

Archaeology's quest for a seat at the high table of anthropology

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Abstract

Between 1900 and 1970, American archaeologists perceived themselves as second-class anthropologists because the archaeological record suggested little not already known ethnographically, archaeology served anthropology by testing ethnologically derived models of cultural evolution, the archaeological record was ethnologically incomplete as a result of poor preservation, and archaeologists used but did not write anthropological theory. Ethnologists of the period agreed with these points and regularly reminded archaeologists of their limited role in anthropology. A few archaeologists claimed in the 1950s that archaeology could contribute to anthropological theory but they were ignored. The claim was reiterated by new archaeologists of the 1960s, and by the 1970s worries about the poor preservation of the archaeological record had softened. However, most archaeologists after 1970 (and before 1990) used anthropological theory and did not write new theory on the basis of archaeological data. The root cause of American archaeology's ninety-year absence from anthropology's high table of theory seems to be the discipline-wide retention of the ninety-year old belief that archaeology is prehistoric ethnology and the (unnecessary and constraining) corollary that archaeologists must use anthropological theory to explain the archaeological record.

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It is hardly possible to understand the significance of American archaeological remains without having recourse to ethnological observations, which frequently explain the significance of prehistoric finds. (Boas, 1902, p. 1)

It seems to me that American Archaeology stands in a particularly close, and so far as theory is concerned, dependent relationship to general anthropology. (Phillips, 1955, p. 246)

In 1984, evolutionary geneticist Maynard Smith (1984, p. 401) reported that previously the “attitude of population geneticists to any paleontologist rash enough to offer a contribution to evolutionary theory has been to tell him to go away and find another fossil, and not to bother the grownups.” He added that the role of a paleontologist had until recently been, in the opinion of many biologists (referred to as *neontologists* by paleontologists), “to show that the facts of paleontology were consistent with the mechanisms of natural selection and geographic speciation proposed by the neontologists...rather than to propose novel [evolutionary] mechanisms of his own” (Maynard Smith, 1984, p. 401). A critical

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thing to note is verb tense. Maynard Smith (1984, p. 402) was welcoming paleontologists to the “high table” of biological evolution (where the elite doyens—the neontologists—of the discipline sit) from which, in his view, the paleontologists “have too long been missing.”

Historian Sepkoski (2005) presented a history of how paleontologists came to claim their place at the high table. He reiterated that prior to the 1970s paleontology was perceived as little more than the unglamorous user and confirmer of evolutionary theory by both neontologists and (most) paleontologists. One point he did not raise was paleontology’s difficulty in gaining access to the high table as a result of its academic home being in geology rather than in biology (Eldredge and Gould, 1977; Nowlan, 1986; Youngquist, 1967). This demanded an intense struggle by paleontologists who sought to demonstrate their worth to a discipline in which they felt they should have been housed originally. American archaeology, too, for the first 80 or so years of its existence as a professional form of inquiry, was not allowed at the high table of anthropology despite its academic home being within that discipline (consider Boas’s and Phillips’s remarks in the epigraph). That situation began to change in the 1960s as some American archaeologists developed a program they thought would earn them a seat at the high table.

In this paper, I review the history of American archaeology, relative to cultural anthropology (hereafter, *ethnology*), and track the history of each subfield’s relationship to the field of anthropology as perceived by practitioners of each. Along the way I examine whether or not cultural anthropologists (hereafter, *ethnologists*) believed archaeologists should be excluded from anthropology’s high table. I begin with a brief definition of a disciplinary high table. Then, I explore whether pre-1970 archaeologists themselves thought they deserved a seat at anthropology’s high table before reviewing what ethnologists thought about high seats prior to 1970. I conclude with a review of what ethnologists and archaeologists thought about the latter’s potential for a seat at the high table after 1970. I show that between about 1970 and 1990 many archaeologists echoed their predecessors; they used anthropological theory rather than build unique anthropological theory on the basis of archaeological data, despite their explicit recognition that they have unique data upon which unprecedented theory might be built.

The high table and theory

Some time ago Hodder (1981, p. 10) stated that “a mature archaeology means an archaeology involved in, and contributing to, wider debate in the social sciences.” He suggested that to make such contributions archaeology has to keep pace with and be fully integrated into the social sciences in general. Hodder’s is one way to define a high table and to specify how to gain access to it. This definition is potentially unhelpful in the present context, however, because contributions may merely include archaeological confirmation of models and theories that originate in nonarchaeological contexts.

Prior to the 1980s, paleontology’s access to the high table was, in paleontologist Eldredge’s (1995) view, restricted. Paleontologist Simpson (1944, p. xv) noted in the context of the neoDarwinian evolutionary synthesis that geneticists had previously observed that “paleontology had no further contributions to make to biology, that its only point had been the completed demonstration of the truth of evolution, and that it was a subject too purely descriptive to merit the name ‘science’.” Simpson (1944) thought otherwise, and introduced what he called “quantum evolution,” a unique tempo and mode of evolution that was clearly visible to paleontologists from the coarse-grained temporal resolution they had but which neontologists and geneticists thought was nonsense given the fine-scale temporal resolution they enjoyed as they tracked genetic change in living fruit flies. Simpson backed down (he was later replaced by Niles Eldredge, Stephen Jay Gould, Stephen Stanley, Elizabeth Vrba, and others [Eldredge, 1999; Sepkoski, 2005]), for it was clear that paleontology was not welcome at the high table in the 1940s, 1950s, and 1960s (Cain, 1992; Laporte, 1991). Neontologists believed that they knew all that there was to know about evolution, and most paleontologists of the era accepted their appointed role as mere confirmers of evolutionary theory. Paleontologists were simply users of evolutionary theory rather than writers of or contributors to that theory.

The preceding sketch, along with Maynard Smith’s (1984) remarks, suggest that access to a discipline’s high table requires that a subdiscipline must contribute *theory* to its parental discipline’s mix. Maynard Smith was welcoming paleontologists to the high table because the second generation of Simpsonites (Eldredge, Gould, etc.) had proposed a unique theory of evolutionary tempo and mode, one

that only paleontology's uniquely coarse-grained temporal resolution revealed. That theory is punctuated equilibrium (see Eldredge (1985) and Stanley (1981) for histories of the theory), a theory that has not only gained significant support from the fossil record but a significant number of individual supporting paleontologists. It has also served as the catalyst for a tremendous amount of research on the evolutionary process in general and underscored the historically contingent nature of that process.

The concept of theory is contentious among archaeologists (Johnson, 1999; Schiffer, 1988). As used here, the term theory signifies a set of integrated general principles about relationships between variables or units with observable manifestations. Principles may be explanatory; relationships may be of the cause–effect sort. For purposes of this paper, theory consists of two levels. Neither is the archaeologically familiar middle-range theory. What I have in mind is a structure consisting of “subtheories” as less comprehensive, particularistic parts that in combination comprise more comprehensive, general theories (Wilson, 1998, p. 287). Mayr (1991, pp. 36–37) provides an example when he speaks of the five (sub)theories comprising Darwin's grand theory of evolution—evolution as such (descent with modification), common descent, multiplication of species, gradualism, and natural selection.

As Schiffer (1988, p. 465) implies graphically, theories can be very comprehensive and have little empirical content, or be not so comprehensive and have much empirical content. Rather than such a continuum (although I suspect Schiffer's model is realistic in this respect), here I envision two levels of theory, one near each extreme of Schiffer's graphic characterization. Of particular relevance to the discussion that follows, conceiving of theory at two levels allows distinction of two levels of abstraction; the lower pertains to particular scholarly disciplines or subdisciplines and the higher or more comprehensive to combinations of lower-level theories that constitute a more general discipline. Thus, we might distinguish several social (sub)theories subsumed and melded together within a general social-behavioral science. Ethnological theory concerns human (non-linguistic) behavior; linguistic theory concerns the behavior of human language; together, these two bodies of (sub)theories make up a large portion of anthropological theory (Fig. 1), or what Schiffer (1988) has referred to as “social theory.” Similarly, the neontological (sub)theories of evolution and the paleontological (sub)theories of evolution together

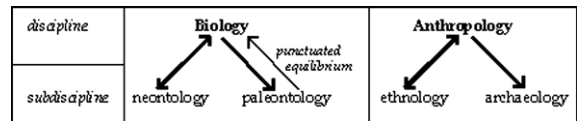


Fig. 1. Relationship between the disciplines of biology and of anthropology, and their respective subdisciplines. Bold arrows indicate major direction of theory flow.

makeup a relatively abstract and comprehensive biological theory of evolution.

The distinction of different levels of theory, including a high table of comprehensive theory, is not restricted to evolutionary biology. Although the term “high table” and the distinction of (sub)discipline specific (sub)theories are not always explicit, discussions of the epistemology of geology, especially discussions of the history of geological thinking, indicate that there is a high table of geological theory (where, for example, the notion of uniformitarianism resides, alongside other comprehensive concepts) comprised of various less abstract, more specific (sub)theories (Albritton, 1963; Rudwick, 1990). A similar distinction of levels of theory is found in ethnology (Salzman, 2001) and archaeology (Schiffer, 1988; Trigger, 1989).

In this paper, I use the term ethnology to signify cultural anthropology as a subfield of the comprehensive discipline of anthropology. The issue I explore is whether archaeology, as a distinct subfield of anthropology, contributes *anthropological* theory at the same level that ethnology is thought to. To do this, I consider what archaeologists and ethnologists think and say about such contributions rather than what those contributions might actually comprise. My intention is not to argue that archaeologists should not use extant anthropological theory. I tell my students that “anthropological archaeology” is in many ways redundant; what other kind of archaeology is there, whether the particular research questions asked are historical, functional, structural, ecological, or something else? My goal here is to determine whether or not archaeologists and anthropologists think that archaeology has contributed unique “social theory” (Schiffer, 2000) that was constructed largely on the basis of its own unique data.

We now know what to look for when we search for evidence, or the absence thereof, of a seat for archaeology at the high table of anthropology. The facts that Simpson's quantum evolution was sharply criticized by neontologists, and his intellectual descendants seem to have succeeded in gaining a

seat, prompts me to start the discussion of anthropology's high table within archaeology itself. Did archaeologists think they deserved a seat at the high table, like Simpson thought paleontology did, or did archaeologists believe they had reservations only for the kids' table?

Beginning in archaeology

Many (but not all) individuals who were doing anthropology at the end of the nineteenth century thought there was no great time depth to the American archaeological record prior to the discovery of the Folsom, New Mexico, site in 1927 (Meltzer, 1985). Thus, the general notion was that the cultures represented by the archaeological record would be virtually the same as those documented by ethnologists, so there was little reason to pursue archaeological research, except to fill in gaps in the ethnographic record. As Boas (1902, p. 1) noted, "it seems probable that the remains found in most of the archaeological sites of America were left by a people similar in culture to the present Indians." And a few years later Kroeber (1909, p. 3) remarked that the culture "revealed by [archaeology] is in its essentials the same as that found in the same region by the more recent explorer and settler." Archaeological research would contribute little to what could be learned about American Indian cultures more thoroughly and more efficiently by ethnographic research. To use a baseball metaphor—strike one (against archaeology).

The second important point about the origins of modern archaeology in the Americas is that it was conceived to be part of a four-subfield discipline. This conception was shared by individuals who founded and professionalized anthropology, including Frederic Ward Putnam (Browman, 2002), Franz Boas (Darnell, 2001), Alfred Kroeber (Jacknis, 2002) and Clark Wissler (Freed and Freed, 1983). Although there is some evidence that the "four-subfield" discipline is a myth (Borofsky, 2002), introductory ("Anthropology 1") textbooks continue to present the discipline as comprising four kinds of inquiry, each demanding its own methods. There are fewer than a half-dozen departments of archaeology in North America. Archaeologists are known professionally as archaeologists and they belong to the Society for American Archaeology. Yet the vast majority of advanced degrees awarded to individuals who do archaeological research as a significant portion of their graduate training are degrees in

anthropology rather than in archaeology. And the discipline has often demanded that archaeologists learn basic cultural anthropology as well as the ethnography of their research areas because they are anthropologists first, though they are anthropologists who study static "material culture" rather than dynamic human behavior.

Once the temporal bottom of the American archaeological record dropped out with the discovery of late Pleistocene cultural materials at Folsom, New Mexico, archaeologists could have made their claim to the majority of the time period of human existence in the hemisphere. They could have told their ethnologist brethren that archaeologists uniquely command the necessary skills and methods for studying the prehistoric past. No such claim, so far as I know, was immediately made by an archaeologist, though it would be made with increasing frequency starting in the 1950s. Instead, archaeologists interpreted the artifacts and sites they studied in rather general historical terms (e.g., Kidder, 1924). In addition, the fact that archaeology was but a subfield of anthropology was repeated by anthropologists and archaeologists for decades (e.g., Smith, 1910; Smith, 1911, p. 448; Dixon, 1913, p. 558; Phillips, 1955, 246–247; Willey and Phillips, 1958, p. 2; Braidwood, 1959, p. 79; Jennings and Norbeck, 1964, p. 4). Ethnologists and archaeologists alike identified anthropology (or ethnology) as the gold standard to which archaeology should aspire (Bennett, 1943; Steward and Setzler, 1938; Taylor, 1948). Further, during the first half of the twentieth century many ethnologists and archaeologists examined the relationship between ethnology and archaeology (e.g., Bullen, 1947; Parsons, 1940; Strong, 1936). Ethnological and archaeological methods were distinct, their data were distinct, but problems and solutions were similar—they were anthropological. Archaeology was, in terms of research questions and explanations, prehistoric ethnology. Archaeology was a service discipline relative to anthropology. "Archaeology contributes to the understanding of factors that cause civilizations to come into being, to flourish, and then to collapse," and anthropology's general task is to "understand all facets of [human] life" (Martin et al., 1947, p. 4). Archaeology contributed to anthropology, but its contributions were to flesh out models and theories based on ethnological research and data. "The findings of archaeology have usually been employed for supplementary and confirmatory rather than critical purposes [in anthropology]" (Strong, 1936, p. 359).

Archaeologists also regularly bemoaned their inability to do real anthropology by identifying how archaeological data were ethnologically incomplete. Griffin (1943, p. 340) said almost all that could be said on this topic:

It is axiomatic in archaeology that the remains recovered from an aboriginal site are in no wise sufficient to reconstruct the living culture of the people who left the remains. The artifacts and tangible associations have merely retained the artificial form given them by their makers, and interpretations made by archaeologists are inferences based upon similar materials used in an analogous but not identical cultural group. An archaeologist may recover the material but not the substance of aboriginal artifacts. The exact meaning of any particular object for the living group or individual is forever lost, and the real significance or lack of importance of any object in an ethnological sense has disappeared by the time it becomes a part of an archaeologist's catalogue of finds. The objects of material culture employed by a preliterate community are but a small part of the culture of the group, and of these objects only a small proportion survives the processes of decay.

I said Griffin said almost all that could be said; what he did not say was that archaeologists typically sample a site, and so even if preservation was perfect, they (typically) do not collect everything. Such issues of sample representativeness had been recognized much earlier (Smith, 1911). Data quality, including representativeness, comprises strike two.

American archaeologists recognized early on that conceiving of the archaeological record as an ethnological record forced the archaeologist to admit that the archaeological record was of poor quality. "Archeological material, being necessarily fragmentary, readily lends itself to misleading [ethnological] reconstruction" (Smith, 1911, p. 445). They repeated the "archaeological data are not worthy" lament, or the *preservation mantra*, over and over again throughout the early and middle twentieth century (e.g., Nelson, 1938; Wormington, 1947, p. 18; Taylor, 1948, p. 111; Ehrich, 1950, p. 469; Smith, 1955, p. 7; Spaulding, 1955, p. 12; Griffin, 1956, pp. 25–26; Thompson, 1956, p. 35; King, 1958, p. 134; Willey, 1960, pp. 112–113; Rouse, 1965, p. 5; Wauchope, 1966, p. 19; Braidwood, 1967, p. 31; Chang, 1967, pp. 12–13; Adams, 1968, p. 1190; Ascher, 1968, p. 43;

Deetz, 1968, p. 285; Trigger, 1968, p. 10; Anderson, 1969, p. 138; Bayard, 1969, p. 377).

Several of those who repeated the preservation mantra noted that archaeological investigation of cultures with written records had the advantage that those written records assisted with the interpretation of artifacts. Part of the popularity of the direct historical approach resided in it providing historical evidence that was directly—some said, metaphorically, "genetically"—linked to archaeological material, thereby warranting and strengthening inferences of ethnic identity and behavioral function of that material (Lyman and O'Brien, 2001). In the Old World, too, the preservation mantra was repeated (e.g., Thompson, 1939, p. 209; Childe, 1946, p. 250; MacWhite, 1956, p. 3; Clark, 1957, p. 219; Piggott, 1959, pp. 7–12). And there, too, the value of written documents for assisting with the interpretation of archaeological materials was highlighted (Hawkes, 1954).

Some American archaeologists worried that archaeology could contribute little to anthropological theory. One explicit statement on what archaeology had to offer anthropology was by archaeologist Creighton Gabel (1964, pp. 1–2) who remarked that the "ethnologist's explanation of the processes of culture as seen in many different areas of the world provides the archaeologist with working theories that help him interpret in the most comprehensive fashion his sparse material evidence of cultural development and change." Archaeology was a user, a consumer of anthropological theory, not a producer of such theory.

Most archaeologists themselves thought that archaeology did not warrant a place at the high table of anthropology prior to 1970. Archaeology was but a handmaid to anthropology; it was not supposed to contribute grand theories, or even contribute unique parts to theories of cultural development or to discover cultural processes of large spatio-temporal scale that were invisible to ethnographers. Strike three; game over for archaeologists, they must go sit at the kiddie table, and take their trowels with them. Ethnologists alone—sniff—sit at the high table.

The perspective of ethnologists

Archaeologist Willey (1984, p. 10) reported that in the 1940s, as he worked toward completion of his doctoral dissertation, he realized that he "had always been somewhat awed by my ethnological and

social anthropological professors and colleagues. These were the people who controlled the core of theory, and, unwittingly or not, they let us feel that archaeology was something second rate.” In his view, “by and large, archaeologists did not have a high intellectual rating on the American scene” (Willey, 1984, p. 10). He identified Kluckhohn (1940) as someone who “scolded” archaeologists, but Kluckhohn was dismayed not just with efforts of archaeologists to be anthropologists, but with both archaeologists and anthropologists for failing to explicitly develop and use theories of human behavior and of culture in their research (Kluckhohn, 1939). Did ethnologists consider themselves to be the sole occupants of seats around the high table? Did they, as Willey alleged, identify the second-class status of archaeologists within the more comprehensive anthropological field? It is easy to show that anthropologists indeed did think of archaeologists as representing a scholastically lower class.

Kroeber (1930, p. 163) stated early on that the “ultimate purpose of archaeology is the same as that of history, the authenticated presentation of a series of human events.” At the time, American archaeology’s goal was the same as that of American ethnology—writing historical ethnographies (e.g., Goldenweiser, 1925; Radin, 1933). Given a common goal, could archaeology contribute to anthropological theory (Fig. 1)? An early statement by Julian Steward suggests the answer was no. Steward encouraged collaboration between archaeologists and ethnologists but implied that archaeology was something of a second-class enterprise relative to ethnology. In Steward’s view, use of the direct historical approach “will serve to remind both archaeologists and ethnologists that they have in common not only the general problem of how culture has developed but a large number of very specific problems. If archaeology feels that applying itself to cultural rather than to ‘natural history’ problems seems to relegate it to the position of the tail on an ethnological kite, it must remember that it is an extraordinarily long tail” (Steward, 1942, p. 341). Archaeology was not the kite; it was but the tail on the kite, long or not. Ethnologists who dabbled in archaeology seem to have been less willing than Steward to acknowledge any value that archaeological research might have for building anthropological theory.

Kroeber (1948, pp. 624–625) noted that our knowledge “of the social and religious life of the earliest man is naturally filled with the greatest gaps,

and the farther back one goes in time, the greater is the enveloping darkness.” Herskovits (1948, p. 116) characterized archaeology by stating “The intangibles that are so large a proportion of human civilization can never be recovered. The ideas of early man about the tools that are dug up, or how he used them, must remain a secret, like his social and political institutions, his concept of the universe, the songs he sang, the dances he danced, the speech-forms he employed.” Hoebel (1949, p. 436) said archaeology “is always limited in the results it can produce. It is doomed always to be the lesser part of anthropology... when the archeologist uncovers a prehistoric culture, it is not really the culture that he unearths but merely the surviving products of that culture, tangible remains of the intangible reality.” Other ethnologists and anthropologists were equally pointed (e.g., Kluckhohn, 1949, p. 50; Beals and Hoijer, 1959, p. 11).

Perhaps the harshest statement by an ethnologist regarding the role of archaeology in anthropology was that by Elman Service. Although he criticized ethnologists for ignoring previously collected data and leaving “historical reconstruction” to archaeologists, the majority of his venom was reserved for archaeologists. Service (1964, p. 364) wrote that the “work of the archaeologist (as an *archaeologist*, not as an archaeologist turned general anthropologist or philosopher) is to dig up remains of peoples and their cultures, to map, measure, describe, count, and so on, and in his report to make an interpretation of what life was like ‘then’.” The implication that archaeology is merely method is one to which we will return. Service (1964, p. 366) also berated archaeologists for not consulting with ethnologists about what was going on in anthropological theory, and contended that “greater sophistication among more archaeologists about the actual nature of culture is what is necessary now, and this sophistication can be achieved by a better acquaintance with some ethnological findings.” Finally, while genuflecting toward Strong’s (1936) use of archaeological data to overturn a long-held model of cultural evolution, Service (1964, p. 364) was at pains to demonstrate that there were “ways in which archaeological theory and method could profit from greater attention to ethnological fact.” This message was repeated a few years later by ethnologist Heider (1967). As an archaeologist, reading these two articles back to back made me feel like a not-so-wise teenager being scolded by his much wiser parents.

Prior to 1970, ethnologists thought that archaeologists had little to contribute to anthropological theory. Service (1964) argued that archaeologists were but technicians and only anthropologists (even if they dug rectangular holes) could explain human behavior and build anthropological theory. Yet, the ethnologists' belief that archaeology could not contribute to anthropological theory for various reasons was not solely dependent on clever semantics, such as defining archaeology as only method. As we will see, the "archaeology as method" mantra was stated by archaeologists themselves. It is beyond my scope here to determine if ethnologists originally thought archaeology could contribute little, or if they merely repeated what archaeologists told them. My suspicion is that ethnologists thought little of archaeology from the get-go; witness Boas's comment in the epigraph. Whatever the case, in the middle of the twentieth century, some archaeologists began to argue that they could indeed contribute to general anthropological theory.

Archaeologists' argument for a seat at the high table

Despite the proclamations of many archaeologists themselves that they did not warrant a seat at the high table of anthropology, and despite the finger wagging of many ethnologists, a few archaeologists thought otherwise. One was King (1958, p. 134), who noted that the "restricted nature of archaeological data, although at times awkward for archaeologists, may nevertheless be their salvation." What King had in mind was the fact that archaeologists had data from which hypotheses could be generated; those hypotheses could in turn be tested with "other social science data" (King, 1958, p. 134). This was, I suspect in the view of many archaeologists and ethnologists, a weak argument to prepare a seat for archaeology at the high table. Interestingly, ethnologist Spicer (1957), when commenting on the same series of archaeologist-authored papers as King (1958), thought that some of those papers made major contributions to anthropology. King (1958, p. 132) explicitly stated that one of the papers had "achieved [its] purpose of deriving anthropological theory from archaeological data." Access to the high table was, perhaps, possible.

Meggers (1955, p. 28) noted that by the time archaeology had evolved from antiquarian pursuits to developing anthropological models and testing hypotheses concerning cultural development (the 1940s), "culture was being redefined as essentially a

psychological phenomenon. From this point of view, archeological results were stigmatized as being hopelessly deficient and relegated to secondary importance." Meggers went on to state that the "results of recent years indicate that archeologists are no longer convinced that they are inevitably doomed to being second-class anthropologists" (p. 128). In her view, "American archaeology [had] come of age" (p. 129). Here was an explicit claim for a seat at the high table, but it was one to which few anthropologists or archaeologists listened. In my view, Meggers can be equated with George Gaylord Simpson, and like with the history of paleontology, whereas few acknowledged Meggers's claim, the next claimant for a seat at anthropology's high table would not fade quietly into the background.

In a series of papers published in the 1960s, Lewis Binford argued that archaeology indeed did deserve a seat at the high table of anthropology, and he outlined ways in which a reservation there could be earned (e.g., Binford, 1962, 1963, 1964, 1965, 1968). His most explicit statement indicated that a seat at the high table was not something archaeologists deserved, yet. This was so because the goals of anthropology were to "explicate and explain the total range of physical and cultural similarities and differences characteristic of the entire spatial-temporal span of man's existence" (Binford, 1962, p. 217), but archaeology had not yet attained the explanation goal. A seat at the high table could, however, be earned because it was archaeology's "responsibility to further the aims of [anthropology]" (Binford, 1962, p. 217). Explicate meant to make known as in describing the archaeological record; explain meant to demonstrate the "constant articulation of variables within a [cultural] system and [to measure] concomitant variability among the variables within the system. Processual change in one variable can then be shown to relate in a predictable and quantifiable way to changes in other variables, the latter changing in turn relative to changes in the structure of the system as a whole" (p. 217). Given our knowledge of the "structural and functional characteristics of [modern] cultural systems," we could explain "differences and similarities between archaeological complexes" (p. 218). This, and only this, would allow archaeologists to "make major contributions in the area of explanation and provide a basis for the further advancement of anthropological theory" (p. 218) and earn them a seat at the high table.

Binford (1962, p. 218) suggested that “we cannot excavate a kinship terminology or a philosophy, but we can and do excavate the material items which functioned together with these more behavioral elements within the appropriate cultural sub-systems. The formal structure of artifact assemblages together with the between element contextual relationships should and do present a systematic and understandable picture of *the total extinct* cultural system.” Two years later he rephrased this: “The loss, breakage, and abandonment of implements and facilities at different locations, where groups of variable structure performed different tasks, leaves a ‘fossil’ record of the actual operation of an extinct society” (Binford, 1964, p. 425). This was followed by Binford’s (1968, p. 23) claim that the “practical limitations on our knowledge of the past are not inherent in the archaeological record; the limitations lie in our methodological naivete, in our lack of principles determining the relevance of archaeological remains to propositions regarding processes and events of the past.” In Binford’s (1968, p. 22) view, “data relevant to most, if not all, the components of past socio-cultural systems *are* preserved in the archeological record. Our task, then, is to devise means for extracting this information from our data.” Archaeology *is* prehistoric ethnology despite preservation issues.

Binford’s counter to the preservation mantra of traditional archaeologists was picked up and repeated by his students. Longacre (1970, p. 131), for example, wrote

If one adopts the view that culture is a systemic whole composed of interrelated subsystems, then it is reasonable to assume that all material items function in a most intimate way within the various subsystems of a cultural system. It follows, therefore, that the material remains in an archeological site should be highly structured or patterned directly as a result of the ways in which the extinct society was organized and the ways in which the people behaved. Thus, the structured array of archeological data will have a direct relationship to the unobservable organization and behavior of the extinct society.

What Binford meant by his statements regarding the preservation mantra would later be said by him to have been misinterpreted by others (Binford, 1981), but at the time, the statements quoted in the preceding paragraphs did two things. First, they partially negated the preservation mantra. Second, they

provided a rallying cry for archaeologists who sought a seat at the high table. For example, a volume edited by Meggers (1968), the original Simpsonite, entitled *Anthropological Archaeology in the Americas*, appeared in 1968. (The term “paleoanthropology” [Longacre, 1968] also was used but did not gain popularity.) Anthropological archaeology has as its goal “the understanding of past cultures, and the explanation of differences and similarities found among them” (Watson et al., 1971, p. ix). The implication was clear: Archaeologists could do anthropology, rather than just dig up artifacts. “The primary goal of prehistoric archeology is to make contributions to the larger science of anthropology” (Longacre, 1968, p. 389). “Archeology must remain as closely and intimately bound up with general ethnology as possible and constantly contribute to understandings of social man” (Deetz, 1970, p. 115).

Binford presented an argument that recruited bright young archaeologists who believed they could earn a seat at the high table. One recruit, Schiffer (1995, p. 3), reports that “by dismissing much of archaeology as traditionally practiced, Binford was wiping the slate clean, saying in effect that a young person entering archaeology could write on that slate something significant. A great teacher, Binford inspired me to join his crusade to transform archaeology into a science.” Further, Schiffer (2000, p. 13) later remarked that “I have long been an optimist that archaeology has unique theoretical contributions to make to the social sciences.” Similarly, Longacre (2000, p. 294) found Binford’s addition to the faculty at the University of Chicago where Longacre was beginning his doctoral research “electrifying to the archaeology graduate students.” Longacre reports that Binford was “very supportive of [Longacre’s and James Hill’s] initial attempts at ceramic sociology but added new directions” (p. 294). Schiffer, Longacre, and other graduate students of the 1960s learned what was required to earn a seat at the high table of anthropology—write new anthropological theory.

That the preservation mantra was (only) partially negated by Binford is easy to show. Longacre (1968, p. 387) observed:

The very nature of the data imposes severe limitations upon the archeologist. The challenge of these limits has been responsible for the development of a multitude of ingenious techniques. Every archeologist must be constantly aware of the boundaries that his data impose, but he

should likewise constantly seek to bridge boundaries through the use of sound scientific methods and judgment and, perhaps above all, imagination.

Deetz (1970, p. 117) noted that the incompleteness of the record poses “a real problem in very many cases if one expects all portions of a whole cultural system to be represented in some way or another in the archeological record.” His solution was to abandon the notion that “an archeologist is an anthropologist who digs [because] the traditional division of responsibilities within anthropology has unnecessarily restricted the archeologist in achieving maximum results” (p. 123); this is the archaeology as method mantra. Archaeologists, in Deetz’s view, should do ethnoarchaeology—study the interaction of artifacts and human behaviors in an ethnographic context and establish linkages between them. The latter would grant “understandings of the relationship between the material and nonmaterial derived from maximum information well controlled [that] can then be fed back into traditional archeological contexts for more precise inferences” (p. 123). That is, use ethnological and anthropological theory to explain the archaeological record.

Binford also began to do archaeological research in ethnographic contexts in order to develop what he called “middle range theory” (Binford, 1977, p. 6). Such theory would allow archaeologists to make statements about the past based on ethnographically observed linkages between behaviors and artifacts that would guide conversion of the static archaeological record into an ethnological record of dynamic human behavior (Arnold, 2003). Binford (1977, p. 7) suggested that building middle-range theory should proceed “hand in hand” with the development of “general theory”—theory that concerned our “understanding of the processes responsible for change and diversification in the organizational properties of living [cultural] systems.” Binford and Deetz exemplify a major trend in American archaeology after about 1970. This trend involves documenting linkages between artifacts and behaviors. It exists because archaeologists conceive of themselves as users of anthropological theory, so they must rewrite the archaeological record into something an ethnologist will recognize (hence the necessity of middle-range theory). Archaeologists use anthropological theory to explain the conversion product. I explore this trend and its nuances in the following.

After the middle 1970s

Archaeologists working after 1965, many of whom were newly initiated into the profession, claimed that archaeology provided unprecedented data in the form of several million years of cultural evolution accessible only to an archaeologist (e.g., Longacre, 1968; Woodbury, 1968). This claim was easily countered with the observation that archaeologists merely confirmed the conjectural evolutionary history erected on the basis of ethnological data in the nineteenth century by Lewis Henry Morgan, Edward Burnett Tylor, and others (Carneiro, 2003; Sanderson, 1990). This was the role attributed to paleontologists by biologists and geneticists prior to the 1970s (Maynard Smith, 1984; Sepkoski, 2005). Performing that role would not gain archaeologists a seat at the high table, regardless of their unique access to the deep temporal record of cultural development.

Reid and Whittlesey (1982, pp. 701–702) were explicit about the role of both middle-range and ethnological theory in archaeology: “We strongly maintain that only when past behavior is securely reconstructed can archaeologists proceed with confidence to explain it. At this point archaeologists compete with other anthropologists and behavioral scientists to provide explanations of phenomena that are interesting and useful to a social group larger than ourselves.” On the one hand, today paleontologists compete mostly among themselves (rarely with neontologists) to explain the paleontological (paleobiological) record; archaeologists on the other hand, compete with “other anthropologists and behavioral scientists” (Reid and Whittlesey, 1982, p. 702) to explain the archaeological record because what they are explaining is no longer archaeological. It is, instead, reconstructed (inferred) behavior—something an ethnologist would recognize (e.g., Schiffer, 1988, p. 465). Thus no archaeological theory is necessary (other than middle-range theory); an ethnologist’s theories not only suffice as explanations but are appropriate (once the application of middle-range theory has reconstituted the static archaeological record into a dynamic behavioral system).

Many archaeologists subscribed to the notions in the preceding paragraph. A few archaeologists, however, expressed concern. Price (1982, p. 714) remarked that “What is surprising is that the ‘new archaeology’ begins its downward deductions at so resolutely middle a level, precluding significant

generalization and producing a corpus of work remarkable for its intellectual conservatism. Interest in the higher levels has, if anything, dwindled.” The focus on the middle level may have been perceived as a necessary prerequisite to the building of laws about culture and human behavior, as it was suggested in the middle 1970s that “archaeologists have not yet developed any major theories” and these were dependent in part on laws (Read and LeBlanc, 1978, p. 310). The focus on middle-range issues led to a softer version of the preservation mantra—a notion that, as I suggested earlier, Binford only partially killed. It is worth exploring the softer version because of where it leads. I consider this softer version to be the new archaeology’s first banner, by which I mean that it was something big and flashy, meant to gain attention. The second banner was the familiar one of archaeology conceived as anthropology. I consider these two banners in turn.

A softer preservation mantra

Wobst (1989, p. 139) perceived a “glaring” and “explicit absence of higher-level [explanatory] theory” in archaeology. So, too, did others, some of them blaming the absence on the nearly universal reliance on anthropological theory, some blaming other things (e.g., Binford, 1977; Dunnell, 1978; Meltzer, 1979; Moore and Keene, 1983; O’Connell, 1995; Schiffer, 1996; Simms, 1992). Why should such a lacuna exist? Clark (1987, p. 31) argued that “What we [archaeologists] have now, and have always had, in place of archaeological theory is a partial and eclectic, at times even idiosyncratic dependence upon selected aspects of social anthropology, and other social and natural sciences, that define and validate problems for different segments of the discipline.” The dependence, I suggest, results from the notion that archaeology *is* prehistoric ethnology (see also Sullivan, 1992, p. 248).

It seems as if the belief that archaeology is anthropology or it is nothing is thought to have a requisite corollary that reads something like *the archaeological record can only be explained with anthropological theory*. Of course, it has long been recognized that the corollary is unnecessary (e.g., Gumerman and Phillips, 1978). Yet, the corollary remains in the sense that the archaeological record cannot, in a sense, speak for itself; it must be viewed through the tint of lenses provided by anthropological theory, or so it seems to be thought by many. The dependence on anthropological theory is not fatal to

the discipline; it is, however, a form of constraint. As Sullivan (1992, p. 248) noted, there is a “considerable gap between archaeological data and the descriptive lexicons of archaeology and sociocultural anthropology.” Translation of archaeological data into ethnological data limits explanatory theory to that of anthropology and other social-behavioral sciences. Several individuals have identified this constraint (e.g., Deetz, 1970; Lamberg-Karlovsky, 1970; Meltzer, 1979; Sullivan, 1992; Wobst, 1978).

Trigger (1973, p. 109) stated that he was “convinced that one of the weaknesses of much of the current theorizing in archeology can be traced to the tendency of some archeologists to treat their discipline as simply the ‘past tense of ethnology’ or a kind of ‘paleoanthropology,’ rather than defining its goals in terms of the potentialities of its data and asking the kinds of questions with which the data of archeology are best equipped to deal.” Trigger then argued that archaeologists can do little else for two reasons. First, “archeologists should be able to explain the archeological record in terms of processes such as innovation, diffusion, and adaptation, which can be studied fully and completely in any contemporary society” (p. 109). But it was paleontologists’ use of neontologically based evolutionary theory that denied them a seat at the high table; only when they identified unique processes of evolutionary tempo and mode (based on the coarse temporal resolution of the paleontological record) did Maynard Smith (1984) welcome them to the high table. The second reason archaeologists can do little more than prehistoric ethnology is found in Trigger’s (1973, p. 109) rephrasing of the preservation mantra: “Archeological evidence is a far more intractable source of information about many, if not all, areas of human behavior than are studies of contemporary man.”

The softened, less pessimistic version of the preservation mantra is in part a result of ethnoarchaeological research. A particularly telling example of the softening is found in two editions of a popular introductory textbook. In the first edition, Hole and Heizer (1969, p. 30) state “By virtue of their incompleteness, prehistoric data are unlikely to lead to the generation of new theories except where they come into conflict with models of what ‘should’ be that have been derived from other fields in the social sciences.” Eight years later, Hole and Heizer (1977, pp. 82–83) were not so harsh: “It is commonplace to assert that archeological remains represent only a portion of the things used by people in a culture, and

that even if we were to include all things, we would still omit a great deal of the essence of a culture. Under these circumstances archeology cannot be all that anthropology is nor can it routinely employ concepts of anthropology. Prehistorians must adapt these concepts to their particular data.” Archaeology is anthropological, if not as thoroughly ethnological as an ethnologist might hope for. Statements by others also reveal a softer version of the preservation mantra (e.g., Deetz, 1988, p. 16; Spaulding, 1988, p. 267; Upham, 1988; Cowgill, 1989, pp. 74–75; Cordell and Yannie, 1991, p. 100; Jochim, 1991, p. 308; Kelly, 1992, p. 255; Schiffer, 1992, p. 236). The softer version of the preservation mantra was, in my view, the only issue that archaeologists after 1970 felt might facilitate their access to the high table. It reflected the fact that the archaeological record was now no longer strictly conceived as an incomplete ethnographic record, but that it was a record that, with sufficient tenacity, middle-range knowledge, and cleverness, could be interpreted in ethnological terms.

The second variable that contributed to archaeology's poor self image was repetition of the “archaeology is anthropology” mantra, as implied by the quote from Hole and Heizer (1977, pp. 82–83) above. Because archaeology is anthropology, anthropological theory *must* guide archaeological research. This notion becomes the second banner of the new archaeologists, though by the 1960s it was an old banner. How or why archaeologists thought that use of anthropological theory would gain them access to the high table is unclear, apparently even to those archaeologists who wanted a seat there. Exacerbating the problem was the fact that this banner had a ribbon associated with it stating that archaeology is but method.

Archaeology as anthropology, and as method

Deetz (1988, p. 19) argues that there is no archaeological theory, but only anthropological theory. He (p. 21) also states that “at the level of theoretical formulation, archaeologists are operating as ethnologists,” thus echoing (without acknowledgment) Service's (1964) archaeology as method mantra. Deetz is actually paraphrasing Taylor (1948). In Spaulding's (1988, pp. 268–269) view, archaeology is “prehistoric ethnography as a part of scientific cultural anthropology.” Two beliefs result from this perspective. First, archaeologists believe that the archaeological record must be transmogrified into

something an ethnologist would recognize. Second, archaeologists are in sole possession of the transmogrifier, resulting in turn in the archaeology as method mantra. I explore each of these issues briefly.

Gumerman and Phillips (1978) attribute the tradition of borrowing from anthropology to the belief that archaeology is anthropology or it is nothing. They agree with Taylor (1948), Bayard (1969), Rouse (1972), and Woodbury (1973) that archaeology is but method, “independent of any specific theory in a behavioral science” (Gumerman and Phillips, 1978, p. 188). Others have more recently reiterated this opinion (e.g., Deetz, 1988; Moore and Keene, 1983; Rothschild, 1992; Spaulding, 1968, 1973, 1988). Gumerman and Phillips (1978) suggest archaeology can build its own theory, but not all archaeologists agree.

Moore and Keene (1983, p. 4) believe that the emergence of the new archaeology resulted in general, discipline-wide acceptance of the axiom that the variability evident in the archaeological record “is the subject matter of general anthropological theory.” The result was that archaeologists “pirated methods from the entire range of social and natural sciences” resulting in archaeology being largely a body of methods rather than the study of prehistory or cultural process (Moore and Keene, 1983, pp. 4–5). Anthropological theory sufficed as a source of explanations. Consider the two editions of what is arguably one of the seminal textbooks of the new archaeology of the 1960s and 1970s.

Watson et al. (1971, p. 164) suggest that the unique contribution of archaeology to theory will concern prehistoric cultural evolution and be “independent of those in other sciences that derive from different subject matter.” On this basis they argue that “thus, there is in a sense an ‘archeological theory,’ although it might be better characterized as evolutionary anthropology.” Thirteen years later, they were more hesitant to argue that there was distinctly archaeological theory. “Archeologists as anthropologists and social scientists explain how the archeological record was emplaced, and also they use archeological data to derive and test generalizations and to construct theories about cultural processes that are represented in the archeological record” (Watson et al., 1984, p. 249). Only archaeological data “can be used to help devise and test possible laws and theories about various aspects of prehistoric cultural change because archeological data contain the only records of long term information about the technology, social and political

organization, art forms, and so on of past nonliterate human societies” (p. 249). But, they lessen this unique value when they state a soft version of the preservation mantra: “In many cases, the archeological record may not provide the best data for testing possible generalizations or explanations, even if the methods of testing have been worked out. Some problems may be solved more readily with the use of ethnographic, sociological, or historical data” (Watson et al., 1984, p. 250).

The preceding makes clear that in the 1970s and 1980s even archaeologists who were strong advocates of anthropological archaeology were hesitant to claim a seat at the high table of anthropology. They may have been hesitant because, as Meggers (1955) suggested, culture was being redefined by ethnologists as something (cognitive) that archaeologists believed they could not access. This seems unlikely, however, given the emergence in the 1980s that that something might be accessible (see the review in Watson, 1995). I find it much more likely that the hesitancy originated in the (constraining) belief that archaeology is prehistoric ethnography, which in turn demanded two things. First, the static archaeological record had to be converted into dynamic ethnography, which demanded an archaeological conversion kit (middle-range theory). Second, anthropological theory was sufficient and *necessary* to explain the converted archaeological record, reducing archaeology to mere method.

The opinions of ethnologists

After 1960 ethnologists were not as vocal as they were four to six decades earlier about the second-class nature of archaeology. Yet, it is easy to illustrate what ethnologists thought of the new archaeologists’ claims that archaeology could contribute anthropological theory. Ethnologists of the late 1960s cited the preservation mantra (DeVore, 1968; Lee, 1968) as a weakness of anthropological archaeology, and they variously suggested that archaeologists wishing to be anthropological had to be more thoroughly educated and experienced ethnologists (Aberle, 1968; Fried, 1968; Lee, 1968). The latter would, it was thought, facilitate the derivation of cultural and human behavioral meaning from the archaeological record. When archaeologists began to do ethnography, they did so not because of the recommendation of ethnologists, but rather because they wanted to learn how the archaeological record was formed and about the linkages between artifacts

and behaviors. Ethnologists were concerned that archaeologists did not know the complexities of those behaviors, nor did archaeologists know how to build anthropological theory based on observations of those behaviors. Later ethnologists expressed other concerns.

Hoebel (1972, p. 131) reiterated his opinion of 1949: “Prehistoric cultures...may be reconstructed in only their thinnest outlines. So much rests on inference from limited facts that enthusiastic opinion and intellectual prejudice frequently run far beyond the reasonable limits of the evidence.” Ethnologist Leach (1973, p. 767) remarked that the “data of archaeology are the residues of ancient social systems and the most that the archaeologists can hope for is he may be able to establish, with reasonable confidence, the work pattern that produced these residues...social organization as the social anthropologists knows that term, must forever remain a mystery.”

When he argued that ethnology was an historical science, ethnologist Aberle (1987, p. 556) remarked that the “past is preserved in the present, but it is imperfectly preserved, both because of entropic transformations of the structures of the past and because of loss of information.” It is easy to imagine what he would say of archaeology. Social anthropologist Frederick Gearing “found it difficult to imagine that some archaeologists still believed that material culture can be mapped isomorphically with the social and ideological dimensions of culture, as even a radical change in the former may not cause changes in ideology or social interaction” (Zubrow, 1989, p. 48). Finally, cultural anthropology doyen Harris (1997, p. 121) remarked “Archaeology is to anthropology as paleontology is to biology. Without archaeology, anthropologists could neither describe nor explain the course of cultural evolution. As a result of the great sweep of time and space studied by archaeologists, anthropology enjoys a unique position among the social sciences because it can observe the operation of long-range trends and can formulate and test causal theories of cultural evolution.” Archaeology *served* anthropology by using its unique data to confirm or refute speculative evolutionary models derived from ethnological data; it did not contribute unique theory, only anthropologists “formulate” theory.

Conclusion

Archaeologist Gosden (1999, p. xi) suggests “archaeology has been an importer of anthropological

ideas and suffered a balance of trade problem due to the lack of export of archaeological results and theories. Things are changing, however.” According to Gosden the change is occurring because anthropologists are realizing that the two subfields concern different time spans. “Anthropologists are increasingly aware that the traditional 18 months of fieldwork provides a snapshot of society, which it is difficult to understand in isolation. The need for historical context is especially acute in areas of the world with short written histories” (Gosden, 1999, p. 11). A realistic conception of the requisite time span for observing human behavior ethnographically or archaeologically depends on the question being asked (Brooks, 1982; Lyman, 2007). Gosden has hit upon one of two key points, but the one he has hit upon is the one others identified previously.

It is only partially because archaeologists have access to a much more temporally extensive record than ethnologists that they can build unique theory, a point made by many cited here. It is also partially the *scale of temporal resolution* to which they have access that gives archaeology the potential to make unique contributions, a point few I have cited explicitly make. The scale of temporal resolution is much coarser in archaeology than the day-to-day, season-to-season, year-to-year resolution afforded ethnology. A coarse scale of temporal resolution is precisely what provided paleontology with the admission fee to the high table of evolutionary theory. In particular, the unique tempo and mode of evolution as manifest in punctuated equilibrium was perceptible *only* with a relatively coarse, millennium-to-millennium, temporal resolution. That is why neontologists reacted negatively to punctuated equilibrium; they were not used to conceiving or perceiving processes at that low power of temporal resolution. It was because the data of the paleontological record did not fit the neontologist theory but instead suggested unique tempos and modes of evolutionary processes that Eldredge, Gould, Stanley, and others wrote new evolutionary theory and were eventually welcomed to the high table.

Modern archaeologists are not timid about borrowing models and theories from modern social science (see references and discussions in Hegmon, 2003; Schiffer, 2000; VanPool and VanPool, 2003), whether or not they still seek a seat at the high table. That archaeology has thus far contributed little to anthropological (or general social science) theory has been suggested by many and is evidenced by the fact that the list of cultural processes

mentioned today is virtually identical to that mentioned prior to 1950 (Lyman, 2007). To gain access to the high table, we should follow a suggestion made more than three decades ago. Plog (1973) argued that frequency seriation had not been fully used to explore tempos and modes of cultural change. I agree and suggest that using frequency seriation as a form of time-series analysis (Lyman and Harpole, 2002) would reveal much about the particulars of the tempo and mode of change, perhaps even tempos and modes that are only visible archaeologically.

I am not suggesting that archaeologists abandon use of ethnologically documented cultural processes as explanatory tools, nor am I suggesting that archaeologists abandon traditional ethnological and anthropological theories. What I am suggesting is that a little explored arena that is likely to contain evidence of unique processes—particularly, tempos and modes of change, to borrow Simpson’s (1944) wording—is the temporally coarse-grained archaeological record itself. It is there that ethnologically imperceptible large-scale processes may be revealed. And, if the history of paleontology is any guide, it is precisely those sorts of revelations that will gain archaeologists a seat at the high table of anthropology. To gain those insights, archaeologists must occasionally discard the tint of the *archaeology is prehistoric ethnology* mantra and consider the archaeological record as potentially revealing something invisible to an ethnologist. It may reveal nothing, but how will we know unless we look?

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