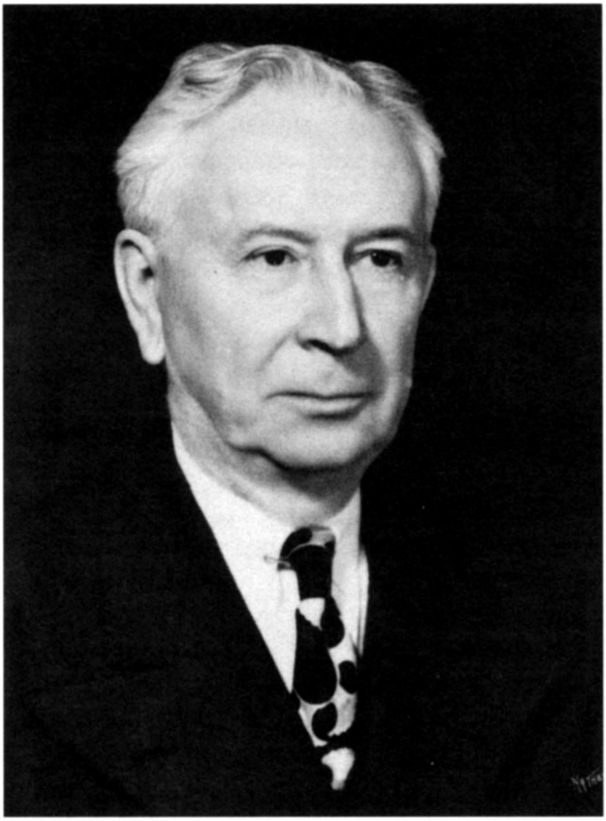

Figure 7. Fay-Cooper Cole, of the University of Chicago, ca. 1950. (Photo courtesy University of Chicago Press)

Fay-Cooper Cole (Figure 7), who received his doctorate from Columbia in 1914 and had assumed the chairmanship of the anthropology department at the University of Chicago in 1929, discussed proper procedure for excavating a mound, using a procedure we elsewhere (Lyman and O'Brien 1999) refer to as the bread-loaf technique, after Gordon Willey's (1936) notation that excavating in such a manner was like slicing a loaf of bread:

> If [the site] is a mound it is staked out in squares (five foot squares are usually most convenient). A trench is started at right angles to the axis of the mound and is carried down at least two feet below the base. The face of the trench is now carried forward into the mound itself by cutting

> thin strips from top to bottom. At the same time the top is cut back horizontally for the distance of a foot or more. If this procedure is followed it is possible to see successive humus layers as well as to note all evidences of intrusions. . . .
>
> A village site is best uncovered by a series of trenches much like those used in mound work. A cut is made down to undisturbed soil and the earth is thrown backward as the excavation proceeds. Horizontal and vertical cutting should be employed in hopes of revealing successive periods of occupancy. The worker should never come in from the top. He should never be on top of his trench, otherwise lines of stratification will almost certainly be lost. (Cole 1932:76, 78)

This method has a long history in Americanist archaeology, dating back to the late nineteenth century and the influence of Frederic Ward Putnam, but it is quite evident from reading the literature that what Cole had to say in 1932 must have appeared revolutionary to most southeastern archaeologists.

It is unclear how much of a result the methodological presentations by Cole, Judd, Strong, and others actually had on southeastern archaeology, but the same cannot be said of some of the papers presented in the sessions of December 19, especially those by Walker on Louisiana, Swanton on southeastern Indian groups, and Collins on Mississippi. The intellectual tradition of the Southeast was in large part set in motion by what they had to say.

Winslow Walker and Louisiana Prehistory

Walker's point was simple: everything that an archaeologist wanted to do necessarily hinged on the ability to order remains chronologically. By 1932, seriation and superposition had been used as ordering methods in the American Southwest for almost two decades, but this was not the case in the Southeast. In fact, seriation never caught on there, despite statements to the contrary (e.g., Ford 1962), and it was stratigraphic excavation and the accompanying use of sherds as index markers that would form the backbone of archaeological dating (O'Brien and Lyman 1998, 1999b; O'Brien et al. 2000). Walker (1932:48), however, had a different strategy in mind when he noted that "it is futile to attempt a classification of pre-historic mound cultures in the lower Mississippi Valley until we know more definitely whether or not they have any connection with the principal [historical] tribes found there. . . . Some of these Indians we know were builders of mounds, but just which ones, and through what stages of development they may have passed, are problems requiring further attention."

The link between peoples living during the prehistoric period and those occupying the region during historical times was what Walker referred to as the "proto-historic" period—a temporal unit about which, Walker (1932:48) admitted, "we are completely in the dark archaeologically." How did one deal with the protohistoric period? Walker (1932:48) had the answer—one that had long been apparent to Smithsonian Institution archaeologists working in the Southeast: "The clue to this phase is the identification of sites visited by the Spaniards in 1542 and by the French in 1682. Special investigations should be made of all relics purporting to date back to either of these periods of exploration." Walker (1932:48) also addressed the investigation of prehistoric remains: "Sites known to contain only prehistoric material should not, of course, be neglected, as there is much work to be done in determining the relationships of the northern and southern mound cultures. But it is more important to establish first the succession of historic and proto-historic cultures, before attempting to say positively just what cultures belonged strictly to prehistoric times." Walker was advocating the use of what his colleagues in the Bureau of American Ethnology and National Museum had been using for years: the direct historical approach.

As Gibson (1982:259) noted, what Walker had to say about the promise of Louisiana's archaeological record and the future directions that should be taken in an effort to understand that record apparently had a profound effect on two young men in attendance—James A. Ford and Fred B. Kniffen, the latter a newly appointed faculty member at Louisiana State University who had trained under Kroeber and geographer Carl Sauer at the University of California. Both Ford and Kniffen immediately began orienting their work in some of the directions in which Walker was pointing (O'Brien and Lyman 1998, 1999b), one direction being the correlation between archaeological-site location and river channels—or more precisely, using the history of river channels to date archaeological sites. Kniffen had already begun exploring the relation between site location and geomorphic features in southern Louisiana, especially relative to land subsidence, as part of Richard Russell's coastal-environments program at Louisiana State University, but he would soon develop several other innovative techniques (Kniffen 1936, 1938), in part because of Walker's influence (O'Brien and Lyman 1998, 1999b).

John R. Swanton and Southeastern Ethnohistory

The success that Walker and other archaeologists working in the Southeast had in applying the direct historical approach was based in large part on the work of John R. Swanton (Figure 8), a Harvard-trained archaeologist-turned-ethnologist who spent his career with the Bureau of American Ethnology. Swanton's early work was on North American Indian languages, and although he continued to produce linguistical texts throughout his career (e.g., Dorsey and Swanton 1912; Gatschet and Swanton 1932; Swanton 1919, 1940; Swanton and Halbert 1915; Thomas and Swanton 1911), he became better known for his ethnohistorical work, especially as it related to the route Hernán de Soto took during his southeastern entrada. Swanton was an archaeologist's dream—someone who both spoke the language and was sympathetic to the goals of prehistory. More importantly, Swanton was someone who could place individual Indian groups in particular places at particular times. This was no small feat in the Southeast, where Indian tribes had experienced centuries of contact with a succession of white groups—Spanish, French, British, and American—resulting in the constant movement of aboriginal groups from one locality to another. It took someone like Swanton, who Kroeber (1940a:3) characterized as "exhibit[ing] a streak of historical genius," to sift through the myriad historical documents on the Southeast and to figure out where particular aboriginal groups were at different times in the past. It was because of the perceived importance of Swanton's work to archaeology that Albert L. Barrows, assistant secretary of the National Research Council, asked Swanton not only to look over the preliminary program for the conference in Birmingham well in advance of the meeting but also to brief council chairman W. H. Howell on his thoughts—all in an effort to make the conference "as useful an occasion as possible in advancing the interests of archaeological research in the southeastern part of the United States."[25]

Swanton addressed the broad issue of southeastern prehistory in two papers he presented in Birmingham, one titled "Southeastern Indians of History" and the other "The Relation of the Southeast to General Culture Problems of American Pre-History." Neither was particularly earthshaking but rather a synopsis of what he had been advocating to archae-

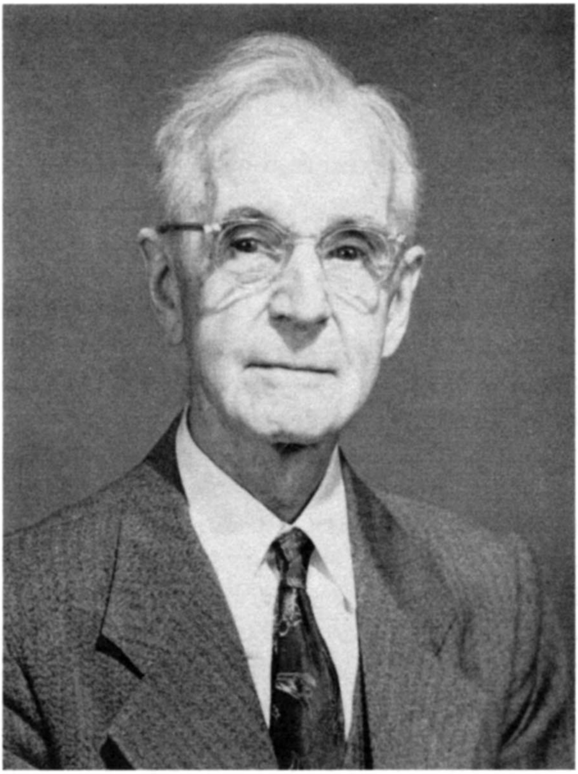

Figure 8. John R. Swanton, longtime ethnologist with the Bureau of American Ethnology, ca. 1945. (From Biographical Memoirs *National Academy of Sciences* 34:328–29 [1960], Columbia University Press, used by permission)

ologists for years: use the ethnohistorical record as a starting point—the chronological anchor—for the reconstruction of prehistory in the Southeast.

Henry B. Collins and Southeastern Culture History

Henry B. Collins (Figure 9) paid homage to Swanton in his paper on historical-period sites in Mississippi: "Our knowledge of the ethnology of the Mississippi Indians is based almost entirely upon the work of Dr. John R. Swanton, whose careful researches have thrown much light on the linguistic and cultural affinities of the Muskhogean and other southern stocks" (Collins 1932:37). However, Collins (1932:37–38) also noted that "There yet remains the task of determining the limits of various groups

Figure 9. Henry B. Collins, assistant curator of ethnology, U.S. National Museum, Smithsonian Institution, ca. 1930. (Negative no. 11,033-A; photo courtesy National Anthropological Archives, Smithsonian Institution)

in pre-historic times [and] their relations one to another and to other southeastern groups, an undertaking that as yet has been hardly begun." Collins (1932:38) believed the most immediate problem facing southeastern archaeologists was the lack of a "basis for chronology," and like his colleagues at the National Museum and Bureau of American Ethnology, he advocated using the direct historical approach. Collins had done the same in an earlier paper on Choctaw village sites in Mississippi, in which he stressed how important it was for southern archaeologists "to seize upon every available source of tribal identification of the cultures represented, and [that] to accomplish this end there is probably no safer beginning than to locate the historic Indian village sites and to study their type of cultural remains for comparison with other sites of unknown age" (Collins 1927:259–260).

By the time of the Birmingham conference, Collins was convinced that of all the "available source[s] of tribal identification," pottery held the most hope for developing chronological ordering:

> potsherds are of decided value as chronological determinants and, if present in sufficient quantities to show the entire pottery range of the site, are of far more significance than a number of complete vessels which might not happen to show such a range. In fact, the obliterating effect of white civilization has reached such a point that at many aboriginal sites potsherds are the only really useful material that the archaeologist is able to salvage. The lowly potsherd thus seems destined to bear much of the weight of the chronology that we all hope may sometime be established for Southern archaeology. (Collins 1932:38)

As we discuss in detail elsewhere (O'Brien and Lyman 1998, 1999b, 1999c), Collins (1927) also believed that a pottery type designates an ethnic group, such as a tribe, that ethnic groups have histories, and that a pottery type designates a specific period in the history of an ethnic group. These were common assumptions among southwesternists (Lyman et al. 1997), but they were novel thoughts from someone working in the Southeast. In short, they provided the epistemological warrant for application of the direct historical approach (Lyman and O'Brien 2000).

Of all the federal archaeologists working in the Southeast, Collins would have the most significant and lasting impact. His work in Louisiana and Mississippi during the 1920s is of particular interest because of the impact it had on succeeding generations of archaeologists—an intellectual genealogy that can be traced from Collins through Ford, who from the late 1930s to the middle 1950s was the dominant force in southeastern archaeology. Collins trained Ford in the late 1920s when the latter was still a high-school student, and Ford later used what he learned while working in western Mississippi as he set about the arduous task of carving up prehistoric time in the lower Mississippi River valley (O'Brien and Lyman 1998, 1999b; O'Brien et al. 2000).

The Legacy of the Conference on Southern Pre-History

Ralph Linton (1932:3), in the remarks that opened the second day of the conference, stated explicitly the research questions that would soon guide much of southeastern archaeology: "The worker in any of the surrounding regions finds evidences not merely of diffusion, but of actual migrations coming into his particular area from the southeast, but until the history of that region is better known, it is impossible for him to tell when such migrants left the southeast, what part of it they came from, what their cultural or racial affiliations may have been, or how they are linked to other cultures marginal to the same area."

Stirling (1932:20–21) reiterated Linton's remarks, thereby reinforcing them in the minds of those in attendance. He also specified the procedure for addressing the issues Linton raised: "The first problem in developing the archaeology of the given locality is to isolate the known historic cultures leaving a residue of unknown pre-historic, should such exist. Both vertical and horizontal stratigraphy can usually be applied. . . . From our knowledge of the pottery used by the historic tribes, many significant hints are offered regarding pre-historic movements of peoples." This procedure was nothing more than the direct historical approach. Stirling (1932: 22) also offered the important caution that "the inter-relationship of cultures [is] a flow rather than a series of static jumps." The significance of that caution was lost not only on archaeologists working in the Southeast but on those working in the Americas generally (Lyman and O'Brien 1997; Lyman et al. 1997; O'Brien and Lyman 1998, 2000).

Collins (1932:37) indicated, for example, that one could determine "the limits of the various [ethnic or tribal] groups in pre-historic times," and he stated that typological differences in pottery denoted "cultural differences" (Collins 1932:40). This was, in short, a way of saying that his understanding of the archaeological record was derived from ethnological theory and ethnographic data. Tribes were viewed as discrete chunks of humanity that bore distinct cultural traits and had particular locations in time and space. Assuming that it was possible to identify cultures in the archaeological record (usually on the basis of some typologically distinctive artifacts), when such an identification was made, each prehistoric culture must, it was thought, represent a discontinuous ethnic unit, such as a tribe. This way of thinking was simply the notion of culture areas, popularized in the earlier work of Clark Wissler (1914, 1916, 1917, 1923b, 1924) and having its roots in the culture-classification work of Otis T. Mason (1896, 1905), in Cyrus Thomas's (1894) regional groupings of mound forms, and in William Henry Holmes's (1886, 1903) regional groupings of pottery. This approach was already coming under close scrutiny by several midwestern archaeologists, and its replacement would form the central focus of the third regional conference organized by the CSAS.

With the benefit of hindsight, the ontological parallels between the concept of biological species and the concept of prehistoric cultures are remarkable (Lyman and O'Brien 1997; Lyman et al. 1997). The analytical problem is one of identifying the historically antecedent species or cultures that were also ancestral (in an evolutionary sense) to historically or ethnohistorically documented species or cultures, respectively. In other words, between about 1910 and 1970 phyletic histories of cultures were determined in precisely the same sense that prehistoric *Homo ergaster* is today conceived of as having evolved (perhaps) into *Homo sapiens.* The procedure for determining these phyletic histories was introduced in the Southeast so that culture history could be written there as it had elsewhere in the Americas.

The procedure focused on *homologous* similarity, or similarity resulting from shared ancestry. Thus, for example, Frank Setzler's (1934) work at Marksville, Louisiana, resulted in the conclusion that the people who occupied that site were culturally and biologically related to people who deposited artifacts assigned to the Hopewell culture of Ohio (O'Brien and Lyman 1998). By the end of the Birmingham meeting, the Bureau of American Ethnology and the National Museum had successfully diffused to southeastern archaeologists the general idea that typological similarity denoted homologous similarity. The idea made sense from the perspective of Swanton, Linton, Walker, and Collins, all major figures in the discipline at the time, and everyone in attendance adopted it. The take-home message was simple: work from the known to the unknown so that you have (a) a chronological anchor for your temporal sequence of cultures and (b) the most recent evolutionary descendant of a cultural lineage to use as a comparative base for determining historically antecedent cultures. This was not really a new message, but southeastern archaeologists adopted it wholeheartedly and took the direct historical approach to heights unparalleled in Americanist archaeology (O'Brien and Lyman 1999c).

If a picture is worth a thousand words, then it would take about 30 pages of text to explain what the seven figures included at the end of the Birmingham report show very neatly: the state of southeastern archaeology in 1932. After looking at the figures, can there be any doubt that the concept of culture areas was basic to everyone's thinking? Although several presentations in Birmingham, like a few in St. Louis three years earlier, made mention of temporal differences between segments of the archaeological record, the conference as a whole was, as James B. Griffin (1976a:19) later characterized it, "a Culture area approach." Of particular interest from a historical point of view is Figure 7, which is a map showing the distribution of some archaeological complexes in the East. This is what Griffin (1976a:19–20) had to say about the map:

> Stirling refers to it once but in the wrong context. I do not know who made the map. It contains the regions delimited in Stirling's paper but some of them do not follow his boundaries. In addition, there is located on the map Hopewell in Ohio, Illinois and the Upper Mississippi Valley; Fort Ancient and Adena, which were probably put in by Setzler. . . . In Illinois we see Black Sand, Cahokia, and Illinois Bluff. The latter is probably the Spoon River Mississippi material. This could have been put on by Setzler or Walker. . . . There are also added the terms Lake Michigan and Upper Mississippi derived from [W. C.] McKern. W. D. Strong had recently joined the Bureau of American Ethnology and undoubtedly helped to add Signal Butte, Mill Creek, Nebraska, Glenwood, and Upper Republican. The map was used in J. R. Swanton's second talk of the conference from the standpoint of attempts to identify the tribal groups responsible. At a later date Kroeber was to commend the map for indicating the presence of Hopewell culture in three different areas.

One might well pose the question, With this heavy reliance on culture-distribution maps and the direct historical approach, wasn't anyone interested in prehistoric chronology? The answer is, yes they were, but they weren't sure how to go about creating a strictly prehistoric chronology. The answer perhaps was beginning to buzz around in the head of one of the youngest attendees at the Birmingham meeting, but that answer was still a few years off. That attendee was Ford, who before the end of the decade would, with Gordon R. Willey, create a prehistoric chronology for the lower Mississippi Valley (Ford 1935a, 1935b, 1936a, 1936b, 1938; Ford and Willey 1940, 1941). But in those crucial years between 1932 and 1935, a group of midwestern archaeologists decided to try a different tack in their relentless pursuit of making sense out of a vexingly complex material record. They decided to ignore temporal differences in the record, at least for the moment, and to concentrate on formal similarities and differences between and among sets of artifacts. Maybe, if assemblages could be categorized into groups that minimized intragroup difference and maximized intergroup difference, this would tell them something important. Efforts to explore the usefulness of this method culminated in the third and final NRC-sponsored conference, this one held at the Marrott Hotel in Indianapolis on December 6–8, 1935.

Carl Guthe prepared the preface to the mimeographed report that emanated from the Indianapolis conference (NRC 1937), and in the second paragraph he laid out the purpose of the meeting:

> The conference was called for the specific purpose of discussing the technical problems relating to the comparative study of the archaeological cultures in the upper Mississippi Valley and Great Lakes region. Detailed descriptions of the results of the investigation of individual sites were not pertinent to the meeting. The group of delegates was purposely kept small in order to insure the freedom of informal discussion, and was confined to research students who were interested either in the archaeological problems of a restricted part of the area, or in the comparative significance of these problems with relation to similar ones in other areas. (Guthe 1937:v)

The number of attendees, 19, was indeed small, pared down in number from the 40 persons who attended the Conference on Southern Pre-History and well short of the 53 individuals at the St. Louis conference. With two exceptions, amateur archaeologists were not invited to Indianapolis. Three anthropologists from Washington, D.C., attended—Frank M. Setzler of the U.S. National Museum and Frank H. H. Roberts and John Swanton of the Bureau of American Ethnology—but the majority of those at the meeting were from midwestern institutions: Guthe, Emerson F. Greenman, and young archaeologist James B. Griffin (Figure 10) from the University of Michigan; Lloyd A. Wilford from the University of Minnesota; W. C. McKern from the Milwaukee Public Museum; Charles R. Keyes from the State Historical Society of Iowa; Thorne Deuel from the University of Chicago; and Glenn A. Black and Paul Weer from the Indiana Historical Society. Also from Indiana were two nonprofessionals, E. Y. Guernsey and Eli Lilly. Despite the latter's technically nonprofessional status, his contributions to midwestern archaeology—both monetary and in terms of research—were significant (Ruegamer 1980). Cole was absent for health-related reasons but sent a letter that was read to those in attendance.

In his preface Guthe touched on some of the "technical problems," as he put it, related to the comparative study of archaeological cultures in the upper Mississippi Valley and Great Lakes region. The greatest need was for "a uniform methodology and a greater correlation" of the various investigations that had been taking place with increasing frequency over

Figure 10. James B. Griffin (*front center*) of the University of Michigan examining pottery from the Kincaid site, Massac County, Illinois, ca. 1939. Front (*left to right*): Irvin Peithman, Fay-Cooper Cole, Griffin, Charles R. Keyes. Back (*left to right*): W. M. Krogman, Richard Morgan, and Roger Willis. (From Cleland 1976, used by permission of Academic Press; photo courtesy Museum of Anthropology, University of Michigan)

the previous decade (Guthe 1937:v). McKern (1937a:1), who presented the opening paper at the conference, was more specific: "I can't discuss local Wisconsin problems without touching on general problems. These center around an inadequacy of analytical and systematic methods and terminology. Our major problem is determining how to cooperate to mutual advantage with students of cultures similar to those in Wisconsin. We have great difficulty understanding each other because we do not do things in the same way, and lack a systematized terminology. My specific problems relate to cultural manifestations and their place in the classification."

McKern was feeling the effects of a problem that went far beyond the borders of Wisconsin. By the 1930s Americanist archaeologists had come to something of an impasse over the means and terms used to describe and discuss assemblages of artifacts. The term "culture" was ubiquitous in the role of a grouping unit, but it varied tremendously in scope and meaning from one application to the next. McKern (1943:313) later recalled that this "vague and varying use of the word 'culture' to describe manifestations which were so unlike in scope and character, of which some were culturally correlative—but in different degree, while others lay wholly outside the specific field of relationship, led logically and necessarily" to his becoming interested in developing a method of categorizing archaeological phenomena so that they could be discussed and compared systematically.

How to formulate and implement such a method was the key topic addressed at the conference, and in reading through the discussions one gains an appreciation for the complexity of the issues facing archaeologists in the 1930s—not just those working in the Midwest but in all parts of the country. How could archaeologists communicate without a standardized set of terms? How could "cultural manifestations" be classified in terms of time, space, and form if everyone was using a different system? Or, as was beginning to be asked, was it even wise to try and keep track of those three aspects simultaneously? Was it perhaps more practical, given a lack of detailed regional chronologies, to concentrate foremost on form and then bring time and space in as they became known? By 1935 this was a key question in certain quarters, and it was beginning to be answered more and more in the affirmative. The method that grew out of that question and that was formalized in Indianapolis set midwestern archaeology on an interesting course, but one that was to produce little in the way of enduring results, despite statements to the contrary (e.g., Guthe 1952). The chief navigator of that course was McKern.

W. C. McKern and the Midwestern Taxonomic Method

Carl Guthe (1937:vi) noted that the "Indianapolis Conference holds a significant place in the history of the development of Middle Western archaeology," but to understand that significance one needs to backtrack several years to at least 1932 and the first of several unpublished papers of which McKern was the major author. Before arriving at the Milwaukee Public Museum as assistant curator of anthropology in 1925, McKern (Figure 11), who received his undergraduate degree from the University of California, had served research stints at several institutions, including the Bishop Museum in Honolulu and the Bureau of American Ethnology in Washington, D.C. In scanning McKern's early publications based on his research in Wisconsin, one gets the feeling that he was frustrated by the lack of any systematic means of comparing archaeological materials from the state with those from other regions. This impression is corroborated by Alton K. Fisher, who worked with McKern in the late 1920s and early 1930s:

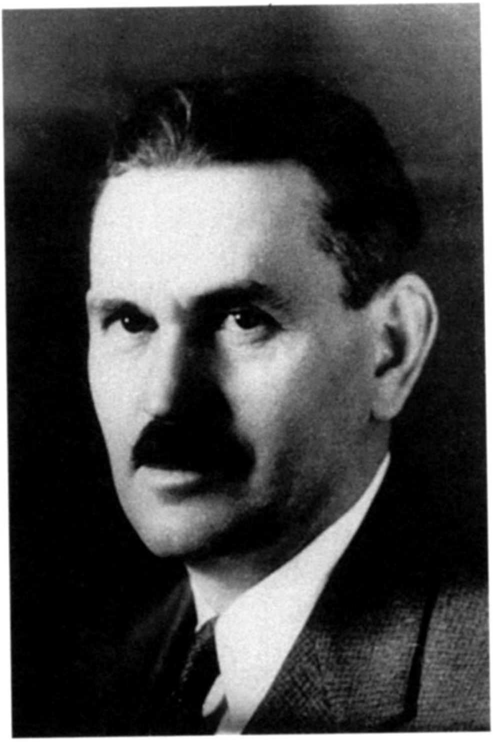

Figure 11. W. C. McKern, longtime archaeologist with the Milwaukee Public Museum and the guiding hand behind the development of the Midwestern Taxonomic Method, ca. 1940.

> By the end of the 1929 field season. . . . some cultural distinctions were becoming apparent [across the region]. . . . However, there was no comparative system in general use in the Midwest at that time to facilitate analysis of subtle as well as overt culture traits so as to suggest possible relationships among them. . . . While McKern had every reason to be pleased with the results of his field work between 1925 and the end of 1929, he was not entirely satisfied with his accomplishments. He had not found the means of defining the cultural relationships he felt must exist but which he had not yet been able to demonstrate. (Fisher 1997:118)

Fisher's remembrances of the time he spent with McKern are important because they give us critical insights into not only the problems of the day but also some of the thought processes that went into the formulation of what eventually became known as the Midwestern Taxonomic Method—

a method that was so synonymous with McKern's name that it often was referred to simply as the "McKern classification" (e.g., Griffin 1943). It was discussion of this classificatory method that held the attention of archaeologists at the Indianapolis meeting. As Fisher (1997:119) recalled,

> After the close of the 1929 field season our noon-time discussions began to concentrate on how a cultural classification system could be designed to serve the archaeological needs of the Wisconsin area. It was recognized at the outset that temporal considerations would have to be ignored because no means was available for the relative dating of what had been found. Certain assumptions could have been made about how the prehistoric culture traits had evolved and then one could have arranged the collected data to fit these assumptions. A hypothetical culture sequence could have been created by that approach but that was rejected by both of us as interestingly speculative but not worth the time that would have been required to develop it. What was wanted was a cultural classification system the criteria for which could be agreed to as valid by all who chose to become familiar with it and to use it. When it became unavoidably clear to both of us that temporal and developmental or evolutionary considerations could not be incorporated in the system, it was finally admitted that the system that was needed so urgently would have to be based on morphological or typological considerations alone. A feeling that was more hopeful than optimistic began to grow that when sufficient facts had accumulated, patterns of arrangement could emerge that would not only suggest cultural relationships but perhaps evolutionary sequences as well. Recognition of the restriction imposed upon the search for the needed classification system actually stimulated the search process.

Fisher's recollection underscores the position midwestern archaeology was in during the 1920s and 1930s—a position similar to that occupied by southeastern archaeology during the same period. Although there were hints as to chronological ordering—for example, it was clear what the chronological position of Hopewell and Adena were relative to one another, as it was clear where Fort Ancient fell chronologically—there were few instances of clear stratigraphic orderings, and those that had been found were often idiosyncratic. Missing were repeated orderings at multiple sites—the kind of evidence that ensured that the suspected orderings were not simply fortuitous occurrences. As proposed so forcefully in Birmingham in 1932, one way out of this chronological dilemma was through the use of the direct historical approach, which anchored the chronological ordering in the recent past and allowed the archaeologist to use overlapping traits to extend the sequence backward in time. Seemingly forgotten was the key chronological work of Kroeber (1916a, 1916b) in the Southwest, which demonstrated that sequences could be constructed, through seriation, without turning a single spadeful of dirt.

Swanton's presentation in Indianapolis was on Siouan tribes in the Ohio Valley, but unlike in Birmingham, the whole notion of using ethnohistory and linguistics to sort out the archaeological record received much less attention in Indianapolis. The Midwest had never witnessed the amount of ethnohistorical and ethnological work that the Southeast had, although this had not stopped archaeologists from concocting all manner of schemes to tie their archaeological manifestations to ethnic groups. If anyone doubts either the complexity of the problem or the speculative nature of efforts to tie the midwestern archaeological record to ethnic groups, Griffin's (1943) Appendix A in *The Fort Ancient Aspect* makes convincing reading. After detailing the myriad proposals that had been put forth for the placement and movement of ethnohistorically known groups in the Ohio Valley, Griffin (1943:313) stated that the "confusion of theories mentioned above results from the fact that no one is in position to interpret intelligently the prehistory of the area in terms of tribal migrations." Apparently, at least from Griffin's point of view in 1943, things had not changed significantly in the decade and a half since McKern discarded the "interestingly speculative" notion of constructing any "hypothetical culture sequence" (Fisher 1997:119) for the upper Mississippi Valley and turned instead to a method of classifying archaeological phenomena that relied solely on formal similarities and differences.

Early on it appeared to McKern that to develop a useful classificatory system, time would have to be jettisoned. And if time went out the window, why should space be retained? If it, too, were discarded, then one could concentrate on a comparative examination of empirical units—that is, on artifacts and the attributes they exhibited. Thus form-related units, which anyone could see and measure, would be the building blocks of the classification. Importantly, there could finally be agreement over units; no longer would archaeologists argue about whether shell-tempered pottery was Siouan in origin or grit-tempered pottery Algonquian in origin. *Perhaps,* as Fisher (1997:119) intimated, at some future point "patterns of arrangement" would emerge that would suggest not only "cultural relationships but perhaps evolutionary sequences as well," but for the present archaeologists would have a method of systematizing the artifacts and features encountered in the record—a method that at the very least would facilitate communication and comparison.

In principle the method McKern devised was simple—a branching taxonomy with successively higher levels of inclusiveness—but it was misunderstood from the start. We think part of the misunderstanding stemmed from the fact that in its unadulterated form the method had nothing to do with time and space—two of the three central foci in almost any archaeological endeavor. Prehistorians from the early nineteenth century on were interested in questions of when, where, and what, and to ignore two of the three was viewed in some quarters as foolish if not downright heretical. Thus there was a backlash against the method that continued into the 1940s (e.g., McGregor 1941; see McKern [1944] for a rebuttal), with the most strident criticism coming from Julian Steward, who defended the contributions made to archaeology by the direct historical approach: "[I]t is difficult to see what is gained by scrapping a scheme with historical terms and categories in favor on a non-historical one" (Steward 1942:339; see McKern [1942] for a rebuttal). Although the Midwestern Taxonomic Method was designed to keep time out of the equation, in practice it rarely did. The temporal dimension was too ingrained in Americanist archaeology for it to have been otherwise, despite the best intentions of the method's chief architect.

Perhaps another reason for confusion stemmed from the fact that the method was used almost exclusively in the Midwest and Plains. It was, after all, labeled the *Midwestern* Taxonomic Method (McKern 1939), which made it sound as if it was applicable only in one region of the country. Of course, it wasn't limited to a single area (e.g., McGregor 1941), but the parochialness implied by the name was still an obstacle to overcome. The Southwest had its own classificatory systems and sets of nomenclature, such as the system proposed at the first Pecos Conference in 1927 (Kidder 1927) and the one that emanated from the Globe Conference of 1931 (Gladwin and Gladwin 1934), and in several respects those systems resembled McKern's. There was, however, one major difference, as McKern (1944) well knew: the southwestern schemes admitted time and space, whereas his did not. Southwesternists were not going to give up their classification systems, which were built around all three dimensions of interest—time, space, and form—in favor of one that was built around only form.

As with most methods, the Midwestern Taxonomic Method went through several iterations—and we touch on a few aspects of the different drafts below—but the basic outline of the method remained unchanged from about 1932. The building blocks of the method were called *components,* defined as assemblages of associated artifacts that represented the occupation of a place by a people. Thus a component was not viewed as being equivalent to a site unless a place had experienced *only* a single occupation (McKern 1939:308, 1944:445)—a key point missed by some archaeologists (e.g., Setzler 1940). Artifact trait lists were used to create higher-level groups. An archaeologist polled available components and identified those traits that linked—were shared by—various components, which were then placed together in a group. Simultaneously, one used those same trait lists to identify traits that could be used to isolate one group of components from another group. Five levels of groups were eventually recognized. From least to most inclusive, these were *focus, aspect, phase, pattern,* and *base.* Three kinds of traits were distinguished: *linked traits,* which were common to more than one unit; *diagnostic traits,* which were limited to a single unit; and *determinants,* which were traits that occurred in all members of a unit but in no other unit. If this sounds rather confusing, note that even those who worked alongside McKern in refining the method were confused on occasion, not only over the different kinds of traits but over how they were to be identified. Figure 12 is our effort to slice through the confusion and show the difference among linked traits, diagnostic traits, and determinants.

According to Fisher (1997:119), it was he, not McKern, who first proposed the method of classification that would become synonymous with his supervisor's name:

> About that time I recalled my relatively recent studies in biology during which I had become quite familiar with the taxonomic system of Linnaeus. It was based primarily on relationship of form, originally applied to the classification of plants but later extended to animate creatures with equal success. If that classification system could show morphological relationships between animal forms as diverse as mastodons and earthworms, might it not be possible to show some relationship between the creations of man as demonstrated by form or structure alone?

This insight was significant in that it eschewed any question of equating archaeological remains with ethnic groups and instead *ultimately* sought evolutionary, or phylogenetic, relations among sets of artifacts (Lyman et al. 1997; O'Brien and Lyman 2000). But for the initial sorting, time was ignored in favor of morphological similarity. Fisher was correct:

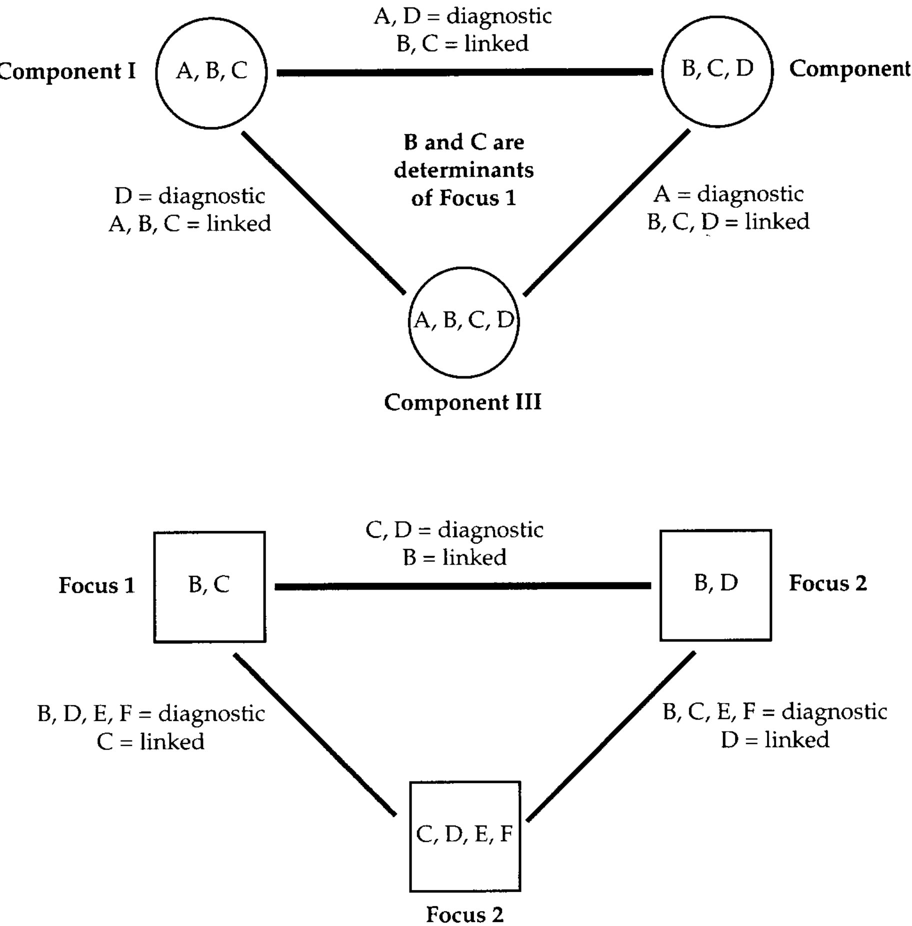

Figure 12. The analytical relations among traits, components, and foci in the Midwestern Taxonomic Method (after Lyman et al. 1997).

in basic principle what he proposed to McKern *was* similar to the way in which Carolus Linnaeus approached the taxonomic classification of organisms in the late eighteenth century. Both methods produce nested categories, and one could make a rough correspondence between components and populations, foci and species, aspects and genera, and so on. Strictly speaking, however, the Linnaean taxonomic system is not an evolutionary scheme; certainly Linnaeus had no evolutionistic pretensions when he first developed the method of classification. That the classification was later shown to have phylogenetic implications had nothing to do with how and why it was created. Similarly, McKern's taxonomic method was not devised to show evolutionary relationships, although it was admitted from the start that various formal relationships that it revealed *might* be phylogenetic.

Alice Kehoe (1990:34) noted that there "is an interesting parallel to McKern's method in the currently controversial method of cladistics in biology." Such a statement is based on an ill-informed view of what cladistics is and is not. Although both are based on the identification of varying degrees of morphological similarity in character states, McKern's method was an application of numerical phenetics, or numerical taxonomy (Lyman et al. 1997; O'Brien and Lyman 2000), in most cases without recourse to the actual quantitative measurement of similarity. Indeed, some of McKern's contemporaries (e.g., Kroeber 1940b, 1942) were quick to point out that the failure to quantitatively measure similarity was a major flaw of the method. In unrelated fashion, cladistics is based solely on the ability to differentiate between not only analogous and homologous traits but, with respect to the latter, shared derived traits and shared ancestral traits. The Midwestern Taxonomic Method made no attempt to separate analogous traits from homologous traits. This was no deficiency of the method; McKern never intended it to do so.

Fisher (1997:120) indicated that at first McKern was skeptical of the method, but after

> considerable discussion and thought on the matter . . . he began to test the idea with data he had collected, and he was pleased to find that it often was successful. . . . When it became evident that there might be a reasonable prospect of success at designing different levels or degrees of relationship between lithic and bone artifacts, pottery, earthworks, and burials and between complexes of such cultural manifestations, the need to become specific in defining the various proposed categories of relationship claimed [McKern's] attention.

McKern's pilot run at introducing the method formally was to be at the annual meeting of the Central Section of the American Anthropological Association, which was held in Ann Arbor, Michigan, in April 1932, but illness precluded his attendance (Griffin 1943:327). Instead, the first presentation was made at a meeting of the Illinois State Academy of Science held at the University of Chicago the following month. McKern revised his paper in light of suggestions he received, and Guthe circulated it to interested parties. The paper was titled "A Suggested Classification of Cultures." McKern revised the paper again, incorporating more suggestions, and on December 10, 1932, he and a small group of archaeologists—Samuel A. Barrett, a former Kroeber student at the University of California and director of the Milwaukee Public Museum (and the man whom McKern had replaced as curator of anthropology); A. R. Kelly of the National Park Service, who after receiving his doctorate at Harvard in 1929 had worked at the University of Illinois until 1933; and Cole, Deuel, Griffin, and Guthe—met at the University of Chicago to discuss the paper. McKern revised it yet again, and on April 4, 1933, the paper was sent out under the authorship of McKern, Deuel, and Guthe (McKern et al. 1933).

McKern revised the paper once more, changing the title to "Certain Culture Classification Problems in Middle Western Archaeology." He presented it the following year as his presidential address to the Central Section, and the CSAS issued the paper through its *Circular* series (McKern 1934). It was the content of that paper that formed the major points of discussion at the Indianapolis meeting in December 1935 (McKern 1937b). The paper assumed such a key role at the meeting that it was appended, without modification, to the published report on the conference. Guthe (1937:vi) had this to say about McKern's paper in his preface to the proceedings: "This paper constitutes the first concise statement of the principles upon which this classification is based, and the detailed methods by which it may be applied. It is included here because the discussions at the conference assumed that the delegates had a knowledge of its contents."

The delegates indeed had a knowledge of its content—several of them had made significant contributions to the paper—just as they had a knowledge of both McKern's deep commitment to the method and how he had defended it:

> I have received such questions as this: Why call the cultural manifestation of the pre-literate Iroquois, Upper Mississippi, or any name other than Iroquois? In some instances we may have sufficient data to verify identification with a known historic group, such as the Iroquois. However, in most instances, we cannot immediately bridge the gap . . . and in many instances we cannot hope ever to be able to do so. . . . The only taxonomic basis for dealing with all cultural manifestations . . . is that of culture type as illustrated by trait-indicative materials and features encountered at former habitation sites. If in the future it becomes possible to name the historic ethnic group of which the pre-literate group is the progenitor, no

> confusion should result from the statement that, for example, Upper Mississippi Oneota is Ioway Sioux; no more so than from the statement that *Elephas primigenius* is the mammoth. (McKern 1937b:70)

McKern chose a poor analog for his last point. Mammoth and *E. primigenius* are simply different names for the same creature; one does not have to show any kind of a connection between the two names to use them interchangeably. This is decidedly not the case with Upper Mississippi Oneota, an archaeological manifestation, and Ioway Sioux, an ethnic and linguistic unit. Here it must be demonstrated that two very different kinds of units have an equivalence. McKern's mammoth example would have been better had he said something like, "No confusion should result from saying that a particular set of fossils represents *Elephas primigenius*," because this would have underscored the necessity of definitive criteria for distinguishing between the fossils of mammoth and those of some other large quadruped. His comments are strong evidence that he viewed his archaeological units as equivalents of ethnic units; he just didn't know which archaeological unit went with which ethnic unit, and until he did, he didn't want to guess.

McKern was determined to leave critics of his proposed method defenseless. He attacked two of the prized possessions in the archaeological tool kit of the early twentieth century: the direct historical approach and the culture-area concept. In attacking the former, he stated,

> Aside from the inadequacy of the direct-historical method in supplying the archaeologist with a means of attachment to the ethnological classification, the latter, even if applicable, would not ideally answer the needs of the archaeologist. One ethnological classification divides the aborigines into linguistic stocks which are first subdivided into more specific linguistic groups and, finally, into socio-political groups. The criteria for classification are social, primarily linguistic. The major portion of the data available to the archaeologist relates to material culture, and in no instance includes linguistic data. Consequently this ethnological classification does not satisfy archaeological requirements. (McKern 1937b:71)

McKern then went after the culture-area concept:

> It may be said that we have the ethnologically conceived culture areas to supply a basis for archaeological classification. However, these so-called culture areas involve two factors which the archaeologist must disregard in devising his culture classification if he is to avoid hopeless confusion;

these are the spatial and temporal factors. First, the culture area attempts to define, or at least limit, geographical distribution. Unfortunately, the American aborigines did not always succeed in confining their cultural divisions within a continuous area, or in keeping culturally pure an area of any important size. Second, the archaeologist considers the American Indians from the standpoint of all time, and certainly, there can be no cultural areas devised which can include an unlimited temporal factor. (McKern 1937b:71)

Applying the Midwestern Taxonomic Method

Armed with these caveats, participants at the Indianapolis conference got down to the business of using McKern's method to sort out the archaeological record of the Midwest. From our perspective the reports from the Saturday sessions are the most interesting because they chronicle the difficulties that archaeologists encountered in actually trying to use the method. Up to that point few attempts had been made to do so. The ones with which we are familiar are McKern's discussion of data from Wisconsin that appeared in the 1934 draft of his paper on the Midwestern Taxonomic Method and four treatments that appeared the following year: Griffin's (1935) preliminary analysis of the Fort Ancient Aspect; Strong's (1935) and Wedel's (1935) treatments of Plains data; and Deuel's (1935) handling of data from the upper Mississippi Valley, which was roundly criticized by several of his colleagues (e.g., Griffin 1943; Guthe 1936; McKern 1938). Deuel's paper and the subsequent treatment of Illinois data by Cole and Deuel (1937) highlight the conceptual difficulties that archaeologists had in actually applying the Midwestern Taxonomic Method to a set of data. In the work of Strong and Wedel we do not see a pure application of the method but rather something of a hybrid of the Midwestern Taxonomic Method and the direct historical approach.

At the time of the Indianapolis conference there were four levels in the classificatory system of the method—basic culture, phase, aspect, and focus—the same four that were in the 1933 draft (McKern et al. 1933). At one of the Saturday sessions Guthe suggested dropping the term "basic culture" because of the confusion surrounding the term, and in its place he suggested "base," with a new level, "pattern," to be inserted just below it. Pattern was a term that had been discussed for some time, and its eventual insertion created the five-tier system that appeared in the published version of the paper (McKern 1939). It was a common misconception among those who were not part of the group that devised the Midwestern

Taxonomic Method that "component" was the sixth, and lowest, tier in the system. McKern and others consistently warned that this was not the case. Rather, a component is "the manifestation of a given culture at a single site" (McKern et al. 1933:4), or "the manifestation of any given focus at a specific site" (McKern 1937b:73). This unit "serves to distinguish between a site which may bear evidence of several cultural occupations, each foreign to the other, and a single, specified manifestation at a site" (McKern 1937b:73–74).

Conference participants had a difficult time deciding whether known cultural manifestations should be labeled as aspects or foci, and some were irritated that their favorite manifestation might lose its primacy. Take, for example, the following exchange:

> McKern: It seems to me that the majority of Hopewell traits are un-Woodland.
>
> Deuel: Outside of Ohio, our Central Basin largely consists of Woodland characteristics, and there are a number of sites called Hopewell that have traits like Marksville [Avoyelles Parish, Louisiana] and others which cannot be placed.
>
> Roberts: Would you say that in your Central Basin, except for Ohio, you have about an equal division of Woodland and Mississippi traits? It seems to me that your separate Pattern here is Hopewell. You may find out that it is a northern extension of your southern pattern. Why not make the Pattern Hopewell?
>
> Guthe: As a matter of convenience, what is there wrong in thinking in terms of Aspects and Phases? Include a Hopewell Phase under the Central Basin Pattern.
>
> McKern: Why can't we say an unnamed Pattern under which we get Hopewell?
>
> Setzler: Why not use Hopewellian Phase instead of Hopewell?
>
> McKern: Hopewell is also a Component in itself. Use the Scioto Valley [Ohio] as Focus. (NRC 1937:61)

There is also clear evidence that try as they might, many of the participants couldn't shake their tendencies to hold to subjective impressions of evolutionary relationships between various units. For example, Setzler stated, "I want a single Pattern called Mississippi, with all pottery-agriculture divisions listed under it." He then asked, "Can't you make your divisions under Phases instead of the Pattern?" (NRC 1937:60). Deuel realized what Setzler was getting at: "It seems to me what is bothering

Setzler is the fact that he sees a genetic relationship between the Gulf cultures and the Mississippi cultures, which should be if the two are classified on the basis of their inherent traits" (NRC 1937:60).

It was the identification of these "inherent traits" that was the final undoing of the Midwestern Taxonomic Method. That and deciding not only what a trait was but whether any particular trait was a linked trait, a diagnostic trait, or a determinant, any of which could be "inherent" to a given unit regardless of whether that unit was a focus, an aspect, or a phase. This problem apparently became so acute that in what was a well-thought-out application—Griffin's (1943) *The Fort Ancient Aspect*—the author took an entirely different tack: "The concepts 'determinant,' 'determinant trait,' 'determinant complex,' 'diagnostic,' 'diagnostic trait,' 'diagnostic complex,' and 'link traits' have not been seriously employed in this paper, partly because of the confusion and contradiction in the present use of such jargon and partly because there was no apparent need for such terms" (Griffin 1943:335). Although Griffin's monograph was published in 1943, the analysis was completed in 1939, three years after he finished his doctoral dissertation at the University of Michigan and while he was assistant curator of archaeology at the university's Museum of Anthropology. Even as early as 1939 Griffin must have seen that applications of the Midwestern Taxonomic Method were hopelessly confused and tautological—Cole and Deuel's (1937) *Rediscovering Illinois* being a case in point. Although Griffin (1943:338) did not refer to that work by name, he obviously had it in mind when he commented on how some archaeologists working in the Mississippi Valley chose determinants: "A few 'determinants' are chosen from a small number of sites, and these same sites are then used to illustrate that the selected list recurs at these same sites." The alternative Griffin selected—establishing a complex of traits and ignoring determinants—became the cornerstone of archaeology throughout the 1940s and 1950s, culminating in the formulations of Philip Phillips and Gordon R. Willey (1953; Willey and Phillips 1955, 1958) that emphasized the temporal and spatial dimensions of archaeological phenomena. The phase unit they proposed came to dominate Americanist archaeology in the 1960s and is, in many respects, simply the result of jettisoning the higher-level units of the Midwestern Taxonomic Method and of modifying the focus to include explicit temporal and spatial parameters (as suggested by Harold S. Gladwin [1936] and Harold S. Colton [1939]).

Epilogue

At the last session of the Indianapolis conference, McKern noted, "It seems to me before we depart that we have gotten a great deal out of this meeting. It seems advisable that we should have such meetings at least once a year" (NRC 1937:69). Guthe, however, announced that "We are confronted with several problems regarding further meetings of this sort. The National Research Council is trying to withdraw from projects it has supported for a long time. According to present plans, the CSAS will go out of existence in June or July 1937, which means that . . . [the] machinery will not exist so that we can get money from a central organization" (NRC 1937:69).

Times and interests change, as do federal funding priorities, and by late 1935 the NRC felt it had supported archaeology long enough. Besides, other branches of the government had become heavily involved in archaeology—the Federal Emergency Relief Administration was created in May 1933, the Civil Works Administration later that year, and the Works Progress Administration in 1935—primarily in an effort to stabilize the economy and get people back to work. Archaeology, being a labor-intensive endeavor, was the perfect vehicle for employing large numbers of people. Ironically, the CSAS disbanded shortly after these programs began and just as millions of federal dollars were starting to pour into local and state coffers to fund archaeological projects. In some quarters the work that resulted from relief efforts was highly innovative (Haag 1985; Lyon 1996; Setzler and Strong 1936), but in others it was less than spectacular (Johnson 1947, 1966). At the point where a strong central body such as the CSAS could perhaps have done the most good in terms of quality control, it was dissolved. But not for long, for early in 1939 the Works Progress Administration asked the National Research Council to create a committee to examine the state of archaeology in the United States and to determine whether federal relief archaeology was producing the kind of results it should. Out of this request grew the Committee on Basic Needs in American Archaeology. And to whom did the NRC turn for assistance in organizing the committee? None other than the tireless Carl Guthe.

When the CSAS was dissolved, no one thought that the immediate problems facing archaeology in the East had been solved. In fact, the majority of sentiment ran in the opposite direction. Midwestern archaeologists had a new method for classifying archaeological manifestations, and southeastern archaeologists had the direct historical approach to help solve their chronological problems, but some of the problems on which the committee had focused from the beginning were as bad or worse in 1935 than they had been 15 years earlier. One of these was the destruction of archaeological sites, which if anything had accelerated in the 1930s despite the best efforts of Guthe and his colleagues. This is how Setzler and Strong (1936:308–309) saw the problem in their mid-1930s assessment of federal relief efforts:

> The present actual status of archaeological conservation in the United States . . . is deplorable. . . . The Antiquities Act of 1906 forbids unauthorized archaeological excavation on public lands, but the law is difficult to enforce and, so long as archaeological specimens can be sold on the open market, can have at best a very limited effect. . . . It is a sad paradox that at this time, when trained men are becoming available and new techniques for determining archaeological history are reaching a high pitch of development, the materials themselves should be vanishing like snow before the sun.

One bright spot in the mid-1930s was the creation of yet another organization, which in many respects acted in the same capacity as the Committee for State Archaeological Surveys had since its inception. However, the new organization differed in structure in that it was a national body and was composed of nonprofessional as well as professional archaeologists. The genesis of the organization was a query posed to the committee in 1933 as to why there was no national society dedicated solely to archaeology in the Americas (Guthe 1967). The committee agreed to look into forming such an organization, and in April 1934 a prospectus was mailed to 192 persons with whom the committee corresponded (Griffin 1985). These included nonprofessionals as well as professionals because, as Griffin (1985:265) later pointed out, if only the latter had been included, their dues would have been prohibitively high in order to fund publication of the journal that the organization proposed to publish. Given the mix of the membership, what should the society be called? After toying with several names, the committee decided on the Society for American Archaeology, the organizational meeting of which took place on December 28, 1934, following the annual dinner of Section H (Anthropology) of the American Association for the Advancement of Science, held that year at the Hotel Roosevelt in Pittsburgh (Guthe 1935).

Despite worries on the part of some of the founders that the new organization would be viewed by some as a vehicle for moving archaeology away from the more traditional societies such as the American Anthropological Association (Guthe 1935), this was not the intent: "the Society was not the expression of a separatist movement, but an attempt to bring anthropologists using the archaeological method into closer contact with the public, and to establish a wider appreciation of the methods and principles of scientific study" (Guthe 1967:438). Further, it was felt that under the conditions, "the original objectives of the Committee [on State Archaeological Surveys] would have a better chance of attainment through such a national membership organization" (Guthe 1967:438). We assume that by late 1934, a year before the Indianapolis conference, Guthe could see the handwriting on the wall: the NRC was going to shift its support away from the committee, and there would have to be an organization capable of assuming its duties. To that end, he approached the Carnegie Corporation, which had helped fund the activities of the CSAS since 1929,[26] asking if the last round of funding could be shifted to the Society for American Archaeology. The corporation agreed,[27] and the newly created organization assumed the duties that had previously been the charge of the Wissler-Kidder-Guthe committee. That committee was discharged at the end of June 1937, having been in existence for 17 years.

In assessing the accomplishments of the CSAS, especially as those are reflected in the three regional conferences the committee sponsored, we are struck by the parallels between Americanist archaeology in the 1920s and early 1930s and Americanist archaeology today. The destruction of archaeological sites did not abate after the Society for American Archaeology took over the functions of the CSAS in 1937, and those in the discipline today are as concerned with the problem as their forebears were. Similarly, chronology is as important today as it was during that earlier period, and although modern archaeologists have access to a battery of methods that earlier generations of archaeologists could not have imagined, some local chronological sequences in the Midwest and Southeast are only slightly more developed than those of the mid-1930s. Today's archaeologists are also as interested in classification as McKern, Guthe, and Griffin were when they were debating the finer points of the Midwestern Taxonomic Method. The descendant of that method—the phase-centered approach to categorizing archaeological manifestations—has, since the early 1940s, been integral to archaeological systematics as used over much of North America. Discussions at the Birmingham conference showed there was considerable need for a systematic method of categorizing archaeological remains. Subsequent discussions at the Indianapolis conference demonstrated what one such method might look like, but it also demonstrated the incredible complexity of the archaeological record and the difficulties involved in fitting it into a taxonomy.

As we peruse the discussions that took place at the various conferences, we often catch a germ of an idea that would later become a central focus in Americanist archaeology. Or maybe it was a simple statement or suggestion that foreshadowed events to come—events that became milestones in terms of how they moved the discipline forward either methodologically or in terms of its knowledge base. For example, considerable debate at the Indianapolis conference revolved around the concept of Middle Mississippi—both how to recognize it and how to classify it. During the discussions, Swanton asked, "How are you going to get anywhere with Middle Mississippi until you investigate the Arkansas–west Tennessee district?" (NRC 1937:64). At least one person in the room must have thought about that question, because within a few years Griffin, along with Philip Phillips and James A. Ford, would begin a decade-long project in the Arkansas-Mississippi-Tennessee portion of the Mississippi Valley that resulted in a monograph (Phillips et al. 1951) that in our opinion is one of the most important works ever written in Americanist archaeology.

Taken in the aggregate, the three volumes that emanated from the conferences sponsored by the CSAS contain an extensive array of information on how archaeologists working in the eastern United States during the 1920s and 1930s organized their study of the past and how they arrived at some of their conclusions about the past. Some of that information is contained elsewhere, either in monographs written during that period or in the reminiscences of those who worked during those times, but it is not the same as reading the actual exchanges that took place at meetings and hearing the way in which ideas were shaped through discussion and debate. In closing, we note that our sentiments are identical to those of Griffin (1976a:171): "If historians of American Archaeology really want to know what a significant number of American archaeologists were working on [between 1929 and 1935] and their views of the then current knowledge of the participants, these reports need to be read."

Notes

1. Clark Wissler to C. E. Seashore, letter, October 14, 1921. NRC Archives, CSAS, Washington, D.C.

2. Franz Boas is often portrayed as the leading figure in American anthropology during the period 1900–1920, but in our opinion this is based in large part on his flamboyant personality and the quality of students he produced at Columbia. Clark Wissler, who claimed fewer students and whose manner was much more reserved, produced work that would endure far longer than Boas's. For a readable account of Wissler's professional life, see Freed and Freed (1983).

3. Clark Wissler to W. C. Mills of the Ohio State Archaeological and Historical Society, letter, November 4, 1921. NRC, CSAS, Washington, D.C.

4. Reprinted as number 97 in the *Reprint and Circular Series of the National Research Council*, Washington, D.C. (1930).

5. The *Proceedings* were published as part of the *Transactions of the Academy of Science of St. Louis*. Missouri Historical Society minutes for June 17, 1880, p. 2. Missouri Historical Society Archives, St. Louis.

6. Archaeological Institute of America Archives, vol. 1:23, Missouri Historical Society Archives, St. Louis.

7. Archaeological Institute of America Archives, vol. 1:23, Missouri Historical Society Archives, St. Louis.

8. Wissler to C. E. Seashore, letter, March 20, 1922. NRC Archives, CSAS, Washington, D.C.

9. Wissler to Charles H. Danforth, letter, October 25, 1921. NRC Archives, CSAS, Washington, D.C.

10. Wissler to the CSAS, memorandum, November 7, 1921, reporting on a joint letter from R. J. Terry and C. H. Danforth of the Anthropological Society of St. Louis. NRC Archives, CSAS, Washington, D.C.

11. "Report of the Chairman on a Trip through the Mississippi Valley, September, 1928," unsigned but written by Carl E. Guthe. NRC Archives, CSAS, Washington, D.C.

12. The complete list of summaries appearing in *American Anthropologist* is as follows (titles and volume numbers can be found in the reference list): 1921 (Wissler 1922), 1922 (Wissler 1923a), 1924 (Kidder 1925), 1925 (Kidder 1926), 1926 (Kidder 1927), 1927 (Guthe 1928), 1928 (Guthe 1929), 1929 (Guthe 1930a), 1930 (Guthe 1931), 1931 (Guthe 1932a), 1932 (Guthe 1933), 1933 (Guthe 1934). There apparently was no summary for 1923.

13. Wissler to Seashore, letter, March 20, 1922. NRC Archives, CSAS, Washington, D.C.

14. Wissler to Albert E. Jenks, director of the Division of Anthropology and Psychology, letter, January 2, 1924; Jenks to Wissler, letter, January 4, 1924. NRC Archives, CSAS, Washington, D.C.

15. Kidder served simultaneously as chairman of the CSAS and chairman of

the Division of Anthropology and Psychology for the period July 1, 1926–June 30, 1927. In 1927 he said of his tenure, "I believe that all chairmen go through four periods: (1) bewilderment, (2) a great burst of energy, (3) discouragement, and (4) a return to normalcy. The greatest problem of the chairman is that he is given a large handsome machine and no gas to run it" (Stevens 1952:123).

16. A. V. Kidder to Vernon Kellogg, memorandum, June 14, 1927; Kellogg to Kidder, memorandum, June 29, 1927. NRC Archives, CSAS, Washington, D.C.

17. Undated manuscript (probably late 1927) by Guthe titled "The Ceramic Repository for the Eastern United States, at the University of Michigan, under the Auspices of the National Research Council." NRC Archives, CSAS, Washington, D.C.

18. "Report of the Chairman on a Trip through the Mississippi Valley, September, 1928," unsigned but written by Guthe. NRC Archives, CSAS, Washington, D.C.

19. Knight Dunlap to Edmund Day, director of the Laura Spelman Rockefeller Memorial, letter, April 11, 1928. NRC Archives, CSAS, Washington, D.C.

20. In the official list of attendees, Futrall is listed as the president of the University of Arkansas at Batesville. This is incorrect; there was no branch of the university at Batesville. Futrall was president of the University of Arkansas at Fayetteville from 1913 until his death in 1939. In addition to being a classicist and an avocational archaeologist, he founded the university's football program, serving as coach for its first three seasons. He is also credited with helping form the Southwest Conference for intercollegiate athletics.

21. A number of anthropologists mentioned in this essay—more than just archaeologists working in the East—were influenced early in their careers by Putnam. For example, Berthold Laufer, Gerard Fowke, Roland Dixon, A. L. Kroeber, and John Swanton were at various times all members of the Jesup North Pacific Expedition sponsored by the American Museum of Natural History. Franz Boas, who at the time was assistant curator at the American Museum, more than anyone set the scientific direction for the expedition, but Putnam certainly had a hand in the project's formulation. Further, it was Putnam who brought Boas to the museum in the first place.

22. There actually was a meeting that took place between the Conference on Midwestern Archaeology and the Conference on Southern Pre-History, but technically it was not sponsored by the CSAS. We say "technically," because although the committee did not publicize or fund it, many of the same archaeologists who participated in the sponsored conferences attended the meeting held in Vermillion, South Dakota, on August 31 and September 1, 1931. A two-page summary

was published in 1931 as number 9 in the committee's *Circular* series. The meeting is of historical interest because even in the short summary statement one sees how archaeologists working in the upper Plains and Midwest were beginning to wrestle with the problem of cultural classification—the single issue that led to the third NRC-sponsored archaeological conference, which was convened in Indianapolis in 1935.

23. The report must have been printed in 1933, but it carries no date other than that of the meeting. We cite the papers in the report as 1932.

24. Brannon later served as the director of the Alabama State Department of Archives and History in Montgomery. He was a prolific author, publishing numerous articles in the *Alabama Historical Quarterly* between 1930 and 1962. His most widely cited publication is *The Organization of the Confederate Post Office Department at Montgomery* (1960; published privately).

25. Albert L. Barrows to John R. Swanton, letter, July 13, 1932. NRC Archives, CSAS, Washington, D.C.

26. Report made to the Carnegie Corporation by the CSAS covering the period 1929–1934. NRC Archives, CSAS, Washington, D.C.

27. Report made to the Carnegie Corporation by the CSAS covering the period July 1, 1935–June 30, 1936. NRC Archives, CSAS, Washington, D.C.

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