ARCHAEOLOGICAL METHOD AS PROBLEM SOLVING:

A RETROSPECTIVE VIEW

David A. Fredrickson
Anthropological Studies Center
Sonoma State University
Rohnert Park, CA 94928

ABSTRACT

Three "revolutions" in California archaeology have affected methods. The first rejected early stage concepts, such as paleolithic and neolithic, in favor of the empirical concept that it was the assemblage itself that made up the archaeological culture. The second rejected the empirical in favor of an abstract view that identified archaeological materials as residues of cultural behavior. A major marker of this approach was an emphasis on variability. The third was forced upon the discipline through the advent of legally mandated archaeology which brought radical transformations, though with little intellectual guidance. The emergence of a significant regional archaeology is one effect of this revolution. Each revolution created a problem-solving environment that stimulated rethinking of appropriate methods.

INTRODUCTION

In keeping with the theme of SCA's 25th anniversary, I reviewed the July 1961 issue of American Antiquity, dedicated to the 25th anniversary of the Society for American Archaeology. Particularly important was Clem Meighan's paper, "The Growth of Archaeology in the West Coast and the Great Basin, 1935-60". Significantly, the paper dealt only with prehistoric archaeology. Aside from the growth of historical archaeology in the past few years, discussed elsewhere in this volume, it was apparent that our substantive knowledge has increased astronomically during these past 30 years, that our theoretical breadth has become impressively wide, and that our methodological rationales have become increasingly sophisticated.

CONCEPTUAL FRAMEWORKS

Assessments of California's prehistory have also changed over the years as we can see when we encounter Meighan's (1961:33) statement that the Far West "can be categorized as having rather simple levels of aboriginal technological and social development" and that its archaeological cultures tend to

be "relatively meager, simple, and slow-changing." He further remarked that "this situation sometimes makes the archaeologist feel that he is spending a mountain of effort to a attain a molehill of interpretation." The origins of this somewhat negative view have considerable time depth, dating back to the 1920s and 1930s as well as into the 1960s (e.g., Elsasser 1960:5), when the absence of pottery throughout most of California's prehistory and the lack of substantial architectural remains were evoked to account for the difficulty encountered by early archaeologists in detecting change in California's archaeological record. I suggest that if this did contribute to the difficulty, it was not the crucial factor. More important was the conceptual framework that affected interpretation, specifically, ideas as to the nature of significant change. In writing of archaeology conducted during the first several decades of this century, Elsasser (1960:3; see Kroeber 1909:15) pointed out that the early conceptual framework was involved with broad schemes, such as paleolithic versus neolithic. Indeed, I recall this remaining a valid topic for discussion even during the late 1940s and early 1950s.

THE FIRST REVOLUTION IN ARCHAEOLOGY

My intent here is to make the observation that methods are intimately related to the interpretive framework. The ability of California archaeologists to discern significant change in the archaeological record in the late 1920s and early 1930s, which constituted a quiet revolution, was in large part due to an implicit rejection of those earlier broad concepts of culture and to the acceptance of an empirical approach, at times self-consciously atheoretical - even antitheoretical, intimately linked to the comparative method, that in effect accepted as meaningful discernible patterned change in artifacts, including form, morphology, and stylistic detail. Because of the absence of conventional materials (e.g., ceramics and distinctive architectural features) with which to build chronologies, those in California mustered their problem-solving skills to develop a method based in part upon the use of beads and ornaments and other stylistic detail, as well as variation in mortuary patterns, as sensitive markers of temporal variation (Beardsley 1948; Lillard et al. 1939). The definition of culture during this period was founded upon the empirical patterning of archaeological remains, far removed from the broad concepts of paleolithic and neolithic. It was the assemblage itself that made up the archaeological culture. This definition, of course, reflects the descriptive and classificatory phases in the development of American archaeology (Willey and Sabloff 1974).

THE SECOND REVOLUTION IN ARCHAEOLOGY

A more vocal and strident archaeological revolution occurred during the 1960s. The archaeological controversy and

confrontations of this period (which, incidentally, contributed to the gestation of the SCA) resulted in general acceptance of the idea that culture is an abstraction, with material remains being in large part the residues of cultural behavior. Although self-consciously referred to as a "revolution" at the time, this productive conceptual change could not have happened without the earlier achievement that was based upon an effective empiricism.

Understanding Variability

An important result of the rejection of the idea that the archaeological culture and the assemblage were identical resulted in an emphasis on the study of variability in the archaeological record, both intersite and intrasite, both diachronically and synchronically, and over a broad geographic region (e.g., Binford 1962; Schiffer 1976). Thus, in my view, the multitude of theoretical formulations that have since emerged can be seen as alternative explanations of this variability. That there is no single explanatory framework, and that there is often controversy between different explanatory approaches, can be considered inevitable and healthy, insofar as the archaeological record, manifestly incomplete, ambiguous and obscure, remains firmly linked to the explanation. While no explanation is complete or fully verifiable, each approach has potential to provide deeper understanding.

To Win the Revolution

The SCA not only had its period of gestation within the controversy of the 1960s, it was born before those changes had become fully entrenched. And once born, the organization fully supported the new paradigm (as it was called in those olden days) and acted to ensure that the old paradigm lost the battle for the younger archaeological mind. As I recall, an effort was made to "purify" papers given at annual meetings, to keep out the "show and tell", which is the characterization often given papers presented in earlier years. On the other hand, data sharing meetings emerged, at least partially, in response to membership perception that abstract and programmatic papers were frequently less satisfying than ones that dealt with data.

One of the major efforts of the 1960s was to transform archaeology into a science (e.g., Watson et al. 1971). Although some believed it was already a science, its earlier methodology was not based upon logical positivism. It is of interest that positivism in archaeology has been seriously challenged in recent years, and that approaches other than the hypothetico-deductive are gaining ground (e.g., Hodder 1985; Leone et al. 1987). Although the desire for certainty in explaining archaeological phenomena (cf. Dunnell 1984:501) supported positivism, this desire was bound to be frustrated, not only because of ambiguity and incompleteness in the archaeological record, but also, in my view, because of the inappropriate hope for certainty. Since there is no agreement in explaining the cultural systems of the living, how can we expect to explain the cultural systems of peoples long absent? Establishment archaeology, as reflected in

proposals emerging from the Office of Historic Preservation, seems to remain loyal to positivism (California Office of Historic Preservation 1991:5).

Consciousness-Raising and Methods

Methodologically, the shift from the assemblage itself being the archaeological culture to the assemblage being a distorted and incomplete reflection of human behavior, and the effort to emulate the logical procedures of the physical sciences, brought with them a pronounced awareness of the shortcomings of methods previously in fashion. This is evidenced, for example, by questioning the adequacy of both survey and excavation methods, which Meighan, in his 1961 review, also recognized in his statement that excavation was "still too much on a 'hit or miss' basis". Although many of the newer methods had been foreshadowed in one way or another by earlier workers, the articulation of the relationship between problem and method became much more explicit. There emerged a methodological awareness with a linked emphasis on problem solving.

With respect to methods since the birth of the SCA, many advances resulted from exploring implications of changes introduced during the early 1960s. For example: methods for sampling archaeological sites received considerable attention, with efforts made to apply probability sampling not only to reduce observer bias but also to obtain statistically reliable samples (e.g., Mueller 1974). I see one significant shift in site sampling to be the replacement of standard trenching by the standard excavation unit, which has since evolved into today's "telephone booth", which is, in my view, as inappropriate as trenching when used as a routine approach. Techniques, such as both dry and wet screening of archaeological deposits, that today may seem to be routine and pedestrian, were seldom employed prior to the 1960s. Retaining and analyzing the residues of everyday living, e.g., faunal remains and debitage, also became routine rather than having status as idiosyncratic activities.

THE THIRD REVOLUTION IN ARCHAEOLOGY

As mentioned earlier, the archaeological values propounded during the 1960s were sometimes credited as effecting a revolution in archaeology. I see another revolution in archaeology occurring in 1973 with the advent of routine, state and federally mandated archaeology, what we refer to as CRM. I believe that this shift had importance equal to that involved in the conceptual changes of the late 1920s and of the 1960s. Although the 1960s concepts of culture continue to drive CRM today, the scale of the work has been so enormous, that qualitative changes in the practice, science, and art of archaeology have occurred, especially in the emergence of a significant "regional archaeology."

It should be apparent that CRM has provided extensive and

large scale testing in a real world context of ideas and methods, both old and new, believed to contribute to effective archaeology. Programmatic and traditional methods were put to the test and although often effective, were at times found to be wanting. Received knowledge was often found to be false; accepted methods frequently yielded unsatisfactory results, a situation sometimes not recognized by the practitioner. What appeared to be lacking at times was an understanding of the relationship between material remains and abstract questions with perhaps too much dependence upon conventional methods. The data potential of different types of remains were not perceived, and analytic methods to convert those remains into pertinent data were not necessarily available. Further, the remains were frequently expected to answer questions that were not relevant to their information potential. Some of the major parameters of CRM that have contributed to its revolutionary status are discussed below; I believe that many CRM attributes which have often been criticized may actually contribute to CRM's importance.

The CRM Challenge

CRM has been criticized in that an undertaking determines the size and location of a project area, not the archaeologist, and that financial need may motivate practitioners to work in localities where they lack experience and knowledge regarding local research issues. In addition, constraints of time and money are explicitly defined for each project. Although these constraints bring up questions as to whether an adequate job can be done, they also demand careful planning and focused research, as well as steps such as consultation with local experts.

The requirement that all archaeological sites be identified forces attention on all site types, not merely those whose research value is self-evident. One criticism of this requirement is that money is poorly spent on sites with little or poorly defined research value. In response, it is the scholarly imagination of the archaeologist that is required to explore and define meaningful research questions.

Complaints are heard that research importance is subordinate to proper management of the resource, resulting in situations where clearly important sites are not investigated while others of less importance are. The criticism inherent in this situation has been resolved conceptually through concepts such as "site banking", i.e., a commitment to long term preservation and management. This is not the forum to explore fully the constraints and opportunities of CRM. I point out that each of these parameters has required active engagement of the archaeological imagination, i.e., problem solving, and requires methodological examination.

The Archaeological Survey

One of the issues we continually face is the definition of what constitutes an archaeological site. Despite years of discussion and debate, there is no established standard for site

definition. On the positive side, through the coordinating efforts of the Office of Historic Preservation (California Office of Historic Preservation 1989), site record information, however the site may have been defined, is routinely more complete and descriptively explicit than ever before. I believe that the concept of "site" involves major methodological issues and that the theory and practice of archaeology would benefit as a result of organized exploration of the concept (cf. Van Bueren 1991).

Land parcels subject to archaeological survey range from relatively tiny plots to vast areas of contiguous acreage. A major methodological advance in the organization of survey information was the establishment of the statewide system of Information Centers. However inefficient or erratic some Centers may be, the organizational framework is in place and at times works extremely well. A continuing failure is the absence of organized curation procedures and facilities for materials collected during surveys.

The SCA was instrumental in establishing procedures for archaeological survey at the very beginning of the CRM era through its joint publication with the UCLA Archaeological Survey, "Recommended Procedures for Archaeological Impact Evaluation" (King et al. 1973). Survey methods have received a great deal of attention and major advances, such as the use of closely spaced transects, have been made. I caution, however, that techniques which prove effective in one context may not prove effective in another. Routine application of the transect method, without regard for variables such as topography, geomorphology, site formation processes, and known and potential prehistoric uses of an area, has proven to be counterproductive.

The opportunity provided by the survey of large contiguous parcels, at times approaching 100 square miles, has prompted the development of site classification systems, with initially limited uses for want of temporal control. In many localities the addition of obsidian hydration to gain temporal control added the diachronic dimension necessary for fuller understanding. The obsidian hydration method, in regions where obsidian occurs archaeologically, has also elevated the research potential of the troublesome flake scatter and, I believe, in some regions has contributed significantly to a fuller understanding not only of their temporal placement but also, combined with other methodological advances, of their important place in the settlement-subsistence system.

Excavation as Discovery

I find the so-called "telephone booth" excavation unit of particular interest. At the beginning of the 1960s the standard approach to excavation was through the use of trenches and occasional arbitrarily placed excavation units (cf. Meighan 1961). As the conceptual challenges of the 1960s emerged, and questions pertaining to site structure and variability surfaced, the excavation units that comprised the trenches were broken up

and spread more extensively over the site surface to test for variability. The 1 meter square unit emerged in 1964, when archaeology in California converted to the metric system. It is questionable whether the one-by-one as it is often used today is truly effective for discovering the importance of a site except in relatively homogeneous vertical deposits.

The inappropriateness of rote use of the one-by-one was pointed out to me by historical archaeologists. With the short time depth of California's historic past, relatively few historic sites have deep or stratified deposits (wells and privies excepted). Sites, particularly rural ones, tended to grow horizontally, rather than vertically. Thus, the typical one-by-one would yield very little, whereas more extensive, shallow horizontal excavation would provide much more useful information. The realization emerged that the same was true for many prehistoric sites, especially in the mountains where I had worked for many years, and into which I had placed innumerable one-by-ones with frustrating results. Although foreshadowed by other minor excavations, the work of Hildebrandt and Hayes (1983, 1984) on Pilot Ridge and South Fork Mountain in northwestern California successfully made use of shallow, horizontal excavation units in search of small pockets of deeper deposits where vertical excavation was appropriate. It was considered unlikely that standard excavation methodology would have yielded equivalent results. Variations of this approach (cf. Jackson et al. 1988) have since been employed successfully in many other contexts.

Analysis, Techniques, and Understanding

With respect to analytic procedures, several developments over the past several years have been significant. First, technological analyses of flaked stone tools continue to evolve and in so doing now contribute to understanding of cultural dimensions beyond the strictly technological. One example is Betsy Skinner's Sierran work on the role of scavenging in the procurement of obsidian. Other developments involving obsidian deserve mention. As you all know, after an initial wave of enthusiasm in the early 1960s, the obsidian hydration method fell into disuse for a variety of reasons, some having to do with overly high expectations on the part of the user, and some with problems at the technical end. During the period of relative inactivity, the method was kept alive at UCLA through the commitment of Clem Meighan (e.g., Meighan et al. 1974) and at Penn State through the work of Michels (e.g., Michels 1973), and was brought back to the attention of field workers especially through the work of Jon Ericson (1981). Obsidian sourcing and hydration studies, stimulated by CRM, have renewed credibility and are now in widespread use, as a result of additional seminal research conducted by individuals such as Richard Hughes (1986), Tom Jackson (1975, 1986), Rob Jackson (1984), and Tom Origer (1987). The recent work of Kim Tremaine (1989) in developing source comparison constants is another useful contribution.

CONCLUSIONS

The major point I wish to make is that archaeological methods are not "fixed form", that is, there is no one way to do archaeology, to conduct a survey, to excavate a site, or to analyze materials. Please be assured, I do not reject the cook book metaphor; I believe, that the archaeological cook, like any good chef, must be familiar with many recipes and have the imagination not only to develop others, but also to modify existing ones according to taste. All of us understand, however, that some cooks produce more tasty food than others. An archaeological excavation is like a dinner with many courses, each designed to complement the others. The archaeological courses, or, to abandon the metaphor, the archaeological methods, must take into account the frequency of occurrence of different types of materials within an archaeological site, and the archaeologist must be cognizant of methods, from excavation through analysis, most appropriate to recover each type of material and to realize fully its data potential. There is no "right way" to dig a hole or to describe a projectile point.

In conclusion, I wish to pass on an observation that Sonia Tamez once shared with me based on her review of planning literature (Tamez and Fredrickson 1984). Archaeology is a field where there is no necessary agreement as to goals. Further, even when goals are shared, there is no necessary agreement about the methods appropriate to attain those goals. Of course, our understanding this will not necessarily make us all more tolerant of those who differ from us with respect to goals and methods. Yet despite the feelings we may have that standardization of goals and methods would improve archaeology, I suggest that progress in understanding the past comes through creative problem solving and imaginative diversity, not through conformity.

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