

FORUM

## Historical Archaeology Adrift?

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### ABSTRACT

This commentary cites the remarkable progress made in the field of historical archaeology during the time since the founding of The Society for Historical Archaeology in 1967. Despite new sophistication in the technical aspects of the discipline, we have witnessed a perplexing tendency to focus research on unique events and thus have largely abandoned the search for the regularities of cultural practice. It is suggested that if historical archaeology is to achieve its full potential as a discipline, it must develop a methodology which draws from its unique strengths. The opposition of archival and archaeological data sets in the context of scientific inquiry is recommended.

### Introduction

My purpose is to offer some observations on the current status of the field of historical archaeology. These views are offered from the perspective of a long association with the discipline during its formative years and with high expectations for the future of historical archaeology. Since it is my hope to focus attention on the broad concepts and problems of historical archaeology, I include few references to the specific literature. Likewise, I have tried to direct my commentary away from discussion of specific theoretical positions.

Conversations with graduate students and younger historical archaeologists convince me that there is very little recognition among these colleagues of the tremendous progress that has occurred in historical archaeology since the founding of The Society for Historical Archaeology (SHA) in 1967. Although that date hardly marks the beginning of the study of historic sites, it is an important benchmark for the

development of the field of historical archaeology as a formal discipline.

Most of the SHA founders who gathered at Dallas were not historical archaeologists. Most were prehistorians and some were historians, all drawn together by an interest in sites occupied during the era of written history as well as the artifacts they contained. None of these founders had ever taken a course in historical archaeology, and few had any formal instruction in historiography. In all honesty, most did not know a pintle from a sear and few could offer a reasonable guess date for the appearance of transfer printed ceramics. I include myself among the initially uninformed.

On the other hand, it was relatively simple to find out what was known about historical archaeology. Two weeks of hard reading would get a serious student through the entire published literature on this side of the Atlantic. If an answer were not to be found in the literature, one could always count on one of the dozen or so real historical archaeologists like Harrington, Cotter, South, Deetz, Jelks, Pilling, Noel-Hume, Woodward, Kidd, and the Hesters. Most of them had, in fact, only recently passed over from prehistory. By today's standards, most of the remaining founders were, to put it politely, marginally competent to even dig an historic site. But after all, this was the point of getting organized in 1967—to build a literature, to train competent students, to learn how to use archival records and to identify historic artifacts.

The measure of how well these objectives were met is to be found in the vitality of the field today. Obviously, there are a lot more of us and we are a lot more competent. This is largely due to the fact that opportunities for undergraduate and graduate education in historical archaeology developed at a rapid rate. Today students have the option of many fine programs that offer degrees in both historical and marine archaeology and even subspecialties in these fields.

Unlike the early days when one struggled to even identify artifacts, our literature provides a competent typology for practically every conceivable artifact class. There are also specialized

consultants who go with types of material culture—thanks in no small degree to the contributions of Parks Canada.

The founding of The Society for Historical Archaeology also led to the establishment of a formal publication outlet for articles devoted specifically to historic site and marine archaeology. Today we can be proud of the journal *Historical Archaeology* and its world class companion newsletter. Beyond these there is an enormous number of journal articles, books, and monographs that together provide the luxury of a vast literature. In fact, many major presses even offer series specifically devoted to historical archaeology.

The last twenty years have likewise seen a growing specialization in the practice of historical archaeology. From a time when most historical archaeologists practiced colonial or fur trade archaeology, today our colleagues might identify themselves as military, urban, plantation, or underwater archaeologists, all specialties which barely existed before the founding of the society. As the world of historical archaeology compounds itself, even the specialties are producing specialists, e.g., industrial archaeologists interested in mining or underwater archaeologists who specialize in marine architecture. Kuhn (1962) has noted that the proliferation of specialties is the sign of a maturing discipline.

Our colleagues have themselves changed. From a beginning where practitioners in the field were perhaps 99% white male, attendance at an annual meeting demonstrate a dramatic but still inadequate increase in the number of colleagues of different races, nationalities, and ethnic groups. Most notable, however, is the way historical archaeology has attracted women. Not only have the number of women who are practicing historical archaeology increased, but women have taken a very strong leadership role in the governance of The Society for Historical Archaeology and in the intellectual life of the discipline.

It could be argued with some justice that the rapid development of historical archaeology is simply a product of normal growth and development. The kind of expansion one might predict given a virtual pedagogical vacuum in the midst of an affluent society poised to construct an historic heritage. Although we should keep this context in mind, what we have witnessed, is

I believe, more than what we might expect when new field hands plow the *terra incognita*. Our growth and success has also been due in large part to the society's leaders who, over the years, have consistently supported high ethical standards, the cause of professionalism, the conservation of historic sites, and the dissemination of scholarly knowledge. The Society for Historical Archaeology and the development of the discipline is to a significant degree the product of planned growth.

Despite the extraordinary advances in the means by which we go about doing historical archaeology, I must confess a certain amount of disappointment in regard to opportunities still being lost. As I survey the field today, it seems to me that most of what historical archaeologists produce, though highly competent, merely flaunts an ability to use typological and histographic skills to produce a mountain of empirical data about the past. To put it another way, we have been so concerned with historic particulars that we have lost sight of our ability to use historical archaeology to address the larger cultural questions so worthy of our attention. To my regret, historical archaeology has developed as our founding, but not infallible, hero J. C. Harrington predicted it should—"as a handmaiden to history." If anything, historical archaeology could be characterized as much for its undeveloped potential as its past achievements. As a discipline it has the theoretical and methodological power to contribute to our understanding of past cultural process in ways that are not available to prehistory or to history. With few exceptions we have not met this challenge.

In making this observation I do not offer a blanket indictment of the work of my colleagues. Quite obviously there are some historical archaeologists who have maintained their focus on bigger problems but I do not perceive this as the overall trend.

#### Time, Event, and Cultural Practice

As historical archaeologists we share a topical orientation, that is the study of the material remains associated with sites occupied since the expansion of the Europeans into the non-western world starting in the 15th century. The various subfields of historical archaeology share a methodology which involves some combina-

tion of the use of excavated, written, and oral evidence for understanding the site as well as the dynamic natural and cultural processes which produced the sites and give them local, regional, and global context. Beyond specific sites we are also interested in developing common theory by which we can address regularities in cultural systems themselves. In order to achieve any of these goals, we must frame our inquiry in the context of three components—time, event, and cultural practice. It is to these that I turn my attention.

Of these three components of archaeological analysis, time would seem to have the most apparent meaning and to be the least controversial in theoretical terms. Thanks to the presence of descriptive documents and referenced diagnostic artifacts, historical archaeologists are usually able to control time with great precision. Unlike our colleagues in the field of prehistory where the chronology of events, which led to the creation of the archaeological record, is only rarely known with accuracy, historical archaeologists are often able to order depositional events to the year, sometimes to the month, and on occasion, to the day. With such knowledge historical archaeologists are usually confident and think of time in strictly chronological terms.

There are, however, other aspects of time which are not often appreciated. This is the context where historical archaeologists must find cultural contrast between the practice of the present and practice of the past. Unlike ethnologists who seek cultural distinctiveness in space, historical archaeologists, like historians of Western culture must seek cultural distinctiveness in time. Because sites occupied by “natives” of the western tradition are familiar to us, we implicitly assume cultural familiarity as well. Because the artifacts are familiar and because we may understand the language of the actor, and because we have contextualized excavated sites within our own historicity does not justify the collapse of time. To do so ignores or underplays the vast cultural differences which often accompany the passage of time within any particular cultural traditions. Put another way, it ignores the distinction between the emic and the etic. It is in fact axiomatic that cultural change is most rapid and fundamental as we

move toward the present. If not, it is at least a hypothesis historical archaeology is in a position to investigate. Within our own century and within our own North American society we might ponder the effect of the passage of time on behavior as behavior was in turn the product of vastly different beliefs about such things as germ theory of disease, economic dependency on kin relations, mobility, political nepotism, cousin marriage, pride in craftsmanship, and on conceptions of time itself. If the purpose of historical archaeology is to understand the cultural practices of the past, the worst mistake which can be made is to assume we somehow understand those practices *a priori*.

The division of time is, of course, itself a cultural construction which is quite arbitrary, yet also certainly has an immense effect on how we as archaeologists think about the past. Categories such as the “industrial age,” the “nineteenth century,” the “Civil War era,” the “roaring twenties” or our current favorite, the “new millennium” all constrain how we think about the relationship between time and events. Each of these characterizations of time is also an underlying or assumed hypothesis about a relationship among events. When we refer to the “industrial age” or the “industrial revolution,” we give priority to such factors as the development of social class, maximization of capital, industrial standardization in production, and the development of markets. There is no inherent problem here except insofar as all sites of the “industrial age” are not always best understood in the context of these specific categories. Slaves most often labored in a market system with strong class divisions, but are slaves motivated to maximize scarce resources? Can we assume they have unlimited demand for consumer goods? Perhaps and perhaps not. That which motivates the master may not motivate the slave.

Historical archaeology, like prehistoric archaeology, can use the passage of time as a variable in exploring the nature of events, their duration, magnitude, and interregional effect; thus, the implicit advantage of *la longue durée*. It is, after all, only through retrospect that we can observe the structure, process, and transformation in culture.

### The Edification of Event

The remains studied by archaeologists are the products of historical events and event sequences. Fogelson (1989), who devised a typology of historic events, defined an event as “a product of unique action which occurs at a specific time and place.” Events are different from material things although things in this context, most commonly artifacts, are themselves the products of events. The archaeological record is formed by a trajectory of events and thus the construction of a building, the burning of the same building, the burial of a deceased relative, the discard of trash, or the sinking of a ship constitute events or event chains. Sometimes events are the product of human action, both intentional and unintentional, and sometimes they result from natural occurrences such as fire, flood, or earthquake. Where events result in the deposition of cultural remains, an archaeological site is formed.

As the first step of understanding the archaeological record, it is necessary to understand the taphonomic relationship between event and the pre- and post-depositional history of the site. For analytic purposes, that is, relating events to each other in time or space, prehistoric and historic archaeologists have developed distinct methodologies for dealing with event. In the case of prehistory where it is seldom possible to arrange the order of events in precise temporal sequence and where few specific details are known relative to any specific event, analysis proceeds by attention to the general case. Thus, for example, the architectural remains of 20 prehistoric houses, each the product of poorly known event sequences, is best understood by a description of architectural norms and its more idiosyncratic variation. Historical archaeologists may draw these same generalizations but the ability to access written and oral records and impose tight temporal control on artifact types usually permits events to be understood on a much more specific level. An analogy may be found in the judicial system where a case built on physical evidence corroborated by an eyewitness testimony will always be stronger than one built on physical evidence alone. Considering the remains of 20 historic houses it is likely that the archaeologist may have access to construction plans, know who constructed the houses,

who even lived in them, and how and when they were finally destroyed. The precise control of event in the archaeological record is what provides historical archaeology with its methodological distinctiveness and its analytical power. It is also the reason that recent calls for the methodological rapprochement of prehistoric and historic archaeology should be rejected. A theoretical reunion on the other hand, is another matter.

It would seem at first glance that historical archaeology's fascination with event is a natural consequence of the availability of the archival record and working in the context of a familiar cultural tradition. This is not the case. The methodological treatment of event is a matter of theoretical choice. It is my contention that historical archaeology has seen a shift in analytical focus away from attempts to explain culture process to a fixation with the elucidation of event. This is an entirely unfortunate trend and not one unique to historical archaeology by any means.

To a degree, focus on event is a trend related to the fact that historical archaeology is still a rather young discipline. In its early years close attention to event was necessary to build a common methodology and develop the artifact typologies crucial to the development of the discipline. It is also true that historical archaeology is almost exclusively funded by a system designed to promote national or regional hegemonies. Thus, funding is largely tied to specific sites and prominent personages and often to the “reconstruction” of an idealized past for the purpose of recreation. In this way historical archaeology has been coaxed away from research which centers on general social theory, to research which fills in the details of specific and often predefined histories. Our very modest revolt from “heroic” archaeology, as inspired by the postmodern critique, has not released us from our fascination with event. We seem fond of justifying our disciplinary existence by telling ourselves that our mission is to tell the story of the economically and politically marginalized masses whose imprint is not to be found in our written histories. It is doubtful that even this noble cause, to adopt a phrase from Bernard Cohn (1980) “proctological” archaeology—archaeology from the bottom up, will free us from event driven research. In fact, both

“proctological” and “heroic” archaeology supports a kind of non-event eventfulness. That is, even though “events” may never have taken place, they are so plausible that they should have!

So what is wrong with event-centered archaeology? In the sense that knowledge of specific events, whether gained from written, oral, pictorial, or excavated sources provide the basic data of historical archaeology—nothing. Insofar as explanation of event becomes the end point of research—everything.

I hasten to add here that while this seems to be the general case in historical archaeology, it is not by any means the exclusive case. There are many excellent studies in historical archaeology in which events are incorporated into broader research agenda to address social process. Here without citing specific contributions I would simply point to studies which have been offered on the subjects of ethnogenesis, class structure, consumerism, urban organization, and plantation systems, and there are more. In the cases where historical archaeology has been extended beyond event it has given a glimpse of the power and relevance of historical archaeology for the future.

What is the evidence for my contention that historical archaeology has become obsessed with event? I would cite most of the papers and special publications to be found in *Historical Archaeology* and almost all of the papers offered at the annual conference of The Society for Historical Archaeology. First of all, much of what passes for historical archaeology is not archaeology. It seems perfectly acceptable to write or present papers that do not involve excavation or even artifacts. From my perspective, papers that deal with such topics as urban landscapes, trails and highways, the architecture of houses occupied by famous persons, or historical play acting, and like topics where there is no pretext of archaeology should be given at more appropriate conferences and published in more appropriate journals. In truth, these studies belong in the domain of cultural geography, urban planning, and social or architectural history. Certainly historical research is a necessary prerequisite of archaeological research design but it is not an end point. Much of event driven research presented as historical archaeology can be laid at the doorstep of attempts to understand the past without disturbing the soil. It may

be interesting but it is not archaeology. I do not, of course, contend that trails, houses, and landscapes are inappropriate topics or orientations for research in historical archaeology. After all, a house is an artifact situated on a site and connected by a roadway to a broader landscape. Research in historical *archaeology* should involve the archaeology of these topics.

Event overload may also flow from sheer lack of imagination, the absence of theoretical context, or in some cases, both. What can I learn by sitting through another paper or reading another article about the archaeological testing of yet another 19th-century farmstead in southern Wisconsin or Iowa or Pennsylvania? Should I be excited to learn the house was built in 1834 or that there is transfer printed whiteware in the privy? Perhaps I should be titillated when informed that a porcelain doll’s head indicates that the inhabitants had children and that extensive faunal analysis shows that lambs were slaughtered in the spring. Is this kind of event oriented archaeology poor research? Not at all. Does it deserve space in a journal or recitation at a professional meeting? Only with appropriate warning labels. It is time to remove idiosyncratic event oriented research from the flag pole of historical archaeology and to find something else to salute.

It should be the goal of each piece of research in historical archaeology to address larger problems and to seek conclusions which advance our knowledge of the cultural practices which are involved in the formation of sites. There is a way to make the remains of a 19th-century Wisconsin farmhouse relevant to broader problems but it must be contextualized as part of a cultural system.

### Cultural Practice

I start with the proposition that the ultimate purpose of archaeological research is to explicate the cultural practice of the past. These practices, or culturally derived behaviors, are those which in large part account for the configurations of archaeological deposits. Leaving aside the natural dimensions of the site formation process, sites are the result of events and event sequences. The order of event sequences and the relationship of one event to another in time and space is in turn the product of patterned behavior. It is the

discovery and explanation of recurrent patterns in the archaeological record which informs us about past cultural practice. As Binford (1977) has pointed out, the process of archaeology must be centered on transforming the static facts which constitute the archaeological record into statements which are related to the dynamic processes through which the record was formed. Raab and Goodyear (1984) as well as Schiffer (1988) have expanded on Binford's application of sociologist R. K. Merton's idea of middle range theory by suggesting in part, that levels of theoretical discourse must be appropriate to the question addressed. Here I follow Schiffer's (1988:463) suggestion that each hierarchy of archaeological principles contain high-, middle-, and low-level theories. The principles within each hierarchy can be related logically and each reflects an analogous degree of abstraction. In this sense, higher level theory subsumes middle, which in turn subsumes lower level theory because each is in turn more less comprehensive. Thus bridging arguments may be built between event and higher level problems.

Historical archaeology presents a good example of an attempt to explicate and test middle range propositions in South's work with "pattern recognition" which he advances in his influential book *Method and Theory in Historical Archaeology* (South 1977). Unfortunately, some historical archaeologists misused South's concept supposing that the object of pattern recognition was merely to find other as yet unidentified "patterns" in the archaeological record. In fact, South was hoping to advance and test propositions which explained artifact distribution in terms of discard behavior, that is cultural practice.

Returning to the discussion of event, it could be observed that the theory of event is at a lower level because unique historical episodes have high empirical specificity but low comprehensiveness. On the other hand, questions which contextualize event in regard to the operation of cultural principles have far greater comprehension. In other words, the complexity of the theoretical proposition must be appropriate to the kind of question being addressed.

Here again a contrast between the methodology of prehistoric and historic archaeology is appropriate. While there are no fundamental differences in the kinds of questions which may be asked of historic as opposed to prehistoric

sites, there is a major difference as to how these questions might be methodically phrased. This is because in the case of prehistoric sites all questions must be posed of the material remains, while in historical archaeology documents provide an important additional data source. If historical archaeology is to make any unique contribution to archaeology as a science, it must develop its own methodology, which builds on this distinctive feature in order to establish the relationship between data availability, data requirements, and problems in theoretical context.

#### A Method of Alternate Oppositions

It is important that the two complementary data sources available to historical archaeologists be used in empirical opposition. There is little if anything to be gained by using either artifacts or documents as a source of subjective confirmation of the other and in any case it must not be assumed that historical accounts are factually correct. Contending that the recovery of a key from the home of Ben Franklin confirms that it really is the home of Ben Franklin because Franklin used a key in his electricity experiment would be an unfortunate teleological error. Artifacts and historical documentation should be used independently as a means to objectively test propositions formulated from the opposing set.

Obviously it is not always possible to create oppositions appropriate to every question. This is true because both archaeological and documentary records are not only often incomplete in themselves but may not provide contrasting data sets for every question of interest.

It is the case that just as the hierarchy of theoretical levels are logically linked, so are the relationships between the hierarchy of theoretical level and the appropriateness of the way the artifact/document oppositions are formed. Consider, for example, that lower-level propositions relate to site formation processes which in turn hinge on unique events. How did specific historical actions result in specific archaeological configurations? For historical archaeologists these questions translate into those relating to event. Is this house ruin the one built by Adam Smith? Was it occupied between 1730 and 1745? Was it occupied as a domicile? Such

questions are best drawn from documentary, pictorial, or oral sources which may be used to form specific hypotheses. These will in turn generate predictive hypotheses as to qualitative and quantitative dimensions of artifacts which should be recovered by excavation. Concern may here be raised about the historic sites which do not have sufficient historical records to generate suitable hypotheses. In such cases the same methodology would be employed as if the site were occupied during the prehistoric era. General descriptive hypotheses about site, size, type, and duration of occupation are always available. To reiterate a point made earlier about event oriented research—it is a necessary and important first step but should never be an end point.

The next step is to consider middle-level questions which move beyond event to the processes of middle range theory. These are the “why” questions which move beyond the static record to the dynamic process. This is the level at which the linking relationships between artifacts and artifact sets and their behavioral correlates are built. Here historical and classical archaeologists maintain the methodological advantage of the written record as a complementary data set, but in the case of middle-level theory, hypotheses are best built from the artifacts. This is true because the archaeological record relating to middle range questions is a more objective, though perhaps a less complete record of actual behavior. At this point we can pose hypotheses which relate to such subjects as the discard behavior of the Smith household, practices relating to the acquisition of consumer goods or the status, gender, and age of its occupants. These hypotheses importantly involve the human actions and the temporal and spatial dimensions of artifact distribution which actions produce. Such hypotheses may be tested with quantitative or qualitative techniques which reveal the patterning of artifact distribution or with historical documentation or preferably both.

There is yet a higher order, more comprehensive set of questions which can and, if possible, should be asked of archaeological data. These are questions concerning the regularities of more complex and inclusive culture behavior which are situated in more expansive realms of time

and/or space. To return to the Adam Smith home we would be interested in questions such as how the architecture of the house relates to changes in the cultural landscape, how economic cycles reflect access to consumer goods and social status of its occupants. Questions of this nature, like specific questions relating to event, are best posed from documentary sources and tested with artifacts and structured details obtained by excavation. This is the case because higher level theory needs to be phrased in multi-site, regional, or even broader perspective.

To recap, the foregoing suggests that historical archaeology as a discipline is in the advantageous position of often having two complementary data sets which should be used as independent means to form and test hypotheses. Further, it is suggested that the nature of the artifactual and documentary record makes each particularly appropriate as a source for hypothesis formation if one considers the logic on which different levels of theory building is based. Given these observations, it must also be observed that what is urged is an ideal and not a dogmatic prescription. There are often cases in which the nature of the data or the questions of interest would indicate departure from the suggested order. Especially, it is not suggested that documentary sources be consulted after the fact since they are also part of a fundamentally inductive process in scientific inquiry.

## Conclusions

Certainly any retrospective view of historical archaeology as a discipline should recognize its remarkable progress in building the basic tools and methods which sets it apart as a new field of inquiry. Concern should be raised, however, when the very core of what makes historical archaeology a science *and* archaeology is drifting to its intellectual peripheries. While event provides the basic data whereby we can discover the past, it is not just the event which we should be intent upon discovering. Each event is a unique occurrence that in itself contributes only one piece of evidence toward understanding broad patterns of human behavior. In our fascination with event, historical archaeologists are certainly exhibiting impressive new skills

but are also replicating those to be found in sister disciplines.

Historical archaeologists need to rededicate themselves to the study of culture and behavior, to science, and to working with artifacts. Likewise, historical archaeology as a discipline could best develop its potential by realizing that its distinctiveness comes not from the fact that we work with sites occupied in a particular time period, but because we have a methodological advantage in the availability of complementary data sources. While this fact is no doubt appreciated, it has not been the topic of much theoretical or methodological discussion and it should be.

This paper suggests that the use of archival and artifactual data sets should, as a matter of methodology, be used in opposition to each other and that the nature of the opposition should be appropriate to the level of theoretical discourse, in order to provide a label—the method of alternate opposition. This method is suggested because while the theoretical level may become more comprehensive in terms of cultural organization, each level must also be part of a hierarchically organized logical system in which higher levels subsume lower ones. It must also be organized to draw on the strengths of various data sets as reflected in the organization of the cultural system and the kinds of questions being asked about it. Thus, event questions which are empirically loaded should be derived from the most detailed data set available, which is usually archival sources. These may then be tested with excavated data. When, however, we turn to middle-level theory which is predicated upon understanding past behavior we move beyond the historical specific to the culturally dynamic. Here the hypotheses should flow from the actual evidence of the behavior in the form of archaeological remains and artifacts. These can be tested by archival data and/or by internal

statistical means. Finally, it is suggested that hypotheses concerning higher-level theory which should be tested with data from multiple sites and in broader spatial and temporal dimensions should be derived from written sources. For historical archaeology to develop its full potential more intellectual effort needs to be devoted to its theoretical and methodological dimensions.

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## Attaining the Full Potential of Historical Archaeology

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### ABSTRACT

Historical archaeology is not adrift; rather through the collective contributions of generations of dedicated scholars it has matured into an inclusive discipline that seeks solutions to a wide range of problems regarding past peoples and their cultural expressions. While it is appropriate to seek to understand cultural regularities we must also explore the rich texture of variation expressed in the material patterns left behind. I agree with Charles E. Cleland's that new and rigorous methodologies must develop in order for historical archaeology to reach its potential. We need not constrain our creativity, however, nor the potential of thorough utilization of archival sources, by limiting ourselves to the construction of oppositions between the historical and archaeological record. Rather, our models should embrace an examination, and testing, of questions derived from the intersection of both sources of information. The full engagement of both history and archaeology in the framing of questions will lead to a more complete understanding of the dynamic nature of cultural expressions recoverable through archaeological inquiry.

Historical archaeology is not adrift, but it needs to concentrate and expand on its strengths to reach its full potential. In this statement one can find elements of agreement and disagreement with the connotations of problems entangled in Charles E. Cleland's assertion that historical archaeology is "drifting to its intellectual peripheries." Historical archaeology has undergone remarkable growth in the number of practitioners and the range of pertinent questions addressed. Moreover, it has become world wide in scope. Topics and areas that were once considered peripheries have become the centers of new networks of knowledge and communication. So long as our scientific and humanistic inquiries are pursued with the highest level of rigorous scholarship we are not adrift—though in nautical terms many of us may have come-about and charted new courses. Over the past decade historical archaeology has engaged new research questions and become more inclusive with regard to both participation and interpretation. These new directions build upon what has been accomplished and, at their best, integrate intellectual curiosity and the pursuit of discovery with a

responsible approach to public policy, public involvement, and education.

The field of historical archaeology has demonstrated remarkable growth since its early years. Cleland's positive assessment of the contributions of the founders of historical archaeology is well founded. For hundreds of historical archaeologists working in contract firms, government agencies, and universities the efforts of the likes of Fairbanks, Cotter, Kidd, Quimby, South, Woodard, Jelks, Smith, Salwin, Noël Hume, Fontana, and Gilmore (to name only the J.C. Harrington Medallist's of The Society for Historical Archaeology up to 1995, representing the first generation of historical archaeologists) cannot be underestimated. To Cleland's statement I must add the observation that a hallmark of the field has been its openness to new ideas and new generations of researchers. The first generation embraced the second, and the second the third. One of the things that attracted me to the field in the late 70s and early 80s was a willingness to entertain new ideas, gender equity, and diversity. The openness of the field is linked to its role as an "upstart" within the broader archaeological community. As an underdog within a field where prehistory was preeminent, historical archaeologists have taken a position of proactive advocacy as they worked on behalf of previously overlooked resources. In this regard, the challenge for current and future generations is to retain the graciousness and open mindedness of our predecessors as we address new problems and train succeeding generations of students.

I share Cleland's concern for the development of analytical methodologies to derive and filter our interpretations of the past but not his assertion that we construct our arguments through the opposition of archival and archaeological data sets. Without analytical tools we lose the power of comparative interpretation as we "focus research on unique events" without grounding the implications of our findings in a broader understanding of cultural processes. Like Cleland, I am leery of generalizations based on incidental attributes of individual artifacts without the establishment of an analytical grounding with which to evaluate the suggested context. I am

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likewise, disappointed when I read papers that are self defined as archaeological contributions but devoid of material findings from sites. I recognized, however, that the “acceptability” of such reportage may simply be a reflection of an analytical pause as our theoretical paradigms shift and we search for analyses that fit the complexities of new approaches to the past and the interpretation of material things.

It is not the opposition of historical and archaeological data sets that will pull the field forward to the next level of quality interpretation; but rather, the ability to better utilize and integrate the potential depth of information from both text and material based sources that will enrich our interpretations. To do this archaeologists have to become better historians—or engage more fully as an interdisciplinary field and involve, more fully, the scholarship and expertise of social historians to utilize the rich, and sometimes overlooked, detailed records available in a diverse array of archives. I have no fear that historical archaeologists doing better history will push archaeology to a more subordinate role in interpretations of the past. Particularly if archaeologists themselves ask significant questions that center on the diversity of living experiences of the past and seek more complete answers from history. What will set archaeology apart is as Charles Orser argues that fact that through detailed analysis of material remains “only archaeology has the power to resurrect the daily lives and cultural patterns of the invisible men and woman of the past” (Orser 1996:10). The best examples of historical archaeology will be those studies that maximize on the combined strength of historic documentation and archaeological analysis, as well as those that utilize pertinent corollary fields, from faunal analysis to landscape architecture, to explore and expand our understanding of the parameters of peoples lives. Our interpretations are inexorably dependent on the quality of our questions and the depth of our analyses.

I do not share Cleland’ concern that historical archaeology has drifted to its “intellectual peripheries,” as these very peripheries are the key to a comparative understanding of cultural variation, and the texture of the human experience. Rather, the problem is to promote an historical archaeology that does not hide behind

the shield of the combined disciplines of history and archaeology and do mediocre archaeology and bad history. In terms of the intellectual domain of historical archaeology, I argue that what historical archaeology can do best is bring forth new insights on topics that were once considered peripheral or lost to history, but in one or another way have been central to the study of anthropology—such as ethnicity, class, status, social structures, gender dynamics, and age. Historical archaeology has the potential of tackling questions that involve dynamic issues concerning culture contact, social interaction, and cultural transformation. To this list I add studies of economic conditions and the mode of production, but caution scholars to realize that there is no single question that fits best and answers “all” for any given site or research problem. Therefore, be open to new questions and do not through out the data that do not fit your model of what the past was “really like.” Furthermore, it is the utilization of the comparative diachronic perspective that brings forth the interpretive strength of archaeological inquiry into historical contexts.

Cleland suggests that he tried to direct his commentary “away from the discussion of specific theoretical positions,” unfortunately—or fortunately—depending upon your perspective, we are all bound by the theoretical structures by which we organize our ideas and ideals. Not surprisingly, Cleland’s own perspectives are decidedly theory bound, and his solutions are driven by definitions of culture and the search for patterns of behavior, or as he terms it “cultural practice” linked to the theoretical constructs of Binford (1977), Raab and Goodyear (1984), Schiffer (1988), and the applications of the processual approach articulated most notably in historical archaeology by Stanley South and his studies of “pattern recognition” (South 1977). South’s goal was arguably the explanation of “cultural practice” on the basis of explanations derived from the patterning and distribution of artifacts. The underlying assumption was that in characterizing the patterns of material use one could reconstruct past behaviors and the cultures they represented.

In my own work, I examine the role of creative choice and variation in relation to adaptive change. The emphasis of interpretation involves

transformative shifts rather than acculturation as it is all too often defined through models of assimilative replacement (Armstrong 1998; Deagan 1998). I see culture as dynamic, fluid, and ever changing, rather than as fixed “whole cultures,” and have attempted to develop flexible analytical models aimed at discerning patterns of behavior based upon the uses of materials rather than the formal order of product manufacturers.

To Cleland, the problem is vested in a perceived cessation in our search for “regularities of cultural practice.” While some may have stopped their search, having assumed that they have alternative answers, my own feeling is that patterns of behavior are at the core of any interpretation and that we must not stop our search for regularities—but must also come to understand the irregularities, or variations, as these may be the key understanding of culture change. I am particularly fascinated with perspectives on the nature of culture, culture change, and particularly in the significance of variation in cultural behavior and its resulting patterning.

This then leads me back to an element of Cleland’s position paper with which I wholeheartedly agree. Namely, that we must define the relevance of our studies of the idiosyncratic actions of people, events, and places of the past and engage in discussions of broader patterns of behavior at the regional and global level. Furthermore, we must define the context the specific findings at a site within a broader cultural and theoretical context. The ultimate conclusions and results that I would expect, however, are not law like generalizations about specific forms of behavior in the classic Binfordian sense, but rather the explanation of how people will integrate creative variations in patterns in order to successfully negotiate the inherent uncertainties of life in a temporal continuum. It is important to realize that there is value in knowledge gained at all levels of interpretation, be they specific examples of case specific cultural, middle-level questions that examine culture process beyond a event that explore the conditions of comparable or variable response (aka middle range theory), or more abstract interpretations involving complex sets of relationships and expressions of behavior. As historical archaeology moves forward one can expect

that our explanation of the past will increasingly shift to present the diversity of cultural contexts and the multiplicity of human experiences encountered in the archaeological record. Given the nature of archaeological data and its complex combination of people and time one can expect a rich array of variation embedded in any discernable pattern of behavior.

It is unfortunate that historical archaeologists are asked to define what they can do that historians cannot, because an archaeologist who professes a knowledge of the techniques of historical archaeology must have an understanding of both general history and the more specific history affecting those who once lived at a site(s). In defining interesting questions to explore using archaeological methods and techniques one should draw upon what is known, and not known, from archival and archaeological sources. Certainly, as Cleland suggests, what is thought to be historically known should be recast as questions and tested against the archaeological record, but it would be an unfortunate limiting of the potential of archaeology, and history, to limit ones questions to tests of these perceived “knowns,” or to place undue importance on staying within the bounds prescribed by such oppositions. Rather, let the research questions of historical archaeology loose on the multitude of “hows” and “whys,” but let the questions derive from the researcher’s ability to test the boundaries of knowledge—and by all means ask new, hard to answer, questions of both the archival and archaeological materials.

An argument for the potential value of historical archaeology is masterfully presented in James Deetz’s *In Small Things Forgotten* (1977, 1996). Deetz sets the stage for the use of archaeology to gain an enlightened view of history and captivates the imagination by presenting concrete examples that set forth the possibilities of archaeology. In illuminating what archaeology can add to our perception of the past he states that “it [historical archaeology] stands in contrast to the study of history or the decorative arts not so much in terms of subject matter as in terms of its analytical approach” (Deetz 1991:37). Deetz intertwines the skill of a story teller with the rigor of an analytical, scientific, approach. The net result is that the reader,

be they new to the field, or scholars with well worn trowels and the letters worn from their computer keyboards, find a common ground in an understanding of what archaeology is capable of doing. Archaeologists have the potential of retrieving the all important details of the past—artifacts that were used and discarded, the ruins of foundations, and the former spatial layout of farmsteads and towns—all forms of “small things forgotten” that more often than not have escaped historical mention. What characterizes quality archaeology is the pursuit of the past following explicitly defined models of interpretation and analytical procedures based on theories concerning culture and society. I suggest that without such grounding the results of historical archaeology can become “the most expensive way in the world to learn something we already know” (Deetz 1991:1).

Historical archaeology has the potential of providing significant insight in the study of what, by broader archaeological standards, is a fairly recent past. Its strength has been, and will continue to be, in the area of comparative analysis and the implications of culture change and social interactions or relations. Personally, I find the opportunities to recover and illuminate the lives and lifeways of persons who were not caught in the spotlight of history to be the most fascinating. The reconstructed stories of the lives of lesser known people, poorly represented underclasses, and undocumented interactions provide clear and exciting challenges that are worth the time and energy of archaeological exploration. The exiting element of historical archaeology is in its potential to add the concrete and real dimension of the materials actually used by people in the past including what James Deetz describes as the “small things forgotten” from innumerable sites and contexts to whole towns and villages that can be reconstructed in peoples minds through the reading of a thorough and illuminating site report or synthetic volume.

In espousing a rigorous approach to problem solving and in the construction of analytical models with which to interpret the past Cleland and I agree. But where he emphasizes a search for the regularities of culture I am enthralled by the explication and explanation of the internal variations and textures of a society and their

manifestation in cultural transformations that can be seen through the variable lenses of time. Moreover, where Cleland wishes to set archaeology in opposition to archival data my view is that one should maximize the information that can be derived from each and use each to assist in answering questions regarding the dynamics of culture. Is there room for both approaches? I say the answer is yes, because each is based on a premise of interpretation through analysis and neither approach needs to exclude the other. Which is more effective for cultural interpretation and the future of historical archaeology? Time will tell the answer.

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## Comments on “Historical Archaeology Adrift? A Forum”

Lu Ann De Cunzo

In “Historical Archaeology Adrift?” Charles Cleland modestly characterizes his purpose as “offer[ing] some observations on the current status of the field of historical archaeology.” His essay in fact offers readers a vision of the goals and methodology that will enable historical archaeologists to meet the discipline’s potential in the next millennium. Cleland’s vision has taken shape over a long and distinguished career, in which he has played a significant part in crafting our discipline of historical archaeology. He remains staunchly committed to a future in which historical archaeology will address “larger cultural questions,” “cultural regularities,” and “cultural practices” in order to contribute to an “understanding of past cultural process.” In this essay, he proposes a “method of alternate oppositions” appropriate to the scope and scale of the task, and in doing so he firmly positions the discipline “in the context of scientific inquiry.”

Cleland begins by expressing concern that students and younger historical archaeologists have not recognized the accomplishments and progress that have marked the 30-plus years since the founding of The Society for Historical Archaeology (SHA). I concur, and preparing these comments has forced me to think about why. I find it easy to forget, and thus to minimize in teaching, the growth historical archaeology has experienced because, as Cleland demonstrates, the essence of our ongoing dialogue about the whats, hows, and whys of historical archaeology has changed surprisingly little. In this forum essay, Cleland strives to jumpstart an historical archaeology stalled in a “handmaiden to history” mode. He elaborates the position he took at the 1987 “Questions that Count in Archaeology” SHA plenary session, that the independence of written/oral and archaeological data sets enables archaeologists to truly employ the scientific method, drawing hypotheses from one data set and testing them with the other (Cleland 1988:15). Here he speci-

fies a “method of alternate oppositions” designed to take advantage of historical archaeology’s special position at the intersection of multiple evidentiary sources, looking back at the *longue duree*.

As Cleland explicates his proposed method, and indeed, throughout the essay, he directly and indirectly raises those same central themes that have framed the discourse in historical archaeology for more than 30 years. He structures the “method of alternate oppositions” (to which I will return) in the dialogue of opposition with which historical archaeologists have become so familiar. As I read and reread his essay, a string of parallel pairs of oppositions emerged as embedded more or less explicitly in the text:

History–Anthropology  
 Humanism–Science  
 Event–Culture  
 People–Patterned Behavior  
 Subjective–Objective  
 Emic–Etic  
 Individual–Society  
 Document–Artifact and Site (Archaeological record)  
 Small–Large (as in the goal of addressing “larger cultural questions”)

Descriptive–Explanatory  
 Finally, the oppositional hierarchy of Low–High that for Cleland encompasses all the others in a summary evaluation of our intellectual enterprise—the low level of event, the high level of cultural process.

This array of oppositions, along with many others with which each reader can certainly embellish the list, has constituted the discursive legacy of historical archaeology. These oppositions have mapped the terrain of a discipline defined by an uneasy straddling of boundaries. Their continual evocation is easily recognizable as marking a process of identity creation and disciplinary boundary maintenance, an endeavor in which historical archaeologists have been perpetually, some have suggested obsessively, engaged.

In discussing one of three essential components of historical archaeological practice—time—Cleland reminds readers of a critical point: conceptions

of time and chronologies grounded in conceptions such as the “industrial revolution” are *cultural* constructs and as such they shape our thinking about the past. They open up productive vistas even as they shut out our view of others. It is equally important to remember that the oppositions cited above are also cultural constructions, and equally useful to examine the impact they have had on our thinking about what we do as historical archaeologists, how we do it, and why we do it.

An opposition of a different kind also underlies the practice of historical archaeology and indeed that of all our sister disciplines, method/theory. In Cleland’s vision, method transcends theory. He intends the “method of alternate oppositions” to have broad applicability, concluding that it is not necessary to discuss it in the context of any “specific theoretical positions.” I cannot readily divorce method from theory, nor in practice does Cleland. Readers possessing only a passing familiarity with the scholarship of historical archaeology will recognize the theoretical influences structuring Cleland’s privileging of an objective scientific archaeology that seeks to explain patterned behavior in the archaeological record. In order for the reader to place my comments on Cleland’s vision in context, I begin with a brief foray into the theoretical discussions that have shaped my recent thinking. I do not intend to present a view of the discipline in sharp opposition to that Cleland proposed. I have been encouraged, however, by some of the efforts to “reinvent” archaeology (Beaudry 1996) that have discouraged Cleland and led to his concern that our field is adrift. These differences in approach do affect my response to Cleland’s vision of historical archaeology as a process of explanation that moves “up” from event to cultural practice by applying a method of hypothesis development and testing that alternately privileges documents and artifacts.

I envision historical archaeology as a transdisciplinary endeavor in search of the stories of the modern era, or as Mary Beaudry (1995:1) elegantly stated it in a volume focused, not coincidentally, on the “written and the wrought,”

the spread of European colonialism in the early modern era, of resultant interactions with indigenous populations (incorporating both indigenous and European responses and accommodations), of *in situ* development of multicultural colonial and post-colonial communities

in various parts of the globe, of the effects of capitalism and the industrial revolution, of post-colonialism and its effects, and of manifestations of social inequality, ethnicity, gender, and race relations. . . .

Building from disciplinary roots in anthropology and history, historical archaeology centers both people and culture. People think and act in cultural terms, through symbolic languages and performances that communicate and give meaning to the self, others, and the world. People live in complex relationships with and within their world, engaged in a continual process of perceiving and remaking the environment. They create, act on, and are influenced by cultural systems shaped by biological, technological, social, economic, ideological, and historical forces. The past lacks unity, as people’s actions and cultural logic embody complex and confusing ambiguity. Its recovery requires constant attention to context. People’s beliefs, actions, and material productions have meaning only in their cultural, systemic contexts, each incorporating many scales, from moments to a millennium, from the individual to the global. Neither “heroic” nor “proctological” in perspective, the historical archaeology I envision aspires to hear the multiplicity of voices from the past, to privilege no single perspective. It is a “postmodern” historical archaeology, interpretive, contextual, informed by social theories that question “totalizing frameworks” (Marcus and Fischer 1986:9). It is, ultimately, historic ethnology (Schuyler 1988), comparative “ethnography in retrospect” (Beaudry 1995). Perhaps most important for pondering Cleland’s formulation, it draws from post-processual and post-structural archaeologies a concern with breaking down oppositional dichotomies and examining the relationships between the two sides (Hodder 1989:70).

Two oppositions in particular structure Cleland’s vision: the hierarchical opposition of event and cultural practice or process, and the related, methodological opposition of artifact and document. Mark Leone and Constance Crosby (1987) first posit an historical archaeology-specific middle-range theory that substitutes the documentary record for the prehistorian’s ethnographic record in opposition to and independent of artifacts and other archaeological remains. The application, however, differs from Cleland’s method of “alternate oppositions.” As Leone

and Potter (1988:14) later explained, historical archaeologists should create “descriptive grids” from the documentary record, “against which to array the archaeological record. One creates the framework, derives from it expectations of the archaeological record, and uses the deviations from the expectations . . . as the basis for a new set of questions about the archaeological record *and* about the documentary record as well.” Cleland proposes that historical archaeologists apply the method at the “middle range” and at the levels of chronicling events and illuminating the processes of culture practice, alternately deriving hypotheses from the documents and the artifacts in their archaeological context. Both assume, however, a strict albeit complementary independence of document and artifact/ site that several archaeologists and material culture scholars in other disciplines have recently urged us to explore.

Historical archaeologists have always acknowledged a certain ambiguity concerning the relationship of documents and archaeological artifacts, in part inherited from our mixed ancestry in history and anthropological archaeology. Charles Orser (1992:95) described the debate over “whether artifacts are historical documents or whether historical documents are actually artifacts” that has characterized the history of the discipline. As students, many of us learned that documents represented subjective, intentional, self-conscious, ideal or at least prescriptive statements, while artifacts constituted the unselfconscious product of actual behavior, thereby embodying a greater measure of objectivity. At the same time, James Deetz and others applied the structuralist’s linguistic model/metaphor to the study of artifacts, arguing for a deeply embedded cultural connection between language (including its written versions) and artifacts.

More recently, the influence of literary theory on archaeology renewed this interest in examining the “artifact as text.” Yet even ardent advocates of the approach such as Ian Hodder consider the important differences between linguistic and artifactual texts. Hodder (1986:123; 1989:72-73) emphasized several distinctions: 1) artifact meanings are simpler than those capable of expression through language; 2) artifact meanings are less logical, more use-bound and

contextual, and thus often non-arbitrary; 3) artifacts are often more practical and their meanings subconscious; 4) material symbols are more polyvalent, ambiguous, and nonlinear; and 5) most artifacts studied by archaeologists have a long duration, making them effective and yet less flexible vehicles through which to control meaning and hence society. These differences effectively delineate the limitations of artifacts as vehicles of communication when contrasted with language, clearly not the prime intent of archaeologists working in this idiom.

In papers contributed to a *festschrift* for James Deetz (Yentsch and Beaudry 1992), Martin Hall and Rhys Isaac celebrate the differences, reassuring historical archaeologists of the rightful place of privilege the artifact occupies in our practice. Hall’s (1992:380) “discourse analysis” of architectural artifacts untangled the multiple, ambiguous meanings of their symbolism. Such material symbols “cannot be verbally defined by those who use them,” and thus they offer “the potential to reveal the world beyond documentary evidence and oral tradition.” They give “objective” form to social interaction such that “the material world *is* social relations.” Isaac’s (1992) view expands beyond the social to the universal, as he argues that the world of artifacts records people’s images of the cosmos in ways that their writings cannot.

In contemporary archaeology, then, the concept of “discourse analysis” has reformulated our understanding of the distinctions between artifact and document, and effectively challenged some of our earlier conceptions. Years before, however, James Deetz had tried to explain that the key issue was not whether documents were artifacts or whether artifacts were documents. In a famous passage, he urged us to remember that Siberian fish hooks, Freaky cereal and the little band of plastic Freakies which dwells in the box, standing rib roast, jumbo jets, topiary, tattoos, and college band formations on a football field were *all* material culture. Every aspect of the environment that people have purposely shaped according to cultural plans comprises material culture, and that of course, includes written documents (Deetz 1977:10-11). A consideration of both document and artifact as material culture, and as “image,” a representation of the world as it is or ought to be (Little 1992:219), blurs the

boundaries we have constructed between them. It leads me to question the viability of Cleland’s proposed “method of alternate oppositions.”

A rich literature in archaeology and the field of material culture studies explores the notion that all material culture forms are active cultural expressions that communicate meaning and enable, and indeed inhibit, action. Creating and using documents, artifacts, buildings, landscapes, graphic images, and performances; people make a living, negotiate social and cultural identity and interaction, and bring order and sense to the universe. As we have seen, they do so differently in verbal and non-verbal media. There are also substantial differences, however, between a trash pit, a butchered cow bone, a teacup, and a courthouse, which are all situated on the “artifact” side of Cleland’s dichotomy. Yet these differences do not compromise the underlying unity of material culture. All material culture requires a cultural competence to produce, use, and comprehend. All material culture embodies some ambiguity and lends itself to multiple interpretations, in the past and in the present. The design and construction of artifacts like houses, tin-glazed chargers, and cities result from purposive and self-conscious as well as unconscious thought and action, just as the design and construction of a newspaper article or probate inventory do. These thoughts and actions are simultaneously individual, social, cultural, and historical.

All material culture is the product of action. The production, exchange, use, destruction, and disposal of all material culture forms consist of chains of events, or actions, as Cleland explains. Furthermore, Cleland and I agree that these events are themselves the enactment of cultural principles, in other words “cultural practice.” These interconnections lead me to close some of the hierarchical distance that Cleland has proposed to open between “event” and “cultural practice/ process,” while accepting as given our need to understand the taphonomic history of the archaeological sites that we study. Beyond taphonomy, once again I have been influenced by the postmodernist turn in archaeology and historical anthropology and believe that the unique and individual coalesce with the cultural in a dynamic process to produce each event-action. It is not the separation and stratification

of individual event and cultural principle that will serve us. To put it much more simply than postmodern social theorists have, we must integrate them into a dynamic theory of culture that explains continuity (pattern) and change (process) in terms of each other and of human agency.

If we do not build our practice on a three-step ladder that rises from event to cultural process, then we must reconsider Cleland’s three-step “method of alternate oppositions.” Because of both the differences that scholars imagine distinguish verbal (including written) and nonverbal (“wrought”) media and the features they share as material culture, I would propose a methodology that does not oppose documents and artifacts in the way that Cleland outlines. On one level if, as for example Hall (1992) and Isaac (1992) suggest, non-verbal material culture symbolically encodes understandings of social order and the cosmos that lie beyond the ability of the verbal to express, can we really “test” one against the other? If, on another level, documents and artifacts result from similar combinations of intentional and subconscious thought and action at once individual and cultural, what would we benefit by opposing a set of documents and artifacts that we would lose by comparing and contrasting a set of buildings and foodways?

Mary Beaudry (1980, 1988, 1996; Beaudry, Cook, and Mrozowski 1991) and Anne Yentsch (1975, 1988, 1994) especially have demonstrated to historical archaeologists the value in taking an analytical, ethnographic approach to documents that moves beyond “historical research” to “documentary analysis” undertaken from both etic and emic perspectives. Methodologically, this involves posing the same sets of emic and etic who, what, when, where, how, and why questions in analyzing a trash pit, a gin bottle discarded in the pit, the fence that once enclosed the pit, a contemporary property plat that delineates neither pit nor fence, and a contemporary treatise on the hygienic benefits of trash pits over open middens. In this way we can construct the many contexts so central to Cleland’s vision.

Many of my colleagues have already explicated an alternative method to the “method of alternate oppositions” (Deagan 1988; Schuy-

ler 1988; Yentsch 1994; Beaudry 1995, 1996). The method requires the integration of all the evidence—verbal, nonverbal, above ground, and below ground—in a process of comparison and contrast at multiple scales of analysis, seeking to explain both the congruities and incongruities that emerge through this construction of context. Such an integrating methodology does not ignore the contradictions and ambiguities between sources. Cleland rightly advocates that we attend to all of them. It does, however, allow the researcher to avoid the pitfall of holding the differences between artifact and document accountable for all the disjunctions. It encourages us to seek the explanations in the phenomena such as gender, class, race, generation, and ethnicity that shape culture practice and process as people respond to hunger, war, environmental disaster, disease, and the “discovery” of new peoples and new places.

Understanding the cultures of any time, place, and social group and situating them in the context of the *longue duree* requires that all available evidence be questioned, analyzed, and evaluated from a variety of perspectives. Historical archaeologists should be careful about the boundaries we construct between the “us” studying material culture below the ground and the “them” studying material culture above the ground surface. I welcome papers at the SHA annual meetings that integrate diverse forms of evidence to address research questions or introduce new approaches to examining specific material culture forms. An open exchange and dialogue among all those who study the material remains of the people, places, and time period that define historical archaeology will enrich “our” interpretations and “theirs.” It will encourage more historical archaeologists to refine an emic and etic methodology that transcends the limits imposed by placing too much emphasis on the opposition of event/ cultural practice and document/ artifact and not enough on the interplay that links them. The SHA annual meetings, *Historical Archaeology*, and other society publications provide the perfect venue to promote this transdisciplinary approach to historical archaeology, an approach that does address the “cultural questions so worthy of our attention.”

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## Historical Archaeology, with Sails Set and Tacking Into the Wind

Gregory A. Waselkov

Archaeologists are, by and large, pragmatists. Not readily enamored of programmatic theories, we tend to steer a middle course, borrowing and adapting and improvising as needs be from science and humanism. This is particularly true in the field of historical archaeology, where the methods of two scholarly disciplines must function together for us to achieve optimal research outcomes. Consequently an inevitable, but constructive, tension will always exist within our hybrid field. I welcome Charles Cleland's thoughts on this topic and share some of his concerns.

A glance back at the earnest (and sometimes amusing) debate between historians and anthropologists recorded in the 1967 *Conference on Historic Site Archaeology Papers* (South 1968) reminds us of the venerable roots of this interdisciplinary tension. Many of the concerns expressed then by our founders have long since been resolved, as Cleland notes in regard to issues of professionalism and training and methodological expertise. But the overall goals of our discipline remain a matter for debate. What do we hope to accomplish with this tool called historical archaeology?

I call our discipline a "tool" because I have always considered archaeology as a means to an end. My own research interests focus principally on the colonial era, so I try to employ archaeological methods and theory to address and resolve problems of anthropological and historical significance regarding that period. Depending on the problem at hand, however, archaeology does not always offer the best means, and seldom is it the only means, to achieve my purpose. In his very fine study, *Rites of Conquest: The History and Culture of Michigan's Native Americans*, Cleland (1992) demonstrates the same eclectic approach to the past, one shared by many others in our discipline.

For this reason, I have always been amused at the outrage generated by J. C. Harrington's

(1955) characterization of historical archaeology as "an auxiliary science to American history," and Ivor Noël Hume's (1964) later taunt, "handmaiden to history." Of course, no one today wants to be considered a handmaiden, or even an auxiliary. Subservience is out of fashion. Such apparently disparaging phrases seem much less objectionable, though, if we take into account the intellectual climate in which Harrington struggled to legitimize a field dominated at the time by architectural concerns. I suspect there would have been few objections in the 1950s if he had characterized prehistoric archaeology as an auxiliary science to culture history. Both disciplines, prehistoric archaeology and historical archaeology, have long since evolved far beyond their original narrow bounds. Today nearly everyone acknowledges that all approaches to the study of the human past can properly benefit from complementary archaeological inquiry. Whether the role of archaeology is major or minor depends on how appropriately archaeological data and methods apply to the question at hand.

The interplay between historical documents (written records, images, recordings) and archaeological data clearly stands at the crux of the issues Cleland raises. He correctly insists that effective historical archaeology demands sophisticated analysis and interrelationship of the two data sets. A few years ago, Kathleen Deagan (1997:4) noted the serious imbalance, caused by "relying largely on either archaeological or historical information," that typifies so much of our literature. Cleland strikes a resonant chord in his critique of conference papers and journal articles ostensibly about historical archaeology that inexplicably fail to mention archaeological data or neglect to address questions aimed at any level of generality. These have been continuing weaknesses of the discipline.

I would extend this criticism a step further. A strong tendency exists among archaeologists of all sorts to favor historical evidence over archaeological evidence. Prehistorians have sometime confided to me that they envy historical archaeologists for the interpretive advantages we presumably enjoy in our historical knowledge

of specific site occupants, knowledge that might range from simple demographics to, in rare cases, introspective reflections on individual lives. Any experienced historical archaeologist could, of course, relate instances in which apparently reliable, site-specific historical “knowledge” turned out to be misleading, because it was either simply erroneous or, at least as frequently, highly biased. Yet I am repeatedly amazed to hear conference presentations and read journal articles in which authors revise their site interpretations solely on the basis of facts gleaned from belated historical research.

Why do we, as a profession, place so little confidence in our archaeological descriptions and explanations unless they are bolstered and confirmed by historical documentation? Why is historical knowledge so often privileged, with veto power over our archaeological conclusions? I am indulging in hyperbole to some extent. Many fine archaeological reports on historical sites do treat historical and archaeological data as independent sources of information on the past. But I suspect that every one of us, in a momentary lapse of critical rigor, has given a nod to an historical “fact” or two that deserved closer comparison with the material record. This unfortunate tendency, I think, derives from our still rudimentary understanding of how archaeological sites relate to the activities that occurred on them. If our archaeological interpretation of a site can be substantially swayed by the discovery of historical evidence concerning the occupant’s ethnicity or gender or occupation (as it assuredly must be, given the primitive state of our discipline’s theoretical development), then I submit that we still have a great deal to learn about interpreting the archaeological record. One might respond that this is precisely the strength of historical archaeology, to provide us with archaeological data linked to historical information that is otherwise unobtainable. Without more efforts at generalizing from the myriad of specific cases, however, we lack the theoretical structure necessary to support sound archaeological inferences, and therefore we repeatedly fall back on the available historical record, to the event rather than the cultural process, in Cleland’s terms.

So I was pleased to see Cleland’s hierarchical guidelines, which can serve as a useful checklist that reminds us of the full range of data that

our discipline generates and the full range of interpretations that our discipline requires. At the most specific level of concern, the type and arrangement of artifacts in a feature can provide the basis for a powerful test of historically derived hypotheses developed to explain the nature of that feature. If this analytic step alone became routine, historical archaeology would gain enormously in credibility with other disciplines. Historians who might otherwise be inclined to use our research find little of interest when historical archaeologists ignore the independent nature of their data and simply correlate material residues with recorded, but otherwise unverified, historical actions.

Likewise, I essentially agree with Cleland’s characterization of middle-range and higher level theory building in historical archaeology, and the alternating roles for archaeological and historical hypothesizing in each. However, I find something missing. There has always existed a strong urge among historical archaeologists to understand the past in humanistic as well as scientific terms. This undoubtedly accounts for the popularity of story telling in recent years. Writing plausible fictions should, I think, remain clearly distinct from historical archaeology. To confuse or meld the two would dissipate the considerable hermeneutic value of the scientific method applied in our field. As Ian Hodder (1999:137-147) has recently reminded us, however, we can still appreciate the role of the individual and individuality in past cultures without loosening all tethers to material evidence. Widespread popular interest in Ice Man and the newly discovered Andean sacrificial mummies derives from their capacity to convey a sense of life and death at a personal level. While we strive, as scientists, to place the individual in cultural and historical contexts, we can also recognize the edifying value of a personal story well told.

Cleland’s guidelines suggest how difficult our profession is. Unless we are fortunate enough to find a congenial historian with whom to collaborate (Wood 1997), most of us must try to master the skills and subject matters of both disciplines. It is an audacious undertaking, but the potential payoff is great. No one understands the range of possibilities open to historical archaeology better than we do. Two decades ago, a major synthetic volume such as Eric

Wolf's (1982) *Europe and the People Without History* contained few citations of the historical archaeological literature. Now social historians and ethnohistorians of the colonial and antebellum Southeast, my own research region, routinely refer to archaeological studies of slaves and Native Americans of those eras to inform their own interpretations (Berlin 1998; Usner 1998). I am not suggesting that we seek validation of our field in the frequencies of reference citations by historians. I do think, however, that our independent research is beginning to produce some insightful and even profound interpretations of the past that can inform other disciplines and interest the public. If we pursue this course, constantly tacking between historical port and archaeological starboard, historical archaeology need not seem aimless and adrift in a sea of particularities and anecdotes.

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## Comments on “Historical Archaeology Adrift”

Donald L. Hardesty

Chuck Cleland is truly one of the pioneers of historical archaeology, and being asked to comment upon his assessment of the field that he helped to create is an honor. “Historical Archaeology Adrift” begins with a favorable overview of what has happened so far in the short life of the discipline. It would be difficult not to agree with the trends outlined by Cleland. They correspond well, for example, with changes taking place in the programs of the Society for Historical Archaeology conferences that I have attended between 1974 and 1999.

The main purpose of the article, however, is to explore the ways in which Cleland believes the practice of historical archaeology today to be off-course and either completely adrift or heading in the wrong direction. He finds fault most of all with the field’s obsession with the description and interpretation of unique historical events, truly the “handmaiden of history” envisioned by J. C. Harrington. Is he correct? Certainly. Whether or not this is a bad thing, however, whether the archaeology of historical events indeed has become the “Frankenstein” of historical archaeology, is likely to be a more contentious question. Cleland’s criticism of event-oriented archaeology lies in his conception of the ultimate goal of historical archaeology, which he believes to be, like all archaeology, the search for regularities in the cultural practices of the past. In this he follows the guiding light of anthropological archaeology and its scientific foundations. Within this conceptual framework, historical archaeology often does not qualify as science because of its preoccupation with event and consequent failure to explore the regularities of past cultural practice. That it does not, of course, will be welcomed by those who see historical archaeology as a humanities rather than a science.

Cleland is absolutely right in finding historical archaeology often guilty of not going beyond the historiography of events. Without question

historical archaeology needs to be connected more strongly to archaeology in a common goal of exploring and understanding the patterns of past cultural practices. This said, he probably also would agree that historical archaeology still can be conducted within a wide range of theoretical and interpretative frameworks not visualized in the heyday of anthropological archaeology. The role of history itself is a major difference. For years, the scientific disciplines discounted historical events and patterns as irrelevant “noise” in the search for the “signals” of ahistorical general laws and principles. Now historical events often play the key role in the explanatory frameworks used to search for regularities even in such hard core science fields as chemistry and biology. Ecologist Stephen J. Pyne’s (1995) fascinating book *World Fire: The Culture of Fire on Earth*, for example, takes an explicitly historical approach in understanding the role of fire in the development of regional biomes. Another example is the development of chaos theory to explore the connections between unique historical events and general laws and principles of the universe. Historical archaeologists, therefore, should consider taking advantage of their strength in having multiple sources of information to acquire detailed knowledge of events to work on the development of “event-centered” theories of cultural practice. Maybe we can have the best of both worlds.

Cleland also argues that much of the current practice of historical archaeology, in addition to not being science, is not archaeology. He makes the point that much of what passes as historical archaeology involves neither excavation nor artifacts. Certainly most of us would agree with him that much of what is published in the journal *Historical Archaeology* or presented at SHA meetings is not traditional dirt archaeology. It would be difficult, for example, not to notice the large number of papers on the program of the last SHA conference that had nothing to do with dirt archaeology. Still, the extent to which archaeology has to be excavation is debatable. Archaeology today certainly accommodates many approaches to the past that do not necessarily involve excavation, such as ethnoarchaeology,

remote sensing, landscape archaeology, and material culture studies. They are, however, methods that help us interpret archaeological data rather than independent sources of information about the past in the same sense as written documents or artifacts recovered from buried deposits. To what extent should papers and articles on method be included in our meetings and journals? Cleland, of course, is lamenting the scarcity of research in historical archaeology that has to do with broad interpretations of the past and that is reasonable. We do need a better balance. Another related issue is preservation. Certainly our data base is in the dirt, but noninvasive methods of data recovery, when possible, better conserve sites as repositories of data than does excavation. Still, the technology of noninvasive archaeology today is yet undeveloped to the point that excavation usually is still the only practical method of recovering most archaeological data.

Finally, Cleland offers the "method of alternative opposition" as a prescription intended to steer historical archaeology on a more fruitful course. He observes that the uniqueness of historical archaeology lies in its multiple and complementary sources of historical information rather than in its focus upon the time period of the modern world. In this sense, we are much more closely allied with the archaeologists of ancient civilizations, who also work with complementary data sources, than with prehistorians. We also may be more closely allied with those historians who work with both oral and documentary sources. We mostly ignore, however, what is happening in those allied fields. Just a cursory review of the literature cited in

several recent issues of the journal *Historical Archaeology*, for example, found almost no references to the vast methodological literature of any of these disciplines. Developing some of the methodological opportunities of historical archaeology discussed by Cleland could come from closer working relationships with other text-aided or multiple data base fields in archaeology. Our interest in the modern world somehow makes us shy away from those who study more ancient civilizations with equally diverse sources of information and, therefore, very similar methodological strengths. We probably have a lot to learn from the methods of iconography and art history and from the methods of oral history, which use both texts and oral testimony as sources of historical information. It is time for the SHA to take on the task of organizing an interdisciplinary conference on the use of multiple sources of information to interpret the past. The conference should bring together historical archaeologists, art historians, epigraphers, Iconographers, oral historians, classical archaeologists, Egyptologists, Sinologists, Assyriologists, and Mayanists, among others. Hopefully, all of us would benefit and thank Chuck Cleland for bringing us back to this methodological issue.

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## Historical Archaeology Adrift?: Comments from CRM/West

When asked to ponder on Cleland's essay, the first thing I did was to look back at the inaugural Forum (Hardesty et al. 1999) to see the level of discourse and rejoinder. It proved to be a dramatic reminder of how similar last year's assessments and predictions by Hardesty and the commentators are to the thoughtful observations offered here. Both illuminate problems in the discipline but also its strength, both of which may well be underestimated.

Chuck's perspective on the growth of historical archaeology as a discipline is a welcome reminder of just how far we have come. As one who was also there then, the published literature pertaining to West Coast sites took even less than the "two weeks of hard reading" it took to assimilate data from the east; moreover, our eastern colleagues felt that there was not much worth the effort in the far west. As for dating bottles way back then in the days before Toulouse, we relied on technology and collector information. It was indeed learn-as-you-go. Precisely because we do have access to so many data now, I do not think that increasing specialization within the discipline is necessarily all bad. While historical archaeologists may fairly be expected to address a variety of sites at a general level, it is incumbent upon us to consult if and as needed with those with particular expertise—for example, with Mission foundations, Chinese materials, or oil extraction sites—precisely because there is so much more accrued information which leads to more and better questions. In other words, we should all know something about everything, and for some, everything about something.

I do not attribute all the growth in historical archaeology to SHA, or ROPA, and only a little to recent academic offerings. Addressing the university curricula first, these seem to be either the creation of a few of our dedicated leaders in the field and still fairly rare on the West Coast, or a direct response to the needs prompted by

environmental legislation and new standards for qualified CRM professionals. The cart has driven the horse, so to speak, and rather reluctantly at that. I doubt that the Society of Professional Archeologists did the field a favor when it abandoned the special categories for the different aspects of archaeology. Historical sites and structures are still being undervalued or even ignored by archaeologists unqualified in historical or industrial archaeology even though they may have RPA after their name, and by agencies or clients who assume that RPA status guarantees expertise or concern for these properties.

Nor do I think that the "mountain of empirical data" emanating from CRM investigations is irrelevant to serving the higher objective of understanding past cultural processes.

Propositions derived from history need a different database to test, or the reasoning becomes circular. At the same time, excavation for the sole purpose of testing an hypothesis may overlook or ignore evidence which may now, or in the future, address some other research objective or support new analytical techniques. Analysis of the plates and pots, the bridges and barns, provides the database for comparative studies on which higher theory is built. Explanations of recurrent patterns ultimately depend on valid data and to this end, multiple rural farmstead studies—for example—are not redundant unless one assumes that each individual responds to a comparable environment or economic challenge in the same way.

That said, the problem becomes dissemination of the data, as Cleland has pointed out, and curation of the evidence. Both *Historical Archaeology* and papers delivered at SHA conferences trend toward discussions of event and theory, rather than archaeology. At least in California, however, responsible professionals deposit all CRM reports from survey to data recovery in the state Information Centers where they are accessible, and some agencies like the National Park Service and the Corps of Engineers do publish and distribute selected reports. Reports about excavation and artifacts, further, will compel more respect if that awful and pejorative term "gray literature" is relegated to the nearest

privy, along with “salvage archaeology.” A bibliography published in *Historical Archaeology* or the *Newsletter* might help to inform the profession about where and how important reports could be obtained.

Precisely because physical remains constitute the primary database of an archaeologist, and new analytical techniques and new theory are predictable, the seeming de-emphasis upon data reports combined with the lack of policy and physical facilities for artifact curation may be hindering the search for process and meaning. We need to grapple with issues about culling bulky collections and defining “representative samples,” and then convince institutions that certain materials should be preserved for their research values, even if they do not constitute glamorous display items. As we achieve better control of chronology and technology, and come to appreciate the benefit of comparative studies in formulating propositions, the worth of earlier assemblages and supporting records from multiple sites should become ever more apparent.

Cleland refers to the relationships between historical archaeology and history, and to prehistoric archaeology. We are greater consumers of history, unfortunately, than historians are of our data but that's their loss. We do, and should, range ever wider in our application of the literature of sociology, geography, economics, and other disciplines. The wealth of applicable data sets and the ability to confirm identifications and dates from sound evidence are among the greatest strengths of historical archaeology. In addition to the added dimension of archival research, there are entirely appropriate distinctions in field methods between prehistoric and historical sites, although these do not imply loss of stratigraphic or other controls in recovering data. The down side, however, of splitting some of the larger investigations between historical and prehistoric teams has occasionally resulted in Native American sites in the historical period falling through the cracks altogether.

Our numbers are growing, our tools are rapidly becoming ever more sophisticated, and dialogues such as these provoke thought about theory and method relative to the ultimate objectives of the research. Cleland points out that we should aim for information which is not better and faster derived from documents, and that framing the underlying hypotheses may proceed from the

written record to the artifacts, or alternatively from the field to the literature, as the level of theory and the nature of a given resource may dictate. Recent dialogues on the internet about the degree to which historical archaeologists should apply anthropological or any other body of theory show that there is a concern for theory. That the emphasis has varied and changed over time, viz.: gender/race/class/processualism/environmental determinism and from Marxist to capitalist (Leone and Potter 1999), demonstrates that people are thinking, that no single body of theory will explain all human behaviors, and that historical archaeology properly “transcends the narrow questions of anthropology or history” (Heite 1999). New approaches, questions, and models will continue to emerge as long as we use all our tools to put the event into a meaningful context. For sites which lack documentation, fieldwork may yield the only evidence in support of even low-level theory, and data from an array of well excavated and reported sites can provide the bridging arguments and predictions for generating middle- or high-level propositions. This assumes that both site reports and the physical evidence are respected enough to be accessible.

It is discouraging to recall that Cleland (1988:13-17) offered many of the same insights and recommendations more than a decade ago, and that Deagan (1988:7) was already warning that “historical archaeology has not produced the original and unparalleled insights . . . that we might expect.” The focus on event may be one consequence of CRM, which so often dictates investigation, i.e., evaluation of significance, of a narrowly bounded linear corridor, a proposed development, one small knoll in an historical landscape, or one site within a potential district. While the limited focus imposed by a restricted scope of work may not contribute to an explanation of culture process, the resulting body of information is still valuable as yet another data set for the comparative studies needed to interpret behavior and support theory, as long as one doesn't drive the conclusions straight off the narrow path by extravagant inference.

Are we drifting? I'd say we are doing a lot more than merely staying afloat. Our numbers are growing; legislation and guidelines have improved; the public agencies are far more sensitive to historical sites, CRM has made

important contributions to method, theory, and an expanded database; and lively internet dialogues on such topics as archaeology as anthropology, archaeology vs. history, and curation demonstrate a valuable level of desirable introspection. That analysis now takes many approaches is a sign of a maturing—not just a young—profession, and is not necessarily to be deplored. Specialists should be welcomed as the body of knowledge expands beyond the ability of any single person to know it all, just so, as Cleland emphasizes, that the focus is set into a broad context. The practice of historical archaeology is increasingly a multi-faceted endeavor as we have progressed far beyond merely weighing and measuring artifacts. Perhaps we have not recognized or exercised our growing powers enough; we should be raising our voices, not just among ourselves, but to our collegial societies and all who have concern for historic preservation and a balanced picture of the past, those who write the laws, assess the importance of historical sites, and interpret historic properties.

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**Reply to Douglas V. Armstrong,  
Lu Ann De Cunzo, Gregory A.  
Waselkov, Donald L. Hardesty,  
and Roberta S. Greenwood**

Charles E. Cleland

I am very appreciative of the time and thought my distinguished colleagues spent in reading and responding to my question, "Is historical archaeology adrift?" Not only are there differences of opinion, but the view prevails in some quarters that historical archaeology is perhaps better off without clear objectives at all. Whether or not this is the case, I believe all of these respondents would agree that discussion of our methodological and theoretical fortunes is an exercise for the common good.

Greg Waselkov raises two very important points. First, he reminds us that archaeology is a means to an end; and secondly, he wonders why we as archaeologists have so little confidence in the information archaeology produces. As archaeologists our task is to use the process of excavation and, in large part, the analysis of artifacts to understand the past. It is, of course, more complicated than that but Waselkov has put his finger on the essential point—that is what historical archaeology is. This is an argument I tried to make as well, since I find it disturbing that some historical archaeologists seem to believe that adhering to the basic nature of archaeology will somehow deprive the field of its humanistic component. A tension has thus arisen between the science which is inherent in the basic method, which constitutes archaeology, and the humanistic component of historic archaeology. Moreover, many believe it is the latter which gives the field its freshness, imagination, and adventure. If this is the case, it is no wonder that we have lost confidence in the ability of the archaeological record (science) to help us understand the past. I find this odd because if I can paraphrase Waselkov, the testimony of the spade is more independent and

objective than the other forms of evidence with which we deal.

Waselkov, along with all of the other respondents, notes that historical archaeologists want to understand the past in humanistic as well as scientific terms. I not only agree, but hoped to advance this objective by suggesting a way to incorporate both science and the humanities into the basic methodology of historic archaeology.

Classical archaeology offers an unfortunate example of a discipline which long rejected science on the one hand, while edifying text and art on the other. Eventually classical archaeologists did little more than document sites, recover art objects, and confirm events recorded in literary sources. S. L. Dyson (1993) and Glen Storey (1999) have both pointed to these failures and suggested that hypothesis testing which involves an interplay between text and archaeology would revitalize classical archaeology by revealing "a fuller, detailed picture of cultural processes in complex societies" (Storey 1999:232).

Part of the fear of science is the perception that it requires the surrender of uniqueness. Don Hardesty allies our fears by pointing out that history and unique events play an important role in even the "hardest" of sciences such as mathematics and physics. The "Big Bang" theory is in this sense the ultimate unique event and also the key to understanding particle physics and the organization of all matter. Chaos theory, where randomized events are explored to find the connection between unique events and the general laws of nature, provides another example. I did not want to give the impression in my discussion of events in historical archaeology that there was something wrong in the study of historic uniqueness. Unique events are useful for both the understanding they give us of their distinctive features as well as the regularities which we may observe from the cause and effect relationships which permit us to reorganize patterns in the order and nature of events. If it is true that cultural practice is the result of patterned behavior and if patterned

behavior is the result of cultural norms, then idiosyncratic behavior can only be understood as behavior which deviates from the expected. Unique behavior cannot be understood in the isolated instance or on its own terms. Maybe this is what Doug Armstrong means when he says that “we must not stop our search for regularities but must also come to understand the irregularities, or variations. . . .”

Roberta Greenwood also provides some balance against my perhaps overzealous attack on event oriented archaeology. After all, she reminds us, “the ability to confirm identifications and dates from sound evidence are among the greatest strengths of historical archaeology.” Point taken.

Greenwood, to my satisfaction, reiterates the important point “that propositions derived from history (text) need a different database to test, or the reasoning becomes circular.” She adds, however, that excavation for the sole purpose of testing a hypothesis may overlook or ignore evidence which may be useful in the future. Well, maybe, but if the implication here is that technically proficient excavations produce “all purpose data,” my experience is that this is almost never the case. Research produces the data it is designed to retrieve and seldom much more.

While Greenwood has proven herself as a scholar and a serious professional historical archaeologist, she is also a contract archaeologist and therefore rightly concerned with the practice of cultural resource management within the context of historic archaeology. In my view, faults in the contract archaeology process are seldom the result of actions taken by archaeologists. Developers and their supporters who are willing to destroy sites without making adequate provisions to preserve and protect our common heritage are at fault. The archaeologists who know better and want to do more are left to complain. They should not have to.

Lu Ann De Cunzo has fundamental disagreements with the position I presented above and I am sure that her theoretical views are shared all or in part by many historical archaeologists. It is this fact which persuaded me to write to begin. To her credit, De Cunzo’s reply is a coherent argument which sets our two views in contrast. To a large degree my differences with DeCunzo have already been played out in

the archaeological literature over the last half decade as part of the commentary on postmodern critique in archaeology. I will not try to reinvent the wheel here but will limit my comments to two or three major points which I hope will emphasize that while we agree on some things, we offer fundamentally different views of the future of historical archaeology.

First there is the question of the role of method as opposed to theory in the discussion at hand. De Cunzo contends that my view elevates method above theory. Armstrong likewise seems to support this notion when he suggests that my methodology is “theory bound.” I did say I hoped to keep my article as free as possible from theory. I do, however, have a strong theoretical position of my own and I know that the problem I am trying to discuss is in part due to conflicting theories. I am operating under the assumption that the problem under discussion is mainly a methodological one which would not be clarified by trying to center its origin or solution in an appeal to a particular theoretical position. In my view, theory, any theory, is only a hypothesis which proposes a relationship between certain variables which in turn purports to predict outcomes. In this sense it is true that method and theory are necessarily linked. I am not suggesting which variables are important or how they relate to each other, only that behavior in the past can best be understood by seeking these relationships among specific variables and testing them in an objective manner.

De Cunzo, however, is espousing something different. The post-modern, post-structural and cognitive based theory she suggests rejects the notion that the regularities of cultural behavior are important for our understanding of the past if they can be understood at all. DeCunzo does seem to send a mixed message in this regard. On the one hand she tells us that people “. . . create, act on, and are influenced by cultural systems shaped by biological, technological, social, economic, ideological, and historical forces.” But on the other “That the past lacks unity, as peoples’ action and cultural logic embody complex and confusing ambiguity.”

I do not want to be unfair to DeCunzo by oversimplifying her argument, but this statement gets to the heart of our disagreement. I suggest cultural practice is regularized by a cultural logic and symbolism within any given society which

permits humans to function cooperatively. Yes, the symbolism of human action is extremely complex, and yes, the messages sent are not always properly received. Yet as archaeologists we must depend upon the embodied regularity of behavior for our understanding of the past. Archaeology is a discipline that studies culture and culture must above all be understood in terms of its shared regularities of behavior. That is what culture is.

Postmodernists, however, value the idiosyncrasy and often deny that cultural action has any reality beyond the personal meaning we gain as observers. The historic play acting which was hopefully a passing feature of some recent SHA meetings reflects these views. Attending the movie, "Jurassic Park", may provide the theater goer with a sense of dinosaurs but it does not substitute or even positively influence our understanding of reptilian evolution in the Jurassic era. That requires earning paleontological knowledge in the old fashioned way.

De Cunzo tells us that she believes that "the unique and individual coalesce with the cultural in a dynamic process to produce each event action." I am not sure what this actually means, if anything, and I am at a loss to see how we can recognize the "coalescence of the unique and the individual with the cultural" in the archaeological record. DeCunzo, like other

postmodernists, aspires "to hear the multiplicity of voices from the past, to privilege no single perspective" what has been described as a "totalizing frameworks." Certainly this is wishful thinking—theory in a methodological vacuum.

In any case, my suggestion is not to devalue the unique and individual action embodied in the archaeological record, but to focus archaeological activity on the generalized cultural schema which produced them to begin with. I do not suggest that scientific epistemology subsumes humanism, but that historic archaeology must develop a methodology which incorporates both. I do insist that such a method avoid tautology and test for predictable outcomes.

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