

WHAT I BELIEVE ABOUT THE USEFUL DIVERSITY OF THEORY IN SOUTHEASTERN ARCHAEOLOGY

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Theory is crucial but has become boring and unintelligible; it is often ignored by most professionals. Archaeology must include a foundation of culture history, processual science, and postprocessual imagination and counteraction of bias. Further, all archaeology should aim for public aspects and practical applications. Theoretical writing must be clear and avoid pretension. Gender bias in Southeastern archaeology is one of the worst distortions of the prehistoric record for what were probably matrilineal societies. Diverse humanistic approaches from many (including non-archaeological) viewpoints can provide worthwhile avenues for investigation with new scientific tools. Narrow interpretive frameworks should be avoided in favor of the delightful banquet of multiple simultaneous or blended approaches.

It is an honor to join this group of scholars commenting upon the current state of archaeological theory in the Southeast, not to mention the greats who are our predecessors (e.g., Watson 1990). Our plenary session organizer, Jim Knight, said that his inspiration for the session title (“What I believe” about archaeological theory) came from comedian Steve Martin; given this and other aspects of theory these days, I am unable to be too serious. As Flannery (1982:278) said, “... archaeology is the most fun you can have with your pants on.” However, recent theory, so crucial to our work, has become boring, confusing, and disconnected from archaeological data, and it has yet to lose its obvious biases. This is a shame when so much of it is useful and imaginative to work with. The great diversity and attractiveness of different theoretical frameworks should offer exciting possibilities at a time when mixing and matching varying approaches becomes more and more acceptable (in archaeology and beyond).

Where Theory Is

A couple decades of teaching and exploring archaeological theory have shown me how it is both used well and abused. Students and other researchers are often

forced to insert some trendy theory into what they write just to get it accepted for a thesis or a publication. Often the theory is poorly related to the data, as well, tacked on at the end like a faunal-remains appendix. Most theory is written by academics, who often make it difficult to understand. However, the bulk of the archaeology done in this country is through contracts, cultural resources management (CRM), preservation, and heritage—subjects still not taught enough in graduate and undergraduate archaeology programs. The integral nature of theory to all these areas, implicit or explicit, is seldom emphasized and rarely part of standard training. This is a shame because theory is crucial to *all* archaeology; after all, we are explaining what humans do. We *are* cultural anthropologists; we just use a totally different method that is unique among all the social sciences. We can approach *any* human problem or issue from a completely alternative and independent perspective—material culture—to see if we get the same results.

Furthermore, since all archaeology is (or should be) public archaeology in some fashion, communication of theoretical perspectives should be comprehensible and at least implied not only in professional work but also in what we portray for more general audiences. Finally, some practical, applied anthropology, whenever possible, should be a major goal for all archaeologists. This can affect theoretical frameworks, whether in interpreting the past for descendant communities, examining identities of peoples who are gone or changed, or explaining human effects upon natural and social environments, as well as the effects of environmental conditions (whether immediate or long term) upon human life. While some archaeologists have recently realized all this and proclaimed that archaeology absolutely must be present in all these important arenas (e.g., Hodder 2004), many have been quietly teaching and doing this for a long time. My home academic program at the University of South Florida (USF) has emphasized applied anthropology, public archaeology, and practical uses of research findings for forty years.

Foundations for Theory

Over 90 percent of the archaeology in the United States is CRM, but most of it is “rather distant from

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theoretical debates carried on by the small minority of non-CRM (ivory tower) archaeologists" (Watson 2009:6). Many archaeologists have an ambivalence toward theory, "if not outright hostility...as new theoreticians appear every so often like snake-oil peddlers coming to town trying to convince the locals that they possess the miracle cure and should abandon last year's concoction" (Skibo 2009:37). Most Southeastern archaeologists do not think about social theory very often, even if it is there in the background. Elsewhere (White 1999a:2) I related this (in true cultural-materialist fashion) to our benign climate, which allows more months for fieldwork and thus brings more obligations for data description and management, and possibly therefore less time for armchair philosophizing. I still believe this, but also think that the very dry and difficult theoretical writing of late has simply turned off too many professionals, especially because most professionals are not academics.

The overwhelming majority of practitioners and writers about archaeology describe what is found, where it is found, how old it is (primary or baseline data), how to reconstruct the human behavior that resulted in its being left as the archaeological record, and how this material record and this behavior in the past might compare with that of other human groups, past and present (interpretation, probably with often only implicit theoretical perspectives). This sounds very straightforward and very interesting, and it is usually labeled "culture history."

Despite the current reputation of culture history as outmoded, old-school, been-there-done-that-and-got-the-t-shirt, or even totally dead (e.g., Lyman et al. 1997), to paraphrase Mark Twain (1906), reports of its demise are greatly exaggerated. We must do culture history first before any interpretation. It is the foundation, the heart of archaeology, the material evidence about whose implications one is theorizing. There are so many places in the Southeast where basic chronologies have not even been worked out yet or data are woefully inadequate, and such work should never be denigrated. As biologists, especially ecologists now realize, despite their prejudices against those who do "merely" taxonomy, identifying new species and such, without this basic work no environment can be understood in systemic or other terms. Without knowing what you have, where it is, and how old it is, you cannot go any further. Most archaeologists are still doing predominantly culture history (Sabloff 1992:267), though they might be considered inferior professionals by some who still fit the stereotype of Flannery's (1982) "Born-Again Philosopher": not good at fieldwork, he stopped doing it to sit in his office and write theoretical and philosophical ideas for others to test.

Science in Theory

Since culture history remains merely descriptive, we need step two: scientific comparison and analysis. The call for the "New Archaeology" of the 1960s and 1970s (e.g., Binford 1962, 1964; Watson et al. 1971, 1984) as a scientific process (including computers, statistics, instrumentation, systems theory, and environmental data) was enormously valid but somewhat ignored the fact that science was already there. Seeking patterns in archaeological data had been going on at least since the time of V. Gordon Childe, who also aimed to write for the public (Trigger 1980:110–114). Gordon Willey, known as the great synthesizer (e.g., Willey 1949) and in my opinion one of the most gracious of the "big men" in archaeology, once told me he did not want just to describe but also to explain; this is science. Lesser heralded work (e.g., Saxe 1970; Whallon 1971), earlier than and contemporaneous with that of Binford and crew, presented quantitative and comparative data and perspectives. As this type of research became better established, it was enhanced with many emerging scientific tools to become what we now call processual archaeology (or as students frequently misspell it, "proccessional" archaeology, with all of us marching along).

Techno-functional explanations did predominate. But scientific archaeology has *always* looked at non-material issues. Good examples are the still-cited classic *American Antiquity* memoir on the "Social Dimensions of Mortuary Practices" (Brown 1971) or Flannery's (e.g., 1972) equally classic work stressing the importance of interpretive frameworks that consider not just ecosystems or economics but also ideology. Yes there is bad science, even a "Bad Archaeology" website (Fitzpatrick-Matthews and Doerer 2013) exposing a lot of it, and archaeology wannabes (such as Jared Diamond [2005]) who abuse archaeological data and see human societies as completely at the mercy of geography and environments. But for most of us there has always been the assumption that culture process means everything is connected to everything else, the social, political, environmental, ceremonial systems, the empirical and the less accessible.

Mother Jones blogger Kevin Drum (2012) says the so-called hard sciences are really easy because they are based on math, and the natural world mostly *does* operate according to mathematical laws and engineering principles. But human communities do not obey simple mathematical laws, though they sometimes come amazingly close in some narrow ways, which we like to discover now and then. Such discovery is a large part of what social science should be. As we presented these papers live at the SEAC meeting in November 2012, the country had just finished electing a president whose campaign was massively data-driven.

It successfully gathered staggering amounts of information and accomplished stunningly complex, daily mathematical simulations for grand predictive modeling and moment-by-moment assessment, changing politics forever (Scherer 2012).

For archaeologists, who have smaller amounts of hard data, an important skill is picking what is able to be inferred from them fairly safely and then stating clearly that the rest is reasonable (or wild) speculation. Without the data, one should not just invent. It is fascinating that humanistic theory involving pure storytelling is pouring out at a time when so many new scientific tools and instruments are available to learn specific, previously inaccessible aspects of the archaeological record that could make the stories more accurate (I return to this particular irony below). Archaeologist Rochelle Marrinan (personal communication 2011; Marrinan and White 1998) phrased it best when she said, "We can make it up or we can dig it up."

If anything, we are perhaps not scientific enough; we are, for instance, not used to quantifying general statements. As an example, many archaeologists, myself included, have studied check-stamped pottery and classified check sizes into large, medium, and small (the sacred number three of Western culture). Several Florida ceramic types have been defined from the beginning based on check sizes (Deptford/Early Woodland, larger; Middle Woodland Gulf Check-Stamped, medium; Late Woodland/Wakulla Check-Stamped, medium to small; early historic Leon or Lamar Check-Stamped, large; Willey 1949). But few studies have included actually measuring and graphing check sizes to see whether there are clusters or peaks indicating the reality of these groupings. When the labor of measuring checks (slightly more fun than watching paint dry) is finally accomplished, it turns out that check sizes mostly fit normal distributions; there are no dips in the curve to distinguish large, medium, or small checks, nor do check sizes clearly indicate time periods (illustrated in Marrinan and White 2007; cf. Brown 1982), not to mention migration, dominance, or anything political (that we know of so far).

Po-Mo/Po-Pro Theory

The unwarranted arrogance and bias of processual archaeologists triggered "postprocessual" archaeology, an approach which really *grew directly out of* scientific archaeology, when many not only rightly recognized the biases of science but also jumped on the bandwagon of trendy postmodernism in social theory. Charles Redman's major address to the American Anthropological Association in 1989, titled "In Defense of the 70s: The Adolescence of New Archaeology and Its

Progeny in the 90s," discussed this phenomenon. This address was later published in the *American Anthropologist* (Redman 1991) in slightly altered form (I know this because I took notes religiously during his talk so I could use them in my theory class). Redman said the reason he was defending scientific archaeology was that it is us. He said he did not like the term "postprocessual," which sounds like something is over (though that comment got cut from the published version), and that postprocessualists or cognitive-mentalist archaeologists saw their interests develop completely out of the scientific archaeology they first did. Ian Hodder is an excellent example of this; all students should read his early scientific, quantitative work (e.g., Hodder 1978a, 1978b).

Redman thought that political and social biases of the present color postprocessual interpretations of the past just as much as they do in scientific archaeology. He went through a list of old New Archaeologists who had said the same kinds of things that postprocessualists were saying. He noted that many postprocessualists, especially in Margaret Thatcher's England at the time, were writing as much theoretical and adversarial material as possible to get noticed, because it was so hard to get tenured and promoted (this comment was not in the published version either—popular calls for reflexivity notwithstanding). He also said that, if some archaeologists are humanists, this is fine, and science can validate or invalidate their stories. He pointed to Childe and Flannery as archaeologists who had this scientist-versus-humanist dialogue in their work all along. Processual archaeologists certainly thought about emic and ideological aspects of their work, but realized that such aspects were much harder to see. Redman was dead-on with a very significant point: that humanistic imagination can provide the raw material for scientific archaeology to work on.

Theory as Torture, Boredom, and Posturing

Some recent archaeological theory has ignored this longstanding call for creativity and vision. It often appears to be simply observations or statements about the past that we already know, even that we take as basic assumptions, but that are expressed in convoluted prose and newly fabricated, pretentious terminology to make them sound impressive and—just as Redman said a generation ago—to get them published. Big words, often freshly manufactured or pulled from philosophical treatises, catch on and become important to repeat. Rhetorical forms such as *proparalepsis* and *epanthesis* (adding a syllable at the end or the middle of a word, respectively) take on a snooty character and mark one's writing as (pseudo-)sophisticated. It is easy to find humor in this; archaeologist Mike Hambacher

(personal communication, 2013) calls it “explication through obfuscation.” It is characteristic of a huge segment of academia, of course, where one can “prattle bombast larded with buzzwords” (Maynard 2009) to succeed and the meritocratic approach rewards mere facility in such practices (Kirn 2009:119–122).

Rather than list such abuses, I note only a few examples (with citations omitted out of respect for colleagues): “problematize,” “structuration,” “performativity,” “situatedness,” “materiality.” The last is one of the worst; for heaven’s sake, this is what we *do* as archaeologists: study the *material* to see how it indicates or embodies everything else about humans. Then there are “habituation,” “habitus,” “doxa,” “practice”—what people do every day and their sense of place as manifested in the material evidence—consisting of no more than new terms for what we have *always* looked at. Likewise, “hermeneutics” and “phenomenology” reference the notion that objects have subjective meanings attached to them; any archaeologist knows this. These and other once-simple terms and ideas get combined into phrases: “genealogies of cultural productions,” “materialized contestations of prehistoric identities” (these examples are composites, so as not to single out anyone). We all do this to some extent, setting forth elaborate language to appear erudite. But these terms are so derived that the reader must look them up all the time to figure out what they mean, like credit default swaps. The worst part is that much of this mutilated language leaves out the actual archaeology, the artifacts or features or ethnobotanical assemblages upon which theoretical statements can be based. Furthermore, the rhetoric is far removed from the day-to-day work of archaeologists in the field or lab, writing the report, or even teaching in the classroom.

This prose is torture. It is not only unnecessary but truly dulls the natural excitement of archaeology for both professionals and wider audiences. And yet postmodern writing is often calculated to intimidate and therefore impress the reader. Cognitive scientist Jim Davies (2012:44) notes that one intent of postmodernism is to obscure deliberately in order to sound imposing. He mentions the hilarious hoax perpetrated by physicist/mathematician Alan Sokal (1996a), who wrote a nonsense article intentionally combining scientific and postmodern jargon and was able to get it published in a leading postmodern “cultural studies” journal. Sokal (1996b) then immediately published another article in a rival journal, revealing that the first piece was all a joke to make fun of obtuse language and thought (and see Sokal 1996c and other works by this author). I have used this case in teaching archaeological theory to caution against incomprehensibility. Unfortunately, Sokal’s work, now over 15 years old, has been forgotten and not really utilized in archaeology or much of the rest of anthropology. For better or worse,

however, we are still saddled with “cultural studies,” essentially anthropology taken over by professionals in literature, history, and philosophy (often anthropologically untrained and unaware). For us to emulate them after they have emulated us is, of course, even richer irony. But we need not write like they do.

The original irony (irony being more or less a foundational principle of postmodernism) of this torturous writing is notable since postmodernism (and, by descent, postprocessual archaeology) came from *literary criticism*. From this source within the humanities, and associated with English departments (the supposed teachers of writing skills), it exploded into the social sciences, eventually including archaeology. So we have small groups of professionals who constitute citation circles and consider themselves on the cutting edge of new thought but do not see that they are just “trending” by echoing the latest verbiage to express standard ideas. Worse, since scholars are often judged by how much their work is referenced by others, “cliques of people who quote one another are formed” (Taleb 2007:217), and “impact factors” of journals and numbers of publications in them can make or break careers. Citation networks, which are powerful forms of social communication, can create unfounded authority in a particular belief system (Greenberg 2009:1) that encourages others to follow.

People everywhere appreciate trendiness, of course; without it artifact seriation or much of the rest of archaeology would be far more difficult. But this kind of scholarly behavior is not exactly like fashion mavens wanting the latest new silhouette for their fall wardrobe. It is seeing something others praise and deciding it must be unquestionably desirable, whether or not it is appropriate. Trend spotting to interpret fad cycles for profit becomes a marketable service (Best 2006a). We constantly latch on to what is lately desirable, but in our professional work we need caution and should not necessarily want to sound like everyone else, or wish to be incomprehensible to the average professional or lay reader. It has always bothered me that popular but crucial publications such as *Archaeology* and *American Archaeology* magazines are not written by archaeologists but by journalists, who sometimes make archaeological mistakes but who are (apparently) perceived to write more clearly, better for the general public, than archaeologists do.

In asking why some scholars might actually prefer to read and write unintelligible prose, Davies (2012:44–46) says that obscurity is often mistaken for profundity. The reader working so hard to figure out ideas will value them more when comprehension is finally reached and because all this effort and individual discovery was required. He notes (Davies 2012:47) that scientific jargon is of course equally difficult to read, but the difference is that, once the terminology is

understood, the meaning becomes unambiguous, while postmodern writing can remain annoying and subject to multiple interpretations (more of that irony).

For archaeologists, much of the newer theoretical prose is not only incomprehensible but also pretty boring compared to accounts of interesting features, beautiful or mundane artifacts, sites, settlement patterns, or other standard archaeology. Much of it is contradictory, too. For example, if "positivism" is the perspective that all is knowable from empirical data if the right tools and methods are developed, then postprocessualists, attempting an "emic" viewpoint, are positivists since they think determining what was significant to past peoples can be done from the archaeological record.

It is easy to make fun of postprocessual excesses, and processual-scientific jargon can be just as mind-numbing; but I do worry especially about students. They are ill-served to have to dwell on this stuff, or on too much theory in general. They need more training in methods, more experience in how to write well, especially in times when jobs are hard to find and hardly anybody finds them in academia. However, in my experience, students are the quickest to glom onto this kind of writing, wanting to sound like the experts whose work they have been assigned to read instead of striving for critical thinking and clarity of expression. Rather than searching for impenetrable phrases to impress readers, we could all have a laugh at the (not only British) humor in the new incarnation of the "Archaeology Buzzword Generator" online (Sweeting 2012). It offers random theoretical terms and arranges them in a sentence or phrase. The goal is not really to enhance thesis acceptance, publication, or tenure review, but to expose the absurd nature of some archaeological prose (last time I did it I got "maximal redistributive morphogenesis" but decided not to put that phrase into my article).

Theory as Diverse, Useful Approaches

Speaking of popular buzzwords, a favorite one of late is "nuance(d)"; it appears in the archaeological literature and everywhere else, including political writing, history, and journalism (and even the names of a software company and a chain of airport duty-free shops). It heralds a time of recognition that there are no simple scientific laws, that explanations have many conditions and manifestations depending upon circumstances. This is the kind of mindset that is helpful to archaeology; we can define and explain our work within multiple contexts. If the terms we already use are too simplistic or have other problems, we can refine them. We already do this: tribes become "middle-range societies" and complex hunter-gatherers are "transe-

galitarians" (an interesting word for the nonspecialist to ponder). We already have enough problems figuring out what we meant by "residual plain" and realize that a good report will define this term according to the particular situation. Just like a ceramic type name can cover great variability, we can have diverse ideas of what is understood by "chiefdoms" or any other concept, but this does not mean we should throw out the word or invent silly synonyms.

If anything, such an approach emphasizes one of the positive impacts of postprocessual theory: the wealth of diverse perspectives and the view toward doing "emic" archaeology. Whether we can actually get into the minds and philosophies of the past people whom we study is always an open question. It is of course more possible with historic archaeology, and mostly impossible in prehistoric. But attempting this does open the imagination to interpretations that could be testable. The "emic" perspective, from the bottom up or from the insider's viewpoint, is one of the major contributions of anthropology, whether in areas of management and policy making or applied technological, environmental, or socioeconomic work. It should be so throughout archaeology, as well, even if it is much more difficult.

Various subtypes (or varieties) of postprocessual archaeology have greater or lesser appeal or feasibility. Among the lesser, in my opinion, are cognitive, Marxist, structural, and agency theory. "Cognitive" archaeology is really impossible; even regular psychology is rarely accurate, let alone "paleopsychology." Motivation and perception associated with individual or group actions are always debatable (though physiological psychology, which holds great promise, can associate brain mechanisms and biochemistry with behavior).

As a lefty product of a working-class family, I appreciate the many type-varieties of Marxist approaches. However, they have several problems. First is that Marxism is not really a materialist philosophy, though it is often touted as such. It is the social and ideological roots of class struggle and resistance to dominance that are predominant in most Marxist interpretations (e.g., Harris 2001). While this is crucial to study, it is also very pessimistic; people are not constantly struggling. But we *may* be able to see resistance in the archaeological record. It might be why there was no sustained interaction between prehistoric Mesoamerica and the Southeast (White 2005), if the latter area actively shunned outside influence. Lynne Sullivan (2009:95) had reasonable evidence to suggest that Tennessee Mississippians resisted Cahokia influences. Late prehistoric Fort Walton in northwestern Florida has Mississippian-shaped ceramic vessels, but they are not shell tempered and they include unusual forms such as 6-pointed

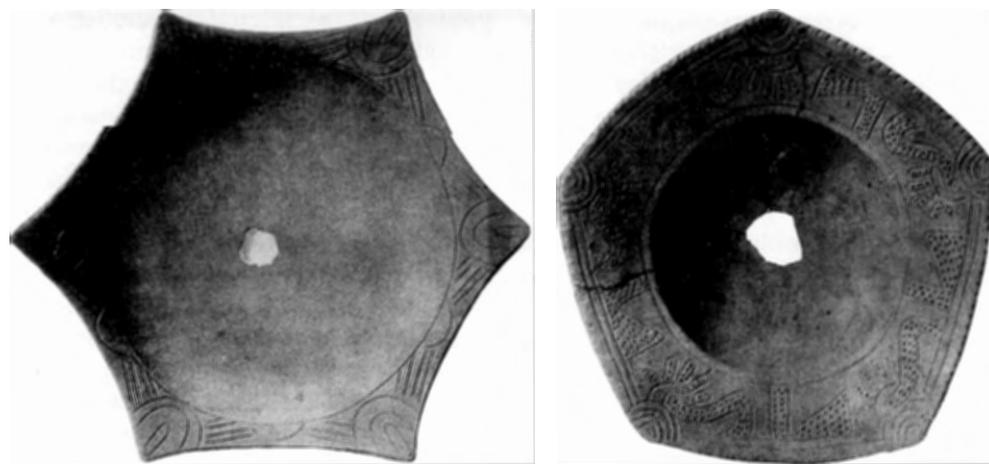


Figure 1. Fort Walton Incised open bowl forms distinctive to Mississippi-period northwestern Florida: left, six-pointed vessel from Mound at Walton's Camp (Fort Walton Temple Mound, 8OK6; adapted from Moore 1901:444, Figure 22); right, five-pointed vessel from Mound Near Jolly Bay (8WL15; adapted from Moore 1901:460, Figure 51).

(sometimes 5-pointed) open bowls (Figure 1), which apparently do not appear elsewhere in the Mississippi-period Southeast (White et al. 2012). No functional reasons (such as lack of shell for temper or use with special regional foods requiring such a vessel shape) exist to account for this distinctiveness in both style and technology. Thus these ceramics might be interpreted as manifesting regional preferences, evidence of isolation or interaction, resistance, or simply identity maintenance. Whatever the case, such unusual artifact types resist purely scientific explanation.

Structuralism is almost a scientific approach, involving identifications of patterns for comparison and sometimes quantification of these comparisons. But when meaning is inserted into the patterns, especially based on some assumed duality of human psychology (e.g., Lévi-Strauss 1969) or other assumptions about what was significant to past peoples (e.g., Hodder 1982), it approaches the realm of conjecture, or worse. Reviewing Hodder's (2004) *Archaeology Beyond Dialogue*, Kowalewski (2006:246) said, sure, Hodder shows that patterns of material culture and the regular actions that made them had meanings that were socially created, shared, and contested, but "why or when would social, culture-bearing, linguistically capable *Homo sapiens* not create meanings out of what they did and the things in their world?" He calls this empty generalization. Instead, structural archaeology should provide foundational models for scientific investigation. For example, Miguel Covarrubias used the art historian's techniques of structural analysis of stylistic elements on Olmec monuments and other artifacts to show that, contrary to prevailing archaeological opinion over half a century ago, Olmec was not a late phenomenon but the antecedent of all Mexico's prehistoric cultural heritage (Smithsonian Institution 2013). Later scientific investigation of this model,

including obtaining real chronologies with radiocarbon dating that had recently become available, confirmed this.

"Agency" is a buzzword for individual people, whom we once learned are very hard to see in archaeology or any branch of anthropology, since we study *Culture* and *cultures*. Aspects of an individual may be manifested in, say, a prehistoric burial, but not necessarily that person's active choices during life. Even in the historic past it is difficult to see causation or explanation for culture change (or lack of same) based on individual or collective deliberate human action (did a war start because someone shot the archduke? Because national leaders were ready to fight? Because economic conditions triggered fear that needed to be channeled? Because religious belief propelled the masses?), though a potter who signs a vessel is clearly asserting an identity. Many archaeologists have forgotten that investigations attempting to see individual choice and idiosyncrasy in prehistory began during "New Archaeology" times with quantitative and qualitative examinations of material technology (e.g., Hill and Gunn 1977). More ideas connected with agency theory are explored below under the heading of gender, mostly because "agents" (why not call them individuals instead of a term that makes them sound like they are selling land or celebrities?) or "actors" (all the world's a stage [Shakespeare 1623:act II, scene VII]) in prehistory are so often assumed to be male.

"Critical theory," an unfortunate term for a valuable perspective, is probably the greatest contribution of postprocessual archaeology and of all postmodern thought. It mandates close examination of the component parts of research so as to identify bias that might render it questionable or invalid. Using its framework to examine current Southeastern archaeology can be useful.

Bias in Theory

As with many other disciplines, the received wisdom of Southeastern archaeology is often made up of the pronouncements of a few middle-class Western white male scholars repeating what is currently in vogue. But critical theory flows logically into the practice of using *diverse viewpoints*, not just opinions of the small number of ubiquitous sources considered to be authorities. Often those who argue for getting into the minds of the natives ignore today's descendant communities and local residents, for example (contract companies are guilty of this often, though it does increase costs). Many other potential sources of differing perspectives are nearly absent as well. Where are the Native American archaeologists in the Southeast? Why are there still so few African-American, Asian-American, and Hispanic archaeologists working in our region? Must one be gay to find queer theory useful? Where are the perspectives of disabled people in the past? Why so few studies of the roles of children (Sullivan 2001 is an exception) in the aboriginal Southeast?

The sources of bias are many and go beyond simply who is excluded or minimized. This is because *the personal is professional is political*. What gets published and becomes important or even who gets what job depends on what is judged by peer review to be good, but also on differing amounts of power and credibility and association, on who cites whom or knows whom or hates whom or even slept with whom. It is very interesting how contemporary historical contingencies and individual agency affect how we think. If you believe violence is part of human nature, you see a prehistoric human skeleton with a point stuck in it as evidence of warfare and infer that, say, Mississippian chiefdoms were warmongers and that is why villages were palisaded, for protection. If you are a feminist pacifist who thinks prehistoric southeastern aboriginal peoples lived in peaceful towns run by clan mothers, you might see the point in the skeleton as evidence of a prehistoric Dick Cheney (the former U.S. vice president who shot a friend in the face in a hunting accident) and the palisade wall as delimiting the dancing ground.

Lowenthal (1985) said, in *The Past is a Foreign Country*, that each generation reinterprets the past according to its own needs. Collingwood (1946) said that we can only guess about the past based on analogies. So ethnographic or historic data are absolutely invaluable for counteracting bias, as are the knowledge of lesser-known archaeologists and other anthropologists, other social scientists, and non-archaeologists such as flintknappers, artists and craftworkers replicating the artifacts, and people who fish, gather, hunt, cure, dance, cast spells or do other contemporary crafts or tasks also done in the past.

My latest good example of using it all is Van de Noort and O'Sullivan's (2006) *Rethinking Wetland*

Archaeology, which combines rigorous science with accounts of people's historical beliefs, myths, and legends, as well as daily tasks, to show the *sacredness* of marshes and swamps. Also, it is clearly written and free of jargon and notes the importance of the findings to understanding modern climate change and other environmental issues. In his later *North Sea Archaeologies*, Van de Noort (2011:v, 65) continues the melding of science and humanistic information, noting how we cannot presume that land and sea were separate or opposite realms, and that many cultures even assign agency to the sea. Work like this, in regions far from the southeastern United States, is worth keeping up with, if only to inspire model building and show good use of historical and ethnographic examples.

Another example of the vital role of ethnography comes from the South. In a new interpretation of late prehistoric people's adaptations in north Florida, John Worth (2012) shows the indispensable nature of ethno-historic data to interpret the Suwanee Valley archaeological culture. It looks dull: rather plain pottery, no evidence for complex political systems, no platform mounds or hierarchies or status markers or craft specialization, no visible Mississippian trappings. One would never know that the Timucuan Indian chiefdoms there when the Spanish arrived had hereditary leaders of multicomunity political units, including many local chiefdoms, central control over land and labor, and signs of rank such as special seating, use of Spanish goods, and exclusion of elites from manual labor.

These are good examples, with assumptions grounded in some kind of data. But much current archaeological interpretation reflects Western, capitalist, male-biased, hierarchical, antiscience, right-wing ideology without such grounding. Those utilizing agency theory are often guilty of this; "big men" or "self-aggrandizers" in prehistory are really often projections of twenty-first-century Wall Street, where monetary compensation and greed grow as the rest of the country languishes in recession. These models ignore leveling mechanisms in traditional societies, such as Richard Lee (1969) described among !Kung foragers in "Eating Christmas in the Kalahari." A decade ago (White 2004) I protested such models and pointed out that building a mound does not necessarily require much social complexity, let alone economic hierarchy. Now I note that those who think one "big man" is needed to direct people in mound building should look at the Arab Spring and Occupy Wall Street movements today, and the many offshoots that become mainstream (Matthews 2012).

Gender Bias

Probably the greatest bias still in Southeastern archaeology concerns gender (as also and indepen-

dently expressed in two other articles of this group, by Levy and Sullivan). This is apparent in many different areas, from the gender of the practitioners (e.g., Bardolph and VanDerwarker 2013) to interpretations of what men, women, and others did in the prehistoric and historic past. Why do researchers not understand the difference between sex and gender? Why do postprocessual archaeologists think they invented the study of gender?

A greater awareness of gender bias hit the social sciences by the 1970s, with anthropological archaeology a little late to the game, but finally gaining ground; Conkey and Spector (1984) had a foundational publication, but many, especially women, and especially students, could not help but be aware of this far earlier (e.g., as discussed in White et al. 1999). However, southeastern U.S. archaeology remained (and remains) a sexist arena, accepting without comment such titles as *Lords of the Southeast* (Barker and Pauketat 1992) with little interest in searching for power wielded by those who were not men.

In the historic Southeast and most likely prehistoric times as well, societies were matrilineal. Men and women had complementary power and authority, and there were more than two genders (unlike in Western culture). As with the Zuni third-gender person (or "berdache," a term now considered derogatory) in the Southwest (Roscoe 1992), Native Americans in the eastern United States had additional gender roles that individuals could inhabit (Callender and Kochems 1983), often associated with spiritual power. Europeans recorded in text and illustration the native southeastern third-gender specialists (whom they called "hermaphrodites"), who carried warriors' provisions and cared for the wounded and very ill, and also buried the dead, utilizing various implements and practices (Lorant 1946:69).

However, gender archaeology is difficult. Radical feminist though I am, I know gender associations are seldom able to be isolated or unmistakably demonstrated in material culture. Even if differently-sexed skeletons have different artifact associations, the reasons may relate to a whole host of other sociocultural roles beyond gender. How can it be determined if a tall adult male in a grave with exotic grave goods and other wealth indicators was a chief or a trophy husband? Recognizing third-gender persons is probably even more difficult, though this has not stopped archaeologists from trying (e.g., Holliman 1997, in California).

We have a fascinating laboratory in the archaeology of the Southeast for studying matrilineal societies before they were forever changed, as they often are by outside colonial powers. Unfortunately, much of the ethnohistoric literature is inadequate because these societies were immediately altered by the Spanish,

British, and others who came from male-dominated Western society, which they quickly imposed, upsetting aboriginal gender systems (Eggen 1966:17–39). The intruders gained the power to make women leave the forests and fields and become more house-bound, performing more stationary domestic tasks. Native women also saw their traditional valuable products replaced by European metal, ceramic, and other goods. The political and economic power of women within the family and the wider social milieu declined considerably and quickly during times of European contact and colonization (e.g., Saunt 1999:143–151).

To understand prehistoric gender roles, it is also useful to study the few matrilineal groups left elsewhere in the world, in the south Pacific (e.g., Lepowsky 1993), Indonesia (e.g., Sanday 2002), south China (e.g., Hua 2001), and other regions (though some of these are called "matriarchies," this term means complete control of society by women, for which anthropologists have so far found no evidence). It is also important to study kinship systems, a subject unfortunately not emphasized in anthropology lately. Kinship is a significant and sensitive indicator of social and cultural change, especially in understanding acculturation or hybridization from both functional and historical perspectives (Eggen 1966:38–39).

More archaeologists of late are looking for gender in the prehistoric past, often (but not always) with feminist perspectives. However, some of these approaches are essentialist, perpetuating stereotypes of women doing the same typical things, whether dealing with plants or tedious craftwork. For example, Paleolithic archaeologists, finding net impressions in fired clay, suggest it means such technologies were available during the Pleistocene. Women are *assumed* to have crafted the nets and other fiber technology. The evidence is also taken to mean that women could indeed hunt by trapping game in nets and clubbing them to death (Adovasio et al. 2007; Pringle 1998). However, it is not suggested that the little lady would actually spear a big old mammoth like men would, even though a 200-pound man is still a mere flyspeck to a mammoth and men in many cultures hunt with (and make) nets too, since it is safer and more reliable (Salopek 2005).

Many of Southeastern archaeology's most passionate arguments since the 1930s, when SEAC was formed, have been among senior men about what we know from ethnographic analogy probably *was* everyday women's work: ceramics. But the emphasis upon agency theory lately has seldom recognized this important role of prehistoric women in providing for the archaeologist. In their matrilineal societies, Indian women of the South made the pots, farmed, owned the home and children, and provided the largest part of subsistence. A husband was "initially a stranger in his

wife's household" (Eggan 1966:25–27). Clan mothers probably made as many decisions as chiefs, and some chiefs *were* women. Probably, to repeat an old southern truism, if mama wasn't happy, nobody was happy.

Therefore, how can we ever accept without question speculations such as that pottery styles moved into areas because women married in, men took in wives? In matrilineal, matrilocal society, women take in the men. Why would theorists so interested in human agency have women be pawns moved around in marriage, when the women might have been moving the men around to marry their daughters and make family, clan, and other alliances?

There has long been an obsession with mound *size* (though assuming size is a male obsession is also essentialist): the bigger (and more numerous) the mounds, the more important and hierarchical the site or chiefdom. Some recent work has counteracted such models, suggesting the existence of heterarchical social entities cross-cutting centralized control by prehistoric elites, as summarized by Blitz (2010:4) for Mississippian cultures. But even accepting the notion that power is associated with bigger temple structures and platform mounds, if the structures were burned and new ones built on top added mound layers every time the leader died, then a society with a queen Elizabeth or Victoria who had a long, peaceful yet powerful reign might have a small mound. A large mound with frequent rebuildings might mean instability, constant change, a sociopolitical fluidity *not* amenable to central or hierarchical control.

Southeastern archaeologists remain hesitant about both sex and gender. Those who bravely tackle some of the issues are indeed stuck with the stereotypes: men hunt, women gather. But in many cultures men obtain plants and women obtain animals. The ethnographic record shows the incredible diversity (White 1999b). Is fishing considered gathering or hunting? Is netting or trapping an animal gathering, but running after it to stab it with a spear or arrow is more macho so must have been done by men? Yes men have greater upper body strength, but one purpose for inventing tools is to enhance human strength. Much more force is possible with a bow or atlatl.

For other gender associations grounded in the ethnohistoric record, a squeamishness remains. At the 1991 SEAC meeting where Pat Galloway first presented her legendary paper "Where Have All the Menstrual Huts Gone?" (Galloway 1991; see also Galloway 1997), she pointed out that we know southeastern Indians had specific menstrual taboos. Women went to special huts and relaxed during their monthly periods, when they were thought to possess unusual power. Yet unusual structure patterns at the edges of villages are never called menstrual huts. Smokehouses, shaman's huts, ceremonial buildings—but we will not want to say out

loud or in print a word such as "menstruation." The large audience was rolling in the aisles laughing as she showed a modern commercial emphasizing that squeamishness by demonstrating how sanitary napkins nicely absorb a *blue* liquid. This work (and that of others in the 1991 SEAC symposium on women) remains unappreciated and is too rarely cited, though unusual structure patterns continue to be uncovered. I was heartened to see at the 2013 SEAC meeting a whole symposium on gender issues (organized by Meagan Dennison and Renee Walker); change is underway, but the pace should not be glacial in the Southeast.

Creative modeling of gender-based ideas in Southeastern archaeology is perhaps increasing to some degree. For example, Cheryl Claassen (2011) sees rock shelters as women's retreats since women are associated with plants, so plants in the shelters mean women were there. Though perhaps a bit tautological and essentialist, this hypothesis is imaginative and great for proposing new questions. It is also testable if DNA could be obtained from the coprolites.

New Tools, Imagination, and Delightful Blending in Method and Theory

Gender does begin (though by no means ends) with sex, and this is possible to detect with scientific methods such as DNA analysis. A new study (Jaouen et al. 2012) shows that measuring both iron and copper isotopes in human bone recovered archaeologically can determine the sex of skeletons. Bone chemistry could even be used to detect matrilineality.

Diverse new tools or those newly applied to archaeological materials may help change other kinds of basic and biased assumptions, just as the invention of the telescope changed astronomy. This happens more often than we realize; nobody even saw Poverty Point's earthen architecture until someone went up in an airplane. Most scientists of any kind would agree that both paradigm shifts and new instruments set research paths (Dyson 2012), and a good argument can be made for *ignorance* as the real motivator behind scientific inquiry (Firestein 2012). Burning, basic questions are what we talk about in the bar at conferences after hearing presentations of what we already know (Cerf 2012), and so many tools from other disciplines are now available to address these questions. New dates (Pike et al. 2012) indicate Neanderthals may have made cave paintings. Pottery is now shown to be 20,000 years old in China (Wu et al. 2012). Sourcing studies and bone chemistry *could* support hypotheses of migration (Tung 2012). New pottery styles do not usually mean migration. Even if artifacts were made elsewhere, that need not mean that the people who

made them moved in. Our houses are full of Japanese electronics.

Given all these new tools obtained from other sciences, we must have multiple approaches. Walter Taylor (1948) long ago noted how archaeologists were too narrow, focused on chronology and description, and should instead have many kinds of data and materials, do wide horizontal excavations, study plants, soils, everything! But he never did this and nobody else could either. Recently Taylor's views, after decades of deliberate neglect and shunning, are being revisited (Maca et al. 2010). Patty Jo Watson (2010) suggests that we might now actually be doing his "conjunctive" approach, trying for everything from economic to cognitive systems. The tools—computers, instrumentation of various kinds, satellite imagery, and so on—are available. Multiple theoretical frameworks are equally indispensable for both accommodating the findings of the new methods and bringing a variety of humanistic approaches into the domain of the scientific. Hauser (2012), reviewing archaeology for year 2011, honors the late Elizabeth Brumfiel's idea that more imagination is needed in linking empirical data with social theory. He thinks the humanist/scientist or deductive/interpretive debates are perhaps easing and the distinctions between "archaeologies" are becoming blurred with all the multiple, simultaneous approaches. This is not even a new idea; a decade or two ago many were calling for a productive plurality of perspectives and an end to posturing among theorists (e.g., Mackenzie 1994:xi–xii) and to polarized factions in archaeological theory (Jones 2001).

For the symposium upon which these articles were based, Jim Knight asked participants, among other questions, "Is society tightly organized or planless?" and "Is scientific causality or historical contingency more important?" The answer to both these questions is yes. All these factors shape human cultures, and we need multiple tools to understand them. We can and should do it all: good materialist science, analogy, humanistic imagination, but less bias and more diverse voices. We need a good mashup of theoretical approaches, and instead of worrying about whether the evidence supports theory, we need to think about whether theory works in the context of the evidence (Lucas 2012:3). I am not by any means the first to support such an approach. Flannery's (1982:274) parable included a statement that the many disparate theoretical/philosophical approaches of archaeologists were useful, especially when held together with the concept of "culture." Jim asked us to describe what social theory informs our work; mine is usually a materialist perspective (we *can do* technology and environment best, after all) tempered by humanistic speculation that I hope can generate testable hypotheses. I think most of culture is structured by what is

available and what people need, but often they need social esteem and something empowering to believe in, and often the accidents of history favor those who are prepared—or they don't.

With all this it is also crucial to move from knowledge to wisdom (Maxwell 1984, 2007) by doing archaeology if possible for the good of humanity, whether studying climate change, environments, technology, identity and resistance, or even social justice. Some examples: Van de Noort's (2013) newest book, emerging from his previous work cited above, uses archaeology and its long-term perspective to address major issues in climate change. Great contributions have been made toward understanding conservation and waste, as the archaeological analysis of modern garbage ("garbology") pioneered by William Rathje continues to expand worldwide. Its legacy of both public archaeology and applied anthropology is carried on by Wilson Hughes and others (Mervis 2012). I am thrilled that graduate students at USF could be a part of this practical archaeology, during Rathje and Hughes's excavation of a Florida landfill (Layman et al. 1991). I am equally pleased that another student used stable isotope analysis on archaeological shells from northwest Florida middens (excavated a decade ago) to demonstrate not only seasonality but also the polluted nature of St. Joseph Bay today as compared with its character during Fort Walton times, when the whelks were collected (Harke 2012). It is not easy to do applicable good while doing archaeology, but if such aims are part of the creative process as well, it will become more possible.

Meanwhile, the emphasis should be upon avoiding one narrow way of thinking, one set of confining theoretical parameters, when there is such a feast of different delicious possibilities. It is important to enjoy as many of them as possible, to play with scientific modeling to get new ideas (Laszlo 2004). Though our organizer Jim mistrusts eclecticism, I am advocating more of a synthetic perspective where one approach complements or feeds into another. And crystal-clear, non-ostentatious writing is also essential. Much of the tedious prose in recent theoretical treatises threatens to dull the passion for archaeology that moved most of us to become professionals; pragmatism, if nothing else, should inspire clarity of presentation.

With an endless variety of approaches, new kinds of tasks might be imagined. It is much easier to reconsider old data now accessible in electronic form with new models, to see if the same answers are reached. Similarly, it is easier to reconcile old, new, and totally different theoretical frameworks. Schiffer (2011:22) advises that, as we continue to follow fads, we should not derive our theory from cultural anthropology, as it "wallows in varieties of vulgar idealism." But his trademark "behavioral archaeology" has evolved over

the years to view not only technological change working on social processes but also social and political processes forcing technology to change. Gifford-Gonzales (2011) admits she has seen behavioral archaeology grow over the decades to include the role of historical contingencies that affect technology. This kind of blending improves archaeology.

It is not good to stand still, nor to move along too restrictive a path. We should be aware that academics' fads do not very often lead to transformation of disciplines and can be harmful wastes of time, turning those forced to participate into cynics (Best 2006b). We should be good anthropologists and know about kinship and language and art and substantive economics and Pacific Island big men and women, as well as maps and ceramic typologies and trace element analyses. With every site, every region, every artifact assemblage, there are commonalities that can be inventoried, measured, quantified, and compared, and there are unique and idiosyncratic elements. This is how culture works, and we are so lucky as archaeologists to be able to bridge the divide between the sciences and humanities to understand our human past. To appreciate the wide range of different delectable possibilities, I paraphrase the famous line from *Auntie Mame* (Warner Bros. Pictures 1958), the old movie with Rosalind Russell as the fun-loving character always worth quoting. She said, "Life is a banquet and most poor suckers are starving to death!" I believe that archaeological theory is a banquet and most poor archaeologists are starving to death. We should try as much of the smorgasbord as possible to do good archaeology, and make sure to have fun as well!

Notes

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