

CHARLES L. REDMAN
*Arizona State University*

# Distinguished Lecture in Archeology: In Defense of the Seventies—The Adolescence of New Archeology

*The core of this article was first presented as the Distinguished Lecture in Archeology at the 88th annual meeting of the American Anthropological Association, November 17, 1989, in Washington, D.C.*

MUCH OF THE THEORETICAL LITERATURE of archeology in the 1980s devotes considerable energy to bashing the 1970s, and the target often turns out to be the so-called New or Processual Archeology. While many of the attacks come from recent theorists who are attempting to replace it with post-processual archeology (Hodder 1982b; Shanks and Tilley 1987), some criticism comes from within what was New Archeology, even from the hand of its original champion, Lewis Binford (1983). If scholars from both outside and inside the theoretical developments of the 1970s are rejecting the New Archeology, why am I defending its importance to us today? The answer is very simple . . . for better or worse, it is us!

As Alison Wylie has recently said (1989), the New Archeology of the 1960s quickly became everybody's archeology in the 1970s. Most of today's faculty members and senior archeologists were the people who, in one way or another, adopted the teachings of New Archeology. Although most archeologists did not claim to agree with all aspects of New Archeology nor could more than two or three people agree on what it was, virtually no one rejected it outright. Typically, each one presented her or his version, often using a New Archeology text as a "starting point" for pedagogical purposes. Few wanted to be left out of the exciting new theoretical movement of those years, and New Archeology was passed on to the succeeding generation of students who reached maturity in the 1980s and are today's young professionals.

Criticisms now leveled against the New Archeology of the seventies do have merit, but by discounting that era as misguided, critics have overlooked its crucial importance. New Archeology had an important historical role in the development of the field we have today, and it has continuing importance because it is still guiding archeology's trajectory into the future. Equally troubling is that some critics ask us to reject the basic tenets of New Archeology and to replace them with a system often called *post-processualist archeology*. I believe this is rhetoric that not only misrepresents the achievements of the New Archeology of the seventies, but also does not successfully articulate the potential contributions of its own position.

To put the New Archeology of the seventies into perspective, it is important to review the decades leading up to its development. In the first years following World War II, archeology was still a small field, but by the fifties and the sixties, it was expanding rapidly and taking itself quite seriously. Since the launching of Sputnik in 1957 there had emerged a frenzy in the United States to make all disciplines more "scientific." Great

*CHARLES L. REDMAN is Professor and Chair, Department of Anthropology, Arizona State University, Tempe, AZ 85287-2402.*

strides were made in bringing science into archeology through new dating techniques, a multidisciplinary approach, early experiments with the use of statistics, and devoting substantial attention to increasing the precision of artifact classification. The sixties provided the nation with both the optimistic Kennedy years, with an emphasis on science and the conviction that we were capable of accomplishing wondrous things, and the cynical Vietnam era. Coming on the heels of a decade of civil rights unrest, the widespread dissatisfaction with the Vietnam conflict in the late sixties molded a generation of young Americans who were distrustful of established authority. In academic life, there was an increasing emphasis on environment, other cultures, and people-oriented disciplines. Anthropology and archeology grew markedly because of these trends. Archeologists were urged to become concerned with sociological issues—the people behind the artifacts.

It was during these decades of rapid change that many of the core concepts of the New Archeology entered the literature. However, they were not, at first, assembled into a program for action that attracted a solid following. Walter Taylor (1948) advocated the conjunctive approach with little effect, while Leslie White's evolutionism (1959) and Julian Steward's cultural ecology (1955) attracted some attention, but largely among cultural anthropologists. Albert Spaulding led a one-man campaign to bring science and statistics into archeology (1953, 1960). But the individual whose work catalyzed the New Archeology movement was Lewis Binford, who incorporated these earlier lines of thinking together with an explicit concern for scientific methods and field research designs. Much of Binford's thinking probably crystallized while he was at the University of Michigan, but it was during his relatively few years at the University of Chicago that he changed the direction of modern archeology (see Binford 1972).

Among the keys to Binford's success was that he attracted a talented group of students who, under his direction, carried out innovative research projects. Binford himself wrote a series of powerful methodological articles setting the guidelines for the New Archeology (1962, 1964, 1965, 1968), and his students filled in with substantive examples (Longacre 1963; Hill 1965; Whallon 1966). The publication in 1968 of the book *New Perspectives in Archeology* (Binford and Binford 1968) marked the crossing of a threshold. New Archeology was discussed and debated by an ever-increasing proportion of the field. Binford's New Archeology caught on because its advocation of a scientific approach was timely and because it represented a major advance over the way things had previously been done. It should be noted, however, that the political climate of the late sixties and early seventies was important in fostering its widespread acceptance as well. There were three important "political" elements to the New Archeology that were right for the times: first, it was explicitly scientific; second, it demanded social relevance; and third, it rejected arguments based on authority alone, thereby providing a means for junior people to assail the establishment on an equal footing.

The advocates of the New Archeology created their own momentum. Binford's writing became clearer, making his message accessible to more readers. Binford's students wrote compelling articles and got teaching positions at important universities where they could directly influence a second generation of New Archeologists. The attractiveness of an explicitly scientific approach and the teaching of New Archeology by this cohort led new scholars to conduct further research reexamining the results of earlier investigations. In this way early studies laid the intellectual groundwork for later studies. This cumulative aspect to several lines of inquiry followed by New Archeologists gave the field a special momentum that made it appear as if real interpretive progress was being made. Nowhere was this more clear than in studies of decorated ceramics, a field I call Ceramic Sociology. Longacre's (1963, 1970) and Hill's (1965, 1970) work was based on insights derived from Cronin (1962) and paralleled work by Deetz (1965) while subsequently providing a model for the later work of Leone (1968), Tuggle (1979), Graves (1981), and others.

If New Archeology had all this going for it, why was it not universally accepted and why is it now out of style? There were, unquestionably, useful innovations in New Archeology, but it was not entirely clear how a researcher operationalized it to achieve the

desired results. The early emphasis in New Archeology on seeking general laws of cross-cultural applicability set our sights high, considering the relatively mundane set of substantive accomplishments. This grand goal led to cynicism from outsiders and active debate among New Archeologists themselves about how realistic their objectives were (Isaac 1971; Clarke 1973; Flannery 1973a). Exacerbating these reservations about the New Archeology was the aggressive manner in which it was promoted, leading to a defensive reaction from many established scholars. Because of this, in its early years the New Archeology was not taught, and I doubt if it was even discussed, at many of the most prestigious universities.

Despite doubts and opposition, there were relatively few articles combating New Archeology during its heyday, and those that did offered an olive branch by acknowledging some of its contributions (Trigger 1973). Especially among younger archeologists, there were few who were not attracted to the New Archeology. Most criticized a few aspects of it and claimed to be following their own version. In this way, they could adopt many of the convincing tenets of the new program, yet they did not have to claim to be following Binford, who may have offended their senior faculty members.

Few scholars today still would call themselves New Archeologists, but much of the New Archeology's program has survived and has become the mainstream conduct of our field. Perhaps above any other influence, the New Archeology's demand for rigorous archeological methodology in formulating both research designs for the field and analytical strategies for interpretation of results has been fully accepted by almost all practitioners today. Which philosophical model is most appropriate is either ignored or a matter of quiet controversy, but it is unarguable that researchers must be able to explain and support the methods chosen. There is now a universal expectation that explicit questions are to be formulated and a research design established before archeological work is carried out. Also attributable to the New Archeology is the recognition of the diversity of material needed to support an interpretive proposition, and the resultant use of sampling in many aspects of field and laboratory work. Another enduring outcome of the increasing scope and precision desired for our observations has been the growing reliance on statistical procedures.

Related to the methodological changes brought by New Archeology was the emphasis on the systemic view that culture is a series of interrelated subsystems and on the importance of ecological relationships. This dovetailed nicely with the diverse data sets defined and sampled through extensive research designs. Emphasis on examining variability among data sets led many scholars to supplement traditional artifact typologies with attribute-based analytical systems, experimentation with artifact classes, data from allied sciences, and an understanding of the operation of material culture in a systemic context, which has, in turn, encouraged the pursuit of middle-range theory and ethnoarcheology.

I believe that New Archeology also had an important impact on the professional structure of our discipline. New Archeology's emergence coincided with a period of great growth in numbers of practitioners and university departments offering anthropology graduate degrees. Earlier, archeological training was available in a limited number of universities where senior professors comprised the acknowledged establishment. Access to the establishment required training and apprenticeship under a senior scholar with an established reputation. This system (which I believe is still dominant in many other countries) restricts access to professional positions and tends to inhibit intellectual innovations. The New Archeology position that new knowledge is validated through explicit testing rather than reference to authority opened the doors for young, unknown scholars to make significant contributions to the field. Such contributions became the major means of attaining credibility for young scholars and for programs at universities without an established tradition in graduate anthropology.

Twenty years have passed since the heyday of the New Archeology debates. Despite the shake-up that the new program caused, New Archeologists who were the *outsiders* of the 1970s now make up much of the establishment. Few people today are beating the

drums of New Archeology, but where have they all gone, what are they pursuing? Many of the people who matured professionally in the seventies during the New Archeology era are now involved in public archeology, either through positions with government agencies or in conducting fieldwork for these units. Their methodological and philosophical focus has been on developing data-recovery strategies and management principles. The New Archeological emphasis on research design and hypothesis testing is fundamental in much of public archeology today.

There also has been an expansion in the substantive domains pursued by anthropologically trained archeologists. For some of us, it has meant carrying out fieldwork in countries that heretofore had not received much attention (McIntosh and McIntosh 1984; Redman 1986), or expanding the study of complex societies where other disciplines have focused on only the elites to a more balanced, holistic treatment (Wright 1969; Marcus 1973; Cowgill 1983; Fritz 1986). Further developments have involved ethnoarcheology, experimental studies, and other ways to gain insight into the meaning of the archeological record, a series of approaches often called middle-range theory (Schiffer 1976; Binford 1978; Raab and Goodyear 1984). There has also been a renewed interest in the material of the archeological record. Chemical and physical studies of ceramics are once again common after receiving little attention in the United States for forty years since their introduction by Anna Shepard (1955). These supplemental sources of information, combined with the objective of discovering differing subsystems of behavior, have prompted a reevaluation of how artifacts and other information are analyzed. The typological approach that yielded easily interpreted nominal sets of units is being supplemented by integrated, attribute-based systems that allow for multiple, overlapping sets of interpretive results. This is also true of distribution studies, in which models and mathematical techniques are being borrowed from geography and ecology to allow for a richer description of the patterns that we discover.

If so much of archeology is peopled with graduates of the seventies, each with her or his own brand of New Archeology, why is there so much commotion about post-processual archeology? And why do these theorists identify what they are doing as *replacing* processual or New Archeology? Who are these "post-processualists"?

In America, for the most part, they come from the ranks of the New Archeologists of the sixties and seventies. There have always been members of the New Archeology cadre that felt the rejection of psychological and symbolic factors was too strong and that the workings of the mind and the style of the way people do things were inadequately treated in most New Archeological works. James Deetz (1977), John Fritz (1978), Mark Leone (1982), and Margaret Conkey (1982), among others, believed that the New Archeology agenda, as most often portrayed, did not do justice to the archeological record and often misrepresented what could be accomplished. These scholars often derived their concern from having backgrounds in history, art, or belief systems. With their additional insights, these post-processualists saw how nonmaterial domains of societies, downgraded by many New Archeologists, were crucial in deciphering the past.

The birthplace of post-processualism, as well as the area of its greatest intensity today, is Great Britain. Just as Binford crystallized and led New Archeology, Ian Hodder at Cambridge University is the central figure in post-processual studies (1982b, 1986). Interestingly, Hodder's early contributions to archeology were in the domain of quantitative methods and locational analysis (Hodder and Orton 1976). Hodder himself has said that he was led to the new way of thinking out of a frustration with the accomplishments of archeology as it was practiced (by which he meant the version of New Archeology defined by David Clarke). He found himself able to devise sophisticated quantitative methods to describe the distribution of archeological artifacts or phenomena accurately, but he still did not have a solid idea about what those phenomena represented. This led Hodder in two directions: first to investigate generalities about human existence; second, to embark upon ethnoarcheological studies, in order to put the objects being investigated into a richer context. Hodder soon attracted a substantial number of young British ar-

cheologists to his way of thinking, as well as some Americans. There has been some ambiguity about who might acknowledge being a post-processualist, but for at least some time, this program appealed to some among those who were concerned with such diverse topics as Marxist archeology, world-systems approaches, critical theory, gender studies, and ethnoarcheology. Each of these domains has found some support in Hodder's approach, but I believe that for the most part, they have maintained themselves as separate, although sometimes overlapping pursuits. What one must recognize then is that the term *post-processualism* does not represent a monolithic approach, but has come to signify a wide range of practices and that even the views of its primary advocate have evolved over time (Hodder 1987b).

The post-processual approach to archeology has attracted a growing and very credible following. As much as I like New Archeology, I concede that it has some significant shortcomings and that the post-processualists offer some worthwhile alternatives. I prefer to use the more descriptive title of contextual archeology, as does Hodder (with due respect to Karl Butzer [1980], who previously used this term to refer to an environmental approach). Critical, symbolic, or structural archeology are also useful to identify other varieties of what have more often been tied together under the single title of post-processual archeology. I do not think processualism deserves a *post*, and I agree with Colin Renfrew, who suggests (1989) that if taken at their word, some would more properly be called *anti*-processualists.

Some of the more vocal adherents to this new movement disagree with archeologists who view interpretation as a rational and objective method. They perceive New Archeologists as focusing on objectivity and validation of things in the past while practicing their profession in the present. Hence, they argue, there is no objective archeological record. Facts can be observed only via living individuals, and therefore, facts about the past cannot be separated from the biases of the present-day observers. This relativistic position leads to historical pluralism in archeological reconstructions and asserts that politics will inevitably enter archeological interpretations (Shanks and Tilley 1987).

The essential point of these writers who emphasize the relativistic nature of data is that we must recognize the importance and the inseparability of the present-past dialectic, which can also be seen as a subject-object dialectic. Their goal is to establish a *situated discourse,* which, with appropriate respect for the gulf between the archeologist and the past, attempts to consider the objects and events of the past in their full context. The most successful examples of these "situated discourses" are with historical societies where many social and ethical linkages to the present can be brought to bear on evidence of the past (Hodder 1987a; Leone and Potter 1988). These relativists believe that the possibility for interpretive richness and control of subjective intrusions offered by their approach are unobtainable via New Archeology.

Having highlighted the importance of the perspective of the investigator, the more relativistically minded among the contextual archeologists view the way that archeologists interact with the archeological record as an active interplay that is often manipulated and hence must be understood in order to be controlled. In this way, archeological method can be seen as a "style" that may involve politics, power, rhetoric, and perhaps even aesthetics. At its most extreme, these contextualists maintain that we are so bound up by our own perspective and style that we cannot know the past at all, but merely present our own views of the present as if they were an interpretation of the past (Miller and Tilley 1984).

There are numerous articles by contextualists that reinterpret an already collected data set from their perspective (Hodder 1982a, 1982b, 1986). They often begin by seeking a recognizable structural patterning in the archeological data. In early studies, this often took the form of demonstrating spatial or typological "polarities" in the data set. The second step would be to relate these patterns to causes that are not normally cited by New Archeologists. These explanations often, but not necessarily, came from anthropological literature or ethnoarcheological studies that the contextualist had been doing among

nearby peoples. The final step might be to draw on information derived from the historical context and various general anthropological ideas that would enrich the original explanation and give it more plausibility. The principles and procedures of post-processualism should not be seen as isolated phenomena. They have parallels with approaches advocated by social anthropologists who focus on meaning or "thick description" (Geertz 1973; Rabinow and Sullivan 1979; Sahlins 1981).

Contextualists are, despite their condemnation of it, descendants of the New Archeology of the seventies. Their own political agenda requires them to claim they are *post*-processualist, but I believe their program is a logical offshoot of processualism. Moreover, many developments happening at about the same time within New Archeology, including middle-range theory as advocated by Binford and others, show how some of our trajectories are at least parallel (Watson 1986). The post-processualists' continuity with New Archeology can be demonstrated in numerous areas.

First, post-processualists call for a *reflexive archeology,* in which archeologists must be concerned with how their *style* affects their conclusions. The New Archeology's first rule, however, was to have an explicit concern with methods and how conclusions were reached because data collection and interpretation were recognized to include subjective elements. Ironically, many of the original critiques of New Archeology accused it of being over-reflexive (Clarke 1973).

Second, post-processualists say that it is essential to put the archeological object in its context of ancient meaning. New Archeologists couldn't agree more, but were less certain how to best achieve this. Contextualists have made some interesting new contributions in this area by more broadly seeking contextual relations, but their ideas are not different in kind, or in the intensity of their application, from the New Archeology. Primary among the efforts of the New Archeology was to see objects, features, and sites within their systemic and ecological contexts. Unfortunately, many examples of New Archeological models reflected a rather simple, materialist-functional viewpoint. The key contribution of the contextualists is to expand their definition of systemic context to include broader symbolic and social domains while attempting to avoid the pitfalls of a naive application of functionalism.

Third, post-processualists say that knowledge comes from a dialogue between subject (us) and object (the archeological record). Some see this as a dialectic involving an unbridgeable gap, while others see the subject and object as being inseparable. Both views do not believe that an objective interpretation of the past can exist. In the place of objective interpretations are politically or rhetorically motivated "critiques" that reflect the investigator more than the object. For many years, some New Archeologists have also been concerned about the use of archeological data for political agendas (Ford 1973; Leone 1981). It is something to be sensitive to and to be made explicit in one's writings, but not something to inhibit moving forward.

Belief in the lack of objective reality is often diminished once an archeologist spends substantial time doing fieldwork. Archeological objects are a lot more real out there in the dirt than they are when thought about from an armchair. However, as the contextualists rightly observe, fieldwork sometimes can lead to delusions in the other direction, fostering the belief that the work is extremely objective just because precise measures are used during fieldwork or laboratory analysis.

Fourth, an acceptance of the contextualist position that archeologists cannot attain objectivity in their interpretations undermines the very basis of our research. At the extreme, some contextualists believe there is little chance of obtaining an objective view of the past with available approaches (see Miller and Tilley 1984). Because archeological excavation is necessarily destructive of contextual relationships, if we do not have an effective method for understanding these relationships, it could be argued that all excavations should stop. Despite these relativist claims, the generation of systematically collected and carefully reported data has mushroomed in recent decades. The consistency

of much of this material has led most scholars to accept it as a sufficiently objective set of data to serve as the basis for interpretations and further research.

Fifth, and probably the most important divergence between the approaches, is that some contextualists believe the New Archeology's primary failing is its overemphasis on validation and efforts to be objective (Shanks and Tilley 1989). Even if not espoused by all contextualists (Hodder 1987b), I believe that this is a useful point for current New Archeologists to ponder. In the early years of the New Archeology, major concern with idea generation was explicitly rejected; the focus was on confirmation as the method of science. Ironically, in the same book about the New Archeology in which Patty Jo Watson, Steven LeBlanc, and I (1971) heralded Carl Hempel's (1966) positivist focus on confirmation, we lauded Norwood Hanson's (1958) and Thomas Kuhn's (1962) realist treatments of the complexities of idea generation in the history of science. By presenting both we overtly recognized the distinction between context of discovery and context of validation without providing a means for integrating the first with the second. More energy should be devoted by scientific archeologists to generating ideas and perceiving contextual relationships, but we should not reject efforts at validation and a systematic approach to being objective, replacing them with uncontrolled storytelling, as has been said of some extreme examples of contextual archeology.

I would prefer to transfer this debate to the well-worn format of: Science versus Humanities. What troubles me most about the current argument, as well as many earlier ones on this theme, is that it is not really a conflict, and I believe the best possible solution would be integration, but the more likely resolution is coexistence (see Spaulding 1988). These approaches, each encompassing great diversity, are essentially complementary aspects of the study of the human condition. One of these approaches does not have to replace the other; I see them as *alternative systems of knowing,* each with its own contributions.

Among the reasons that New Archeology emerged was to dispel a widespread belief that archeology could advance as a science solely by achieving more precise measurement and not revising its faulty interpretive ideas. Another major reason for New Archeology's formation was to combat the acceptance of plausible stories as the truth, as long as they were put forward by distinguished scholars. The need was seen for careful consideration and explicit justification of methods of inquiry and the results that were obtained: a scientific approach. A scientist assumes that the world is knowable, and that it operates in an orderly manner, which can be understood by reference to widely accepted rules or laws. Whether human phenomena are amenable to general laws, similar to those that explain the physical properties of the world, is yet to be demonstrated, but the pursuit of general principles, statistical as well as universal, remains the cornerstone of a social-scientific approach. For archeologists this has meant, among other things, a general acceptance of uniformitarian principles, not a preoccupation with precision.

There is a perfectly acceptable alternative path to knowledge, however, which for simplicity I will call humanistic knowledge. Acknowledging that there are extremely diverse approaches followed by those who call themselves humanists, I will attempt only to characterize the practices of a substantial portion of the field. Like scientists, most humanities scholars also believe that there is a real world, but usually acknowledge the relativists' position that we come to know it only through the present. In fact, their explicit objective is often to enrich the present through perspectives generated by studying the past. Some post-processualists claim that this type of mixing of the past and the present while making interpretations is an inherent shortcoming of archeology. To the contrary, I view this as the essential relevance of archeology.

Many humanists do not seek a precise answer to what went on in the past, but rather are interested in opening a *dialogue* with the evidence we have of it. That dialogue is clearly reflexive, being between the evidence in the past and the scholar in the present, analogous to the situated discourse of the post-processualists. Testing, replication, and rigorously argued evidence are not requisite methods to many in the field of humanistic knowledge.

This does not undermine the possibility of knowing the past, but it puts the work of many humanists in a perspective of not seeking a final truth, but rather seeking what is most convincing or stimulating. To the extent that humanists, too, claim to know the past as it was, they tacitly use interpretations that rely on uniformitarian principles.

The essential frustration many of us have had with the New Archeology of the seventies is that it raised our hopes for grand interpretive breakthroughs that have not occurred. The rigor, scientific method, and explicit approach of the New Archeology were oriented toward verification, not creation of new ideas. In the earliest New Archeological publications, science was explicitly defined as a verification procedure, not a hypothesis-generation procedure (Binford 1968; Watson, LeBlanc, and Redman 1971). Concern was not with the origin of an idea, only with its confirmation. Lacking a methodology for generating stimulating ideas and applying them to our data, New Archeologists looked to other fields where there were already established laws, or at least promising ideas being applied. Once again, this was disappointing.

One solution to this problem was offered by philosophers Wesley and Merrilee Salmon (1979), who had devised a new viewpoint on how social science operates. As one of their test cases, they observed what New Archeologists actually were doing, rather than what they said they were doing. The Salmons called their perspective the statistical relevance model of scientific explanation. This approach allowed for building up confidence in ideas through an examination of the probative quality of all relevant information. Hypotheses are informally rated by archeologists on their prior probabilities for being true, and the most likely are pursued. The difficulty of rigorously assigning prior probabilities to information, as well as other inferential pitfalls, has kept many in the science camp of the New Archeology from accepting the Salmons' approach.

I believe that many of the best ideas in archeology have emerged from studies that lack scientific rigor or extensive documentation. Gordon Childe's work (1965 and others) is a perfect example. Relying on only the scantiest of empirical information, he promulgated the most insightful, sweeping views of the human career to date. More recently, Kent Flannery has made several of the greatest contributions to new ideas in archeology. In articles that are among his most exciting—on origins of agriculture (1969, 1973b), archeological systems theory (1968), house forms (1972b), and evolution of civilization (1972a)—Flannery engages in what I would call a *dialogue* with the data. Although his propositions are surely prompted by the empirical record, Flannery gives these dialogues substance and importance by his personal insight, not by rigorous use of evidence in a process of scientific validation.

The Hodder school has rightly recognized the simplifying assumptions and lack of interpretive progress in New Archeology and has tried to correct them by advocating a contextual approach. However, I believe that some contextualists have erred in the other direction by focusing their major effort on documenting the formulation of their idea, with little systematic effort devoted to further validation. This has led some contextualist studies to be ignored by most archeologists because of a lack of testing, while other contextual studies rigorously document a boring pattern, something that New Archeologists did quite well without them.

The obvious solution to archeology's malaise would be to amplify the interpretive, idea-generating aspect of scientific approaches, as has been suggested by Earle and Preucel (1987) or to systematize and firm up the evaluative efforts of the post-processualists, as is attempted by Hodder in his most recent works (1986, 1987a, 1987b). There is great promise in both of these efforts and what might very well emerge is an "ideal" combination of the best of the two approaches. It is my belief, however, that although there may be a few cases where this marriage succeeds, it will not work out for the field at large. There are real differences in beliefs concerning appropriate knowledge that cannot be easily bridged. Hence, even though I hold out hope for the emergence of a newly unified archeology, I do not think it is realistic nor necessary for our advancement. Rather, I

would like to see a diverse, but more cooperative discipline with shared goals and results, but distinct approaches.

I suggest that, as a discipline, we encourage those who are gifted in creative dialogue to think great thoughts: to brainstorm, unconstrained by the rigor of normal scientific methodology; to look at the world from all angles, both the obvious and the less obvious; and to put forward interesting propositions, providing enough information for others to determine whether a dialogue should be pursued. I certainly would not hope to turn the entire discipline into storytellers, but we need our share! The key is that they recognize that the pursuit of knowledge is not over once they have promulgated their story.

The majority of us in archeology should do what we can with generating new ideas, but stick to scientific verification as our primary activity, seeking new ways to measure behavioral processes with evidence from the past (Watson, LeBlanc, and Redman 1984). Read widely, expose ourselves to the ideas of the new thinkers, but bring them and their ideas back to earth! Be aware of the possible pitfalls that contextualists have identified, because surely there is culture-bound contextual meaning to all we study and to ourselves. To the extent that ideational factors that may have been contingent on specific cultural traditions played a substantial role, we must apply uniformitarian ideas with appropriate caution. But we must not let that be a hindrance to progress in understanding the past. As scientists, we should continue to proceed under the banner of the general validity of uniformitarianism. This does not mean that we have to limit ourselves to projecting present patterns into the past, but at least we should use them as starting points or building blocks for a new view of the past; there is no other way. Yes, we may misinterpret some things, but looking back from our culture-bound present over the past hundred years of scholarship, archeologists have developed many ideas about the past that have withstood careful scrutiny for generations. There *are* patterns in the past, and these patterns *are* reminiscent of things we can understand in the present—*uniformitarianism does work*!

I do not claim that we can know all of the past, or even know any particular piece of it with absolute certainty. At some level, however, there are shared elements to being human: to perceive opportunities, to categorize, to elaborate, and to cooperate for a common good. These are among the factors that have led to our position of preeminence in the animal kingdom, and it is what is most likely to be knowable through uniformitarian studies. If we were to reveal only those ideas to the world, archeology would truly be a worthwhile endeavor.

I would like to conclude this discussion by departing from archeology in isolation and say a few words about our present-day political context: how it may have led many postprocessualists to reject New Archeology and how at the same time it is creating new opportunities for our discipline. New Archeology was born as part of the optimism of the sixties. All was possible: racial tensions could disappear, the Cold War could thaw, the poor could be made wealthy, and archeologists could devote themselves to testing general laws that had social significance. Unfortunately, in the broader world, as in archeology, the problems proved too difficult to be solved at that time.

The eighties in the United States can be generally characterized as an era of economic expansion and getting on with your work. There were more of us, more jobs, and more research money, at least in cultural resource management. However, in Great Britain, growth did not spread to the academic ranks, and the restrictive, hierarchically controlled university system has remained largely in place. The era of New Archeology in Great Britain saw innovations in thinking, but not the massive structural shift in the discipline that accompanied it in the United States. Because of this, antiestablishment feelings are still active, and the subject of their acrimony now focuses on New Archeology and some who once championed the British version of it, such as the legacy of David Clarke. Young scholars who are seeking to "break" into the establishment are more inclined to emphasize the distinctiveness of their approach, rather than its continuities with what came before.

But the eighties are over, and we find ourselves in a new and rapidly changing world situation. There must now be a shift in our energies from conflict to cooperation, just as there is in the broader political scene. New political alignments are forming, material goods are flowing over old barriers, and global interaction is intensifying. The ultimate resolution of these processes is difficult to foresee, but it is certain that a new world order is emerging.

What do these changes mean for the course of archeology in the nineties? It can mean business as usual, or it can mean responding to a rare opportunity. I believe that people in many parts of the world are ready to embrace new perspectives on world history and certainly a new explanation of the world order. As Binford and many others have said, we as archeologists are especially well suited to view long-term change and to view it within a perspective unavailable to the textual historian, the ethnographer, or the sociologist (Binford 1968; Plog 1973; Watson 1973). This is an enormous challenge and fraught with difficulties. To achieve this lofty objective, archeologists must cultivate the renewed humanistic focus on idea generation and accept the value of thoughts stimulated by the past, but written in the present. At the same time, I believe that real progress can only be made if this more freewheeling individualistic approach to knowledge is carried forward in cooperation with others who pursue the continued growth of rigor and validation. Whether we call it New Archeology, Post-Processualism, or even a New Processualism is of little importance. The essential element is that we encourage serious scholars to do what they are best at doing and to coordinate diverse thinking to form a loose but lasting alliance for new knowledge of the past and present.

*Acknowledgments.* I want to thank the executive board of the Archeology Division for conceiving the idea of the Distinguished Lecture in Archeology and asking that I deliver the first. The thoughts contained here derive from my interaction with many gifted individuals. I am especially grateful to those who fruitfully commented on an earlier draft of this article: Michael Shanks, Patty Jo Watson, George Cowgill, Barbara Stark, Geoffrey Clark, Alison Wylie, Thomas Chadderdon, Steven Savage, and Richard Hodges. Most of all I want to thank the many co-travelers of the seventies who made it such an exciting time to be an archeologist.

## References Cited

Binford, Lewis R.
1962 Archaeology as Anthropology. American Antiquity 28:217–225.
1964 A Consideration of Archaeological Research Design. American Antiquity 29:425–441.
1965 Archaeological Systematics and the Study of Cultural Process. American Antiquity 31:203–210.
1968 Archaeological Perspectives. *In* New Perspectives in Archaeology. Sally R. Binford and Lewis R. Binford, eds. Pp. 5–23. Chicago: Aldine.
1972 An Archaeological Perspective. New York: Seminar Press.
1978 Nunamiut Ethnoarchaeology. New York: Academic Press.
1983 Working at Archaeology. New York: Academic Press.
Binford, Sally R., and Lewis R. Binford, eds.
1968 New Perspectives in Archaeology. Chicago: Aldine.
Butzer, Karl
1980 Context in Archaeology: An Alternative Perspective. Journal of Field Archaeology 7:417–422.
Childe, V. Gordon
1965 Man Makes Himself. London: C. A. Watts.
Clarke, David
1973 Archaeology: The Loss of Innocence. Antiquity 47:6–18.
Conkey, Margaret W.
1982 Boundedness in Art and Society. *In* Symbolic and Structural Archaeology. I. Hodder, ed. Pp. 115–128. Cambridge: Cambridge University Press.

Cowgill, George L.
1983 Rulership and Ciudadela: Political Inferences from Teotihuacan Architecture. *In* Civilization in the Ancient Americas: Essays in Honor of Gordon R. Willey. Richard M. Leventhal and Alan L. Kolata, eds. Pp. 313–343. Cambridge, MA: University of New Mexico and Harvard University Press.
Cronin, C.
1962 An Analysis of Pottery Design Elements, Indicating Possible Relationships between Three Decorated Types. *In* Chapters in the Prehistory of Eastern Arizona, Vol. 1. P. S. Martin et al., eds. Pp. 105–114. Chicago: Chicago Natural History Museum.
Deetz, James F.
1965 The Dynamics of Stylistic Change in Arikara Ceramics. Illinois Studies in Anthropology, No. 4. Urbana: University of Illinois Press.
1977 In Small Things Forgotten: The Archaeology of Early American Life. New York: Anchor Press/Doubleday.
Earle, Timothy K., and Robert W. Preucel
1987 Critique. Current Anthropology 28(4):501–538.
Flannery, Kent V.
1968 Archaeological Systems Theory and Early Mesoamerica. *In* Anthropological Archaeology in the Americas. B. Meggers, ed. Pp. 67–87. Washington, DC: Anthropological Society of Washington.
1969 Origins and Ecological Effects of Early Domestication in Iran and the Near East. *In* The Domestication and Exploitation of Plants and Animals. Peter J. Ucko and G. W. Dimbleby, eds. Chicago: Aldine.
1972a The Cultural Evolution of Civilizations. Annual Review of Ecology and Systematics 3:399–426.
1972b The Origins of the Village as a Settlement Type in Mesoamerica and the Near East: A Comparative Study. *In* Man, Settlement and Urbanism. Peter J. Ucko, Ruth Tringham, and G. W. Dimbleby, eds. London: Duckworth.
1973a Archaeology with a Capital S. *In* Research and Theory in Current Archaeology. Charles L. Redman, ed. Pp. 47–53. New York: Wiley.
1973b The Origins of Agriculture. Annual Review of Anthropology 2:271–310.
Ford, Richard I.
1973 Archaeology Serving Humanity. *In* Research and Theory in Current Archaeology. Charles L. Redman, ed. Pp. 83–93. New York: Wiley.
Fritz, John M.
1978 Paleopsychology Today: Ideational Systems and Human Adaptation in Prehistory. *In* Social Archaeology: Beyond Subsistence and Dating. Charles L. Redman et al., eds. Pp. 37–59. New York: Academic Press.
1986 Vijayanagara: Authority and Meaning of a South Indian Capital. American Anthropologist 88:44–55.
Geertz, Clifford
1973 Deep Play: Notes on the Balinese Cockfight. *In* The Interpretation of Cultures. Pp. 181–223. New York: Basic Books.
Graves, Michael W.
1981 Ethnoarchaeology of Kalinga Ceramic Design. Ph.D. dissertation, University of Arizona.
Hanson, Norwood R.
1958 Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science. London: Cambridge University Press.
Hempel, Carl G.
1966 Philosophy of Natural Science. Englewood Cliffs, NJ: Prentice-Hall.
Hill, James N.
1965 Broken K: A Prehistoric Society in Eastern Arizona. Ph.D. dissertation, University of Chicago.
1970 Broken K. Pueblo: Prehistoric Social Organization in the American Southwest. Anthropological Papers of the University of Arizona, No. 18. Tucson: University of Arizona Press.
Hodder, Ian
1982a Symbols in Action. Cambridge: Cambridge University Press.
1982b The Present Past. New York: Pica Press.

1986 Reading the Past: Current Approaches to Interpretation in Archaeology. Cambridge: Cambridge University Press.
1987a [ed.] Archaeology as Long-Term History. Cambridge: Cambridge University Press.
1987b *Comment on* Processual Archaeology and the Radical Critique, by Timothy K. Earle and Robert W. Preucel. *In* Current Anthropology 28(4):516–517.

Hodder, Ian, and Clive Orton
1976 Spatial Analysis in Archaeology. Cambridge: Cambridge University Press.

Isaac, Glynn
1971 Whither Archaeology? Antiquity 45:123–129.

Kuhn, Thomas S.
1962 The Structure of Scientific Revolutions. International Encyclopedia of Unified Science, Vol. 2, No. 2. Chicago: University of Chicago Press.

Leone, Mark P.
1968 Neolithic Economic Autonomy and Social Distance. Science 162:1150–1151.
1981 The Relationship between Artifacts and the Public in Outdoor History Museums. Annals of the New York Academy of Sciences 376:301–314.
1982 Some Options about Recovering Mind. American Antiquity 47:742–760.

Leone, Mark, and Parker Potter, Jr., eds.
1988 The Recovery of Meaning: Historical Archaeology in the Eastern United States. Washington, DC: Smithsonian Institution Press.

Longacre, William A.
1963 Archaeology as Anthropology: A Case Study. Ph.D. dissertation, Department of Anthropology, University of Chicago.
1970 Archaeology as Anthropology: A Case Study. Anthropological Papers of the University of Arizona, No. 17. Tucson: University of Arizona Press.

Marcus, Joyce
1973 Territorial Organization of the Lowland Classic Maya. Science 180:811–816.

McIntosh, Susan K., and Roderick J. McIntosh
1984 The Early City in West Africa: Towards an Understanding. African Archaeological Review 2:73–98.

Miller, Daniel, and Christopher Tilley, eds.
1984 Ideology, Power and Prehistory. Cambridge: Cambridge University Press.

Plog, Fred T.
1973 Diachronic Anthropology. *In* Research and Theory in Current Archaeology. Charles L. Redman, ed. Pp. 181–198. New York: Wiley.

Raab, Mark, and Albert Goodyear
1984 A Review of Middle-Range Theory in Archaeology. American Antiquity 49:255–268.

Rabinow, Paul, and William Sullivan, eds.
1979 Interpretive Social Science: A Reader. Berkeley: University of California Press.

Redman, Charles L.
1986 Qsar es-Seghir: An Archaeological View of Medieval Life. New York: Academic Press.

Renfrew, Colin
1989 Comments on Archaeology into the 1990s. Norwegian Archaeological Review 22(1):33–41.

Sahlins, Marshall
1981 Historical Metaphors and Mythical Realities: Structure in the History of the Sandwich Islands Kingdom. Ann Arbor: University of Michigan Press.

Salmon, Merrilee H., and Wesley C. Salmon
1979 Alternative Models of Scientific Explanation. American Anthropologist 81:61–74.

Schiffer, Michael B.
1976 Behavioral Archaeology. New York: Academic Press.

Shanks, Michael, and Christopher Tilley
1987 Social Theory and Archaeology. Cambridge: Polity Press.
1989 Archaeology into the 1990s. Norwegian Archaeological Review 22(1):1–54.

Shepard, Anna O.
1955 Ceramics for the Archaeologist. Washington, DC: Carnegie Institute of Washington.

Spaulding, Albert C.
1953 Statistical Techniques for the Discovery of Artifact Types. American Antiquity 18:305–313.

1960 The Dimensions of Archaeology. *In* Essays in the Science of Culture in Honor of Leslie A. White. Gertrude E. Dole and Robert L. Carneiro. Pp. 437–456. New York: Thomas Y. Crowell.

1988 Distinguished Lecture: Archeology and Anthropology. American Anthropologist 90:263–271.

Steward, Julian H.

1955 Theory of Culture Change: The Methodology of Multilinear Evolution. Urbana: University of Illinois Press.

Taylor, Walter W.

1948 A Study of Archaeology. Menasha, WI: American Anthropological Association Memoir No. 69.

Trigger, Bruce G.

1973 The Future of Archaeology Is the Past. *In* Research and Theory in Current Archaeology. Charles L. Redman, ed. Pp. 47–53. New York: Wiley.

Tuggle, H. D.

1979 Prehistoric Community Relations in East-Central Arizona. Ph.D. dissertation, Department of Anthropology, University of Arizona.

Watson, Patty Jo

1973 The Future of Archaeology in Anthropology: Cultural History and Social Science. *In* Research and Theory in Current Archaeology. Charles L. Redman, ed. Pp. 113–124. New York: Wiley.

1986 Archaeological Interpretation, 1985. *In* American Archaeology, Past and Future. D. Meltzer, D. Fowler, and J. Sabloff, eds. Pp. 439–457. Washington, DC: Smithsonian Institution Press.

Watson, Patty Jo, Steven A. LeBlanc, and Charles L. Redman

1971 Explanation in Archaeology: An Explicitly Scientific Approach. New York: Columbia University Press.

1984 Archaeological Explanation: The Scientific Method in Archaeology. New York: Columbia University Press.

Whallon, Robert, Jr.

1966 The Owasco Period: A Reanalysis. Ph.D. dissertation, Department of Anthropology, University of Chicago.

White, Leslie A.

1959 The Evolution of Culture: The Development of Civilization to the Fall of Rome. New York: McGraw-Hill.

Wright, Henry T.

1969 The Administration of Rural Production in an Early Mesopotamian Town. Ann Arbor: University of Michigan Museum of Anthropology, Anthropological Papers, No. 38.

Wylie, Alison

1989 A Proliferation of New Archaeologies: Skepticism, Processualism, and Post-Processualism. (Unpublished ms. in author's possession.)