Old Wine and New Skins: A Contemporary Parable

I HAVE been asked to comment on the new, the not-so-new, and the downright old in contemporary archaeology in the United States. The specific question was put: “what happened between 1948 and 1970?” The task has been rather uncongenial and the writing difficult. I have tried to navigate the waters between a Scylla of adverse criticism and a Charybdis of self-approbation—only to end up with what sadly seems to be a bit of a shipwreck on each side of the straits. This may be the neatest seafaring trick of the week. But that is the way it is, and I offer my results without further apology.

Contemporary archaeology in the United States is certainly in a state of flux. Archaeologists having traditional goals and using traditional theory are still very much with us. There has also developed a sort of “neo-traditionalism,” an archaeology having traditional goals but working with an expanded range of data and modern techniques which have evolved in response to a somewhat modified, but still recognizably traditional conceptual scheme. Then there is the so-called new archaeology which, justifiably or not, has set itself apart from traditionalism of any kind and trumpeted its breakthrough to science and the scientific method. Other heterodoxies have been proposed. Rouse, usually rather conservative in his outlook, has suggested the radical possibility that in the not-too-distant future archaeology may separate into two professions, one technological and the other interpretive (1968:12). Chang has argued for a “science of prehistoric so-

...
ing hypotheses which must be tested and refined by specifically programmed investigations.

3) Culture is integrated, or “systemic” as modern jargon has it, to such an extent that cultural manifestations cannot be truly depicted or understood apart from their contexts—with the corollary that construction of cultural context is an absolute requisite for anthropological archaeology (or archaeological anthropology).

4) Cultural context consists not merely of material objects, singly or in categories, but includes relationships between and among cultural and noncultural phenomena, which relationships or “conjunctives” serve to connect the meaning as well as the construction of archaeological contexts.

5) One of the most efficient and productive ways of utilizing these conjunctives is by multiple categorizations based upon the many inherent characteristics and relationships which each archaeological datum has.

6) The concrete, empirical findings of archaeology can be manipulated and interpreted to provide evidence of cultural behavior, of the nonmaterial results of cultural behavior, and of culture itself—with the corollary that this evidence, specifically and explicitly argued, may be used to enrich the cultural contexts and to support subsequent and consequent studies of culture itself and of cultural process.

If these tenets sound familiar to contemporary archaeologists, especially to those of the “new” or “processual” school, it does not surprise me at all. Much of the “new archaeology” is operating with a conceptual scheme which is virtually identical, in its basic ideas, with that anticipated in A Study of Archeology. What does surprise me, however, is that it has taken the many years since 1948 for that conceptual scheme to take hold! Perhaps it is as one colleague predicted: that A Study of Archeology would not be widely accepted until a new generation of archaeologists had come along without so much subjective and emotional involvement in the then status quo. Perhaps the popularly held view that my criticism of Americanist archaeology was a polemic aroused such partisan and defensive animosity that the message of the rest of the volume was lost. Perhaps it is somehow indicative that, only a few months ago on the latest of not a few similar occasions, I heard a colleague pay formal, public tribute to the influence upon him of A Study of Archeology, only to discover that his most recent theoretical publication cites the monograph only once, a parenthetical page reference, but does quote at considerable length more recent works by other authors as the sources of some of the most basic and distinctive ideas of A Study of Archeology.

Perhaps there have been academic-pedagogic-generational reasons for the lag: the older generation taking umbrage but maintaining a dignified silence and largely ignoring my insurgency, at least in public and in print; the next generation (that of my peers) in some cases taking up the cudgels which their mentors and idols had declined to wield, in others tempering their own traditional viewpoint to accept some of my ideas and consequently being more tolerant of their own students’ attitudes and actions; and the third, the present generation, once removed from the traditional archaeologists and with more permissive instructors, accepting the insurgency to varying degrees and in varying segments of their archaeological theory.

Perhaps another obstruction has been the object-mindedness of many American archaeologists (Taylor 1952). Gordon Willey is a self-identified example of this and, in discussing Lewis Binford, says:

... however, with the outlook of one who grasps the tangible example more readily than the abstract theoretical statement, I await with interest some large-scale demonstrations of [Binford’s] principles with the data of New World prehistory. (1968:53)

And Paul Martin was saying practically the same thing a number of years earlier:
I think Taylor’s ideas would have been far more favorably received and more widely accepted if he had first put out an archaeological report embodying his ideas. . . . To me a concrete example is more easily grasped than an abstraction or a theory; and we who teach could then point to the applications of his principles. I still await with pleasure Taylor’s publications of his archaeological work in Mexico.
(1954:571)

It is unfortunate that Martin must still wait for my final report on the Mexican material and that Binford has yet to publish a full-scale archaeological monograph. But it has always seemed to me that both Binford and I have provided our colleagues with enough pertinent material for them to chew on for quite a spell. The fact that they have not chosen to do so and have been waiting so patiently for us to provide them with spelled-out applications using masses of material objects appears to me to be more a commentary on their outlook and standards of value than a justifiable demand upon us.

I for one regret as much as they not having produced a monograph based upon the conjunctive approach, but I cannot see that my default explains or condones the complete lack, in the literature of American archaeology, of any objective, thorough critique of A Study of Archaeology. Had there been such, it would have constituted a scholarly response rather than, as actually happened, an emotional reaction in defense of a theoretical contradiction inherited by the critics and their cohorts (e.g., Woodbury 1954). In view of our stated aims, the fact that Binford and I have not seen fit to play their game, but have gone ahead with our own, cannot be justifiably held against us.

And finally perhaps, as some have said, there may be intrinsic and/or practical difficulties which have worked against acceptance and use of the conjunctive approach. With present methods, this may possibly be true, although in the absence of any full-scale test I do not see how the critics can be so sure. My own experience leads me to believe that any such difficulties can be largely overcome through developing and applying new, more efficient methods and techniques, both in the field and in the laboratory. What is needed is a flexible, ever-changing, “lively” approach which will demand, and thus have a better chance of getting, more productive methods. This is precisely what the “new archaeology” has demanded and, in many ways, gotten—although of recent days I have an uneasy feeling that their approach is becoming less flexible and more dogmatic, a situation not unusual among evangelical sects. Let me say once more that what we need is not one approach but a series of approaches which will remain receptive to new ideas and which, thus, can be both broadened and refined as we go along. Only by constantly making more specific and more stringent theoretical demands upon our data can we realize their full potential.

But whatever may or may not have been the influence from A Study of Archeology upon contemporary archaeological theory, many of the latter’s methods and techniques are new or are appreciable advances over anything discussed or even envisaged in 1948: the use of computers and backhoes for example. But after all, the conjunctive approach was explicitly stated to be just that, a “theoretical foundation for a viewpoint, a point of attack, or an approach rather than for a particular method” (1948:7). I could and did expect that practical implementation would take turns which, at that time, were unforeseeable. Therefore, despite mutterings of denial from some of its practitioners, I allow myself the presumption of looking upon much of the “new archaeology” as practical application of a basic conceptual scheme, the earliest more or less complete expression of which was the conjunctive approach. When progeny will not own their parentage, it becomes the undignified and distressing but incumbent responsibility of parents to claim their posterity as they understand it. False modesty that obscures genealogy can leave a serious blot on the ‘scutcheon’!

Then what can be said of contemporary
archaeology as it is being practiced? Is it possible to accept it without reservations? The answer, as might be expected, is no, at least in my opinion. As an example, if we take a close look at the “new archaeology,” which is the substance of this volume, I believe that several aspects invite comment and a gentle caveat. I feel unhappy about certain matters of theory and would like to elaborate briefly upon two of them here. But before doing so, however, let me emphasize what I have said elsewhere: “Archaeology in the United States today is a remarkably different discipline from what it was in 1948 and, from my viewpoint, the outlook for the future is tremendously encouraging and exciting” (1968:1).

Perhaps my principal worry concerns the new archaeology’s attitude toward the construction of cultural contexts. It was not to plead for storybook pictures of ancient life that I so strongly advocated cultural context as the minimum performance incumbent upon archaeology as history or as social science, as a part of anthropology. Ford, among others, quite misunderstood my point when he said that I had made a “plea for a more vivid reconstruction of cultural history on the basis of archaeological evidence” (1952:317). I made no such plea, nor was vividness or lack of it any part of my argument! My point was very explicitly stated to be that context is needed because cultural isolates can neither be understood nor properly used without it. This is the old Boasian doctrine and should not have required a lengthy disquisition in order to be comprehended! Furthermore, it should also have been abundantly clear that by the “fullest possible cultural contexts” was meant integrations ranging from the most elaborate narrative descriptions of past lifeways to the immediate matrix of some artifact, event, cultural system, cultural-ecological relationship, or other such isolate: in other words, as much or as little context as required to set the subject of research in its temporal, cultural, and natural perspective.

But it is precisely context of any sort that is skimmed or lacking in the published work of the “new archaeologists.” They seem to be so impatient to get on with their hypothesis-testing that they do not bother to provide either themselves or their readers with the contexts which alone can set their tests and results in an appropriate and necessary relevance. In fact, some of them have gone so far as to deny the possibility of ever making what they rather anachronistically call “reconstructions of past lifeways.” The fallacy of using the word reconstruction was dealt with at some length in A Study of Archeology (Taylor 1948:35), and to use it today is to set up a straw man, to saddle other archaeologists with an admittedly impossible goal and then deny its possibility. In addition to perpetrating this dubious circularity, they have fallen into the very trap against which they were warned:

Finally, . . . it will be well to comment on the fallacy and mental hazard engendered by the use of the term historical reconstruction. The words reconstruction and resynthesis are fundamentally erroneous and have been responsible for much loss of confidence, particularly among the anthropologists. . . . If it cannot be told for sure whether past actuality has, or has not, been recreated in detail or in essence, it cannot be claimed that these contexts are, or are not, resyntheses or reconstructions. These terms imply a re-building to exact former specifications which, from the above, are not verifiable and, hence, not knowable. The unknowable cannot be taken as a standard of value. Therefore, arguments both for and against historical reconstruction in anthropology or in any other [historical] discipline are irrelevant, and it becomes apparent that the work of all historical disciplines really leads to construction and synthesis, not reconstruction and resynthesis. From this, it is further apparent that the real task of the students in historical disciplines settles down to seeing how sound, how plausible, and how acceptable their constructions can be made. Neither the anthropologists nor the historians should use the term reconstruction and thus make himself feel inadequate because he knows that his research will never permit him actually to reconstruct the life of past times with certainty and com-
pleteness. Rather, he should realize that even the contexts written from the best and fullest archives are constructions and that the differences lie in the nature of the respective data, not in the procedures or basic theoretical factors. (Taylor 1948:35–36)

And also in the same vein:

Some archeologists forget that proof of culture and cultural relationships in the past is simply not possible. In archeology we are dealing, and perforce must deal, with probabilities and, we hope, with “ever closer approximations” to some finitely unknowable reality. Our only tribunal is professional acceptability or our personal values. If this is remembered, then much of the steam is taken out of arguments about “knowledge of the past,” “verifications,” and other such absolutistic value concepts in archeology. (Taylor 1969: 384)

Can it be that their love affair with science has not only led the “new archaeologists” to reject what they know (and everybody else knows!) they cannot prove but has also blinded them to what is the only solution to this predicament: a recognition and an acceptance of the essential nature, the relativism, of their research materials? I hope that as they mature, individually and as a school, they will relinquish their shibboleth of proof and their denial of cultural context to become more relaxed and understanding and tolerant of their lot. If they do this, I believe that they will have a much greater chance of attaining their goals. If they do not, I feel that they will only raise more problems.

For example, it has been stated by Binford and others that the ultimate goal of the “new archaeology” is the formulation of laws of cultural process, which goal is to be attained by the explication and explanation of cultural similarities and differences. But I would ask: how are they going to describe these cultural similarities and differences, much less explain them, without recourse to the very cultural contexts which they claim are impossible of achievement? Or must we believe that they will come upon these explanations a priori or by some decree ex cathedra? Once again I sense an impatience with what other anthropologists consider to be a most necessary procedural step. The “new archaeologists” seem to wish to deal with abstract, universal concepts without dealing with the individual, partitive cultural contexts from which alone such abstractions can be derived or have meaning. In his reply to Leslie White, Robert Lowie made much the same point with regard to the work of Tylor and Morgan, asking how their evolutionary formulae had been derived:

Are they empirical inductions? In that case they must rest on observations of the history of specific tribes. Or are they all a priori constructs. . . . the Boasians do claim the right to check evolutionary generalizations by the facts they are meant to explain. . . . (1946:231)

In other words, if the “new archaeologists” claim that it is impossible to make what they call “reconstructions of past lifeways” from the data of archaeology, then on what bases are their processual studies to be made and checked? Adapting a phrase from Lewis Binford, I may say that they appear to have painted themselves into a theoretical corner!

My second point of issue concerns the concept of culture. I do not believe that it is coincidental that the “new archaeologists” should find themselves aligned with the evolutionary anthropologists in this matter. Leslie White, avowed champion of Evolutionism, has obviously been a guiding theoretical spirit behind the “new archaeology.” Many of its practitioners use his concept of culture: the extrasomatic means of man’s adaptation to his environment. Whether or not this definition is practical in working with living cultures is not a moot question here. But its application to archaeological research most certainly is—and I have serious doubts as to the utility of White’s definition in our field. His definition is explanatory and hence dynamic; it tells what culture does. It does not tell what culture is, what culture consists of; it is not a descriptive definition. Now the ethnographer can observe culture in action in the form of cultural behavior and in its adap-
tive relation with the environment. Therefore, he can use a dynamic, explanatory model from the inception of his studies. This is not possible for the archaeologist. His empirical data are static and, in order to accomplish his first task which is to delimit his empirical data, he must use a static model, i.e., a descriptive concept of culture, not a dynamic one. Only after he has identified his data and then only by use of inference from that data, can he apply a dynamic, explanatory model. By using only a dynamic model, that is by using it first, the “new archaeologists” are once again skipping a necessary procedural step and weakening their results.

I should like to close on a rising pitch. Despite what I have said about certain faults I find with the “new archaeology,” I have no inclination to throw the baby out with the bath. I am excited and enthusiastic about what the “new archaeologists” are producing. Given the brief time-span of their work, they have made great strides and notable contributions. I believe their future holds much promise. I hope that they will take these few thoughts of mine as they are meant, as a way of showing my interest and, hopefully, of making a small contribution to their work, to “our” work.
CONTEMPORARY ARCHAEOLOGY

A Guide to Theory and Contributions

Edited by Mark P. Leone

SOUTHERN ILLINOIS UNIVERSITY PRESS
Carbondale and Edwardsville
Feffer & Simons, Inc.
London and Amsterdam