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Archaeology, Anthropology, and the Culture Concept

What follows is the text of the Distinguished Lecture presented at the 93rd Annual Meeting of the American Anthropological Association, held in Atlanta, Georgia, in November 1994.

ALTHOUGH I HAVE belonged to the American Anthropological Association since 1953, my first year in graduate school, I have been so deeply immersed in my own archaeological corner for the past 20 years that I hadn't noticed, until I began thinking about this talk, how *very* different the current anthropological landscape is from the one in which I came of age in the discipline. That fact makes the present assignment a considerable challenge: to say something that might hold the attention of an audience representing the diversity of 1990s anthropology. So I decided to structure much of my discussion around something central to anthropology and anthropologists since the formational period of the discipline: culture.

As a University of Chicago graduate student, I encountered the anthropological culture concept not long after my commitment to a particular form of Protestantism, as a matter of personal faith and belief, had faded away. So it is perhaps not surprising that during my pre-M.A. period I concluded culture was a crucial tenet of anthropological faith. It seemed to me absolutely necessary to commit myself to one of the many definitions of *culture* then under discussion (Kroeber and Kluckhohn 1952) before I could be confirmed as a real anthropologist (before I could pass the comps). After that, I would earn a Ph.D. and live my anthropological career in accord with my own personal understanding of culture, which might also be Kluckhohn's or Kroeber's or Linton's. As a matter of fact, it was Robert Redfield's version of E. B. Tylor's classic definition that I chose to cleave to. Tylor said,

"Culture . . . is that complex whole which includes knowledge, belief, art, morals, law, customs, and any other capabilities and habits acquired . . . as a member of society" (Tylor 1871:1). In Redfield's rendering, "Culture is 'an organized body of conventional understandings manifest in art and artifacts which, persisting through tradition, characterizes a human group'" (Redfield 1940; see Kroeber and Kluckhohn 1952:61).

Redfield's definition is a little shorter and snappier than Tylor's, and hence easier to memorize for a person struggling—as I was then—not only with detailed culture-historical sequences in several parts of the Old and New Worlds but also with Murngin, Naskapi, and Nuer kinship systems; with how to tell a phoneme from a phon; and with how precisely the Australopithecine pelvis differs from ours and from a chimpanzee's. Also relevant was the fact that Redfield was a senior member of the Chicago anthropology faculty and someone my adviser (Robert J. Braidwood) respected. Moreover, Redfield's definition specifically mentions manifestations of culture ("art and artifacts") and explicitly invokes duration through time, two characteristics that appeal strongly to archaeologists.

Secure in my grip on the culture concept, I passed my comps, got an M.A., and went on to dissertation research in Near Eastern prehistory. Redfield, Eggan, Tax, Braidwood, Washburn, and McQuown taught us that anthropology was a unitary enterprise made up of four equal parts: social anthropology or ethnology, archaeology, physical anthropology, and linguistics. A prominent Harvard archaeologist, Philip Phillips, also formally emphasized the close ties between archaeology and the broader field of anthropology in an influential article published in 1955, concluding that "American archaeology is anthropology or it is nothing."

I wholeheartedly accepted all this and identified with anthropology as fervently as with archaeology. Sometime during the late 1950s when I was completing my Ph.D. dissertation, I received an initial reality check concerning

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the relation between archaeology and anthropology. Having attended a lecture and subsequent reception for Ruth Landes, whose Ojibwa ethnographies I had read and admired, I introduced myself to her as an anthropologist. She asked what my specialty was and I said Near Eastern prehistory, at which point she turned away abruptly saying, "Then you're *not* an anthropologist, you're an archaeologist." Her remark was my first inkling that the anthropological world was not as well integrated as my mentors had led me to believe.

I had ample opportunity to confirm the inkling while carrying out research in the Old World, and then later as I transferred my fieldwork locale to eastern North America. By the early 1980s I knew of at least two North American departments of archaeology completely separate from anthropology (Calgary and Simon Fraser) with another (Boston University) on the way. There were also separatist themes clearly voiced in the literature by several archaeologists.¹ A few years later a full-scale anti-"archaeology as anthropology" assault was launched from England and northwestern Europe.² "American archaeology as anthropology" was rejected along with other tokens of American imperialism. And, of course, during the 1960s and 1970s I had noticed that the subdisciplinary balance in my alma mater department at Chicago had become markedly asymmetric in favor of one kind of sociocultural anthropology and against archaeology and physical anthropology.

All this I knew, but until I heard Kent Flannery's distinguished lecture at the annual meeting of the American Anthropological Association in December 1981 (Flannery 1982), I had not noticed that the other foundation of my basic anthropological training—the culture concept, even culture itself—was under attack within American sociocultural anthropology. Flannery quotes Eric Wolf's 1980 assessment:

An earlier anthropology had achieved unity under the aegis of the culture concept. It was culture, in the view of anthropologists, that distinguished humankind from all the rest of the universe, and it was the possession of varying cultures that differentiated one society from another. . . . The past quarter-century has undermined this intellectual sense of security. The relatively inchoate concept of "culture" was attacked from several theoretical directions. As the social sciences transformed themselves into "behavioral" sciences, explanations for behavior were no longer traced to culture: behavior was to be understood in terms of psychological encounters, strategies of economic choice, strivings for payoffs in games of power. Culture, once extended to all acts and ideas employed in social life, was now relegated to the margins as "world view" or "values." [Wolf 1980]

Flannery mourns the loss of an integrating concept of culture in ethnology, and fears the threat of such loss in archaeology. Now, somewhat more than ten years later,

it appears that the culture situation in ethnology and sociocultural anthropology is even more problematic.

Back in 1952, Kroeber and Kluckhohn (1952:149) noted that after Tylor published his definition of *culture* in 1871, there were no other formal definitions offered for 32 years. Between 1900 and 1919, they found six; between 1920 and 1950 there were 157. The word *culture* had great currency throughout this whole time, including the three decades post-Tylor, but it was being used without explicit definition.

According to a recent summary volume (Borofsky 1994), research on or about the culture concept, or "the cultural," now ranges from linguistic, cognitive, and psychological approaches to a variety of postmodern and post-postmodern experimental efforts on the literary side to politically, historically, empirically, and/or methodologically oriented work, to that which focuses explicitly on the nexus of biology and culture, of natural science and human science, and to that which concentrates on intercultural encounters in premodern, modern, or postmodern world systems.³ I return to this issue below, in the concluding section, but first take up something with which I am somewhat more familiar: recent travels of the culture concept in archaeology.

The culture concept in anthropological archaeology has followed a well-marked but nonlinear trajectory over the past several decades. After a freewheeling and primarily data-free speculative period in the 19th century (Willey and Sabloff 1993: ch. 2), North American archaeology developed around a culture-historical approach parallel to but separate from concurrent processes in European archaeology (Trigger 1989:187, 195). At the turn of the century, "the term culture was first applied to groups of sites containing distinctive artifact assemblages in the Ohio Valley. By 1902 William C. Mills had distinguished the Fort Ancient and Hopewell cultures" (Trigger 1989:187).

At this time in North American archaeological parlance, Trigger says a "culture" was mainly a geographical entity—a taxon for one of several synchronic units—because so little was known about chronology. The period between World War I and World War II was characterized by intense concern with temporal relations and by a great deal of historical particularism in North American archaeology. Trigger notes, as have other scholars, that Americanist archaeologists of the 1930s and 1940s paid no attention to human behavior, to function, ecology, or even quantification.⁴ There was no interest in culture per se, although widely used classificatory units (foci, aspects, phases) were implicitly understood to be cultural units, possibly reflecting ancient tribes or groups of related tribes. Archaeological cultures in North America were believed to be conservative, changing slowly if at all in response to diffusion of objects and ideas, and/or to migration of large and small human groups. Walter Taylor's

detailed critique of Americanist archaeology, published in 1948 and promoting a very different view of culture to and for archaeologists, was a radical departure from mainstream 1940s archaeological practice.

Taylor's argument (1948: ch. 4) included a view of culture as composed of two concepts, one holistic—Culture—and one partitive—cultures. Holistically speaking, Cultural phenomena are distinguished from natural phenomena, both organic (nonhuman biological) and inorganic (geological, chemical). Cultural phenomena are emergent, more than the sum of the partitive parts, they are in a realm of their own, a realm created and maintained solely by human cognitive activity.

Partitively, the culture concept also denotes a specific *piece* of the whole of human Culture, *a* culture. Either way, C/culture “is a mental phenomenon, consisting of the contents of minds, not of material objects or observable behavior” (Taylor 1948:96). Cultural content is cumulative: “The culture-whole existing today owes its form and at least the majority of its content to what is called the cultural heritage” (Taylor 1948:98). The (or a) cultural heritage consists of mental constructs. “Mere physical form is extraneous as far as culture is concerned, being a property of the world of physics and not of culture” (Taylor 1948:99). What was once called “material culture” (as distinct from “nonmaterial culture” or “social culture”), according to Taylor, is not culture and is in fact two removes from the real thing: the locus of culture is mental, ideas in people's minds.⁵ Artifacts and architecture are the results of behavior, which itself derives from mental activity. “Culture [the first-order phenomenon for Taylor] is unobservable and non-material.” Behavior (second-order phenomenon) is observable but nonmaterial, and only with third-order phenomena resulting from behavior do we come to artifacts, architecture, and other concrete materials making up the archaeological record: “this [third] order consists only of objectifications of culture and does not constitute culture itself” (Taylor 1948:100).

Taylor's handling of the culture concept is seemingly a departure from the position held more or less contemporaneously by Kroeber, who says that materials and objects are all part of culture equally with ideas and customs: “We may forget about this distinction” (Kroeber 1948:295–296). If one reads Kroeber's whole discussion, however, one realizes that his view is probably the same as Taylor's (and Redfield's). He says,

What counts is not the physical ax or coat or wheat but the idea of them, their place in life. It is this knowledge, concept, and function that get themselves handed down through the generations, or diffused into other cultures, while the objects themselves are quickly worn out or consumed. [Kroeber 1948:295]

So it is not difficult to see how Taylor, beginning with the traditional, then-current views on culture, and think-

ing about how to transform observations on the archaeological record into information about culture, came to the formulation outlined above. If only the ideas and knowledge in people's minds are culture and the ultimate source of culture, then archaeologists who want to contribute to cultural anthropology, the discipline that studies culture, must address their thrice-removed materials in ways calculated to delineate past cognitive patterning. The archaeological record can reveal ancient culture—the mental activities of long-dead people—if skillfully interrogated. The archaeologist as archaeologist is merely a technician digging up physical materials and their associations, in space and time, but the archaeologist as anthropologist is uniquely qualified to produce truly cultural information about ancient peoples and extinct societies throughout time and space.

One might think that to be an exciting and appealing prospect, but virtually no one heeded Taylor's call to reshape the practice of archaeology and make it more anthropological. Nothing happened even after two eminent, well-respected members of the archaeological establishment, Gordon Willey and Philip Phillips, repeated Phillips's earlier admonition that “American archaeology is anthropology or it is nothing” in a widely read and highly influential volume, *Method and Theory in American Archaeology* (Phillips 1955; Willey and Phillips 1958:2). Why not?

One very immediate and practical obstacle was the *ad hominem*, or straight-to-the-jugular, technique Taylor used to highlight the sins and errors committed by living, active, and highly influential senior archaeologists, who, he said, preached anthropology but practiced “mere chronicle,” sterile time-space distributions of selected artifacts. Such personal assaults are almost never successful as a long-term strategy. In a published Ph.D. dissertation, they are suicidal.

Another *a priori* reason why Taylor's program was never implemented, not even by Taylor himself, is that the demands it placed upon field and laboratory recording and analysis were simply impossible to meet at the time *A Study of Archaeology* was published. Even now, with quite powerful computer hardware and software available to archaeologists, and with greater knowledge of site-formation processes as well as more widespread interest in ancient ideational patterns, Taylor's conjunctive archaeology is a rather tall order.

As Dunnell (1986:36) has pointed out, there is yet another possible explanation why Taylor's reform call was virtually totally ignored, and that is the concept of culture he provided as the source and center of his formulation. Taylor asserted, with most sociocultural anthropologists of his day, and indeed since Tylor, that the locus of culture is mental. Artifacts are not culture, they are only *objectifications* of culture at several removes from the real thing. Moreover, he insisted that the highest goal

archaeologists could aspire to was eliciting cultural anthropology from archaeological remains, which meant the mental processes (the true, the real culture) of those past peoples. This argument easily led to a view of archaeology as being highly marginal within general anthropology.

As indicated earlier, Taylor's views also ran counter to the basic operating assumptions of most Americanist archaeologists at the time he was writing (Binford 1987:397), many of whom did not believe that the original meanings—to their creators—of the items they excavated could be retrieved, and most of whom were less immediately interested in this proposition than they were in basic time-space systematics. In 1943, Griffin matter-of-factly stated,

The exact meaning of any particular object for the living group or individual is forever lost, and the real significance of any object in an ethnological sense has disappeared by the time it becomes a part of an archaeologist's catalogue of finds. [Griffin 1943:340]

Almost exactly 20 years after Taylor completed the dissertation published in 1948 as *A Study of Archaeology*—a closely reasoned, devastating critique that seemingly sank without a trace—another reformer published a much shorter and much more successful appeal, similar in some ways to that of Taylor but quite different in others: Lewis Binford's 1962 *American Antiquity* article, "Archaeology as Anthropology," initiated a period of dominance by processual archaeology, or "the New Archaeology," as it is often called.⁶ Like Taylor, Binford and the New Archaeologists were intent upon expanding the goals of Americanist anthropological archaeology beyond those of typology and stratigraphy. Although Binford insisted that *all* aspects of past societies could be investigated archaeologically, in practice he focused almost exclusively upon subsistence and ecology. Processual or New Archaeology came to be a kind of neo-evolutionary "econothink" (Hall 1977) with heavy emphasis on hypothetico-deductive method, quantification, computers, and statistics. Binford's concept of culture, appropriate to the general tenor of New Archaeology and quite different from Taylor's, was that of his professor at the University of Michigan, Leslie White: "culture is man's extrasomatic means of adaptation" (Binford 1962; White 1959:8, 38–39).

Binford himself—like another of his Michigan professors, James Griffin—had little interest in the meanings archaeological materials might once have had for their makers and users, and he paid no serious attention to ideational issues, regarding them as epiphenomena at best. Thus, under his highly influential leadership, Americanist archaeology was materialist, functionalist, and evolutionist in orientation, overtly anthropological and scientific in its aspirations. This trajectory was very successful during the 1960s and 1970s. In fact, it still represents the mainstream of practicing archaeology in the United States

(Willey and Sabloff 1993:317), partly because of the great initial success of the New Archaeology and partly because of the 1974 federal legislation (the Moss-Bennett Bill, or the Archeological Conservation Act) mandating the inclusion of archaeology in federally funded environmental impact assessments. This legislation formalized and routinized archaeological procedures in an early-1970s mode that persists throughout the United States today.

In the late 1960s, however, Binford's attempts to understand the morphological variation in Middle Paleolithic (Mousterian) assemblages in France resulted in his turning the full force of his research into ethnography in northern Alaska and elsewhere (Binford 1983:100–106). Largely, although not by any means entirely, owing to Binford's influence, ethnoarchaeology became a standard research focus during the 1970s and 1980s for Americanist and other prehistorians and is now an established, productive sub-subdiscipline.⁷

Meanwhile, in the late 1970s and the 1980s, the few anthropological archaeologists who were not entirely swept away by Binfordian, processualist New Archaeology with its heavy methodological emphasis received powerful reinforcement from British and European advocates of postmodernist (postprocessualist) directions in archaeology, wherein ontological issues were central. The most influential among these—at least in the anglophone world—is usually said to be Ian Hodder (1982a, 1985, 1991a, 1991b). Although Hodder strongly opposes nearly everything Binford advocates, and Binford wholeheartedly embraces their adversarial relations, both are deeply committed to ethnoarchaeology as an essential archaeological technique.⁸ Obviously the foci of their ethnographic observations differ. Binford, to whom culture is humankind's extrasomatic means of sustaining themselves in a wide array of physical environments through space and time, documents the interplay of climatic, topographic, floral, faunal, geological, and other natural factors with human hunter-gatherer-forager subsistence and technology. Hodder, to whom culture is mental (symbolic), material, social behavioral, and the recursive relations among all three, takes note of the important roles played by artifacts in the complex, dynamic tensions characterizing human social and societal encounters. He insists on the primacy of archaeology as archaeology and archaeology as history, rather than archaeology as anthropology, and stresses an empathic, particularistic approach to understanding the past, much like that of R. G. Collingwood (1939, 1946).

Binford rejected the traditional anthropological culture concept (Tylor's, Kroeber's, Redfield's, Taylor's) because it was not appropriate to his goals and practice as an archaeologist, not even as an explicitly anthropological one. Hodder is committed to a fluid semiotic version of the traditional culture concept in which material items, artifacts, are full participants in the creation, deployment,

alteration, and fading away of symbol complexes. Hodder advocates a contextualist archaeology—as did Walter Taylor—but one in which artifacts are not just objectifications of culture, they *are* culture.

Like Binford's earlier explicit rejection of an archaeologically unworkable, mentalist-idealist concept of culture in favor of Leslie White's functionalist, neo-evolutionist formulation, Hodder's move is clever and strong; but it is in the opposite direction of Binford's. Hodder begins with the mentalist concept of culture, then takes archaeology from a completely peripheral position with regard to that concept and places it squarely in the center of symbolic-structuralist inquiry. Artifacts—their creation, use, and discard—are “symbols [i.e., Culture] in [social] action” (Hodder 1982a). Hence, archaeology with its primary focus on material culture is very centrally and strategically located in the arena of social theory.

Binford does not deny that artifacts had intrinsic meaning, semiotic content, for their makers and users, but this does not interest him. He rejects the traditional archaeologist's narrow focus on artifacts solely as markers of time and space, and he also rejects Taylor's focus on artifacts as mere *clues* to—objectifications of—cultural patterns in minds long gone, as he rejects the further implication that, no matter how hard they try, archaeologists who accept Taylor's program can never be more than cultural anthropologists *manqué*. Binford views artifacts and associated non-artifactual/ecofactual information as the essential means to interpret the interactive dynamics of paleoenvironments and human paleoeconomies in synchronic and diachronic detail, important work that only archaeologists can do. To make the artifacts and ecofacts comprising the archaeological record speak substantively to these issues, however, those artifacts and ecofacts must be approached via site formation processes and ethnoarchaeology, all of which Binford refers to as “middle range theory.”

Hodder is not interested in matters of subsistence and brute livelihood. Rather, the intrinsic meanings with which the artifacts were imbued, the roles they once played in complex social actions and interactions, are central. He agrees with the symbolic anthropologists and other social theorists that symbol systems are what distinguish the human primate from all other beasts; those symbol systems include and are importantly shaped by material objects and architectural forms. Hodder approaches these issues of symbolic systems, past and present, via ethnoarchaeology (Hodder 1982a, 1982b).

So what is this thing called ethnoarchaeology, upon which the most influential representatives of contemporary Euro-American archaeology have converged? Ethnoarchaeology is one of the multitudinous ways in which archaeologists obtain information relevant to creating and expanding their inferences from archaeological data, and to making those inferences more plausible. Ethnoar-

chaeology can be as simple as collating descriptive and functional details about objects and processes archaeologists frequently encounter—stone scrapers, bone awls, sherds from wheel-made pots, metallic ore, and slag—from archival sources, such as old ethnographies, ancient histories, museum exhibits and collections; or from published and unpublished photos, drawings, paintings. But, classically, ethnoarchaeology means designing and carrying out ethnographic research in one or more contemporary locales, chosen for their relevance to some archaeological problem. Binford picked the Nunamiut of northern Alaska because he believed the caribou hunting techniques they practice in an arctic environment are relevant to his archaeological interpretation of Middle Paleolithic caribou hunters in arctic western Europe during the Late Pleistocene. The Nunamiut also instructed Binford about the dynamics of mobile, successful hunting-gathering groups in close touch with relevant natural resources in their landscapes. Binford's books and articles on lessons learned from the Nunamiut were, and are, highly influential among Americanist archaeologists, as is the other ethnoarchaeological or actualistic research he has done in the interests of middle-range theory: “the relationship between statics and dynamics, between behavior and material derivative.”⁹

Hodder initially chose East Africa as a suitable place to investigate, for archaeological purposes, spatial patterning of artifacts in relation to ethnic boundaries (Hodder 1982a), but then he was distracted by other aspects of the contemporary scene in Baringo and turned to the study of material objects, symbol systems, and their intersection with archaeological interpretation. In examining ideas about spatial patterning of material culture, ideas that were widely held among archaeologists, Hodder found that his observations among several East African groups (the Njemps or Ilchamus, the Lonkewan Dorobo and Samburu, the Lozi, the Nuba) contradicted these ideas, or at any rate made them appear highly problematic. For example, most archaeologists would readily agree that material culture reflects the degree of interaction between groups: the more interaction, the greater the similarity of artifacts, and vice versa. Hodder noted that the nature of the interaction and the degree of competition between the groups play an important role in how basketry or styles of ear decoration are used “to constitute and reproduce ethnic group distinctions despite the long history and high degree of inter-ethnic flows” (Hodder 1982a:35). He also found that the symbolic status and functioning (the cultural meaning) of material items, such as the spears carried by young unmarried men and the calabashes decorated by young married women, determine the morphology and distribution of those items within and beyond a single society (Hodder 1982a: ch. 4, 1991a:109–119). Finally, he was strongly impressed with the fluidity and activity of symbolic loading on and in

objects of material culture, which are continually created but also continually act back on their creators, users, and perceivers to maintain or to disrupt culturally defined boundaries within and between social groups (young men versus old men, men versus women, Samburu versus Dorobo):

material culture transforms structurally rather than reflects behaviorally . . . refuse and burial patterns relate to social organization via such concepts as purity and pollution. . . . So, how material culture relates to society depends on the ideological structures and symbolic codes.¹⁰

Hodder and other postprocessualists are also very concerned about the sociopolitical setting of contemporary archaeology. They urge archaeologists to be aware and self-critical about their biases and preconceptions, lest they unwittingly create a past in the image of their own present, a past that then helps legitimate contemporary social or political themes (Hodder 1991a: ch. 8; Shanks and Tilley 1988: ch. 7).

In sum, regarding the two men and their programs: one may, and should, quarrel with Binford's narrow "econothink" focus, as does Robert Hall (1977:499), who coined the word in reference to 1970s New Archaeology (see also Fritz 1978; Redman 1991). And one may object to the ahuman (no people in it) ecosystemic orientation (Brumfiel 1992), and the general theoretical underpinning of Binford's position (P. Watson 1986a, 1986b; Wylie 1985), but his influence has instigated and continues to impel a considerable amount of fruitful archaeological research. That is, Binford has been successful in defining goals and methods that many archaeologists find feasible and rewarding.

Much of the work of Hodder, his students, and his postprocessualist colleagues has been heavily dependent on ethnographic and historic information, and the method he advocates has yet to be comprehensively demonstrated for purely prehistoric data, although such a demonstration is perhaps forthcoming from the work he is currently directing at the famous site of Chatalhöyük in Turkey. Meanwhile, however, Hodder and other postprocessualists (by now a diverse group scattered through Europe, Australia, and North America) *have* certainly influenced contemporary archaeological practice in the heartland of the old Binfordian New Archaeology, and even in parts of the cultural resource management universe. There is much more interest now than even five years ago in semiotic approaches and in critical theory applied to the archaeological record and to the practice of archaeology. It is perhaps too soon to see a comprehensive synthesis emerging, but some manner of rapprochement is definitely underway (see Willey and Sabloff 1993:312–317).

Beginning in 1989, the Archeology Division of the American Anthropological Association requested a

prominent archaeologist to deliver a distinguished lecture at the yearly divisional get-together during the annual meeting of the association. Very conveniently for my purposes here, the four lectures published so far all address this very issue.¹¹ The four distinguished archaeological lecturers provide a series of authoritative opinions and examples concerning relations between archaeological theory—past and present—and the actual doing of archaeology (fieldwork, laboratory and library work, interpretation and publication). Each speaker focuses upon crucial themes in archaeological theory and practice, past and present, and provides suggestions about how to improve our present understanding of the past.

Redman (1991) begins the series by pointing out how much continuity there is between 1970s and 1980s archaeology. He also notes that although contextualist or postprocessualist archaeology and New Archaeology (processual archaeology) are obviously complementary, it is unlikely that there will be significant integration. He thinks coexistence is the best we can expect because a major impetus for postprocessualist critiques comes from fundamental differences between archaeologists with humanistic goals and those committed to science. He advocates making the most of both approaches, and recommends that "we encourage serious scholars to do what they are best at doing and to coordinate diverse thinking to form a loose but lasting alliance for new knowledge of the past and present" (Redman 1991:304).

In spite of Redman's well-founded reservations about explicit integration between processual and postprocessual archaeology, Bruce Trigger (1991) sets himself the task of indicating what such a synthesis might look like. He characterizes processual archaeology as neo-evolutionism and ecological determinism, counterposing it to postprocessualist emphases on "the contingent, psychological, and mental aspects of human experience" (Trigger 1991:553). In other words, the confrontation is between "reason" and "culture" (Trigger 1991:551, 554). Trigger then discusses external and internal constraints on human behavior: ecological, technological, and economic factors and forces being the most familiar external constraints, whereas cultural traditions made up of mental constructions—some unique to specific societies, some much more widespread cross-culturally—are the internal constraints. Because cultures are "historical precipitates," the invention of new concepts is not random, but is strongly affected by earlier concepts and their history. The best means archaeologists have to get at the cultural meanings of historically related archaeological evidence is to develop the direct historic approach, Trigger says (1991:562), admitting that we will probably never know the specific meaning that Upper Paleolithic cave art, say, had for its creators. Nevertheless, he urges archaeologists to embrace wholeheartedly "the study of cultural traditions as well as of ecological and systemic con-

straints . . . to take account of the constraints imposed on human behavior by cultural traditions as well as by rational accommodations to external factors," thus synthesizing the ecological determinism of processual archaeology with the historical particularism of postprocessual archaeology (Trigger 1991:562–563).

Trigger's optimism about the possibility for synthesis is encouraging, but he fails to give any consideration to the very significant problems involved in deciding what is "cultural" ("internal") and what is "natural" ("external") in ethnographically or archaeologically documented societies. Hence, this part of his discussion misses the whole point of the anthropological enterprise, which is to obtain knowledge about that very conjunction: How is it that human individuals and human societies—past and present—intricately blend and intertwine nature and culture?

Brumfiel's distinguished lecture (1992) is a clear and eloquent argument about the importance of paying attention to social change in ways that the dominant ecosystemic orientation of New Archaeology discouraged or disallowed. She is especially concerned with gender, class, and faction, and argues three points:

First, the ecosystem theorists' emphasis upon whole populations and whole adaptive behavioral systems obscures the visibility of gender, class, and faction in the prehistoric past. Second, an analysis that takes account of gender, class, and faction can explain many aspects of the prehistoric record that the ecosystem perspective cannot explain. Third, an appreciation for the importance of gender, class, and faction in prehistory compels us to reject the ecosystem-theory view that cultures are adaptive systems. Instead, we must recognize that culturally based behavioral "systems" are the composite outcomes of negotiation between positioned social agents pursuing their goals under both ecological and social constraints. [Brumfiel 1992:551]

In the body of the address, Brumfiel succeeds in showing how thoughtful archaeologists could actually begin to forge the synthesis Trigger speaks of, or at least the "loose but lasting alliance" Redman hopes for.

Cowgill's distinguished lecture to the Archeology Division (1993) is an even more explicit attempt to bring together and build upon the most successful aspects of processual archaeology and the most exciting promises of postprocessual archaeology. In describing the achievements and shortcomings of processual archaeology, Cowgill notes that one characteristic of most archaeologists is underconceptualization of the past at different levels: On the lowest level there are no people at all, just pots or potsherds, projectile points, or other artifacts. At the second level, people are present but they have no individuality; they are Ruth Tringham's "faceless blobs" (Tringham 1991). At the third level, people are "rational actors." Cowgill points out that we badly need a fourth level, where people not only find food, shelter, mates, allies, and enemies while creating, using, modifying, losing, breaking,

and discarding material things, but also these people perceive, they think, they plan, they make decisions, and in general they are ideationally active. In the rest of his paper, Cowgill discusses how archaeologists might hope to approach the ideational realms of prehistoric peoples by trying harder to get at ancient ideation; by becoming more sophisticated about direct historical approaches (here he obviously agrees with one of Trigger's points); and by working imaginatively and responsibly to develop what he calls "Middle Range Theory of the Mind." By this he means, in part, seeking out widespread aspects or principles of symbolization, attempting to link design properties (in art styles or architecture) with social features and/or culturally specific cognitive maps, and in general taking seriously what he dubs "psychoarchaeology."

What is most interesting and heartening to me about this suite of distinguished lectures is that all four explicitly, creatively, and thoughtfully address the major schism in contemporary Americanist archaeology, and all four explicitly, creatively, and thoughtfully recommend ways to bridge the schism at various points, as well as ways to advance archaeological knowledge using methods from both sides of the fault line.

Another very promising development is the new generation of ethnoarchaeological fieldworkers who are undertaking and completing longer-lasting, finer-grained investigations than those of Binford and Hodder. Of many good examples I note just three here: the 30-year trajectory of ethnoarchaeology among the San of Botswana from the work of Yellen and Brooks to that of Hitchcock, Weissner, and Kent; Longacre's 20-year-long Kalinga ceramics project in northern Luzon; Herbig and Dietler's ten years of research on Luo pottery and on Luo settlement biographies in western Kenya.¹²

As regards the other focus of this paper, is there an edifying conclusion to be drawn from comparing the odysseys of the culture concept in Americanist sociocultural anthropology/ethnology and in archaeology? Yes, there is. In each subdiscipline, certain practitioners took that concept very seriously, not just as a more or less meaningless piece of antiquated anthropological dogma. Because archaeologists of the 1930s did not attempt to operationalize the prevailing culture concept, but rather ignored it while absorbed in creating time-space frameworks essential to North American prehistory, Walter Taylor (1948) made a strenuous effort to align Americanist archaeology with Americanist sociocultural anthropology by taking the traditional, Tylolean culture concept as a central tenet in his argument. He had very little immediate influence on his archaeological colleagues, in large part because that culture concept could not be implemented or operationalized in ways congruent with archaeological concerns of the 1940s and 1950s. Binford enjoyed much greater success in the 1960s and 1970s by insisting with Taylor that archae-

ology must be anthropology, while highlighting a non-Tylorean, nontraditional concept of culture, that of Leslie White. Hodder has gone back to something like the traditional culture concept but modified it to place artifacts, architecture, and archaeology in the center of anthropology and social theory, while explicitly rejecting Phillips's conclusion that "archaeology is anthropology or it is nothing." "Archaeology is archaeology," he and the postprocessualists insist, even as portions of their program are being incorporated into both academic and cultural resource management Americanist anthropological archaeology, partly to reinforce certain minority themes present there before the postprocessualist movement, and partly to further syntheses between processual and postprocessual goals in archaeology.¹³

The revisionists in sociocultural anthropology and ethnology eventually found that the traditional culture concept was not very useful to them, so they modified it to suit their purposes. Many of them, past and present, are quite explicit about this, and many of them were quite successful at initiating productive research lines based upon their new formulations.¹⁴ In sociocultural anthropology over the past 40 to 50 years, there has accordingly been a proliferation in approaches to culture from the earlier essentialist concept to cultures as configurations of a psychological sort, as a series of distinctive cognitive maps, as symbolic and/or adaptive systems, as infinitely varying surface phenomena that may reveal deep truths about universal human thought processes, as social knowledge networks, and as trait complexes defined and studied within neo-Darwinian frameworks.

Does this mean that the center of anthropology—belief by all anthropologists in some widely sanctioned variant of a unified culture concept—has been destroyed? If so, does the lack of unanimity about culture—what it is and where it is and whether it matters—mean that anthropology itself as a holistic discipline is, or is about to be, no more?

More than 20 years ago, that was Rodney Needham's prediction for the very near future about academic anthropology (Needham 1970). He thought that pieces of anthropology would be redistributed among neighboring disciplines. That was Wolf's conclusion 14 years ago (Wolf 1980), the theme picked up by Flannery in his 1981 American Anthropological Association Distinguished Lecture; and apparently James Clifford (1986:4) was of the same opinion eight years ago when he remarked that "Man" as *telos* for a whole discipline" has disintegrated. Clifford Geertz, in his *Current Anthropology* interview with Richard Handler (Handler 1991) says 50 to 75 years from now academic anthropology departments will no longer exist because anthropology will have evolved into several different disciplines.

Perhaps these conclusions are correct; perhaps general, integrated anthropology is already or soon will be

gone. Although I care deeply about this issue, owing to my 1950s imprinting in holistic anthropology, I cannot get too worked up over the disintegration prediction. Anthropologists have been worrying about this for at least 40 years and recently went through another bout of explicit fretting in the pages of the *Anthropology Newsletter* (see Givens and Skomal 1992). Those who contributed to the discussion are pro-integration and pro-four fields. Givens and Skomal (1993) conclude that a four-field holistic anthropology is, at the present time, both myth and reality.

Another reason that I manage to remain calm in the face of savage attacks on the old-time culture concept, attacks supposed by some to mark or presage the disintegration of anthropology, is that the sociocultural subdiscipline, and, ultimately, all of anthropology benefits from the culture conceptual shifts briefly referred to above. In sociocultural anthropology, as in archaeology, each new research trajectory counterposed to some aspect of the traditional culture concept results in new data, new insights, and new knowledge. Moreover, the old-time culture concept still plays an integrating role as a central reference point even for the radically revisionist anthropologists, for whom it is variously a *bête noire*, a punching bag, or a springboard to alternative perspectives on the human condition, past and present.

Finally, the 1950s characterization of anthropology is true enough and strong enough to bear the weight of most contemporary, intradisciplinary construction and reconstruction. Anthropology is still the only human science all about humankind, from four million years ago to the present: Who are we? Where did we come from? What happened to us between origin and now? What is the scope in all its compelling detail of past and contemporary human physical and cultural variation, and what does that variation mean in biological, social, and cultural terms?

No other discipline has ever asked these questions about the entire spatial and chronological sweep of the human past and present, as well as about the particulars concerning specific portions of that sweep. And certainly no other scholarly band ever set out to actually obtain answers to such questions. In spite of episodic skeptical crises within anthropology, and a chronic agoraphobia about where our center is and where our boundaries are, anthropology is still here—even Geertz gives it another half-century: an undisciplined discipline, an unruly semi-aggregate, but one with research methods and research results of enormous global importance and great intrinsic interest.

Notes

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1. Dunnell 1980; Gumerman and Phillips 1978; Meltzer 1979; Wiseman 1980.
2. Chippindale 1989:69; Clarke 1968:13; Hodder 1991a: ch. 9; Shanks and Tilley 1988:213, item 6.5; Shennan 1986; Tilley 1989: especially 110.
3. Barth 1994; Bernard 1994; Bloch 1994; Goodenough 1994; Harris 1994; Keesing 1994; Kottak and Colson 1994; Marcus 1994; Sahlin 1994; Salzman 1994; Scheper-Hughes 1994; Strauss and Quinn 1994; Tambiah 1994; Tishkov 1994; Wolf 1994; see also A. Watson 1992-94; J. Watson 1994; R. A. Watson 1964; R. R. Watson 1978; R. S. Watson 1994.
4. Bennett 1943; Binford 1962; Kluckhohn 1940; Taylor 1948.
5. See also Kroeber 1948:295-296; Kroeber and Waterman 1931:11.
6. Caldwell 1959; P. Watson, LeBlanc, and Redman 1971, 1984; R. A. Watson 1972.
7. Gould 1978; Kleindienst and P. Watson 1956; Kramer 1979, 1994; Longacre 1974; P. Watson 1979; Yellen 1977.
8. Binford 1987, 1988; Binford and Stone 1988; Hodder 1988, 1989, 1991a; P. Watson 1991.
9. Binford 1976, 1978a, 1978b, 1980, 1981, 1982. Quotation comes from Binford 1981:29; see also parts 3 and 4 in Binford 1989.
10. Hodder 1982a:210-211; see also pp. 155-170 for his detailed discussion of bone refuse disposal and burial customs among the Nuba.
11. Redman 1991; Trigger 1991; Brumfiel 1992; Cowgill 1993.
12. Kramer 1994 provides references and commentaries on the San work; see Longacre 1991 for overviews of the Kalina research, and of other work in ceramic ethnoarchaeology, some of which long predates the era of New Archaeology; Herbich and Dietler's project is summarized in Herbich 1987 and Herbich and Dietler 1991.
13. See, for example, Fritz 1978; Hall 1976, 1977; Kehoe and Kehoe 1974; Marshack 1972.
14. For earlier and more recent examples, see Auger 1992, in press; Benedict 1934, especially ch. 3; Geertz 1973:4-5; Fox 1989; Harris 1964; Kroeber 1952: part 1; Lévi-Strauss 1955, 1962; Tyler 1969.

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