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*CURRENT AND FUTURE DEVELOPMENTS IN ARCHEOLOGICAL
THEORY BUILDING WITHIN THE CONTRACT FRAMEWORK*

by

Albert C. Goodyear
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CONSERVATION ACT: RETROSPECT AND PROSPECT. Forty-first
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INTRODUCTION

The scientific goals of archeology have been described and emphasized over and over again in the literature of the past decade. The primary goal, that of the elucidation and explanation of cultural processes, firmly links archeology to the broader goals of social science and to anthropology in particular. As archeology has begun a new professional growth period into what is referred to as cultural resource management, or contract archeology, it is both appropriate and necessary to continually examine the progress we are making in the realm of theory construction. If theory is conceived as the total informational system we control at any one time about events of the past, we can argue for the primacy and presence of theory in every aspect of doing archeological research. Few would argue against the notion that the main purpose for doing archeology is to create knowledge about the past. Granting this, it is possible to evaluate the overall success in all operations connected with archeological research in light of their contributions or potential contributions to archeological knowledge.

Theory used in this way is a rather broad concept. In a stricter philosophical sense, formal theory with a set of covering laws is the ultimate form we wish our theorizing to take. Causal-explanatory theories and contemporary Environmental Impact Statements while admittedly rather disparate affairs at the moment, need not be and must not be if we are to more effectively integrate contract archeology into the mainstream of current archeological research trends. But to simply conceive of theory in the causal-explanatory sense is an oversimplification of scientific research in general and archeological research in particular. Let us consider for the moment that theory relates to all aspects of

hypothetical or conceptual activity (cf. Dunnell 1971: 26-34). In this way nearly every facet of research has at least an implicit or latent theoretical nature. In the absence of a formal codified set of laws and demonstrated theories, experimental laws, empirical generalizations, and explicitly and implicitly made assumptions seem to comprise the repertoire of current archeological theory. Since many of these are implicit but have a great effect on the direction of research, they need to be identified and made explicit. Where they are absent they need to be created. This is particularly critical since we often see contract related research done in service of no apparent theoretical goals.

As Schiffer (1975a: 1) has pointed out in the introduction of the Cache River report, traditional salvage archeology and to a great extent contemporary contract archeology have made few contributions to archeology's store of method and theory. While such a statement is not intended to negatively criticize or denigrate the substantive contributions of many hard working, well intentioned archeologists, the statement remains descriptively true. The task before us is to devise ways in which to allow archeological research in the contract setting to make more theoretical and methodological contributions to archeological science.

Much of what is called contract archeology currently has only the most tenuous connection with theory building or with theory using. Many projects seem to be done with the sole purpose of generating funds to keep a research outfit financially solvent. There seems to be the tacit assumption that finding sites in an impact zone and making preliminary identifications of the artifacts is somehow sufficient or will hopefully add up to something. In other cases it seems that archeologists believe that they basically "know" what the remains mean, and what is needed is

more data of that type. This type of strategy is most often associated with a culture-historical approach to research emphasizing a rote pigeon-holing of artifacts into time-space categories. In addition to being implicit and heavily inductive, the debilitating effect is compounded by the behaviorally empty character of most culture-historical schemes.

When so-called problems are defined in culture-historically oriented work, they are relatively pedestrian and refer to such descriptive questions as who lived here, how old is the site, and what artifact "types" were found. It is not that this type of information is not useful or relevant, but it cannot be the primary focus of anthropologically based research. When such questions provide the orientation, a great particularizing effect takes place, and the development of behavioral or systemic problems is stifled. The answers to questions concerning site histories can usually be resolved in the course of more behaviorally oriented research.

One of the basic tasks of every contract funded project concerns the assessment of archeological significance. Determining significance for archeological remains is a thoroughly theoretical matter. From a social science viewpoint, the significance of a site or set of sites relates directly to its information content about the structure and function of past cultural systems. Therefore, depending on the questions asked or the problems posed, significance will vary depending on the perspective. Although the information content of a set of remains is probably not infinite, it is doubtful if any one research design or set of designs ever comes close to exhausting the data base. We approach the significance and potential significance of archeological remains

through problems, hypotheses, and models that may be relevant to those remains. Since the cultural systems that produced such remains were complex and diverse, it follows that in order to effectively realize the information potential of a set of data, we must examine them from several problem-oriented perspectives. At least that much is due our data base, non-renewable and dwindling before our eyes.

THE NEED FOR RESEARCH DESIGN

One of the chief weaknesses in contemporary archeology relates to the weak or nonexistent notion of research design (Goodyear 1975). The utilization of explicit research design in normal scientific activity is commonplace. Binford (1964) was an early proponent of research design and since that time several papers have been published exploring the relationships among theory, method, and data. There has been a great deal of lip service paid to using research design in archeology, not just contract archeology. But in many cases the so-called design can be seen to be no more than a reenactment of what has been traditionally done. A research design does not consist solely of a budget for field operations or a projection of man-days and equipment costs necessary to dig sites, although a well constructed research design will make careful provisions for these tactical or logistical problems. The sine qua non of a research design must be the questions, problems, or hypotheses which are being formulated and tested, and which can be linked to methods adequate to their evaluation.

Explicit, written research designs help identify problems, hypotheses, or unexplained patterns in the data. By knowing these before field research, there is a greater likelihood that relevant data can be collected toward their resolution. A written research design helps publicly to monitor and control the biases of research. Written designs, therefore, allow the evaluation of progress and efficiency of a piece of research as well as an assessment of success or failure. While it is at least possible to conduct research around specific problems without written designs, as the problems become more complex and the data requirements more comprehensive, a certain inefficiency prevails which detracts from the overall effort.

It becomes intellectually impossible to adequately integrate the crucial conceptual and empirical components of successfully conducted research.

In essence, a written research design is a complex enactment of the hypothetico-deductive method. As King (1971: 258) and others have pointed out, however, under the usual process of hypothesis testing, the site or data base is chosen for its probable relationship to that hypothesis. While it must be admitted that this poses an obvious constraint on the type of problems which can often immediately be investigated, it is certainly not the case that significant and worthwhile problems cannot be posed and examined in a contract framework for we have too many examples to the contrary. What is required in these situations is serious research planning beforehand which will yield important problems and relevant approaches. For example, the fact that ecological and settlement-subsistence approaches can and have been taken to practically any contract situation and yielded significant results is an indication of the relative strength of these strategies. It can be interpreted as a sign of great progress when any contract project, regardless of size or setting, can be plugged into a series of standing research designs such as those treating regional adaptations, activity reconstructions, paleoecological studies, regional sampling methods, and many others. The point is when we are at the stage of having developed a multiplicity of ongoing designs, models, and problems, then the constraints of having to do archeology in areas dictated by the needs of impacting agencies will be greatly reduced. By having standing designs that treat general problems many of the small-scale projects will also begin to have more value since they will serve as individual tests of broader patterns and predictions.

TYPES OF RESEARCH DESIGNS

Several examples of research have already appeared in contract archeology which illustrate the relative success and value of implementing problem-oriented research at every level. I would like to briefly review some of these designs, their goals, and areas of success.

Perhaps the broadest type of design might be referred to as General Research Designs. An example of this type would be a general overview that I wrote upon assuming the job as Highway Archeologist at the Institute of Archeology and Anthropology, University of South Carolina. Before actually starting any fieldwork related to highway impacted sites, I made a thorough review of the strengths and limitations of doing archeological research in what might be referred to as a "narrow right-of-way." Given the conditions of sampling inside a transect-like corridor, I began formulating general problem domains that could be approached primarily around the intensive analysis of individual sites. These broad problem domains include (1) cultural identification, (2) activity analysis, (3) subsystem reconstruction, and (4) cultural ecological analysis (Goodyear 1975). Since South Carolina is relatively unknown archeologically, these domains allow us to bring to every site or sites a set of relevant problems which can be attacked early on at the E.I.S. stage of research. Of course problems lead to the further recognition of additional problems from which hypotheses and models can then be constructed. In the event of mitigation by excavations, there are plenty of relevant questions ready to be posed before the site is dug. The general research design such as that written for the overall operation of a specialized program such as our highway program, is not intended to take the place

of additional designs needed to accommodate the particular substantive aspects of a site or series of sites. Rather, it is designed to facilitate systematic research at the earliest possible stage and to allow every project regardless of scale to regularly feed information back into our general fund of knowledge. This is a particularly efficient exploitation of the archeological record since many projects do not lead to further field investigations after the E.I.S.

Another class of designs which is beginning to emerge from contract research can be called Topical Research Designs. Again, like general research designs, such plans are not necessarily restricted to any one project or set of sites. Since institutions conducting contract supported archeology more often than not are doing research in regions and under conditions with which they are not familiar, many of these designs relate to methodological problems, especially inductive sampling schemes. For example, regional and intrasite sampling strategies often must be developed anew for each project in order to obtain the best fit between suspected archeological populations and intervening conditions such as buried sites, type of vegetation cover, plowed sites, and appropriate sampling fractions. At the Institute of Archeology and Anthropology we are currently experimenting with a long-range study of intrasite sampling methods on a large multicomponent prehistoric site which has a heavy history of plowing and soil erosion. We are trying to determine the effects of plowing and erosion on the horizontal distribution of artifacts, as well as the vertical, by using a stratified unaligned probability sample. Data from the surface of this site are being analyzed spatially with computer mapping programs such as SYMAP and SYMVU, as well as statistical studies. Yearly surface collections are being made using the same sampling design by randomizing

the locations each year. Thus, we are interested in determining if broad and repetitive intrasite patterns can be successfully identified each year and secondly, if plowing and erosion have noticeable effects on these spatial patterns. Also being experimented with are different methods of subsurface sampling to be used for site detection during E.I.S. phase research. We must develop appropriate methods of sampling per region adapting the sampling design according to plant cover, depth of sites, and probable underlying settlement patterns.

Yet another type of topical research design might refer to a specific cultural system of broad areal distribution, such as Hopewell or Middle Mississippian. Prior to the recent Arkansas Archeological Survey E.I.S. study on the Cache River, Dan Morse had developed a model for the early Archaic Dalton settlement pattern which was further tested during the Cache River Project by House and Schiffer. The testing of this model turned out to be one of the more important theoretically-related activities of the Cache River Archeological Project (Schiffer and House 1975). In South Carolina and adjacent states, the areal manifestations of the late prehistoric Mississippian pattern is referred to as South Appalachian Mississippian. Leland Ferguson of the Institute of Archeology and Anthropology is adapting his doctoral dissertation (1971), which concerned the broad geographic patterns of this cultural system, into a topical design format focusing on the conditions of the growth, spread, and decline of South Appalachian Mississippian.

A truly etic research design was written by Mike Schiffer (1975b) for use in the Cache River Project which treats the effect of occupation span on site content. Schiffer derives a set of laws in equation form that specifies the relevant variables to be measured and controlled for

determining occupation span. This type of research design is timeless and spaceless in the sense that all archeologists must allow for the complicating effects of occupation length of a site when making behavioral inferences based on the contents of a site.

Finally, another class of designs that is showing increasing popularity due to their workability can be referred to as Regional Designs. Regional considerations of artifacts have been implicitly used for years in archeology, having been explicitly developed in part by Willey and Phillips (1958) in their use of archeological areas, regions, and localities. One of the first and most comprehensive attempts to establish a regional orientation to a single state was that instituted by the Arkansas Archeological Survey in 1969 under the guidance of Charles McGimsey. Each regional archeologist provided a summary of the then known knowledge of his or her respective area and identified significant problems for future research. I understand this is now being done again. There are several benefits in doing periodic summaries of regional research. In addition to explicitly recognizing relevant problems, formulating hypotheses or constructing models to be examined as future projects take place, there is also the good effect of explicitly and succinctly summarizing that which we already believe we know. Long term regional designs are extremely useful for taking advantage of many already known culture-environmental relationships. The identification of critical environmental variables and their geographic extent are rather obvious first order questions in regionally oriented research. As Struever (1971) and others have pointed out, the region must be the spatial focus for useful culture-ecological analysis. The region can be examined at various scales and when looked at very broadly in areal terms, can

encourage the formation of areal or regional research groups who team up to research problems of mutual interest. A good example of a successful interregional or areal effort would be the Southwestern Anthropological Research Group or "SARG," who are interested in studying broad patterns of adaptation to the arid Southwest. A similar group has formed comprised of archeologists from states near the Central Mississippi Alluvial Valley to exchange research designs and information. This group is known as the Central Lowlands Archeological Seminar and Symposium (CLASS). We have always known that contemporary state boundaries should not get in the way of our research, although they seem to anyway since we have so few examples of multi-institutional and multistage organizations of research. Groups sharing a single or several research designs are extremely valuable for the study of specific regions or areas.

Before leaving the topic of research designs I would again like to point out their utility in that they summarize current and previously acquired knowledge and accordingly form a contribution to knowledge in their own right. Secondly, there is a beneficial cumulative aspect to them as well. If a thorough background research is performed on a topic, a region, or cultural system, that much has been done and does not necessarily need repeating next project around. Many research designs develop problems that are not readily solved and require several projects in time toward adequate resolution.

CONCLUSIONS

In several ways this paper has been more about the hypothetical or the methodological rather than the theoretical aspects of doing archeology in a contract situation. This is understandable in light of the dearth of tested and confirmed explanations archeologists of all manner have thus far produced. It is important to realize, however, how the more successful natural and social science disciplines have been able to construct theories. An explicit awareness of the scientific method is one outstanding feature of successful research leading toward theory building. If archeology, and I mean archeological research regardless of the source of funding, is to become a successful social science as it aspires to be, then the entire research enterprise must be coordinated with theoretical goals. One of the exciting facets of American archeology as it expands under the aegis of federal legislation and funding, is the increased number of professional archeologists present to do the job and the large sums of money available to carry out diverse aspects of research. Many of the big questions archeologists have been fond of asking over the years will now have the research support for their thorough investigation. Processually oriented research in a comparative sense is expensive research. More than just "diagnostic" potsherds from a few key sites is required to design and test models of cultural systems. The only way we can justify spending these unparalleled sums of money is to produce a body of information that contains both adequate descriptions and explanations of the past.

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