



Sally Jo Watson



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Archaeology and Anthropology: A Personal Overview of the Past Half-Century

Patty Jo Watson

Missoula, Montana 59803; email: pjwatson@artsci.wustl.edu

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Abstract

Having begun graduate work in anthropology and prehistoric archaeology at a time (early 1950s) and place (University of Chicago) where the two were closely linked, I subsequently participated in work devoted to early agricultural economies in Western Asia and Eastern North America; to the relations among archaeology, history, and science; and to the place of anthropological archaeology in the contemporary world. In this article I discuss my personal experiences within each of these areas of endeavor.

AAA: American Anthropological Association

NSF: National Science Foundation

INTRODUCTION

When I entered the University of Chicago's pre-MA program in the fall of 1952, I encountered a small community of faculty and students pursuing a bewildering array of research trajectories: Totonac phonetics and phonemics, agricultural origins in ancient Mesopotamia, functional anatomy of nonhuman primates, contemporary Mayan kinship systems and village life, domestic economies among the Sauk and Fox Indian tribes. This was very exciting but also confusing for someone fresh from the cornfields of northern Iowa who just wanted to learn how to be an archaeologist. It helped a great deal, however, that these scholars were all self-declared anthropologists, members of a single discipline dedicated to the study of humankind throughout time and space. Before the end of fall quarter, I thought of myself as an anthropologist committed to investigating the cultural histories of prehistoric human groups: an anthropologist first and an archaeologist second. I joined the American Anthropological Association (AAA) in the spring of my first year as an anthropology grad student and have been on its rolls ever since. Thoroughly imprinted in the unity of the discipline, I completed a PhD in 1959 and began my career in the 1960s as an anthropological archaeologist specializing in Near Eastern prehistory, especially the origins and early development of agropastoral economies.

ORIGINS OF AGRICULTURE IN SOUTHWEST ASIA, THEN AND NOW

Then (1950s)

The center of my grad school orbit was Robert J. Braidwood's office on the third floor of the Oriental Institute (known to habitués as the Orinst, its cable address, or the OI).

I enrolled in all Braidwood's seminars—held in his OI office—but also took courses in social anthropology (from Eggan and Redfield, plus a class on Aboriginal Australia offered by Lloyd Warner, which featured the Murngin controversy, and one by Simon Ottenberg on African

ethnology), physical anthropology (Washburn), and linguistics (McQuown).

Braidwood was well known as the excavator of Jarmo in the foothills of northern Iraq, then the oldest known food-producing community in the world. The 1950–1951 Jarmo field season (Braidwood 1953; Braidwood & Braidwood 1950, 1953) had been a big success, and plans were well underway for the 1954–1955 season by the time I became a Braidwood advisee. Braidwood's views on how, where, and when the first West Asian plants and animals were domesticated (the Hilly Flanks theory) centered on regional floral, faunal, and climatic data. These data indicated to him that the earliest evidence for farming and herding should be found in geographic locales where wild ancestral populations were native, i.e., in the rain-watered uplands above the Fertile Crescent, stretching from the Levantine coast across northern Mesopotamia to the Tigris and Euphrates.

For the 1954–1955 Jarmo season, Braidwood wanted to take with him to Iraq experts who could identify and interpret physical remains of Jarmo animals and plants, representing as they did the oldest known domestic goats, sheep, pigs, wheat, and barley. He included a zoologist (Charles Reed), an archaeobotanist (Hans Helbaek), and a radiocarbon expert (Fred Matson) in his National Science Foundation (NSF) proposal, in addition to the geologist (Herbert Wright) who had begun work at Jarmo in 1950–1951. This proposal was the first ever received by NSF for archaeological fieldwork in the Near East. It was funded. The natural scientists and other staff members were signed on, and I was included as the most junior member.

Just prior to and during the 1954–1955 Iraq-Jarmo season, Kathleen Kenyon's excavations in basal levels at the big mound of Tell es-Sultan (ancient Jericho) in Jordan produced results that directly challenged Braidwood's Hilly Flanks theory. The long stratigraphic sequence her team documented revealed evidence of a prehistoric settlement at the oasis of Jericho, far from the Hilly Flanks—a settlement not only bigger and fancier than Jarmo, but also several

millennia older (Braidwood 1957; Davis 2008, pp. 11, 118–120, 128–150; Kenyon 1957).

During ensuing decades, many other archaeologists took up the interdisciplinary agricultural/pastoral origins focus pioneered by the Braidwoods at Jarmo. Although Braidwood is still widely recognized as the first Near Easternist to take natural scientist colleagues to the field, he himself always insisted that Raphael Pumpelly (1908) initiated interdisciplinary archaeological fieldwork in Asia.

The state of knowledge now is, of course, far more complex than it was 50 years ago. Besides many more sites and much more data, methods and techniques now provide new categories of information, such as ancient sheep/goat herd structures, DNA analyses of wild-to-domestic animal and plant lineages, and much more precise radiocarbon dating than was possible in the mid-twentieth century.

Now (Early 2000s)

Subsequent to the Jarmo-Jericho debate, Braidwood's interdisciplinary team carried out one season of fieldwork in the Iranian Zagros (the return to Jarmo planned for 1958–1959 having been cancelled owing to the nationalist revolution in Iraq) but then moved to Ergani, Diyarbakır Vilayet, in south-eastern Turkey. The Joint Istanbul-Chicago Prehistoric Project, directed by Halet Çambel and Bob Braidwood, continued for the next 30 years, its major focus being Çayönü, a site significantly larger than Jarmo, more complex architecturally, and at least a millennium older (Çambel & Braidwood 1980).

Another big, fancy, prehistoric Turkish site—Çatalhöyük—had been preliminarily excavated during the 1970s by James Mellaart (1967). A second generation of work at Çatalhöyük was begun in 1993 by Ian Hodder (2007). Çatalhöyük is somewhat younger than basal Çayönü, representing what Braidwood called “established village-farming,” although the site is quite different from the early established village-farming communities known when he coined that phrase. In fact, the whole

latter-day agropastoral origins archaeological enterprise is so large and active that topical, regional, and general syntheses appear frequently (e.g., Cappers & Bottema 2002, Colledge & Conolly 2007, Denham et al. 2007, Naderi et al. 2008, Simmons 2007, Zeder 2008).

The general picture is that most of the animal and plant species identified as key players by Braidwood's Jarmo colleagues are now known to have been domesticated during the early Holocene at various times and places within the Hilly Flanks and the southern Levant: sheep and goats by ~11,000 years ago, pigs by ~10,500 years ago, cattle by ~10,000 years ago; wheat, barley, peas, and lentils by ~10,500 years ago. Accelerator mass spectrometer (AMS) radiocarbon techniques provide precise dating of the floral and faunal remains themselves, whereas new analytical procedures enable archaeobotanists and archaeozoologists to detect subtle patterns in bone and plant remains indicating “predomestication” managing many centuries prior to the emergence of morphological traits clearly attesting to domesticated status (Hillman & Davies 1992, Hillman 2000, Weiss et al. 2006, Zeder 2008). The entire Near Eastern complex of domestic plants and animals was present by 9500 years ago in the earliest established village-farming communities. Furthermore, many of those communities were very large and sustained complex ritual activities.

AGRICULTURAL ORIGINS IN THE EASTERN WOODLANDS OF NORTH AMERICA

While Braidwood's teams, as well as those of his colleagues, students, and successors, were pursuing the origins of agropastoral economies in western Asia, evidence for a very different food-producing system was emerging in the Eastern Woodlands of North America (Fritz 1990, 2007; Gremillion 2002; Smith 2002, 2006; Watson 1997; Yarnell 1969, 1974, 1994).

Having, in the summer of 1955, married into a speleological group (the Cave Research Foundation) carrying out research within and

adjacent to Mammoth Cave National Park, Kentucky, I was drawn toward dark-zone cave archaeology in the early 1960s (Benington et al. 1962; Watson 1969, 1974, 1999b; Watson & Yarnell 1966). The cave—Salts Cave, Mammoth Cave National Park—where I began research contained many miles of dry passages well known to prehistoric inhabitants of the region. There is a significant amount of historic traffic and disturbance in the upper levels of Salts Cave, but there are several lower-level passages undisturbed since the end of the aboriginal caving period here 2000 years ago. Everything left in such passages is preserved in situ, including ample evidence of prehistoric diet, primarily in the form of human fecal deposits. Desiccated human paleofeces—readily dated by AMS radiocarbon techniques—are marvelous data repositories containing information about the environment (inferred from floral and faunal contents, including pollen) as well as subsistence, sex, and DNA data for the defecators and DNA data for the fragments of plant and animal tissue in each specimen (Poinar et al. 2001; Sobolik et al. 1996; Watson & Yarnell 1986; Yarnell 1969, 1974).

Introduction of wet-screening and flotation techniques to Eastern Woodlands archaeology in the 1970s and 1980s enabled retrieval of charred plant fragments and various categories of microfaunal remains (e.g., fish scales, tiny rodent bones and teeth) missed by standard dry-screening (Chapman & Watson 1993). Evidence accumulated rapidly for a previously unsuspected form of indigenous agriculture east of the Mississippi that began about 4000 years ago and was well developed by the time prehistoric cavers roamed the passages of Salts and Mammoth Caves (Smith & Yarnell 2009).

For most of the twentieth century, scholars of early agriculture in North America focused heavily on Mexico (especially the origins of maize) and the U.S. Southwest. Reference was frequently made to the Three Sisters—maize, squash, and beans—grown by many American Indian groups at the time of European contact. These crops were all believed to have been domesticated originally in

pre-Hispanic Mexico, diffused north as a package to the U.S. Southwest, and eventually across the Plains to the Eastern Woodlands. The retrieval revolution in Americanist archaeology and development of the requisite specialties, as had been the case in the Near East during and after the Braidwoodian era, replaced older understandings with much more detailed knowledge. The Eastern Agricultural Complex was one result and is now known to be among the very few, independently created agricultural systems in the entire human past. The exact number of autochthonous developments of food-producing economies varies somewhat from expert to expert but is fewer than ten. The usual lineup includes Western Asia, Eastern Asia, Oceania (New Guinea), Eastern North America, Mexico, tropical South America, and sub-Saharan Africa.

Emergence of agricultural economies in the Americas—North, Central, and South—is currently a hot topic characterized by considerable difference of opinion on many details (Browman et al. 2009), but it has been clear for some time that the earlier notion of a single Mexican package (the Three Sisters) diffused north and south is wrong. Each region north of Mexico, for example, has its own plant-use story. Some locales include an early-agriculture narrative, whereas others are characterized by only occasional or seasonal use of one or more domesticated (or cultivated or propagated) species to supplement wild fruits, nuts, and other forest, prairie, or desert foods.

Maize did come into the U.S. Southwest from Mexico and was present by 4000 years ago. Squash appears by 3500 years ago, but Mexican beans did not appear until 2000 years ago. The premaize Eastern Agricultural Complex, present by 3000 years ago in parts of the Midwest and Midsouth, included sunflower (genus *Helianthus*), sumpweed or marshelder (*Iva*, a relative of sunflower that also produces oily seeds), *Chenopodium* (goosefoot, lambsquarter), maygrass (*Phalaris*), a thick-walled squash (*Cucurbita pepo ovifera*, pepo gourd), and bottle gourd (*Lagenaria*). Knotweed (*Polygonum*) and tobacco (*Nicotiana*) show up

later, as does maize [*Zea*, which appeared in the U.S. Midwest ~2000 years ago, but at only one site that early (Riley et al. 1994)]. Maize became widespread ~1500–1200 years ago and later, with considerable regional variation and many places where it was not grown at all prior to European contact. Finally, ~800 years ago, Mexican beans are present at a few sites.

While this agricultural origins excitement was going on at ground level in Eastern North America during the 1970s, 1980s, and 1990s, stirring events were taking place in the rarified atmosphere surrounding major methodological and theoretical issues relevant to all of Americanist anthropological archaeology.

A much-discussed topic during the 1970s and 1980s, ethnographic fieldwork in the interests of archaeological interpretation, is one that Braidwood periodically brought up in his 1950s seminars at the Oriental Institute.

FROM ARCHAEOLOGICAL ETHNOGRAPHY TO ETHNOARCHAEOLOGY: BACK TO JARMO

Staff members for the first Jarmo field season of 1950–1951 included a recent Chicago graduate student in Anthropology, Frederik Barth [MA 1949]. Braidwood asked Barth to remain for several weeks after the Jarmo dig closed, so he could document archaeologically relevant architectural and economic details in nearby Kurdish villages. Barth (1953) produced a fine social anthropological monograph, but it contained no data on construction and maintenance of puddled-adobe houses, details of wheat and barley farming, specifics about sheep and goat herds, etc. (see Barth 2007, p. 2, for his account of this early stage in his anthropological career). Hence, when I became one of his advisees, Braidwood was still lacking farming, herding, and architectural details from contemporary Iraqi Kurdistan. After the 1954–1955 Iraq-Jarmo season, he suggested that my dissertation project could be the archaeological ethnographic one yet to be undertaken in the Jarmo locale.

I never did any archaeological ethnography in living villages around Jarmo, however, because the 1958–1959 Iraq season was cancelled subsequent to the 1958 revolution. Braidwood did succeed in fielding his interdisciplinary team in western Iran during a nine-month period in 1959–1960, so it was in Iranian villages that I recorded information meant to aid better interpretations for ancient villages such as Jarmo (Watson 1979).

The empirical aspects of archaeologically oriented ethnography seemed quite straightforward to me at the time I was doing it. In the minds of 1970s archaeological theorists, however, the relations between archaeology and ethnography were highly problematic (even without considering the negative opinions of some ethnologists about ethnographic skills of those archaeologists invading their scholarly turf). The standard reference on epistemological aspects of ethnographic analogy is “The Reaction Against Analogy” by philosopher of science Alison Wylie (1985; see also Wylie 1982, David & Kramer 2001). My own modest efforts to sort through the most troubling problems from the perspective of a practicing archaeologist are detailed in a 1999 essay, “Ethnographic Analogy and Ethnoarchaeology” (also Watson et al. 1971, pp. 49–51, and Watson 2008, pp. 30–31), the essential point being that ethnographic analogies—whether simple or complex—are trial formulations (hypotheses, models) subject to testing like all such propositions.

Ethnoarchaeology was just one of the theoretical issues central to Americanist anthropological archaeology during the later 1960s through the 1970s and 1980s, a period when New Archaeology—also known as processual archaeology—became dominant. Archaeological practitioners within the United States during the mid-1970s were also struggling with the transformation of their discipline in response to passage by Congress of the Archeological and Historic Preservation Act, giving rise to Cultural Resource Management (CRM). This Act formally added archaeological (prehistoric and historic) sites and materials (cultural heritage properties) to environmental protection

legislation, mandating (and recommending a certain amount of federal funding for) archaeological survey with subsequent mitigation (documentation, excavation) of resources scheduled to be disturbed or destroyed by federally funded projects (e.g., roads, dams).

The most pressing problems in the immediate aftermath of the Preservation Act were pragmatic, the first being clear description of requisite expertise for archaeologists certified to carry out work in advance of federally funded projects. Regulations sufficiently general to be applicable throughout the entire country and across the entire prehistoric and historic span covered by the archaeological record in the United States were also necessary. Moreover, Americanist archaeology, a small group of avocational or academic scholars, now had to include a rapidly growing subset of practitioners employed by public agencies (e.g., the National Park Service) or private construction firms, or those who were in business for themselves as contract archaeologists.

Because more than 95% of all archaeological fieldwork in the United States is now CRM archaeology, major impacts have been made on teaching, training, and public outreach (King 2008). Nevertheless, these effects are often rather distant from theoretical debates carried on by the small minority of non-CRM (ivory-tower) archaeologists, to whom I now return.

PROCESSUAL, POSTPROCESSUAL, AND POST-POSTPROCESSUAL ARCHAEOLOGY

New Archaeology/Processual Archaeology (1960s–1970s)

I use the terms New Archaeology and processual archaeology interchangeably, although some scholars make a distinction (Johnson 2008). In any case, Lewis Binford was the leader of this movement that shifted Americanist archaeology from

highly particularistic, historicist foci toward generalizing, explicitly social scientific, anthropological ones.

Archaeology must accept a greater responsibility in the furtherance of the aims of anthropology. Until the tremendous quantities of data which the archaeologist controls are used in the solution of problems dealing with cultural evolution or systemic change, we are not only failing to contribute to the furtherance of the aims of anthropology but retarding the accomplishment of these aims. We as archaeologists have available a wide range of variability and a large sample of cultural systems. Ethnographers are restricted to the small and formally limited extant cultural systems.

... As archaeologists, with the entire span of culture history as our "laboratory," we cannot afford to keep our theoretical heads buried in the sand. We must shoulder our full share of responsibility within anthropology.... (Binford 1962, p. 224)

Binford's call to revise and broaden anthropological archaeology was not the first (Bennett 1943, Kluckhohn 1940, Steward & Setzler 1938, Taylor 1948), but it was certainly the most successful. Although Binford was on the Anthropology faculty at Chicago when he published his 1962 paper, I had left some years prior to his arrival and—not being a New World archaeologist at that time—did not know much about his radical ideas until 1968. During the fall of that year, my husband Richard (Red) Watson and I and our five-year-old daughter Anna, resided in Ergani, Turkey, together with other staff members of Halet Çambel and Robert Braidwood's Turkish Prehistoric Project (Watson 1999c). Red was geological field assistant cooperating with the Turkish Geological Survey to find and document obsidian sources and to sample each one. I was field supervisor for the excavation of a Halafian site (Girikihacıyan, GK for short; Watson & LeBlanc 1990) several millennia younger and a few miles away from Çayönü, the primary focus of the Project.

Two University of Chicago graduate students were also members of the Project staff: Geoffrey Clark and Charles Redman. Geoff and Chuck were steeped in 1960s Binfordian New Archaeology and eager to apply it at Girikihacıyan and/or Çayönü. I was an easier mark than Bob or Halet, let alone the codirectors in combination, so they set about giving me an intense tutorial. Their efforts were aided significantly by the fact that Turkish Antiquities law prohibits one expedition from excavating two sites simultaneously. Hence, the 1968 GK sondage had to await completion of work at Çayönü. There was no rule against nondigging, surface work, however, so we got permission from Halet and Bob to carry out systematic surface collection at GK. The results were very rewarding, and I became an enthusiastic convert to New Archaeology. Halet and Bob were also favorably impressed to the extent that they allowed us to surface-collect Çayönü, too (Redman & Watson 1970).

By the next Turkish season in the fall of 1970, Steven LeBlanc had entered the predoctoral graduate program in Anthropology at Washington University and was in search of a dissertation project. He joined the GK field staff in place of Chuck, who was Çayönü dig supervisor in 1970. Between those two seasons, the three of us had collaborated on a book manuscript. This was LeBlanc's idea. He thought that a substantive but brief exposition of New Archaeology for a general archaeological audience, students and professionals, was badly needed. Chuck secured the interest of an editor at Columbia University Press, and the book came out soon after the 1970 Turkish season (Watson et al. 1971). It was sufficiently controversial to be a big seller, becoming what one critic (pejoratively) called a *locus classicus* of 1970s New Archaeology.

Just about the time it seemed the battle was over and processualist New Archaeology had won, English archaeologist Ian Hodder published an essay describing a distinctly contrasting approach (Hodder 1982). He followed that account of his radical views with numerous others, as well as many talks at meetings in Europe

and the United States. In 1985, he published a synthesis of European perspectives entitled "Postprocessual Archaeology" (Hodder 1985).

Postprocessual Archaeology

Although Binford himself asserted that entire past cultural systems could be and should be inferred from the archaeological record (Binford 1962), in practice Binfordian processualist archaeology of the 1960s and 1970s was focused on paleoenvironment, paleoecology, and subsistence economies of ancient societies. Relations between ancient communities and their physical environments were highlighted; explicitly scientific research designs were stressed and insisted on by peer review panels of the NSF and other funding agencies. The major goal of Americanist anthropological archaeology was to formulate and test generalizations about human sociocultural behavior (Watson et al. 1971), but the generalizations sought were nearly always in the realm of what Robert Hall (1977) succinctly characterized as "econothink." Hall and a few other anthropological archaeologists (e.g., Kehoe & Kehoe 1974) advocated more broadly based interpretive frameworks than those of the New Archaeologists, but they were a very small minority.

A much bigger theoretical/methodological fuss was stirred up by Binford himself and by Michael Schiffer during the 1970s and early 1980s (Binford 1976, 1980, 1981; Schiffer 1972, 1987). The crux of this debate was the nature of the archaeological record. Archaeologists were ignoring the complexities inherent to prehistoric human/environmental relations. They also gave insufficient attention to cultural and noncultural agents, events, and processes that rearrange, remove, or obliterate original cultural deposits and their original sedimentary contexts. Binford's and Schiffer's publications impelled an intense focus on site formation and deformation (e.g., Goldberg et al. 1993; Goldberg & Sherwood 2006; Stein 1983, 2005a,b; Stein & Farrand 1985).

All this site formation debate and work, however, was very much within the prevailing

processualist mode. What concerned Ian Hodder and other British and European archaeologists of the 1980s and later was an altogether different set of theoretical matters, as indicated by Hodder's coining of the term postprocessual archaeology. The phrase covers a multitude of perspectives, some (various Marxian emphases, for instance) fairly congruent with processualist archaeology, others antagonistic (e.g., extreme structuralist-symbolic or critical-theory approaches).

My own initial close encounters with postprocessual archaeology—in 1982 and 1989—made significant impacts on my subsequent thoughts and actions.

LeBlanc, Redman, and I had carried out two explicitly New Archaeology-oriented field seasons in New Mexico during the summers of 1972 and 1973 (Watson et al. 1980), after which the two of them became career Southwesternists while I shifted to cave and shell mound archaeology in the Eastern Woodlands of North America (Watson 1999b).

As already noted, cave archaeology in and around Mammoth Cave National Park fortuitously enabled me to continue research on an agricultural origins story very different from that being pursued by Braidwood, his codirectors, and collaborators in Iraq, Iran, and Turkey. By 1974, I was thoroughly immersed in fieldwork at Salts Cave, Mammoth Cave, and other underground locales, as well as Bill Marquardt's and my investigations of shell mounds along Green River downstream from Mammoth Cave National Park (Marquardt & Watson 2005; Watson 1969, 1974). My job as a new faculty member in the Anthropology Department (recently split off from Sociology) at Washington University in St. Louis was also very demanding. After the main New Archaeology period of the 1960s and ensuing debates about ethnoarchaeology and site formation, I slid away from abstract theoretical issues and scholarly disputes to pressing matters of departmental and campus politics vital to our small Anthropology unit on the one hand and exhilarating field and lab research in my new persona as an Americanist archaeologist on the other.

So I was a little slow to focus on what was happening in European archaeological theory during the 1980s, and even on developments within 1970s–1980s sociocultural anthropology. I received a valuable introduction to some crucial postprocessualist issues, however, during a 1982 visit to Birzeit University in the West Bank/Israeli Occupied Territories. The trip was arranged by Albert E. Glock, an archaeologist who had recently accepted a position at Birzeit. The central administration there wanted to establish a curriculum in regional archaeology, which they hired Glock to create. Having found several promising West Bank students, he was planning to help them secure graduate degrees from archaeological programs in Europe and the United States so they could return to staff the Birzeit department. Glock invited me to come to Jerusalem, stay with him and his wife Lois, meet the students, and discuss current developments in archaeology with them, especially ethnoarchaeology.

I spent several days with Glock and his students, being toured around archaeological sites on the West Bank, talking with them about work they had already done and work planned for the future. Glock was very explicit about his goal. He was preparing this select group of bright, well-educated indigenous young scholars to carry out their own archaeological research in their own country and to guide future generations of Birzeit students in learning about their own cultural history.

My stay in the West Bank made a profound impression. There are few places in the world where every aspect of archaeological research is so politicized and where practitioners must remain so continuously aware of the political intricacies integral to everything they say and do.

Further lessons were forthcoming in other categories addressed by postprocessualists when I participated in a 1989 conference at Southern Illinois University, Carbondale. The conference organizer was Robert Preucel, then Scholar in Residence at the Center for Archaeological Investigations. This appointment meant he could work on his own research all year, except that he was responsible for organizing

and chairing a conference on the theme he proposed when applying for this position. Preucel's theme was Processual versus Post-Processual Archaeology. Proponents on both sides of the Atlantic and of the theoretical/methodological divide were invited, and Preucel asked me to provide general commentary at the close of the conference.

The group assembled for the get-acquainted reception at A Touch of Nature, the woodland retreat that lodged the conference. While talking with a Norwegian archaeologist, Bjørnar Olsen, I received my first clue concerning significant differences between European perspectives and my own. Olsen remarked that he saw no need for consensus or accommodation among proponents of different views on archaeological theory. Why should archaeologists not continually dispute their positions as philosophers do?

As the conference ran its course, I became increasingly anxious about my putative contribution. The presentations (by advocates much more skilled at debating than I) were diverse, sometimes contradictory, and drew on literature of which I was fairly or completely ignorant. The names of the favorite social theorists—even those I'd heard of and knew a little about—were menacing: Adorno, Bourdieu, Giddens, Foucault, Heidigger, Horkheimer, Ladurie, Marcuse, Merleau-Ponty, Ricoeur. I stayed up most of the night before my presentation to compose it.

In spite of my qualms, it turned out all right (interested readers can judge for themselves by consulting Preucel 1991). I used Olsen's comment to make a point about the funding of archaeological research: How can we expect NSF or the National Endowment for the Humanities (NEH) to provide money to a scholarly group who cannot agree about the empirical nature (if any) of the archaeological record, and what the most important goals (if any) should be for archaeology in general? Moreover, given the nature of field archaeology in the United States a decade and a half into the CRM era, how could the large group of CRM archaeologists maintain their professional activities

(successfully bidding on contracts in response to Requests-for-Proposals, carrying out Phase I and/or II, and/or III work, preparing the final report under budget and before deadline) while also keeping up with fast-moving scholarly debates that draw primarily on the vast literature of European social theory?

My experiences at the Touch of Nature conference and on the West Bank were valuable in many ways, central among them being a much clearer understanding of the basic critiques than I would have gained from simply studying post-processualist publications.

So what do, or did, the 1980s postprocessualists want? Some of their accusations and demands for change were justified. For example, processualist archaeologists neglected ideational issues and individual agency in past societies they were studying and virtually completely ignored the sociopolitical context of contemporary archaeology. Hence, processualists were vulnerable to many charges leveled against them by postprocessualists voicing archaeologically relevant portions of the post-modernist program that had surfaced a decade or so earlier in sociocultural anthropology. Perhaps most importantly, postprocessualists denied that direct, unproblematic, unbiased access to "the real past" was possible.

Preucel's edited volume was, of course, not the only product of the processual-postprocessual conflict. Argumentation included direct confrontations in person (Stone 1989) and in print between Binford and Hodder (Binford 1987, Binford & Stone 1988, Hodder 1988). Many books and journal articles addressing the general debate and issues raised by it appeared and continue to do so (e.g., Gero & Conkey 1991, Hall 1997, Hodder & Hutson 2003, Nelson 2007, World Archaeol. Congr. 2005, Wylie 2002).

There is also by now something along the lines of postpostprocessualist archaeology represented by archaeological theorists who have taken up themes from, for example, post-structuralism, embodiment, and neosemiotics (Bapty & Yates 1990, Hamilakis et al. 2002, Preucel 2006).

Meanwhile, CRM continues to be a major focus within the U.S. archaeological community. In 1990, strong impetus in a direction congruent with the postprocessual emphasis on indigenous and descendant populations was provided by passage of the Native American Graves Protection and Repatriation Act (NAGPRA). NAGPRA requires all U.S. institutions to inventory Native American human remains, grave goods, and other relevant materials in their possession and to send copies of these inventories to all 600+ federally recognized Indian tribes. Those tribes can then, via their legally appointed representatives (e.g., Tribal Historic Preservation Officers), request repatriation of such remains and items they establish as part of their cultural patrimony.

There are many examples of negotiations among American Indian tribes, museums, and other curational facilities and between tribal groups and archaeological research teams. Hundreds of human skeletal remains excavated between 1915 and 1925 during A.V. Kidder's Pecos Project (New Mexico) have been repatriated by Harvard University's Peabody Museum and reburied. A single skull labeled "Pawnee," obtained from a nineteenth-century battlefield and eventually donated to the anatomy department of a St. Louis hospital, was formally conveyed to a designated Pawnee representative. In some cases, agreements are drawn up whereby human remains and/or grave goods, ancient or historic, continue to be curated, by the institutions holding them, in a manner deemed appropriate by descendant groups.

Some years ago I was contacted by speleologists working in the Southern Rocky Mountains who had come upon human bone in the dark zone of a cave they were investigating. The skeletal remains turned out to be several thousand years old. Because the cave is on Forest Service land, the Forest Service archaeologist informed the appropriate Native American group so that their cultural heritage representative could join us to assist and oversee the proposed research. We determined that the ancient man had died in the cave, which meant that it was a burial site, hence a sacred site, so the

Forest Service closed it to the public. The skeletal remains were examined and thoroughly documented then formally conveyed to a Forest Service repatriation specialist, who presented them to the cultural heritage representative.

As a scientist, I found this a wrenching experience because the most fundamental axiom of science is public accessibility to the primary evidence. We had done our best to document the ancient physical remains, but nothing is a satisfactory substitute for the bones themselves. Nevertheless, science and scientists function in the real world of the present and are subject to regulations and laws of the nations and administrative boundaries wherein they work. Long before CRM and NAGPRA, archaeology in the United States, as everywhere else, was constrained by many factors: shortage of time and money, inimical weather, hostile flora and/or fauna, sickness and accidents, uncooperative landowners or provincial officials, friction among the staff, and many other major and minor logistical problems, including destruction of some or all the primary evidence [e.g., by fire, floods, looting, warfare (Löw 2003, Emberling & Hanson 2008)].

As already noted, repatriation and reburial or other sequestration are not always the final result of a NAGPRA situation, but even if they are, those results can be ameliorated by benefits to both sides from working closely with each other. In fact, a very salubrious effect of postprocessual critiques together with legislation requiring attention to the rights of indigenous groups is that archaeologists everywhere are now much more aware of their responsibilities to local communities. It has become routine to confer and consult with the local people where one is planning to work. A good example is reported by the Center for Desert Archaeology (Duff et al. 2008; see also Colwell-Chanthaphonh & Ferguson 2007; Killion 2008; Little & Shackel 2007; SAA 2008a,b).

WHAT NEXT?

Anthropological archaeology in the Americas is an enterprise vastly different from what it

was 30 years ago, let alone 50 years ago when I was completing graduate work. Both the AAA and the SAA are much larger than at any earlier time in their histories: ~11,000 members for the AAA and 7650 for the SAA. When the *Guide to Departments of Anthropology* was first published in 1962, only 44 PhD-granting departments containing 522 anthropologists were listed. In that same year, the SAA had 1706 members, including avocational as well as academic and museum personnel.

The AAA was reorganized in the 1980s, taking on its current configuration of multiple units called associations, councils, divisions, sections, or societies. According to the 2008–2009 AAA *Guide*, there are 37 of these (one of the founding units being the Archeology Division), plus six interest groups. The 13-member Executive Board includes representatives for five major subfields plus Student and Minority seats. The AAA governance group, like that of the SAA, has been for many years thoroughly engaged in contemporary national concerns including public outreach.

The SAA was also reorganized during the 1980s, having previously carried out its business affairs via the AAA headquarters office. Under the guidance of the first SAA Executive Director, Jerome Miller (hired in 1983), the mid to late 1980s were exciting years (Fowler et al. 1997). Under Miller's guidance, membership increased, *American Antiquity* was printed and distributed by an independent publishing company, and the SAA established its own Washington, DC, office and hired its own lobbyist on The Hill (after learning that a nonprofit organization can legally devote up to 20% of its income to direct lobbying of Congressional personnel).

Size increase for the restructured SAA was accompanied by increases in diversity, both in ethnicity and in gender. There is still work to be done (Zeder 1997), but SAA membership includes American Indian, African American, and Latin American archaeologists as well as many more women than it had previously. The percentage of members based in public or private sectors (i.e., federal, state, county, or municipal

agencies vis-à-vis free-lance CRM businesses, or large contracting firms that carry out an array of environmental consulting work) is roughly equal to that of academic and museum archaeologists, and CRM training is the norm for student archaeologists. There is also considerable concern with applied archaeology directed to retrieving past technology and knowledge for use in the present (e.g., Erickson 1998) and with other archaeological contributions to solving contemporary problems (Sabloff 2008).

The highly parochial character of early-to mid-twentieth-century ivory-tower archaeology is much diminished. Strong scholarly traditions with striking contrasts from one country to another, or interregionally within a single country, have given way to an international archaeological community (sustained by global email systems) wherein major and minor data-based questions as well as theoretical, methodological, and political ones are discussed and debated. The World Archaeological Congress is the largest of these networks, but there are also many smaller e-lists (e.g., cave archaeology) that are—thanks to the Internet—equally global in coverage. Globally accessible Web sites maintained by individual projects or individual archaeologists are legion. The Internet has (so long as the electronic infrastructure can be maintained) solved the problems of archaeological data manipulation, storage, retrieval, and public access that loomed so large through the 1970s and early 1980s. It has also greatly facilitated international interdisciplinary research.

Themes now common to Americanist archaeology and world archaeology include those central to Old Archaeology (time-space systematics, cultural histories specific to places and peoples), New Archaeology (archaeology as science, evolutionary archaeology; paleoenvironment, paleoecology, paleoeconomy of past societies and cultures), postprocessual archaeology (paleo-cognition, critical theory, multivalent interpretations of the past, archaeology as history, individual action in the past, domination and resistance), and

post-postprocessual archaeology (phenomenological perspectives, embodiment and the human body as a universal referent).

And that is just with reference to archaeological theory and to the sociology and ethnography of archaeological scholars. Archaeological technology has become much more complex, much more expensive, and much more international in scope. Radiocarbon dating, integral to the practice of archaeology since the 1950s, as well as trace element analysis, ancient DNA analysis, and the whole panoply of archaeochemistry and archaeophysics are routinely employed in addition to the subdisciplines now standard to archaeological practice everywhere (archaeobotany, archaeometry, archaeozoology, ethnoarchaeology, and geoarchaeology). It's a brave new archaeological world.

But that brave new world is a fragile one because—like every other scholarly pursuit—archaeology, prehistoric or historic, anthropological or classical, is one very small component

of a global human population that is currently highly politicized and severely threatened by planet-wide problems ranging from fiscal crises to pollution of the oceans and the atmosphere, desertification, epidemic diseases, and inadequate subsistence systems affecting millions of people.

Archaeology and anthropology alone cannot save the real world. Nor does it matter, I suppose, whether we explicitly profess allegiance to what used to be a unified field called “anthropology.” But it is supremely important that anthropologists of all varieties continue to participate in the work that has always engaged anthropological scholars and practitioners, work that no other discipline can undertake: Providing more and better understandings about the whole of humankind—viewed biologically, culturally, socially—from origins millions of years ago to the present day. If a global human community is to be created and sustained on this endangered planet, then the real world needs those understandings desperately.

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