



Archaeography, Archaeology, or Archeology?

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Source: *American Journal of Archaeology*, Jul., 1989, Vol. 93, No. 3 (Jul., 1989), pp. 429-435

Published by: Archaeological Institute of America

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The First Joint Archaeological Congress: The Plenary Session Papers

INTRODUCTION

In the committee charge of the First Archaeological Congress held in Baltimore, 5–9 January 1989, the Program Committee set forth three mandates. The Congress was to 1) celebrate all aspects of archaeology throughout the world and time; 2) address broad philosophical and methodological questions raised by research with subject matter of collective interest; and 3) emphasize and unite our common goals and needs. The Congress was sponsored by the Archaeological Institute of America, the American Philological Association, the American Schools of Oriental Research, and the Society for Historical Archaeology. It was unique for it brought together 3400 archaeologists, philologists, scientists, and educators in 130 sessions and 900 papers!

The Plenary Session papers that follow affirm our

pledge in carrying out these directives. They examine the strengths of archaeological exploration and research in America in the past, present, and future. They convey some of the dynamic thought-in-progress that represents our discipline, and future directions and initiatives we must take.

As Co-Chair of the Congress Program Committee and President of the Archaeological Institute of America, I am pleased to introduce these papers and to express my gratitude to those who thoughtfully spoke their minds for the discipline: James Deetz, James R. Wiseman, Brian M. Fagan, and Hester A. Davis. My hope is that our shared journey is well served.

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Archaeography, Archaeology, or Archeology?

JAMES DEETZ

Delivering a keynote address can be both intimidating and a source of inspiration—intimidating because one wonders what can be said that would justify all of your listening, and inspiring because you *have* to listen, or at least sit there, and the opportunity presents itself to inflict on you all manner of outré thoughts and probably spurious wisdom. So it is that I'm here and you are there, and somehow together we have to get through the next half hour in some semblance of order, and with luck, to our mutual benefit. When this address was first proposed, and in fact as it appears in the preliminary program, the title was "Archaeology and the Past." More recently the title has been changed, but the content will address the earlier title as well. But that title poses a question: What past? The whole of past time to which we archaeologists direct our attention, or the past of archaeology? If the latter, how much of that past? Back to the early 19th century, or to more recent time, say since the discovery of Carbon-14 dating in 1948? Well, I didn't bother to inquire as to just what it was I was to talk about, since at least the title had been changed, but rather made an

arbitrary decision, and a selfish one at that. I like 1948; I graduated from high school in that year and entered Harvard College as an early case of affirmative action, providing for the admission of hillbillies to Ivy League institutions. So archaeology got me and Carbon 14 in the same year, the latter courtesy of Dr. Libby, and the former the result of my failing Chemistry I (my lab notebook burned up at mid-term) and thus having to give up a career in medicine, probably to the benefit of countless thousands of potential patients. Carlton Coon had left Harvard the previous year quite unexpectedly, and Anthropology I-A was team-taught for the first time by an all-star cast, namely the entire Archaeology faculty—Jo Brew, Alfred Tozzer, A.V. Kidder senior and junior, Hugh Hencken, and Stanley Garn. I had enrolled in the course for area distribution, but was immediately hooked. I had no previous interest in archaeology, and knew nothing about it. I had collected butterflies, not arrowheads; studied astronomy, not ancient history. I knew far more about galactic nebulae than about ancient Egypt, the Maya, Inca, or Aztec. But here I am,

40 years later almost to the day that I took my first final exam in archaeology. I received a grade of C on the exam, hardly an auspicious start. But I persevered, spent four years defending my country in the early '50s, finished graduate school, married, had kids, bought a television—all the normal things we all did. Had I to do it over, I almost certainly would have become a more useful contributor to society, say a professional football coach. But done is done, and quite seriously it has been a blast; if work is defined as that which you don't like to do, I haven't done a lick of work in 40 years, save the occasional department or committee meeting. So what I propose to do is, using a popular Marin County (California) verb, to share with you some thoughts about those 40 years from a very personal perspective, what took place and some possible reasons why, and how it has all brought us to where we are today in the post-processual era of archaeological thought.

To return to the title of this address: archaeography, archaeology, and archeology have received varying degrees of attention during the 40 years in question. The extent to which each has been emphasized over the past four decades has in no small part been the result of events and changes in the larger world of which we as archaeologists are a part. But before looking at these events, some definitions are in order, dull and boring as that might be. By archaeography I mean the writing of cultural descriptions based on the material record, buried or not, in a fashion analogous to ethnographic description. Archaeography stands in the same relationship to ethnology, the study of culture holistically, as does ethnography. I used the term archaeography in my *American Antiquity* paper on Walter Taylor's contribution to archaeology,¹ and while I'm not terribly taken with the term, it does have its uses, and one of them is that at hand. Archaeography is what we all do, or at least should do, before we move on to the next level of synthesis and analysis. It requires hard data and lots of it, not because once a certain critical mass is reached the data will speak for themselves, but because without a decent body of material, any explanation runs the risk of missing the mark through oversimplification.

My obligatory quote from Henry Glassie makes the point quite well: "any method of inquiry must include a synchronic statement as a prelude to diachronic interpretation. Time must be stopped and states of affairs examined before time can be reintroduced, else the scholar will be unable to determine his object of

study clearly. He may include too much, or more likely, too little in his research, and when the object of his study mutates he will be caught without an explanation for the change. The architectural historian who operates without a totalizing definition of architecture may focus on isolated details—an object too small. As a consequence he often finds himself having to explain architectural change as a series of unconnected revolutions instead of the gradual development that he would find if he examined architectural wholes."² One can easily substitute the term *cultural* for *architectural* in this passage, and make it apply to proper archaeological method. Furthermore, while the material record is a synchronism in most respects, though the result of diachronic process, both history and cultural process are diachronic. And while one might recover too much data, it can always be reduced. Sufficiency, it would seem, can only be perceived in surfeit, too little can so easily seem enough.

So we excavate our beloved potsherds, measure houses, mark the location of spent cartridge cases on battlefields, and if we are historical archaeologists, spend large chunks of time in the archives perusing day books, probates, orphans' court records, and deed and title documents. As archaeographers, we dirty our boots and our hands, and the success of our efforts is in direct proportion to the extent we are willing to engage in this pursuit.

I also suggested in the Taylor paper that perhaps the word archaeology is redundant, with or without the "a," since ethnology, when properly defined, seems to describe what we do with our archaeological data. But this is a tough prescription and one not likely to be followed, so for today at least, the term archaeology will stand. But it is spelled two different ways, and while I'm certain that such was never the intent, the two spellings can refer to two different kinds of archaeological thinking, one humanistic and the other scientific. I think it is not totally accidental that *Science* magazine uses only the "e" form, while we find the "ae" form in use in journals that include both scientific and humanistic writing. In any case, while we all produce archaeography in our work, those data recovered can be used in both humanistic and scientific approaches to understanding culture. So much for the terms in the title; they are little more than gimmicks, though intended ones.

Those of us who have been doing archaeology for the past 40 years or more know that the field today is little like what it was in the late 1940s, and even less

¹ J. Deetz, "History and Archaeological Theory: Walter Taylor Revisited," *American Antiquity* 53 (1988) 13–22.

² H. Glassie, *Folk Housing in Middle Virginia* (Knoxville, Tenn. 1975) 8.

like it was in the time prior to the Second World War. This was a time that Willey and Sabloff have designated as descriptive, and thus archaeographic; one has only to look at, for example, the final exam for Anthropology I-A at Harvard University in January 1949. No question on that exam hinted at paradigms. Archaeologists were hard at work compiling data—and making students memorize it. Almost impossible mnemonics were of limited use; who today remembers, or ever knew, ACALMASM—Abbevillian, Chellean, Acheulian, Levalloisian, Mousterian, Aurignacian, Solutrean, and Magdalenian. No matter that two of these entities have been retired, and that the sequence is hopelessly outdated; I will carry this set of letters to my grave, and it's a toss-up as to which of two sequences I will give to St. Peter, ACALMASM or 13373541, my Air Force serial number.

And even later, in the mid-'50s, I vividly recall an all-nighter with Olaf Prufer where the two of us struggled with learning the differences between Proto-Panzeleo I and Proto-Panzeleo II. What were these things? They must have been South American, since we were studying for Gordon Willey's course in South American prehistory. My point is that at the time, as I recall it, we were robust in data, but theoretically not only timid, but quite naive. This could contribute to the content of many courses that reached and exceeded a point of terminal dullness, when the professor simply ran through sequence after sequence, listing relevant and apparently important traits. Such, then, was the world of archaeology that I had entered, a world that had begun to change, and that would continue to do so at an ever increasing rate.

At least four happenings in our national history contributed to this change; others might see different ones, or more. The first had actually happened just before I began my interrupted four years at Harvard, and was a direct result of the Second World War. Under the G.I. bill, thousands of folks who otherwise may never have even gone to college did so, and many of them chose archaeology as a career. This marked a departure from the older tradition of archaeology, practiced in part by people of some significant means. I well recall a fellow graduate student, who shall go unnamed, who approached a professor, also to remain anonymous, telling him that he was desperately broke, and asking if he knew of any possible source of support—jobs, loans, scholarships, anything. After brief reflection, the professor replied, "Hard as I know it may be, you're simply going to have to dip into capital." These "dollar-a-year men" constituted a significant portion of the profession, and there were not all that many archaeologists anywhere. The G.I. bill changed all of that, and in the period of the '50s and

'60s, the number of archaeologists grew significantly, as did the number of positions available to them in the early '60s. In fact, it is this cohort of anthropologists who are now moving through the pipeline like a pig through a python, and their retirement during the next decade is supposed to have a significant and salutary effect on the job market.

The second event that had an important effect on the course of archaeology occurred in 1957. The Soviet Union managed to propel a small sphere into space, and the impact that the first Sputnik had on American education was a major one. Suddenly America felt that it had been ignoring science in its educational programs, from primary grades through graduate school. In a way, the space race also became a science teaching race, or at least that's how it was perceived in the United States. And while the National Science Foundation had been in existence since 1950, it was not until 1959 that anthropology was given a program within the foundation. It is probably significant that the first program director for anthropology was Al Spaulding, not only an archaeologist, but as we all know, a scholar with special interest in quantitative approaches and the application of scientific method in archaeology. It would seem, then, that those last couple of years of the 1950s marked a critical turning point in archaeology. The stage was set with all parts in place. There were more archaeologists than ever before, new programs were springing up around the country, and some people were beginning to think in more theoretical terms, although there was not full agreement on what constituted theory. Willey and Phillips published *Method and Theory in American Archaeology* in 1958, significantly the first attempt to integrate all of the archaeographic data that had been accumulating for so long for the Americas. But others within the discipline were taking a more scientific view of the work that they did, in contrast to the earlier generation of humanists who had dominated archaeology from the start. All that was needed was a catalyst, and it was provided by American involvement in Southeast Asia.

I am certain that anyone who was a part of the archaeological community in the early '60s would agree that the appearance of the New Archaeology owed at least a part of its timing to people like Mark Rudd and Bernadette Dorn. And likewise, those here who were not around then probably don't even recognize their names, although Mario Savio might signify something. As Andy Warhol said, by 2000 everyone will be famous for 15 minutes. These people and others in the '60s were key figures in the wave of student protests that swept American campuses. As for the content and style of the New Archaeology, those likewise were

partly a function of a whole set of attitudes and perceptions that characterized the emerging counterculture. "Don't trust anyone over 30." "Question authority." "Down with the establishment." Well, who were the authority figures, the over-30 group, the establishment in archaeology? The senior scholars, that's who, men and women who were humanistic in their approach to the field, who were highly competent archaeographers, although perhaps not theoretically sophisticated in some instances (that's not necessarily bad, as we were to learn), and who occupied senior positions in departments across the country. And of course, the lines of opposition were quite naturally drawn in terms of who used the scientific method and who did not, since the younger generation had at least in part been trained in the post-Sputnik era. Ironically, it was those who espoused the supposedly dispassionate and objective position of scientists who were the more emotional in the many exchanges that took place, and there emerged a style of discourse which was strident, *ad hominem*, and often bordering on ugly. This tendency was most obvious at professional meetings, when proponents of the New Archaeology often deported themselves like members of the Red Guard. It is a further irony that this same style, but in a much gentler form, was what led Walter Taylor into certain difficulties with his peers in 1948. It would appear that Walt was ahead of his time in more ways than one. What was considered as ungentlemanly form in the '40s became the norm of the '60s and '70s.

Now these observations may be seen as mere quibbles, if not totally wrong. But one must agree, I would think, that at the same time there was a slow but steady move away from responsible archaeography, as archaeology with an "e" became ascendant over the "ae" version. Much of this process has been delightfully dealt with by Kent Flannery in his "Golden Marshalltown" article,³ and we all know the born-again philosophers and children of the '70s who have less than a proper respect for hard data, wherever its study may lead us. This weakening of our intellectual center is quite disturbing. Kent Flannery has the Old Timer tell us that the central paradigm of archaeology is culture, and this is absolutely true, and since archaeography is the construction of cultural contexts from the material record, it is the *sine qua non* of what we all do. Culture, not behavior. Certainly objects can only come into being through behavior; they do not make themselves. But this is not the same as saying that the reconstruction of past behavior is our primary goal. Behavior and the material universe both contain

and permit us to perceive the cultural rules responsible for their form, and in any society, these rules are the same for both categories of objectification.

A second trend that has contributed to the weakening of the center in archaeology has been the emphasis on non-archaeographic data to an extent that is perhaps excessive, becoming an end unto itself. I refer here to such things as taphonomy, post-occupational site-formation processes, and ethnoarchaeology. Not that there is anything inherently bad about such concerns; quite the contrary, when properly addressed, they can be immensely constructive. I suspect, however, that many of those who pursue such studies believe themselves to be doing archaeology, with or without the "a," when this is not the case, since such studies lack an archaeological component. Part of this trend was probably unavoidable, since during the '60s there was such a growth in our numbers that increasing specialization was inevitable, if only because remaining a generalist was to risk a loss of competitive edge. But I have a feeling that in spite of our numbers, we are still dealing with a finite amount of intellectual resource, and that archaeologists of the older tradition are relatively if not numerically fewer, and that things have become out of balance.

Ethnoarchaeology is a special case. I have never liked the term, for it seems that proper ethnography entails all that an ethnoarchaeologist might do, namely a complete description of this or that society. Moreover, there seem to be serious flaws in some of the work being done under this rubric. Kent Lightfoot once commented in a talk at Berkeley (it may or may not have been original with him) that prior to the expansion of Neolithic peoples, hunter-gatherers had access to the entire world, including those areas with an abundance of resources. Modern hunter-gatherers occupy marginal areas into which they were forced millennia ago, and mostly live in semi-dependency on communities that are the result of much later European expansion. It is difficult to see how such people could in any way serve as a model for understanding the way people managed their world before 10,000 B.C. Outrageous as the suggestion might be, is it not possible that at least in some cases these marginal dependent people are closer to the homeless street-people of Berkeley, who live in the bushes around the stadium and forage along Telegraph Avenue, than to the majority of pre-Neolithic peoples anywhere in the world? Note, I said closer to, not like, but it is an interesting thought.

A fourth major influence on the course of archaeo-

(1982) 265-77.

³ K.V. Flannery, "The Golden Marshalltown: A Parable for Archeology of the 1980s," *American Anthropologist* 84

logy during the past 40 years has been the emergence of environmental preservation legislation and the rapid growth of cultural resource management. Archaeology had now gone corporate, had become a business, so that a whole new set of constraints emerged. Mitigation of adverse impact often translated into avoidance rather than study, and cynical as it may sound, the more contracts one could negotiate, and the quicker the work got done, the more exorbitant overhead charges could be levied. I personally believe, but don't necessarily expect agreement, that much of what I call the cryogenic approach to archaeology owes to the C.R.M. process. The reasoning goes something like this. Whatever you do, if possible do not excavate. Save the site for later generations, who will have more sophisticated methods, and will be able to do it better justice. This philosophy, very strong in California, is the same as that which says freeze my body when I die, so that in the future, more advanced medical science will be able to thaw me out and cure me. Such an eventuality, even if realistic, depends on a constant power source, and no defrosting. Likewise, if we are to save the sites for some unknown future archaeologists who are going to be so superior to us, who can guarantee the support system? Pulling the plug in this case will take the form of looting, private construction, and the inevitable progress of natural forces. I am not suggesting wholesale excavation of all sites just because they are there, far from it. I have always advocated saving a significant portion of any site for later study. But if we do our work right, and keep proper records, I don't see that we are necessarily engaged in an orgy of destruction. Far better, it seems, would be the very careful, slow, and meticulous excavation of a relatively few key sites that hold promise of providing new data, and a thorough rethinking of what field techniques are most efficient for data recovery. Curiously, there have been far fewer changes in field procedures during the past 40 years than in the areas of theory and method. Five-foot, or one-meter, squares are not divinely provided to us; there may be other ways, and it would behoove us all to give the subject considerable thought.

These thoughts bear directly on the whole question of research designs, also eloquently discussed by Flannery in "The Golden Marshaltown." His analogy between digging a site and shooting an informant is particularly apt, and draws attention again to the parallels between ethnography and archaeography. Clearly any excavation should be conducted in such a way that the maximum number of questions might be answerable from the data recovered. Oh, I know the argument here. How can all aspects be dealt with? We should know something of what we are looking for,

and bring our hypotheses packed with our trowels and brushes. Well, there is danger in both directions, and I am not one to say which is the least problematic, only that full adherence to overly rigid research designs (without keeping the other dimension in mind) is a bit intimidating.

The illustrations in Stan South's *Method and Theory in Historical Archaeology* are delightful and original. In one, he shows us the pole arm of archaeology, a halberd blade with a sharp point and two side projections near the blade's base. The point, the piercing edge, is labeled the hypothetico-deductive nomothetic paradigm. The side projections are labeled, respectively, humanism and particularism. The imagery is clear, and almost but not quite unambiguous. For while humanism and particularism are not placed on the point, the function of those projections on a halberd is in part to keep the point from going too far into whatever is being pierced.

I should like to offer an alternative analogy, which I think more closely approximates what we do as archaeologists. Call it the groundhog stick of archaeology. Picture a stout branch with a couple of projections on one end, something like a snake stick. Where I grew up, we used sticks like this to twist groundhogs out of their burrows, although sometimes we would drop a lit railroad fusee down one hole and shoot the groundhog when he exited by another. Now the thing about a groundhog stick is this. You know the groundhog is down there, but that's about it. So when you stick the pole into the ground and twist, you might get the critter under the arm, or in the groin or even elsewhere, but when you pull, you get the whole groundhog. Likewise in archaeology, you may approach the data from any number of directions, but if you do the job well, you end up with the whole picture. The analogy is messier, but I think perhaps closer to the truth.

So here we are in Baltimore in the second week of 1989. Where are we after the past 40 years? We have watched our discipline change for better or worse. I personally think that there is still a danger of losing our center, but as with the doomsday clock, I think the hands can be set back a bit. Let me cite just a few of the many bright spots around us on the horizon. First, I perceive a refreshing diversity in the way in which we are looking at our data. I am told that we are now in the post-processual and even in some places post-structuralist world. I find these terms rather like non-Christian in the sense that that term includes members of all the world's religions save one. But this kind of ecumenism is most positive, a happy blend of humanism and science; perhaps equilibrium has finally been achieved. No one seems primarily bent on telling the other person what to do, or how to think about it.

There are different schools of thought, to be sure, but at least from where I sit, I sense little of the feeling that was so pervasive in the '60s. Of course, as a historical archaeologist who for the past 30 years has paid more attention to folk life studies, history, and architecture, my isolation from the mainstream of archaeology might color my views somewhat, but I believe there is at least some truth in that observation.

A lot of exciting fieldwork is going on, but I will limit my observations here to just one area, a period of time that has been a sort of archaeological no man's land for quite a long while. I am referring to that period just prior to European contact whenever it took place anywhere in the world, what we sometimes refer to as protohistoric. It seems to have been relatively neglected by prehistorians because it was just too late to be of much interest, and by historical archaeologists since it is prehistoric from their point of view. Yet this is the time when some of the most radical changes were worked on the peoples of the world as a result of wholesale European colonization, and its study, which by necessity must be archaeological, will shed light on many current world problems. To name but a very few of the scholars who are producing excellent scholarship in this area, Carmel Schrire, Ann Romanofsky, Garth Sampson, Merrick Posnansky, and Kathy Deagan come to mind.

Another promising trend is to be seen in our readjusting to the scale with which we view our data in a spatial sense. The archaeological study of landscapes has been receiving more attention of late; Bill Kelso and Rachael Most have edited a book soon to be published by the University of Virginia Press entitled *Earth Patterns*, and the articles in it treat both recent historic and ancient landscapes. I find this a very promising subject. Years ago, Al Spaulding defined a site as that place where an archaeologist digs. He may have been kidding a little, but the definition has significant implications. He digs there because there had been something visible to lead him there in the first place. But all the land between sites in the traditional sense can bear the stamp of culture, and we ignore it at our risk. Landscapes can be powerful cultural statements, intended or not.

When the so-called 1820 settlers came to the eastern Cape province in South Africa, not surprisingly in 1820, they so remade the landscape into a pocket version of the England that they had left behind that, even today, parts of it look for all the world like the Derbyshire moors, except for the Aloe and Euphorbia in the deeper dongas. There is no question that this landscape made a statement to others, especially the

local Xhosa peoples, a statement of power and dominance. To the English inhabitants the same landscape said something quite different. The Reverend William Shrewsbury, writing in 1869, had this to say: "It certainly is pleasing to think that, from my circuit in the heart of Kaffraria I can any time ride on horseback in the short space of five days, to Grahamstown, and behold England in miniature."

I would place battlefield archaeology in the category of landscape archaeology, since such areas are of considerable extent, and require special techniques for their study. Richard Fox's analysis of the detritus of battle from Little Big Horn is an excellent demonstration of how archaeology can provide a very different picture from that commonly presented. In this case, the fabled last stand was probably a chaotic rout, something that Indian witnesses had insisted all along. Kent Lightfoot's careful survey of a very extensive area surrounding a coastal Algonquian midden on Long Island revealed specialized activity areas that provided a clearer picture of the annual subsistence and technological cycle. In my own subfield of historical archaeology, the revision of scale has impressed on us the critical need for an international comparative approach to the study of early European expansion and subsequent colonial developments. Ivor Noel-Hume's comparisons between Virginia and Ulster in the early 17th century, Marley Brown's comparative study now underway between Bermuda and Virginia in the 18th century, and our current work comparing the early 19th-century English frontier in the Chesapeake and on the eastern Cape frontier in South Africa are all examples of this approach. In the latter case, although the project is in its early phases, it is already apparent that upon moving to southern Africa, the early settlers stepped out of the industrial revolution, and constructed a world quite different from that of their American counterparts. Margot Winer will present a paper later in the meeting on her part of this project, the study of folk housing and landscape.

Finally, I am encouraged by recent suggestions that we allow ourselves to become emotionally involved with our work, a clear sign of a resurgence of humanism in our collective outlook. Now there is a big difference in the investment of emotion in our perception of the value of what we do, as opposed to the way in which we communicate with others. Too much of the latter took place in the '60s and '70s. Archaeology is—or should be at least—very important; I've always told my students that it's too important to take seriously.

In a paper written for a Cambridge University seminar on post-structuralism and archaeology, Simon Coleman and Matthew Johnson make a plea for

replacing power with passion in our presentations.⁴ Stating that “in practice post-structuralist writings tend to reinforce the very power relations they claim to subvert,” they ask archaeologists “to recognize that charisma and social context play far more important a role in academic discourse than is usually acknowledged.” They go on to say that “the radical passionate archaeologist is also a romantic in the broadest sense of the term. He or she believes in the possibility of a different and better future, is an idealist in the broad political sense. His or her rhetoric is almost sensual in its acceptance of the possibilities of change.”

I find these views very refreshing, and must confess that they have always implicitly underlain my approach to the field. That this is so is perhaps indicated by their using one of my public lectures as an example

of the use of passion in archaeological discourse. I am not only flattered and honored by this, but also bemused. For now, after 30 years in the business, I have first been a culture historian, then a New Archaeologist, then a structuralist, and now, apparently, a passionate post-structuralist. The fact is, I am not doing things that differently from the way I did in the '60s. I don't think I have changed at all; the transformations have been in the way my work has been perceived by others. Fine! What goes around comes around, but I cannot help wonder what kind of an archae (e?) ologist I will be by the year 2000.

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⁴ S.M. Coleman and M.H. Johnson, “Power and Passion in Archaeological Discourse,” unpublished paper for Cam-

bridge University seminar on post-structuralism and archaeology, July 1988.