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The Nature of Archaeological Fieldwork: Implications and Limitations for Research Procedure and Organization

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THE NATURE OF ARCHEOLOGICAL FIELDWORK:

IMPLICATIONS AND LIMITATIONS FOR

RESEARCH PROCEDURE AND ORGANIZATION

by

Mary B. Struever

A Thesis Submitted to the Faculty of the Graduate School
of Loyola University of Chicago in Partial Fulfillment
of the Requirements for the Degree of

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VITA

The author, Mary Barnard Struever, is the daughter of Carl C. Struever and Nancy (Schermerhorn) Struever. She was born in Hanover, New Hampshire, on March 10, 1949.

She attended Monroe High School in Rochester, New York, where she was graduated in 1966. In September, 1966, she entered The University of Chicago, and received the degree of Bachelor of Arts in September, 1970, with a major in anthropology.
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INTRODUCTION

In the past decade or so archeology, particularly in America, has been going through some important theoretical revolutions. These revolutions are not subtle and underground, but very conscious and well-publicized, with various archeologists taking sides often in the nature of a generation gap dispute or a new religion with its converts and opponents. Lewis R. Binford, a ring-leader in many of the recent archeological innovations, states in an article introducing a landmark collection of essays by proponents of the "New Archeology" that "the changes in archeology" which are currently taking place "are more than simply new methods and new theories; the changes consist of theories and methods developed in the context of a new epistemological perspective on such basic issues as the appropriate scientific procedures to be followed in investigating the past" (Binford in Binford & Binford, eds., 1968:17). Others likewise express a conviction that some significant changes have been happening in archeological goals.

In the last decade, prehistoric research has attained a new level of sophistication in the gathering and interpretation of archeological materials. The revolution that has taken place is a two-fold one, involving the development of new methodological approaches to the gathering and simple description of data...and the construction of new theoretical approaches to the interpretation of those data. (Freeman in Lee & DeVore, eds., 1968:262).

"As general anthropological theory has advanced, new and exciting problems have been conceptualized for archeology" (Struever, 1968:131). Some archeologists describe a very strong personal reaction to the
changes in approaches to archeological material: "A conceptual trans-
formation, a revolution, has taken place for me" (Martin, 1971:1).

The new directions in contemporary archeology are very important
to the health and vigor of the discipline, for they reaffirm the ability
of archeology to address itself to more significant statements about
past human existence than simple trait inventories of objects dug out
of the ground and spatial and chronological maps of variations in these
inventories. The new archeology begins with the assumption that infor-
mation is available in an archeological context to be applied towards,
and eventually to answer questions of, general social scientific impor-
tance. "Laws linking archeological remains to cultural processes and
events enable archeologists to use general processual laws to explain
situations...Such use of the data of prehistory makes it available to
social science as a whole" (Watson, LeBlanc, & Redman, 1971:30). Taylor,
in 1948, resisted the then current tendencies to obscure archeology's
objectives and its relations to other disciplines. He saw "Americanist
archeology included within the discipline of cultural anthropology, yet
having the explicitly stated objective of reconstructing history" (1948:
26). He understood the value then of explicitly defining the relation-
ships between archeological objectives and methods and those of cultural
anthropology. Indeed, it is clear in retrospect that this attention to
what actually are the questions that should be asked, and the appro-
priate methods of data collection and analysis to answer them, has helped
cultural anthropology itself become more explicitly aware of its own
goals and methods, and has opened up new directions for research.
Binford makes a case for the development of a theoretical orientation in archeology, on the grounds that archeology will otherwise be unable to make a contribution to anthropology as a whole.

Archaeology must accept a greater responsibility in the furtherance of the aims of anthropology. Until the tremendous quantities of data which the archeologist controls are used in the solution of problems dealing with cultural evolution or systemic change, we are not only failing to contribute to the furtherance of the aims of anthropology but retarding the accomplishment of those aims. We, as archeologists, have available a wide range of variability and a large sample of cultural systems. Ethnographers are restricted to the small and formally limited extant cultural systems.

Archeologists should be amongst the best qualified to study and directly test hypotheses concerning the process of evolutionary change, particularly processes of change that are relatively slow, or hypotheses that postulate temporal-processual priorities as regards total cultural systems" (Binford, 1962; reprinted in Binford, 1972:31).

Before the kind of information which is useful to social science generalizations can be available through archeological investigation, "archeological methods" must be "adequately developed so that cultural events and processes of significance to general cultural theory can be recognized in the archeological record" (Watson, LeBlanc, & Redman, 1971:25).

If archeology can make very clear--first to itself as a self-conscious, active discipline, and then to the rest of anthropology and other disciplines--that its aims properly should be "the whole of material culture" (not just whatever is old and underground), relationships between remains (not just an inventory determined by presence or absence of traits), and "behavioral reasons for differences in the data" (Deetz, 1971:4), then archeology will be in a position to make a more explicit and productive contribution to academic thinking. Before such a contribution may be effective, archeology must achieve a self-concept as a
discipline with a fresh approach to data that has potential for throwing light on all kinds of behavior. Some archeologists are becoming concerned that their discipline should not be thought of as a quaint and obscure occupation (in other words, that the public, as well as other social scientists, should not equate "archeologist" simply with "excavator of antiquities"), but that archeology should be thought of as a way of thinking about human behavior, a way of approaching everyday activities as a means to understanding the nature of lifeways and the dynamics of human culture. Deetz has remarked that "the archeologist's reluctance to come out of the earth and consider the whole of material culture has probably had some unfortunate effects on the development of archeological method and theory" (Ibid.).

Archeology has always had a special contribution to make to the social sciences, simply in terms of its data base, and now that archeology is turning its interests to dealing with questions of general social scientific interest, this contribution is more important. In order to deal with certain issues, it may be necessary to have data from a wider time or cultural basis than is available for observation either now, or within the span of recorded human history. Archeological data, for instance, can be used to increase the sample size for a problem dealing with a largely extinct culture type such as hunters and gatherers. Archeology is the potential "source of large quantities of independent data for testing laws" (Watson, LeBlanc, & Redman, 1971:31). Archeologically-derived information can provide a unique and critical contribution to the study of prehistoric cultural evolution. "Only archeology has access to
the long time-spans necessary for the formulating and testing of hypothetic laws concerning the development of technology, social and political organization, art forms...in nonliterate societies" (Watson, LeBlanc, & Redman, 1971:162). Archeological data "are the only data that can be used to test and to confirm generalizations about evolutionary change in human societies" (Watson, n.d.:1). Other archeologists have pointed out this particular contribution of time depth which archeology stands to make to the other social sciences. "...[A]rcheology provides a unique laboratory for measuring the interaction of social variables through time" (Leone, 1968:1150).

That part of anthropology known as archeology is concerned with culture in the past—the extinct lifeways of former peoples, how and why they changed and developed, and the significance of this to developmental process and to our understanding of culture. In short, archeology adds a vital time dimension to the study of man" (Deetz, 1970:115).

Archeologists

...have knowledge of the material remains of populations, and thus...can develop techniques for measuring variations in the demographic and behavioral characteristics of such populations over long periods of time in relationship to specific complexes of biological, natural, and cultural features of their ecosystems. (Harris, 1968:360).

"The chronological data are clues to the identification of antecedent and consequent conditions in slow-moving cultural transformations" (Spaulding, 1968:38). In particular, "the archeological record affords many more examples of results of such processes and events than are available historically or ethnographically" (Watson, LeBlanc, & Redman, 1971:30-31). "If anthropology, as the study of culture, is to be able to generalize fully, to base its conclusions on the broadest foundations, it cannot be satisfied with data gathered only from the shallow depth
provided by the ethnographic reach" (Taylor, 1948:154). Insofar as "the archeological record is taken as a giant time-space laboratory for social science," what we can know from archeology is critical to the understanding of the broad sweep of human history, as it allows a time depth not approachable in ethnographic studies. "The archeological record,, can be viewed as one means by which human behavior can be investigated both diachronically and synchronically" (Watson, LeBlanc, & Redman, 1971:24). Archeologically-derived information is required in order to compare the nature and processes of culture in the past with what is known about these things in contemporary groups. Information abstracted from living groups, about societal and cultural dynamics and long range change,

may hold true for living groups, but nothing is thus proved as to its applicability in the past. Whether the same conditions existed then or are recent developments, whether specific aspects are due to demonstrable borrowing or are native as far back as we can go: these are vital questions not to be answered with data from a single time-plane. (Taylor, 1948:156).

The historical dimension which archeology adds to an anthropological study of a people makes the analysis and interpretation that much more interesting; it just adds a whole other dimension. The archeological record chronicles a vastly complex matrix--of human society with networks of carefully articulated structural relationships that make the society able to continue to function on all different levels--and this matrix is constantly in flux. What's more, that rate of flux can shift a great deal, whether to accommodate changes necessary to maintain a society as a viable entity, or all the way up to complete systemic
change. It is this combination of the potential for both a wide range in time depth and a detailing of systems of integration and synthesis of parts, that gives archeology the role of contributing a unique data base to the progress of the rest of the social sciences.

The aims of delineating general cultural laws for human social and subsistence organization and processual change, through information gathered archeologically, helps give archeology new relevance to anthropology and the social sciences in general. "Archeology can aspire to share with social sciences the goal of dealing with general laws, the purpose of general laws being to explain and predict human behavior and the dynamics of cultural process" (Watson, LeBlanc, & Redman, 1971:167). Once archeology moves beyond the simpler aims of establishing chronological and spatial relationships between human groups, and reconstructing past lifeways, and begins to deal with explaining and predicting patterns in human behavior with a context of other humans and a biological and geographical environment, certain problems in the application of scientific methodology become apparent. Archeology shares with the other social sciences the difficulty that, though they deal with generalities about human behavior and patterning of human activities, people are very difficult to experiment with. It is hard to create meaningful test situations that can effectively measure any given parameter of behavior, attitude, etc. Consequently, "controlled investigations of various kinds and diverse observations in many times and places are necessary to provide sources of independent data for testing hypothetical laws and explanations" (Watson, LeBlanc, & Redman,
In archeology this means that no one site can provide conclusive generalities about the nature of human behavior in a given situation; comparison on a regional or cultural level is essential.

There is obviously a consensus that there is a great deal of positive potential in these new approaches to archeology, and that they point in a good direction. However, this paper proposes that a critical gap exists in the application of this theory to actual field practice in contemporary archeology.

Field investigators have suffered from lack of information about the site they are about to dig, and from lack of practical methods for development of specific directions for research. The emphasis on strong theoretical direction of research efforts is still relatively new. "Although the literature of archeology is vast, only a small fraction of it deals explicitly with theoretical issues" (Deetz, 1971:2), and this discussion has centered around models for human behavior and societal functioning, but very rarely has there appeared explicit discussion of what kinds of propositions and tests of actual data will work towards these ends. James N. Hill's article "Prehistoric Social Organization in the American Southwest" (1970) is a rare and notable example. Other, more limited attempts to produce specific test implications for hypotheses are: Deetz (1965), Dethlefsen & Deetz (1966), Longacre (1966; 1968), Hill (1966; 1968), Whallon (1968). The essential paradox is that you almost need to know what you are going to find ahead of time, in order to develop really meaningful hypotheses to direct excavation. That gap is very little recognized and is rarely discussed explicitly, with a view
towards dealing with it effectively. In the author's experience with archeological field research, limitations placed on theoretical orientation and problem development by the excavation situation have been recognized and disparities between professed objectives of research projects and the actual procedure of data collection and manipulation have been noted. It is believed that it would be worthwhile to delimit some of the determining or influencing parameters in each of these fields experiences, and to point out instances where objectives were not really realized in the form originally anticipated or at the juncture in the research process, and to discuss the significance of the actual progress of the research and its relation to a theoretical format.

The goal of the paper will be to analyze the archeological research process (particularly the field situation). Toward this end the methods and techniques for data collection and analysis as structured by the theoretical goals will be delimited and compared to the more immediate goals (problems, hypotheses, questions) as structured by the data. "The task will be to analyze what the archeologists say they have been doing and what they have actually done, and then to see how these two bodies of fact compare" (Taylor, 1948:43).

Like any other human behavior, archeological research may be treated formally as a system of activities subject to behavioral analysis. When research is conceived in this way, it is appropriate to ask, 'How do the actors make decisions?' 'What are the regularities in their decision-making processes?' and 'What set of rules can be specified which models those regularities?'. Once such rules are stated, it becomes possible to evaluate their efficacy as means to solve the research problems which delimit the goals of the research. (Wilcox, in press :1)
CHAPTER I

THE NATURE OF ARCHEOLOGICAL DATA AND IMPLICATIONS

FOR METHODS OF COLLECTIONS AND ANALYSIS OF DATA

In order to handle the masses of information contained within any site, the investigator must necessarily adopt some conventions for the purpose of description; even if he were to attempt the absurd task of describing each individual item or relationship separately, the very words he used would constitute decisions on his part as to the relevant descriptive characteristics. Some sort of categorization of events in the archeological record is necessary in order to begin to recognize regularities and patterning in the data. It is essential to note from the start that any system of classification or taxonomy necessarily imposes certain concepts of what attributes are significant and implies notions of the role of objects within a human system, and thus about the nature of patterning of behavior.

Any system of classification is fundamentally based on the units of description that are used. What possible kinds of units are there and how are they derived? Leslie Freeman's depiction of a research strategy begins with the "slow and painstaking isolation of regular types of associations of materials," and subsequently "their formal equation with activity types" (1968:266). Marvin Harris sees all classification as ultimately based on the assumption that all things which share a quality of "sameness" should be lumped together. The difficulty
with "sameness" is that, though based on a very simple notion, it is often difficult to assess accurately. "Sameness is a purely logical relationship which can be demonstrated for logical constructs, but not for empirical entities" (Harris, 1964:8). Even the system of binomial classification, widely used in categorizing and quantifying archeological data, necessarily shapes the nature of the information collected. Taylor mentions "...it is important to note that this method can be used to present only those relationships which have already been hypothesized by the classifier" (1948:146). The traditional approach to establishing differences in non-verbal data (such as archeology is limited to) is a "particularistic" view: artifacts are seen as equal and comparable traits that are separated into like groups (Binford, 1962; reprinted in Binford, 1972:21). This approach to establishing similarities and differences and to finding patterns in these similarities and differences centers on setting up typologies, and Bobby Jo Williams points out that "the establishment of typologies, by its nature, obscures finer cultural differences" (1968:162). Data units which simply seek locate similar attributes and add them up to get a composite picture of a culture are very limited in their application. "It is impossible to get at the cultural significance of any artifact merely by classifying it with certain more or less similar artifacts and noting its presence within an archeological site" (Taylor, 1948:79).

If the investigator is interested in more than an additive list of the various traits recognizable in a culture, then certainly he must look for other things besides the indications of various attributes in
the archeological record, and he must extract more information than simply their presence or absence. The archeologist's job will be to articulate between a human set of behavior in the past, and his own reconstruction and explanation of the functional operation of that system in the present. The archeologist "must be able to translate the nature and distribution of cultural debris into the behavior of the prehistoric people responsible for it" (Watson, LeBlanc, & Redman, 1971:54). "Archeology... is faced with the task of correlating the structure of material elements of a cultural system with the structure of behavioral attributes of that same system" (Struever, 1968:134). The investigator must make this translation by means of a system of classification and ordering of data that does not necessarily (indeed, can hardly hope to) duplicate the cognitive system of the people whose behavior it seeks to elucidate. Binford has indicated his view that the order in the material record of a social group, as perceived and tested by the archeologist, will reveal an accurate picture of the structure of that group and its systemic operation, whether or not the revealed order is a kind which was apparent to those who originally used and manifested it.

...[G]iven the theoretical tools available to us we may:

1) ask certain questions about the past or about the operation of cultural systems generally;

2) develop classificatory criteria which inform on variables believed relevant to the questions being asked;

3) investigate the archeological record in terms of these criteria and draw valid conclusions, irrespective of the degree of conformity between our criteria and the cognitive systems of the manufacturers of the artifacts we study. (Binford, 1967b;

What new information could possibly be gained about variations in the activity systems of the past by simply fitting archaeological remains into types which are ordered in terms of our preconceptions of what those activities were? Our task as archeologists is to devise analytical means of discovering what past activities were, not to fit artifacts into activity classifications arrived at arbitrarily. (Ibid.:71).

Similarities and differences in archeological remains are to be explained in terms of the functioning of material items in a cultural system, and in terms of processual features of the operation and evolution of the cultural systems, according to L. R. Binford (1968c:273). In this way, artifact forms, associations, and distributions, as they are observable in the ground, are related to the much wider cultural systems.

The patterned relationships among classes of artifacts should document the context in which they were made, used, and lost or abandoned. It is essential to measure the mutual covariation among all classes and types of archeological data; the structure of this covariation, once delimited, should reflect the organizational and behavioral aspects of the society that produced it. (Longacre 1968:91).

The aspect of archeological data that is significant for retrieval of information about human cultural systems is not that material products of culture come in different forms, but that these differences are patterned in the archeological context just as they are patterned in day-to-day usage. "Human behavior is patterned...; and if the patterning has not been disturbed by erosion, plough, or pot-hunters, it can be recovered by proper techniques of limited excavation, that is, by an adequately designed sampling procedure." (Martin, 1971:5).
The variations in form and distribution of artifacts which are observable in the archeological record are owing to a "full range of determinants which operate within any socio-cultural system" (Binford, 1968, in Binford and Binford, eds, 1968:22), therefore many aspects of cultural patterning and operation are discoverable in material remains. Though an activity cannot be reconstructed "unless that activity produced some preserved material evidence," aspects of society not directly preserved are still referred to (and partially elucidated) by more direct evidence. "The mechanism of socialization...influences the patterning of activities in the society, right down to the form of the tools made and used by social units" (Freeman, 1968:265). The way an object appears in the archeological record is a reflection of all or some of the ways in which the object entered into the culture's systems operation, in its individual life history of manufacture, use, discard and deposition. Since this is true, the principles operating on that object that are responsible for its final location in the site, should be discoverable. This is the record of non-material behavior recorded in material remains. The forces acting on the objects are there, and lack only the proper means for seeing or discovering them.

Walter Taylor maintains that the most useful (or meaningful) way to look at houses and their features, and at smaller artifacts, is "as the material environment of human activity" and not as descriptive units discussed "for the purpose of comparisons with other such phenomena at other sites" (Taylor, 1948:72). The latter view would relegate the material paraphernalia associated with a culture to a role as an
identifying tag for a people or a time period, rather than recog-
nizing its functions and different roles in the operation of the cultual
system. The concept of role of objects or activities in the synchronic
or long-term functioning of a social group reveals different attitudes
about what information can be recorded (and hence discovered) in arche-
ological remains. Spaulding maintains that archeology can be "defined
minimally as the study of the interrelationship of form [physico-
chemical properties], temporal locus [dating of prehistoric events by:
time of manufacture, period of use, time of deposition], and spatial
locus [position in the three-dimensional world] exhibited by artifacts"
(1960; reprinted in Deetz, ed., 1971:24). The significant variables in
the archeological record are by this view objects, floating in space
and time—objects whose end location is the result of human behavior,
and perhaps of further forces of subsequent human behavior, disturbance,
or decay,

Walter Taylor though, as early as 1948, recognized another type
of relationship in the archeological record, namely the relations between
objects, features and constructions.

There is one source of archeological data, rarely mentioned or
fully utilized in archeological literature. I refer to what may be called the affinities existing between the material remains:
between individual cultural objects, between groups of objects,
between objects and the natural environment. These affinities
are as much facts and as much integral parts of the archeological
data as are the material objects themselves...Without them there
is possible only description and superficial quantification of
material objects. (Taylor, 1948:111-112).

The recognition of relations as a valuable source of information, in
addition to and apart from entities, implies a particular understanding
of the functioning of items within a culture: that behavior is patterned
and that the patterning is manifested in the flow of objects within a
culture and hence can later be revealed to one who sees only the pat­
terning of the remains and not the behavior itself.

Assumptions about the nature of human behavior and the role of
objects in a system indicate an attitude about the nature of informa­
tion contained within archeological data. Thus one recent archeologist
considers the vital essence of archeology to be "a concern for the
relationships between man's visible and measurable modification of his
environment and his invisible and less easily measured social and
ideological life. Both are regular, patterned, and interrelated" (Deetz,
1971:3). Binford states, "we approach our task by developing
methods and procedures that will permit us to demonstrate order in our
data. It is assumed that the demonstration of order implies a set of
systematic relationships among cultural phenomena that existed in the
past" (Binford, 1967; reprinted in Binford, 1972:49). Watson, LeBlanc,
and Redman hold a fundamental assumption that there is "a real, know­
able, orderly world" (the world of past human events and behavior
patterns), that they maintain is basic to their purpose in searching
for cultural laws to explain such patterning and order, and their
ability to do so. This assumption underlies their attitude that this
orderly world can be investigated and discovered because "archeological
remains and their spatial interrelationships are empirically observable
records of that patterning" (1971:22). Leslie Freeman also states that,
in order to investigate cultural structure, "it must be assumed...that
patterned occurrences of elements of material culture ... can be discovered, and that when, derived from undisturbed contexts they indicate that patterned human behavior was responsible for their existence" (1968:265). "Because, norms are regarded as independent forms and not as functional aspects of the culture, one can only describe them and their travels or distributions" (Watson, LeBlanc, & Redman, 1971:63).

Such an attitude toward the form and potential information in data results in an additive, rather than an interactive composite picture of the site. On the other hand, a theoretical view which views material culture as the products of human behavior involved in a system, will produce a treatment of data as parts of a functioning, integrative whole. Clearly, one's attitude about the nature of variation within archeologically-observable data and the units appropriate for discovering that variation, is closely tied up with one's attitude about the nature of patterning in human behavior, and about the variety and extent of information recorded in the archeological record.

An additional problem in the recognition and recording of information from the archeological record is the comparability of data units. Comparable data units are a desirable goal for field practice, so that information from various individual excavations may be available and useful to the goals of archeology in general. To be productive of information of wider applicability than the explication of the immediate single excavation or series of excavations, the data must be in a form which will allow of comparisons between sites, on a regional or cultural basis. "The lack of" a standardized vocabulary and excavation
technique "disallows by its vaguery, the testing of one's hypothesis against comparable units, the final results of which are unacceptable through the comparison and testing of non-contrastive units" (Lamberg-Karlovsky, 1970:114). It is felt here that the manner of collecting data, the units used to identify and measure information, are a minimal condition that can limit the usefulness of one's data, by limiting the contexts in which it may be compared. It is interesting to note that, whereas in traditional archeology the data units used have not been so critical as the goals of archeology have changed, and particularly as archeologists have aspired to become more precise and "scientific," the selection of units for the collection and recording of data have a new importance. If the ability to compare sites on a regional or cultural basis—measuring variation and patterning within and among localities—is taken as a theoretical goal, then the comparability of data units is absolutely necessary.

In brief summary, the single most characteristic innovation in the concept of the nature of archeological data in recent years is an emphasis on relations rather than entities. Such a switch in concern is essential to the actualization of the goal of studying cultural process; a different kind of information must be gathered in order to be able to speak in these new terms about the functioning and processes of change in human systems.

The development of techniques for the recovery of data in structural terms i.e. relations rather than entities is believed to be crucial, for it is the structure of archeological remains that informs about the cultural system and it is the cultural system which is the seat of process. (Binford, 1964; reprinted in Binford, 1972:160),
CHAPTER II

METHODOLOGICAL PROCEDURES

THE ROLE OF INFERENCE AND THE DEVELOPMENT OF PROBLEM-ORIENTATION

The role of inference in archeological methodology is exceedingly important, as nearly every phase of archeological reasoning depends on inference, or reference to some previously-known incidence. "The data themselves are conspicuously silent and do not inform on the past unless one has acquired a set of relational statements—linking variables of material and space to variables of behavior and organization" (Schiffer, 1971:4).

The empirical data with which the archeologist has to work consist of the material objectifications of culture traits and their empirical attributes. The archeologist who works on undocumented cultures has only three sets of these attributes upon which to base all his studies. They are 1) spatial relationships, 2) quantity, and 3) chemico-physical specifications. For example, most of his temporal relationships are inferences drawn from vertical or horizontal space, the associations of his material are purely spatial, his conceptions of cultural values are taken largely from relative quantity and from chemico-physical attributes leading to judgements of quality, his inferences as to use and function are taken either from spatial associations or from the physical properties of shape, material, etc. His work is entirely a pyramiding of inference based on these foundations... It is in the nature of the archeological materials. (Taylor, 1948:143).

Ethnographic material is a significant part of the information and assumptions that contribute to the development of hypotheses about the nature of the data, and predictions about the recovery of data, prior to and during the early stages of excavation. Surface clues at a
site may reveal affinities with other, previously excavated sites, and with ethnographically-known situations that provide some link with the structural and artifactual clues. In order to plan and organize an excavation, the archeologist must develop some preconceptions about what will be found: he needs to know something about what kind of data will be found (amounts, kinds of patterning) and general relations to other, existing data (relative place in chronological and spatial ordering, etc.). Such preconceptions are necessarily limited (how much can you know ahead of time?), vary in validity, depending on their source and the reasoning that went into them, but are still necessary to the research process. A certain amount of information about the site is necessary in order to deal effectively with data collection and development of hypotheses, questions, and problems to structure the investigation. James Hill has pointed to ethnographic information as "the most immediate and useful sources of ideas for generating propositions" (Hill, 1970:27).

Ethnographic analogs have long been used to elucidate the behavioral context of an activity observed archeologically as the presence and distribution of artifacts and features. In an instance in which ethnographic analogy is being used to demonstrate or explain an event or pattern in the archeological record, an appropriate postulate would be: "The behavioral context of the use of archeologically-known features was the same as that described ethnographically for the analogous facilities" (Binford, 1962; reprinted in Binford, 1972:48).

Bobby Jo Williams has made a study of the relations between
ethnographic and archeological data—the forms of information which are available, and their potentials for revealing facts about behavior. Ethnographic data is largely verbal (descriptions, explanations, and rationalizations), and "after-the-fact discussion...may obscure as well as reveal certain relationships." Archeologically-available data, on the other hand, sometimes is "a more direct reflection of the culture of a people," since it is non-verbal and consists of patterned signs or clues to actual activity, rather than after-the-fact or second person description (Williams, 1968:161). By providing a different kind of information, ethnographic situations have a particular contribution to make to archeological studies in general. "A coherent and unified body of subject matter entirely appropriate to archeology is the study of the material aspects of culture in their behavioral context, regardless of provenience" (i.e. whether or not they have to be dug up) (Deetz, 1970:123). Observation of contemporary activity can reveal information about the relations between the manufacture, distribution and deposition of objects, and human behavior. "Understandings of the relationship between the material and nonmaterial derived from maximum information well controlled can then be fed back into the traditional archeological contexts for more precise inferences" (Ibid.:123). Binford names "the study and establishment of correlations between types of social structure classified on the basis of behavioral attributes and structural types of material elements" (1962; reprinted in Binford, 1972:24) as a major focus of anthropological research which needs to be developed. Archeologists need such correlations in order to be able to handle such
problems as evolutionary change in social systems.

Several workers have pointed out that the principal emphasis should be the use of inference or analogy as a source of concepts or ideas for relating variation in the archeological record to human behavior, but not a dependence on them. Deetz isolates as a problem in the interpretation of archeological data the fact that archeologists "implicitly seek analogies between material categories from the past and behavioral categories from the present" (1970:122). Analogy should not be used directly for interpretation, but should rather suggest postulates and questions from which hypotheses can be derived deductively. Through testing, the hypotheses may generate explanations, predictions, and interpretations. Ethnographically-known instances of behavior, with their material correlates, cannot be transferred directly to archeological circumstances, as explanations. "Ethnographic parallels in fact afford only clues in what direction to look for an explanation in the archeological record itself" (Childe, 1956; cited in Binford, 1972:52). After ethnographic analogy perhaps suggests an explanation, it is up to the archeologist's analytic methods to test and verify that suggested explanation. "The truth value of an argument offered as to the significance of archaeologically observed phenomena to past conditions and events must be determined by the testing of relevant hypotheses against the archeological record" (Binford, 1972:52). Emphasis should be on derivation of explanations or interpretations from the nature of the patterning itself, rather than on the assumption of similar functioning of variables in different cultures.
Freeman has pointed out that "if we utilize models which are only sensitive to the elucidation of parallels with modern groups, the discovery of parameters of sociocultural structure unique to prehistoric time periods is impossible" (1968:262). Binford has further described the limitations that theoretical and methodological outlook put on one's conclusions, with the use of inference and analogy: "If we take an analytical, rather than a descriptive, approach to the past, the limits on our generalizations are set only by the analytical techniques available, and not by our substantive knowledge of the present" (Binford, 1967b; reprinted in Binford 1972:72). Methods and techniques either presently available, or which can and should be developed, can allow manipulation and analysis of the archeological record to infer things about the past which have no observable counterpart in contemporary human societies, or in the recent past which is known ethnographically or historically. Ideally, our knowledge of the past is not limited by our knowledge of the present. The assumption (such as Chang makes (1967a)) that we can interpret archeological patterning only insofar as it can be explained by an analogous situation known ethnographically is limiting, and "such a procedure denies to archeology the possibility of dealing with forms of cultural adaptation outside the range of variation known ethnographically" (Binford & Binford, 1968; reprinted in Binford, 1972:14).

The development of problem-orientation for a site utilizes inference and analogy for source material, and is directed significantly by the theoretical outlook of the investigators. While Gordon Willey
and Philip Phillips suggested that "acceptable field work can perhaps be done in a theoretical vacuum" (Willey & Phillips, 1958:1), many archeologists today would maintain that it is nearly impossible to carry on an archeological investigation without some bias or shape given to the data collected by the theoretical outlook. The archeologist's assumptions and theoretical framework, as well as his concept of the scientific approach, structure and define field operations and final analyses. Very simply put, theory largely determines the kinds of questions we attempt to answer. In stating general aims of the "New Archeology," Binford wrote, "in short, we seek answers to some 'how and why' questions in addition to the 'what, when' and 'where' questions so characteristically asked by archeologists" (1964; reprinted in Binford 1972:135). Hill has similarly pointed out "problems involving the description and explanation of prehistoric social organization are derived from a different set of theories or premises than are problems involving chronology and the historical 'relationships' among culture traits" (Hill, 1970:16).

Ways of Approaching Variation, Patterning and Internal Site Structure

The questions the archeologist asks himself direct the nature of excavation, data collection, and publication of results. For instance, the assumption that "the important factor about a cultural manifestation in archeology is its presence or absence within a given site" (Taylor, 1948:79) leads to disregard of the relative numbers of artifact types or changing associational contexts over time, or other measures of
internal variation. The questions an archeologist asks are highly dependent on his conception of "the determinants of differences and similarities in the formal, spatial and temporal characteristics of artifacts and features and their interrelationships" (Binford & Binford, 1968:2). Data units and methods of collection appropriate to different notions of the nature of variation and patterning occurring as a result of human activity and 'fossilized' in a site have already been discussed in the section "The Nature of Archeological Data" (pp. 10-14).

The view that "the intimate systemic articulation of localities, facilities, and tools with specific tasks performed by social segments results in a structural set of spatial-formal relationships in the archeological record" (Binford, 1964; reprinted in Binford, 1972:136) will lead the investigator to deal with his site as a pattern of information that reflects the systemic operation of the occupants' life, rather than as a collection of details that together add up to a description of the culture. The recognition that roles and locations are differentiated in task performance will lead him to look for patterning in artifact distribution or internal site structure that would reflect such activity areas and role specialization. "People do not cooperate in exactly the same way when performing different tasks. Similarly, different tasks are not uniformly carried on at the same locations. As tasks and cooperating groups vary, so do the implements and facilities" (Binford, 1964; reprinted in Binford, 1972:136).

A significant attitude that has developed recently towards patterning in an archeological site is that the patterning itself can tell
you something. The archeologist may not be able to recognize immediately the meaning of certain patterns, but he can recognize that the patterns occur, and what form they occur in. "We may not always be able to state or determine what specific activities resulted in observed differential distributions, but we can recognize that activities were differentiated and determine the formal nature of the observable variation" (Binford, 1964; reprinted in Binford, 1972:136).

Verification of Hypotheses, Propositions, and Explanations

A major aim of archeology today is to be able to present an "explanation" for the occurrence or functioning of a variable seen through the archeological record, with the end purpose in mind of delimiting laws about the nature of change and status in human society. Binford defines an explanation as the "demonstration of a constant articulation of variables within a system and the measurement of the concomitant variability among the variables within the system" (Binford, 1962:21). Current innovations in archeological theory stand to bring archeology into wider relevance within the social sciences. "Laws linking archeological remains to cultural processes and events enable archeologists to use general processual laws to explain situations... Such use of the data of prehistory makes it available to social science as a whole" (Watson, LeBlanc, & Redman, 1971:30). Others have also pointed to this goal of generating general laws:

From a set of premises, we can frame testable hypotheses whose confirmation will lend support to the postulates and assumptions (premises) on which the hypotheses are based. It is in the testing
of hypotheses as to the relationship between two or more variables that we can raise our hypotheses to the level of general laws. (Binford, 1967b; reprinted in Binford, 1972:70).

Those who expect testing to produce absolute answers will be disappointed. Absolute truth is not expected to be reachable; what is possible is the closest approximation to that truth, which is reached by "those hypotheses which are most adequately confirmed at any one time" (Watson, LeBlanc, & Redman, 1971:22). Flannery has also maintained that "'truth' is just the best current hypothesis" (1967:121).
CHAPTER III
EXAMPLES OF THEORETICAL AND PRACTICAL
APPROACHES: PERSONAL FIELD EXPERIENCES

In this chapter of the paper, I will discuss various approaches to data that I have witnessed in my participation in field work. In the summer of 1968 I dug in Winchester, England, where excavation of several sites was under way to reconstruct the historical background of Winchester and corroborate it with existing records. The following summer I worked with the Anasazi Origins Project, which was attempting to draw together information from many Archaic and PaleoIndian sites in New Mexico. Since this was a previously little-known period of Southwestern prehistory, the project was unable to proceed with any of its ecological-systemic or processual analyses without some preliminary work in establishing a space-time framework. In 1970 I worked on a pueblo site in eastern Arizona, where the major emphasis was on reconstructing patterns of social organization within the pueblo. In 1971 I worked on another late pueblo site in Tijeras Canyon, New Mexico, where the major interest was in settlement and community patterns, and their relation to geographic, topographic, and ecological factors. Each of these projects introduced new problems for gathering data and organizing research objectives; together they present relevant material for a discussion of accommodating field practice to research objectives in archeology.
Winchester Excavations

The British are very interested in documenting and redocumenting their own history, through excavation and archival research. Winchester having once been the ecclesiastical and trade center of England, it was chosen as a likely spot for a comprehensive series of excavations that would explore several of the known significant constructions—the cathedral, Winchester Castle, and the Bishop's Palace. The initial aim was a rather traditional one: to produce additional information to fill in the gaps and to document and enrich the written historical record. Though this was a well-accepted goal in British archeology, Martin Biddle, director of Winchester Excavations, was not satisfied that this was the best use for his information, which was gathered at considerable expense of time, effort, and money. If there is abundant documentary evidence, why bother excavating another source of the same information? What do historical archeologists hope to gain from the particular nature of archeological evidence?

James Deetz, as spokesman for a "school" of American Historical Archeologists had provided an answer to these questions. Deetz feels that historic "artifacts constitute a unique and powerfully controlled context in which to refine and develop archaeological method and elaborate archaeological theory" (Ibid., 125), and particularly that historical archeology "because of its documentary support, gives the archeologist an area in which to develop general theory treating the relationships between culture and its tangible products" (Ibid., 129). In an article about excavation of sites from the historic period in
North America, Deetz pointed to his objectives in seeking archeological correlates to written records:

While it might be argued that one need not excavate so many sites just to determine the general form of seventeenth century colonial culture, since documentary evidence is in fact rather rich, the latter approach only tells us what was available to the early settlers and not what was actually used. (Ibid.,:122).

Determining that "archaeology can serve as a valuable supplement to history, since each discipline has a quite different emphasis" (Deetz 1968:123), Biddle began to emphasize the archeologically-gathered historical material as a check on documentary evidence and, to a lesser degree, as a check on archeological methods. Because of such a shift in purpose that developed in the process of excavation as Biddle recognized the potential of the data he was uncovering, the objectives of the project shifted notably from excavation of the prominent landmarks of ancient Winchester to any part of the ancient city which would become available for excavation. In the reappraised goals of the project, what was happening in the rest of Winchester, while the cathedral and other monuments were being erected was of considerable interest to the total picture of Winchester at that time. Any part of the ancient city was assumed to be able to provide significant information. The Brook Street site—a two-block area in the heart of town where some warehouses were razed to make room for a new post office—became a very important methodological addition to the project.

Such a shift in purpose was soon reflected in a shift in excavation technique. Insofar as archeologists hold a concept of a "Standard Operating Procedure" that would indicate what data to collect, in
what units, what information to look for, etc., such a "standard" method of operating should vary with the expectations of specific type of site. For instance, in England the soil is largely damp loam; it is particularly suited to careful, clean excavation techniques and precise distinction of stratigraphic layers in horizontal or vertical section. Consequently, the excavation method preferred is that of meticulous excavation by natural levels with no screening of the dirt for recovery of artifacts missed in excavation. The effort is put into doing a precise job the first time, without dependence on a safety measure to catch what you missed. Such a bias in digging method and technique is as much a cultural difference (British vs. Americanist archeology) as it is a practicality. Another traditional aspect of British "Standard Operating Procedure" is leaving baulks between artificial grids, so as to maintain stratigraphic control once material has been excavated. At Winchester this method was used for some time, then reevaluated and abandoned. It was determined that such a method obscured the natural units (rooms and structures). Here, excavation methods were evaluated and changed in terms of the kind of information sought, and in terms of the potentials of the emerging data.

Further concrete, operational goals of the Winchester Excavations project were the careful unraveling of the sequence of events in the ancient city, and discovery of how the different social segments fit together in a working system. Instrumental to these goals were such problems as the delineation of modification and remodification of construction plans—the series of constructions which interrelated in
time and space, as for instance has been investigated with some success at Kiet Siel by Jeffrey S. Dean, using tree-ring dates as a chronological tool for fine distinctions (Dean, 1970). In order to deal with these questions, attention was paid to detail in dirt and rubble deposits in relations to structures and the remains of structures.

Particular aspects of the conditions of deposition, disturbance, and preservation played a role in the types of information available and techniques for discovering that information. The soil being quite damp, very little organic matter was preserved (two water-logged timbers being an exception). This eliminated many direct sources of information about foods and building materials, and attention had to focus instead on indirect sources of such information, such as impressions of wattle in daub matrix. The intensity of occupation, and the continuous re-occupation of Winchester obscured the relations between successive constructions in the ground. Often later building episodes intruded into the remains of earlier constructions, or actually used their remains, as building materials were in scarce demand. Consequently, direct evidence of the early occupations is often destroyed or confused by subsequent building events. British archaeologists have most often given up at that point, and considered that information lost. Biddle, however, noticed a clear distinction between the soil matrix and fill in certain distinguishable zones. He recognized a pattern (which he determined to be man-made), and resolved to follow and record that information, in the hopes that as the distribution and pattern of that variable became clearer, an interpretation might be possible. This method turned
out to have considerable value for determining successive occupations, as these trenches were often "robber trenches" (old foundations "robbed" of their building materials and eventually filled in with rubble).

The Winchester project had few aspirations of acting as an effective teaching situation. Though many of the diggers were history or archeology students, they could really only hope to learn about the excavation process from participation in whatever niche they proposed to work in, and observation of as much else as possible. The large number of excavators (c. 150), small staff, and low funding made individual attention hard to come by. Any student who wanted to know what he was doing, or to hear something about the goals of the project, had to make a singular effort to find this out. Discussion about decisions of where to dig or why, what information to collect or look for, took place amongst the staff after hours. Most recording and measurement was carried out by the staff. The Winchester project was ineffective as a teaching situation because of the almost total separation of neophyte students from the processes of development of problem-orientation and research design, and from the decisions which determined the day-to-day progress of the dig.

The Anasazi Origins Project had already been in operation for several seasons in 1969. The objectives had been set as the exploration of the range of adaptations, and how they functioned together, within the broad adaptation known as the Desert Archaic. Further, Dr. Irwin-Williams hoped to trace cultural evolutionary links between the earlier PaleoIndian hunting cultures and the broad spectrum hunting and gathering
adaptation of the Desert Archaic, and to establish a hypothesized continuity between the Desert Archaic and early Anasazi Pueblo groups (hence the title for the project).

In dealing with such a broad problem, the "methodology most appropriate to the study of culture process is a regional approach" (Binford, 1964; reprinted in Binford, 1972:160), Survey is a useful tool for determining the range of sites and which kinds of sites will best provide information to generate and test propositions. A well-structured regional investigation is needed to "gain reliable and representative information concerning the internal structure and ecological setting of successive cultural systems" (Ibid.:160), as well as of the functioning of components in any given adaptive system. In the Anasazi Origins Project, an attempt was made to pick sites which would fill in holes in the growing framework that described the Desert Archaic and its relation to previous and subsequent adaptations, and also to pick sites which would anticipate parts of the subsistence network that were not yet documented by excavation. Certain characteristics of these early sites and the Southwestern terrain determined some of the site and excavation strategy. As there are rarely structural remains associated with PaleoIndian and Archaic sites, locations must ordinarily be determined by associations of lithic debris and areas of prehistoric habitation would be predicted (determined by previous experience of relations between sites, topographic features, and availability of resources). Since a direct relation between surface indications and the actual site is often difficult to ascertain, testing is frequently
used as a means of pinpointing subsurface concentrations. Indeed at many large open sites—such as the Baker Site—backhoe trenches are used as a means of securing such information quickly. Test pits, located on the basis of the trenches, are then put in to determine more exactly how and where the most productive or representative areas of the site lay. Test pits are intended to provide a view of the stratigraphy of the site and the composition of individual levels, to guide later, more extensive excavations. The "unit-level" system for structuring excavation and locating artifacts, features and strata in horizontal and vertical space was used. This system is appropriate to characteristics of the site: there was practically no horizontal structure (rooms or boundaries) to habitation sites, hence an arbitrary grid system was needed to structure the excavation procedure. Natural levels were discernible, but they were often so deep that some means of separating early from later deposits within a single natural layer was necessary. Ten cm. levels (within natural levels) were excavated within meter-square grids. The site of Dunas Altas had unusually thin deposits and was consequently dug almost entirely in natural levels. Digging, per unit volume of dirt, proceeded more slowly than at many of the other sites in the project. From the stratigraphy in the backhoe trenches, it was known that the entire sequence present was compressed into a four to five foot depth between surface and bedrock, and that we had to watch out for these changes in the process of digging. At the Baker Site, on the other hand, the soil was an extremely hard, calcareous matrix, and turned out to be quite sterile just below the surface.
In this case, railroad picks, pick-maddocks, and shovels were used to remove the greatest amount of dirt in the shortest possible time. In this circumstance, the use of screens—ordinarily a routine double-precaution—was absolutely necessary in order to catch what few flakes were encountered. When extensive testing had proven that the stratigraphy in the backhoe trenches had been incorrectly interpreted, and that the levels excavated were in fact pre-Man, the object of excavating the site was abandoned. "...Any archeological scheme is, of necessity, subject to modification, elaboration, and even radical change in response to particular circumstances and the dictates of practicalities such as time and finance" (Taylor, 1948, 150), and this was an extreme case of modification of the original plan.

Though Dr. Irwin-Williams was clearly interested in problems of systemic functioning of sub-groups in resource acquisition, and change in functioning, adaptation, population aggregation etc. over time, the general problem required more command of the distribution of groups through space and over time than was currently known. Some initial chronology building is necessary, before a "research design" is possible. At that point the archeologist is ready to select priorities for further excavation, in relation to specific questions or propositions with which to deal (Lamberg-Karlovsky, 1970:113). Others have also stated

One's first concern is to account for the observed variations within a region...Hopefully by anticipating...variability [such as sites that actually represent seasonal or task-specific occupation, rather than separate 'cultural units'], we may be able to develop means of testing alternative hypotheses and coming out with an approximation of the adaptive system present in the region.
and "Without such a [systematic] framework [of temporal and spatial distributions], the more analytic, causally oriented approaches of more recent decades never could have been initiated" (Adams, 1968:1188). Should a site prove to contain a single cultural entity, "the development and change through time exhibited by a single component will be one of its most important aspects, culturally speaking." In a site where several cultural entities are represented, "a time differential may be one of the important diagnostic features belonging to different components, and, once established, will aid in the interpretation of the separate contexts themselves and their interrelationships, if any" (Taylor, 1948:179), Deetz further points out the necessity of chronological control before any interpretations based on the nature of formal or distributional differences between assemblages can be made:

The caution which I would suggest...is that of making certain that we have very precise chronological control before making too many statements about patterning in assemblages as it reflects social patterning in a synchronic way...We must be careful that we do not compare assemblages which might in fact be hundreds of thousands of years apart in time," since a time span of "more than one generation could well introduce differences which might be seen as a function of synchronic variation but which are in reality a matter of chronological separation" (Deetz, 1968:282-3). Dr. Irwin-Williams understood very well the scope of variation needed to deal with the problems she undertook, and set out valiantly to discover (by survey) the range of kinds of sites which existed within the cultural period she was interested in, and sites to
excavate which were expected to be most productive of the information most useful to these problems and were also practically accessible.

A difficulty inhibiting the success of the whole project was that the problem was just too big and too unknown to tackle at the level of analysis anticipated. At the juncture when I participated in the Anasazi Origins Project, the project was unable to deal with the objectives it set for itself, not because of a lack of understanding of the functioning of the components of subsistence or cultural adaptation, but because the control over the data was insufficient to deal with such a wide range of time and cultural groups. Because "detailed description and precise chronology are indispensable pre-requisites to other kinds of investigations" (Meggers, 1968:ix), and because such control of the variation present was such a vast problem, the project was in essence unable to deal with much else besides the construction of typologies on the basis of stratigraphic information, in the hopes of pinning down some of these chronological and spatial changes. Stuart Struever has warned "chronology-building is an initial step to the solution of broader problems" (but it is "not an end-result of research") (1968:131), and yet that seems to have been what happened in this case.

Part of the trouble with the organization of the Anasazi Origins Project is that, though the project aspired to synthesize a large body of information from many disciplines, with a multivariate approach to relationships and variability within a system, the director clung to a "principle investigator" organization. In order to deal comparatively
and synthetically with a large data base, not only do you have to deal on a regional basis and measure patterning and variation between sites as well as within sites, raising the whole scale of data input, but you also need to diffuse the administration of such wide-scale undertakings, and call in specialists (geologists, botanists, zoologists, etc,) to head their own branches of research that will go into the whole. It is important that "no single individual can control the totality of techniques and methods essential to understand the complexities evident in any single archeologically-defined cultural process" (Lamberg-Karlovsky, 1970:113).

Although the Anasazi Origins Project was billed as a field school for Eastern New Mexico University, instruction was fairly well limited to a few introductory lectures and whatever information was needed to carry out the work each individual was responsible for. However, a variety of jobs, including recording, mapping, and surveying, were open to anyone who wanted to do them. I felt that by circumstance, my personal experience was more valuable than that of those in the other three crews: my crew had to deal with four small, very different sites with very different methodological and technical problems during the summer, and the crew supervisors were exceedingly open and informative about exactly what thinking was going into each decision of where to dig, and how, and what information would be most useful. Here the major block to effective participation in, or understanding of, the research process was certainly not the attitude of the immediate supervisors, but the fact that the scope of the entire project was too large
and complex for clear and explicit formulation of a research design, during the early stages of field work.

Grasshopper Pueblo: University of Arizona Field School

The University of Arizona has been running a well-established field school at Grasshopper Pueblo on the White Mountain Apache Reservation. The site has been excavated with approximately twenty students and ten staff members for nearly ten field seasons, and analysis is carried on into the winter at the University of Arizona. The site is a large (500 room) pueblo, occupied over a short span of about seventy-five years. Masonry structures are clearly distinguishable (sometimes above ground) and serve to partition the site into rooms, provenience units which are meaningful in terms of kin, social, and subsistence groups.

William Longacre, who directs the dig, is interested in the interrelationship of social organization and economy over time. In particular at Grasshopper he wants to watch the covariation of these two items during a period of environmental stress (in this case, the stress is prolonged drought). At Grasshopper then, specific needs for information partially guided the data collection: in order to watch the changes in social organization in stress, he needed to be able to recognize social organization in the archeological record before, during and after stress. Chronological control was thus absolutely necessary. Fine distinctions of absolute time are sought through the use of tree-ring dates, and relative contemporaneity of rooms in construction units
and temporal relations between construction events were sought by a variety of methods (Wilcox, 1973; [in press]). Test implications for the recognition of stress, in occupation levels or segments of the pueblo, have been difficult to isolate, but osteological and palynological material have been collected in hopes that such analysis may reveal these associations. The recognition of the social organization in the pueblo, and particularly being able to distinguish differences at various points in time (re. environmental stress) is a sticky problem.

In James Hill's carefully considered essay "Prehistoric Social Organization in the American Southwest: Theory and Method" (1970), he outlines several stages in general scientific methodology that are applicable to the discovery of prehistoric social organization, using a complement of ethnographic and archeological information (Hill, 1970: 21-2). The first step is to generate propositions and hypotheses, and he makes two important statements about this stage in a research strategy. First of all, there are no rules for how this is to be done; anything, dreams and hallucinations, as Hempel once suggested (1965) may provide a source for initial propositions. Second, preliminary propositions and hypotheses depend on an initial minimal data base--some knowledge of the kind of information available--for their generation. An hypothesis, which is a tentative (unproven) statement, "proposes an explanation for an observation or set of observations, and the explanation is in terms of both general laws and specific conditions" (Ibid.:21). In other words, though Hill presents hypothesis generation as a 'first
step' in a research strategy, his plan in fact calls for some previous investigation, an earlier step. Before an archeologist can generate the most appropriate propositions to cover an excavation situation he intends to become involved in, he must seek out the lay of the land, find out something about the chronological and cultural affiliations of the site or region he is dealing with, understand something of the extent or density of cultural occupation, the kinds of artifactual remains present, the general structure of the site or pattern of settlement, and major features of the natural environment to which human adaptation may be related.

Though there was a great deal of talk at Grasshopper about the delineation of social organization, kin patterns, etc. in the archeological record being excavated, little was heard in the way of concrete suggestions about means of recognizing these patterns. Efforts were centered around a kind of "Standard Operating Procedure" oriented specifically to Grasshopper's needs. An attempt was made to anticipate any and all kinds of information which could possibly reflect human social behavior, or behavior under ecological stress.

The field school has been generously funded, principally by the National Science Foundation and the University of Arizona. Such funding provides for excellent accommodations, food and equipment for the students, and a well-salaried staff, as well as funds for specialists and technical analysis (dating, pollen studies, etc.). The availability of funds allows much of what contributes to the success of Grasshopper as a field school. Since the field school is a well-funded
research project, with excellent prospects for thorough, well-conceived analysis ending in publications, and salaries are good, positions on the Grasshopper staff are eagerly sought and valued. There is considerable continuity in staff from year to year (lack of which is a problem Jim Judge cites as endemic to field schools (1972:6)) and Longacre feels justified in calling the staff together for frequent planning meetings throughout each spring before the field session. Participation of the staff in the research process throughout the year, and their continued involvement in the excavation both as a research problem and as a teaching environment, adds considerably to the effectiveness of the field school.

In addition to a wealth of resources with which to run the excavation, the next single most important contribution to the success of Grasshopper as a field school is that each student is required to take responsibility for his part in the research process. Students, in groups of two, are responsible for every phase of decision-making, and excavation, recording, analysis, and synthesis (writing a final report) for a single room in the pueblo. Such involvement, under close and supportive direction, is the very best introduction to "the mechanics of archeological research" (Taylor, 1948:150).

An interesting aspect of the research at Grasshopper is that over the years, as an unforeseen consequence of an increasingly well-developed problem orientation, the desirable feedback between diggers and directors has been reduced and students are less directly involved in the workings of the dig. The students in recent seasons have felt
much more that their labor and their brains were being *used*, rather than that they were making a direct contribution to the operation as a whole (S. Debowski, personal communication).

**Tijeras Canyon Pueblo: The University of New Mexico Field School**

In 1970, I worked as a teaching assistant on the University of New Mexico field school, in its first year at a new site, and in its first year with a new director. The season was very informative of the problems of organizing archeological research on three levels: the practical considerations which must be met (equipment to be collected, purchased, built; forms for recording, data processing, lab analysis to be devised; transportation; scheduling, etc.), the development of problem-orientation and specific hypotheses to be tested, and the running of a field school.

The site is a medium-sized late Pueblo IV site, in Tijeras Canyon outside of Albuquerque, and in the Rio Grande area. It is built on a small, rather prominent little hill, with outliers at lower levels around it. Masonry and adobe walls structure the site into rooms. Apparently, two occupations are present: an earlier adobe phase, and a later masonry phase. (This is still being tested). The field school operates with a far different student/staff ratio than Grasshopper; there are approximately thirty students and four to six staff.

At the Tijeras Canyon site, the distribution of different parts of the site over the varied terrain, as well as the question of relation
to their agricultural lands and to other communities in the canyon, suggested that here attention to settlement pattern and ecological relations might be a fruitful approach. Such information as the density, agglomeration, scatter, extent, orientation, shape and topographic location of prehistoric communities within the canyon, as well as the proximity, extent and seasonality of wild resource zones and the availability and suitability of lands for agriculture would provide an interesting data base for dealing with a variety of questions and problems. The network of relationships between human settlements in the canyon and throughout a wider region, and their relations to and requirements of the natural surroundings (as well as the interrelation and interaction of these differentiated roles through time) could be investigated with an eye to the "mechanics and effects" (Watson, LeBlanc & Redman, 1971:99) of these relations. An investigation of the operation of these variables in Tijeras Canyon may lead to the generation of specific hypotheses about the abandonment of the pueblo, as "the viability of a system depends to a great extent on its ability to react to the relevant properties of its environment and to adjust its structure accordingly" (Watson, LeBlanc, & Redman, 1971:71).

As often as not, a perceptive assessment of the setting of an archeological site in relation to explanations for changing subsistence productivities and patterns of settlement goes far beyond even the best contemporary data gathered for other purposes, and requires the gathering of additional data as a part of the archeological project itself. (Adams, 1968:1189).

Because the Tijeras site is funded only as a field school by the University of New Mexico, money is not available for surveys of the contemporary biotic, climatological, geological, or topographical situation.
Such studies, made with a view towards providing information specifically useful for answering questions about possible prehistoric adaptations at the site, are conspicuously lacking in the body of available data to be used in analysis. For instance, it would be useful to have such information as where suitable agricultural lands, quarries for building materials or stone for tools, critical plant and animal resources might be located in relation to the pueblo, or what the range of variation in the suitability of the growing season for corn, other crops, or wild plant resources is. Such specific information is not likely to be found in Bureau of Land Management or Forest Service surveys of the natural resources of the area.

The ecological approach is useful "not only as a guide to data collection but also as an interpretive framework for viewing culture" (Watson, LeBlanc, & Redman, 1971:91). Although many things in a culture are determined or shaped by environmental requirements, there is also a wide variety of alternatives, and alternative ways of life, that are possible within the range of given conditions. These choices give an insight into the make-up of that culture. In an ecological study, various subsystems (the economic, political, religious, and so on) are seen in relation to each other and to the biophysical environment. Such a plan for analysis is practical for archeology, as it utilizes data (e.g. topography, flora, fauna, mineral resources) that can be collected and inferred archeologically. The ecological approach, in addition to emphasizing the role of plants, animals, climate, and topography, also gives the perspective of man and nature as participating in a series of
dynamic and interacting systems (Watson, LeBlanc, & Redman, 1971;107). Problems stressed at the Tijeras site are predominantly ecological and demographic rather than sociological, because such questions are suggested by the location of the site and the data emerging at the site, and also very likely in this case, by the interests of the director.

The Tijeras site was probably quite ineffective as a field school in its first year, due as much to the short session of six weeks (the students felt they were "just beginning to get the hang of it" when the session ended (P. Spahn, personal communication)) as to the lack of clear formulation of goals and procedure. Since one of the most useful things students can pick up in a field school is a general notion about the process of decision-making, hypothesis generation, data input and reevaluation, it seems necessary for those directing the investigation to have a clear idea of what they are looking for, and where to look for it. The most valuable contribution that was made as a field school was the attempt to outline the steps in the decision-making process right from the start to the students. We could transmit to the students the thinking behind choices of where to dig, and how, and what standard items of data were worth collecting, but we were unclear ourselves (I think because we knew practically nothing about the form data would be taking) about appropriate questions and hypotheses to propose, and the students were naturally confused and discouraged about the failure of these things to pop up before their eyes.

There were so many purely technical problems (insufficient equipment, poor transportation, a director busy with two other major
responsibilities, lack of an established laboratory or laboratory procedure) that it was difficult to even maintain a smoothly operating excavation and recording procedure. In the following season, improvements were made in technical procedures, clarification of goals and methods, and provision was made for lengthening the field session so that more momentum could be attained and more work accomplished. And the director has recently stated, "We've come a long way from that first year of the field school, or even from last year" (J. Judge, personal communication).

Discussion

From the description and discussion of the process of decision-making and development of excavation logistics on these four projects, it is apparent that all of them relied strongly at one time or another on a kind of "Standard Operating Procedure," as a means of determining what to dig next, and how. In its simplest and most useful application, "Standard Operating Procedure" is conceived of as a way of dealing with the "initial observation of the total pattern of the residue of past behavior," as thoroughly and safely as possible, before analysis is undertaken (Deetz, 1971:5). In virtually every excavation there is necessarily a period in which not much is known about the shape of the data present in the archaeological record at that site, and such information is necessary in order to formulate problems for the direction of the research. Aside from the backlog of archaeological information (sites of a similar adaptation, culture, region, that have previously been
excavated), the archeologist necessarily must derive some of that initial information form beginning excavations, tests at his site. In order to direct that digging procedure, he must needs have some standard, guiding notions of the kinds of information he is looking for, and a clear concept of what minimal categories of data should be sought, in order not to overlook any significant aspect of the adaptation documented at that site. Archeologists are fairly well aware today of the need to preserve a wide variety of information—geographical, geological, pedological, botanical, zoological, climatological, and so on. In the recorded description of the destructive excavation process, most archeologists recognize the need to record precise locations of artifacts in relation to stratigraphic levels and man-made features or architectural structures, and to obtain a variety of samples for possible future dating or climatological analysis for any unit (level, feature, room, house, et etc.) that may possibly prove significant in terms of human occupation or activity. These are all ways in which any conscientious excavator utilizes standard operating procedure to insure the collection of the most thorough and productive data base, or, in another view, to insure the minimal loss of critical data.

In an historical overview of archeological research in the Southwestern United States, Longacre maintains that prior to 1950 a very strong conviction prevailed that there was such a thing as a standard operating procedure, applicable to all excavation situations:

The overriding concern with objective scientific investigations...was based in the assumption that one could excavate a site, and recover all the facts in an unbiased manner. The view seems to have been held that one collected one's data essentially in a
theoretical 'vacuum,' and the facts, once assembled, would 'speak for themselves', (Longacre, 1970a:7).

In other words, archeological methodology at that time assumed there was a 'pure' way to excavate a site and not miss anything. But more recently, archeologists have realized that "to propose a rigid and universally applicable system for the gathering, analysis, and synthesis of archeological materials would be impossible as well as presumptuous," as Walter Taylor has pointed out. "The handling of such data is too contingent a procedure to be amenable to more than a very general systematization" (1948:5). In fact, sites vary as to the type, amount and condition of data, insofar as human occupation of a locus varies in size, cultural affiliation and economic base, and time range, and any archeological deposits, once formed, are subject to differential preservation due to soil texture and chemistry, climatic conditions, and time.

Even if all the material items of a culture are related to its non-material aspects, the archeological remains may be so limited, altered, or destroyed that a complete description of the past cannot be reconstructed from them, not just because our techniques (or intelligence) are limited, but because the complete past simply is not reflected in the material that remains. (Watson, LeBlanc, & Redman, 1971:21).

Or, Binford has warned, "how can we know that an empirical generalization about archeological data is accurate, since there may be pertinent and nonconforming evidence that has been lost?" (1968, in Binford & Binford, eds.:18). "There are certainly variations in the nature and amount of data at some times and places" (Watson, LeBlanc, & Redman, 1971:112).

Watson, LeBlanc, and Redman have characterized hypothesis formulation and data testing as a highly interactive process; indeed, they
seem to accept the fact that neither actually comes first. In this picture of the research process, data in fact constitutes tests of hypotheses and answers to problems that have not yet been explicitly formulated" (Watson, LeBlanc, & Redman, 1971:14). That problems and hypotheses are set out neatly before investigation and data collection begins is "an ideal that is seldom exemplified in practice" (Ibid.:15).
CHAPTER V

CONCLUSIONS

So many archeologists seem to describe their work as "excavating; then describing (and ordering) of data" (Deetz, 1970:116), as if you could excavate material without really ordering it at the time, as if there is a regulation, pure, and complete way to excavate and record that will preserve all important information and make it available for classification "according to any one of a number of classificatory systems" (Ibid.:116). One reason why archeologists who aspire to many of the goals of the "new" scientific archeology often tend to collect their data and then see what they can do with it later, is that this is much easier and far less troublesome than formulating appropriate hypotheses and establishing a research strategy to handle those hypotheses. The problem-oriented approach to the practice of archeology requires derivation of meaningful hypotheses that will serve to explain the particular causal events and patterning that seem most significant in a given archeological situation, and then the choice of data that will most adequately test these hypotheses. The choice of data includes a determination of the proper units and scales of measurement, which in turn will implicate certain excavation and recording techniques.

It is often a thankless process, as frequently hypotheses have to be readjusted to fit the data several times in the course of investigation: "procedure by the method of hypotheses in science always
involves modifying them as data accumulates, and sometimes rejecting them and substituting entirely different hypotheses" (Watson, LeBlanc, & Redman, 1971:13). "The archeologist and his system need to be flexible and able to change with the changing demands of his complicated and disconnected material" (Taylor, 1948:149). It is far simpler to just dig the site, and worry later about what to do with the material. In regard to this particular issue, Walter Taylor pointed out some time ago that "a policy of wait-until-all-the-evidence-is-in can stunt the growth of archeology to a dangerous degree. The man on the job has tremendous advantages over students who might wish, at some later time, to make use of his specimens and records" (Taylor, 1948:156).

Watson, LeBlanc, & Redman have taken the trouble to point out that "misunderstanding about how problems and hypotheses are formulated and modified in the light of data has led to a misguided criticism of explicitly scientific archeology" (1971:14). My discussion is not so much a criticism of the aims of explicitly scientific archeology, or of the method of problem-orientation and hypothesis-formulation, but rather a criticism of the way it is often carried out. Indeed, hypotheses need only to be tentative statements at initial stages in the research, and are subject to continual reassessment and reorganization, so that they will fit the data, and so that the methods of data collection will fit the questions being asked. However, I do not agree wholeheartedly with Hempel's often-quoted statement: "What determines the soundness of a hypothesis is not the way it is arrived at (it may have been suggested by a dream or a hallucination), but the way it stands up when tested,
i.e., when confronted with relevant observational data" (Hempel, 1965: 6). Though a hypothesis may achieve its validation through testing, its initial conception and subsequent transformations throughout the course of the research, are critical to the direction of excavation goals and derivation of data collection methods in line with the problems to be dealt with. And it is my contention that the basis of good fieldwork, dedicated to the elucidation, explanation, and prediction of cultural systemic variables and long-term cultural evolution, is a sound and explicit problem orientation, developed before and during initial stages of investigation and excavation. The value of the body of data collected is significantly determined by the appropriateness and scope of the questions being asked of the data, the openness of the investigator to questions or problems suggested by the emerging patterning of the data, and the appropriateness and thoroughness of the methods of data collection and recording. Thus the success of problem-oriented archeological excavation is to a large measure contingent on a great deal of introspective attention being given to the conception of the problem(s) and the nature of the data. Insofar as archeologists often are not able to freely choose the focus of their investigation (as less and less sites are left undisturbed; or due to grant stipulations or economic constraints; or in the case of salvage work) and often do not know much about what to expect from the site before work is begun, they are often confronted with sites for which they have not the necessary time, information, or inspiration to develop a meaningful problem-orientation. So it is that few excavations today truly actualize their theoretical aspirations in
their work, due to a lack of careful conceptualization of the goals (and means of achieving them) at all stages of the research. Furthermore, few archeologists seem to have a mind of sufficient clarity and single-mindedness of purpose to maintain a plan of action oriented towards their problem(s), in the face of the incredible range of diversity and irregularity—as well as various field emergencies—present in the archeological record. Consequently, the strategy and organization of investigation in the field are actually more of an intuitive, pragmatic attempt to deal with the immediate problematic situation. Though an investigator may have certain categories of information or types of questions in mind as particularly interesting to him (or as fitting the archeological mode of the day), these rarely serve as a guide to research procedure in the early stages. Rather, excavation seems to proceed by some sort of "Standard Operating Procedure" and problems tend to be outlined and hypotheses developed as the nature and availability of the data become apparent, or in fact, once nearly all the data has been collected and assimilated.

Particularly, I would like to point out that "the right attitude"—an interest in the explanation and prediction of patterns of human behavior and in the dynamics of cultural process—does not necessarily mean that the excavator is doing anything more effective than his predecessors to generate the kind of information that is going to help him deal with such topics. These new objectives in archeology require an entirely new approach and a complete reassignment of emphasis of effort in the retrieval and analysis of data from a site. The transition
from the traditional methods of the practice of archeology is not an easy and simple one.

Obviously, the "new" archeologist must be prepared to spend a great deal of time and effort in planning his research protocol, explicitly determining his explanatory objectives, and anticipating the nature of the data in his site and the nature of the information he will need to deal with his questions, before he even goes into the field. Less obviously, the archeologist must also be ready to spend considerable time watching and evaluating the progress of the excavation, being prepared at all times to readjust his objectives and his methods of acquiring those objectives.

In the field, specific questions about the depositional significance of various artifacts, assemblages, stratigraphic events, constructions and disturbances can be raised "based on initial impressions and observed facts...Once the questions are stated, research activity can be focused into a systematic investigation" (Wilcox, [in press]:23). The investigator must continually be aware of the need to determine specific relationships between significant objects and events (floors, hearths, pits, walls, flake concentrations, trash deposits, etc.) once they become identified as such. ("Once a behaviorally-meaningful set is recognized, its relations with other sets may also be systematically investigated" (Ibid.:23)). It is important to realize that, as David Wilcox has pointed out, "the only situation" in which such relationships and determinations may be "objectively and systematically studied and determined is in the field. Ex post facto evaluations are notoriously
tentative" (Ibid.:23).

Walter W. Taylor made the statement, in 1948, that archeologists "should be reminded that their results should depend at least as much upon the work of their minds as upon that of their spades" (Taylor, 1948: 8). To a large degree, the extent and quality of one's answers depend upon what questions were asked of the data, before, during, and after the actual excavation. I am convinced this is the critical clue to the quality and value of archeological reasoning, and as such I have taken issue with Binford's statement that the major fault of traditional archeology is "the lack of any rigorous means of testing, and thereby gaining confidence in, propositions about the past" (Binford, 1968b; in Binford & Binford, eds., 1968:16). I do not see "scientific testing" as a completely dependable and irrefutable method of verifying either hypotheses or conclusions. Rather, your testing is only so accurate as your hypothesis and its test implications (just as your inference is only so valid as the basis for that inference). Test implications may appear to be verified by observed instances, while in fact the correlations are really spurious, and the implications irrelevant to the hypothesis. (A well-designed testing program should catch such fortuitous "support" for an explanation, ideally). Statistical tests are no infallible back-up, as the value also depends on appropriate use of the statistics. In the current theoretical literature, the emphasis appears to be on testing, rather than on the initial conceptualization of the problem, and questions to be asked, and data appropriate to solving those, and continued reconception throughout the research process. Consequently, "archeologists
have difficulty agreeing on just what will constitute adequate confirmation to turn a statement (about general relations or connections which exist between specified events) into a causal or deterministic law" (Watson, LeBlanc, & Redman, 1971:6).

Continuous reevaluation and redesigning are in the nature of the ongoing process of field work, and Wilcox points this out very adequately in his characterization of the research process:

Field work is conceived to be a complex, creative activity system in which the researchers are continuously asking questions, making interpretations, formalizing both of these into a scientific theory, deriving hypotheses and testing them. By continuously interacting with the field phenomena, the researcher attempts to come up with formal concepts which appropriately and meaningfully model the field context and contribute toward the solution of the research problems" (Wilcox, [in press]:20).

In brief summary then, archeological fieldwork to date has tended to be something in which you pick a site to excavate, dig it, and then see what you can find out about it when the massive task of analyzing the data is undertaken later. Ordinarily the archeologist is not in a position to say what is going on at his site, in any detail, until long past the stage of actual "dirt archeology." The theoretical perspective of the "New Archeology" requires a complete reversal in emphasis in order to be effective: fieldwork, or data collection, can no longer be thought of as a detached entity, that can be performed in a standard and efficient way appropriate to all field situations, separate from and previous to any analysis, question-forming, or comparison and compilation of data. It must be understood that there can no longer be any concept of effective field work without previous consideration of the data, surface survey and site testing, and continual
comparison both within the accumulating data and with other sites in
the same regional or cultural compass. Digging should not be undertaken
without full consideration of various possibilities of what might be
found, and what each of these possibilities might imply for further
excavation strategy. In other words, one should never dig "blind."
Understandably, such a reemphasis does not allow so easily for quick
contracting of funds and equipment, or smooth expediting of large num­
bers of workers (particularly untrained ones) in the field, leaving the
heavy head work for later. This attitude towards archeological research
organization requires a great deal of the work to be done before and
during the actual excavation. Watson, LeBlanc, and Redman have pointed
out that "participants in the ideal archeological project...will be in
an excellent position to get their results out quickly because much of
their basic analysis has been completed in the field" (1971:156).

The problem of effectively coordinating the analytic and pro­
cedural goals of the "New Archeology" with the practical contingencies
of field archeology, can be schematically summarized as follows: Some
knowledge about the archeological record at a particular site is a pre­
condition to asking good questions of that site; while some lack of
knowledge is a necessary precondition to digging, as otherwise what
reason is there to dig? "It is unscientific to excavate with no plan or
problem in mind to which the data might contribute a solution, but if
one knew exactly what was in the ground before excavation, there would
be no reason to dig" (Watson, LeBlanc, & Redman, 1971:12).
Problems in the Application of Specific Hypotheses and Questions to the Actual Excavation Situation

The backlog of archeological information (from surface observation and excavation of similar sites in the past) ordinarily provides some knowledge for the proposal of hypotheses. But when excavating, the investigator cannot know if the data he is going to get from the site will be useful for testing precisely those hypotheses with which he began. While excavating without a plan or problem is unscientific (because data cannot be collected usefully without these guidelines), the process of excavation is such that you cannot know in advance whether any given hypothesis, question, or problem is going to be testable with a given body of archeological data. "Practically speaking...there are certainly variations in the nature and amount of data at some times and places" (Watson, LeBlanc, & Redman, 1971:112). There is great variance in size and type of sites, and the kinds of information which is deposited and preserved--just as there are great differences in personality, temperament, and interests of archeologists, and scope and funding of excavations. In other words, sites and investigators come in all sizes, shapes, and colors, and the combinations of these factors will lead to a wide diversity of practical contingencies and dilemmas to be met in the course of an excavation, and consequently to a wide variety of solutions. There is no typical site, and no simple way of describing the range of situations to be encountered or how to deal with them. This is not to say that the practice of archeology is a complete hodgepodge of unexpected, erratic events, but rather that the number and
variety of excavation situations in the field that constitutes "archeological research" provide many practicalities which must be dealt with, and, given the research structure which exists today, these practicalities are often met in the most immediate and expedient way.

Though many of the idealistic "New Archeologists" today maintain that effort is most usefully and productively devoted towards specific research problems, a very strong traditional bias maintains that the excavator must deal with whatever information is there, and make a statement about the archeological record as a whole, as it is preserved. For instance,

...only one objective can be sanctioned with regard to the actual excavation of archeological sites; that of securing the most complete record possible, not only of those details which are of interest to the collector, but of the entire geographic and human environment. That which is not recorded is most often entirely lost. In such a situation, selection implies wanton waste. (Taylor, 1948:152).

Taylor goes on to speak very directly about his opinion of the place of problem-orientation in the research procedure, and of just exactly how much the preoccupation with certain problems or questions should be able to "interfere" with the collection of a complete record of the site.

...Questions of problems and objectives, insofar as they are limiting or abridging factors, should be confined to two stages in the procedure of investigation and, above all, ...they should not inhibit the excavations themselves.

As the first of these stages, the choice of an area of investigation and of the sites to be dug should be made with reference to specific problems...

And the second stage of procedure wherein special problems may determine the nature of archeological research comes after the empirical record has been gathered... The archeologist's own
personal interest may again take possession and guide his further use of the data. (Ibid.:153-4).

But, he maintains, in between these two points in the research schedule, during the actual digging process, the archeologist has no business selecting from "the complete cultural and geographic record contained within the site" (Ibid.:153).

Watson, LeBlanc, and Redman present a convincing argument for problem-orientation in data collection; basically it is that since it is impossible to avoid bias, why not be explicit about it and use it to produce the most useful body of data. They say "one cannot possibly hope to record every bit of minutiae of nonartifactual information or even to be sure that every 'artifact' is properly perceived as such" (1971:115). And then "because it is literally impossible to record everything, it is necessary to emphasize careful research design and clear formulation of questions, together with specifications of the kinds of data necessary to answer them" (Ibid.:115). However, in an earlier chapter of their book, they recognize that in an actual investigation situation, the archeologist is under an implicit obligation to deal responsibly with whatever information his investigation turns up. (He must "take into consideration whatever data results from his excavation, altering his hypotheses if necessary and adjusting his tests in the light of this data" (Watson, LeBlanc, & Redman, 1971:12). Thus they also recognize that the archeologist has some 'moral' burden to preserve information from the site he is destroying by excavation.

Not only must the archeological investigator find some way of dealing with the body of data from his site, including that data not
specifically relevant to his problem-orientation, but he must also face the possibility that the data he does get will be insufficient for what he wants to test. A fear which prevails is that of directing all one's work on a site toward a specific problematic approach, and then being caught with insufficient data to deal with that problem. There is always the possibility that "at some one site there may not be enough data to permit independent tests of certain hypotheses" (Watson, LeBlanc & Redman, 1971:112). And it must be remembered that "the appropriateness of all questions is contingent upon finding something in the field" (Wilcox, in press:23). Each archeologist, as he faces an actual excavation operation, has in the back of his head the possibility that this particular site—either because of quirks of poor preservation or because the type of site does not meet his original expectations—will not provide "the kinds of data necessary to answer his questions" (Watson, LeBlanc, & Redman, 1971:115). Hence it is rare that any investigator will have a final and concrete form of research problem decided upon before he sets shovel to earth. The problem and research methods inevitably are formulated (or formalized) as the characteristics of the site and its data become apparent from routine, systematic excavation or testing.

The Nature of Archeological Fieldwork: Implications for Learning How to Handle Field Decisions and Archeological Research Strategy

Though new theoretical directions and the methods appropriate to them have received a great deal of attention in the last decade, very
few refinements in the process of characterizing these new directions and goals and passing them on to the next generation of workers have taken place. "There appears to be an inverse relationship between the increase in new methods and techniques and the archeologist's training and ability to cope with them" (Lamberg-Karlovsky, 1970:112). I think it is because archeology as a discipline has such a hard time stating clearly and explicitly its aims and purposes, and its methods and techniques which will directly aid in achieving those. Today, the nature of archeological research is conceived either as an abstract, idealistic situation ("One must decide what to recover on the basis of formulated questions, not simply on the basis of an intuitive concept of the nature of the data" (Watson, LeBlanc, & Redman, 1971:115). "Hypotheses are deductively formulated to give direction to scientific investigation. Such hypotheses determine what data are to be collected" (Martin, 1970:199)), or as an intuitive, pragmatic attempt to deal with the immediate problematic situation, I believe it will someday be possible to delineate more precisely how to go about perceiving and outlining the problems at hand, and organizing field work to insure collection of all the necessary data with which to deal effectively with those problems, but to date, this has not been done. Such eventual delineation of aims and procedures will need to be very thorough, and particularly very explicit.

Archeological fieldwork as it stands today is a complex enterprise, operating largely as a vast succession of decisions based on preceding contingencies and discoveries. Decision based on a wide variety of phenomena have to be made constantly, at each new juncture in
the investigation, in order to determine how to handle a specific situation, or to determine in what direction to continue. Such a modus operandi cannot be described easily in the abstract. "Fieldwork is an on-going process demanding methodological decisions based on a running analysis of the data recovered from prior fieldwork" (Binford, 1964; reprinted in Binford, 1972;161). David Wilcox has laid out a step-by-step description of how archeological fieldwork should proceed, in order to be most tuned in to the emerging variation in the data, and to produce new evaluations for the further direction of the excavation and effective production of meaningful data. The description is a good indication of the multiplicity of contingencies which must be met with decisions at all points throughout the excavation, and the constant interaction between data accumulation and hypothesis formulation which must be maintained.

The first step is to state the major goals which orient the research. Second, in light of these goals and what is already known about the site, many general research questions can be asked and preliminary strategies for reaching the goals can be built from these questions. Depending once more on how much is already known or reasonably expected about a site, after research questions have been posed, situations may be specified which have the potential to yield data useful in answering the questions... The fourth step is to select a set of these situations for excavation. Next, once the excavation is underway, it is time to attempt to put together answers to the questions and to test them against the phenomena at hand and against alternative answers. Asking specific questions is part of this process. I believe the most fruitful and objective way to structure an answer is to treat it as a logical argument. Collections and notes then should document a repeating process of initial interpretation, formalization, argumentation and testing. At the end of the field work, collections and notes should include the evidence to substantiate or refute a series of alternative answers to the full range of research questions. A sixth step is continuously to re-evaluate in light of all new information the earlier statements of goals and questions, specifications of situations, and formulations of arguments. This will usually result
in a more or less complete re-structuring of excavation formats (situations), research procedures (answering questions), or collecting and recording policies (documentation). (Wilcox, in press :25).

The nature of decisions that need to be made in the field is complex, hence the best way to transmit an understanding of how that process is to be carried out is by participation in that recurring series of investigating, evaluating, and redirecting. The field is a particularly appropriate place to learn how to handle certain kinds of decision-making critical to the design and operation of an effective research program: "Only in the field...can we learn to ask appropriate specific questions and execute the process of developing arguments to adequately answer them" (Wilcox, in press :23).

Dealing with research decisions in the field is also a somewhat cumulative process. Former experience will often help deal with immediate situations, and will help one anticipate problems, good questions, and sources for answers.

Past experience may make apparent the form of many relevant questions, their hierarchical relations to other questions and to research goals, and classes of observations which would help to answer them. Such knowledge may have an extremely useful heuristic value for figuring out what happened in particular excavation contexts. (Ibid.:23).

Consequently, attention is often given to the nature and extent of an archeologist's field experience in evaluating his potential competence in the field, or in dealing with research planning, and analysis and synthesis of archeological data.
Field Excavation as an Optimum Teaching Medium for Archeology

There are many aspects of archeological field research which are most effectively demonstrated and transmitted in the field. Many aspects of actual excavation technique can really only be taught in the field. Consequently, when faced with the problem of initiating untrained students into archeological fieldwork, archeologists depend almost universally on inclusion of their students on indulgent field projects (ones which do not require previous field experience) or on actual field schools. The field school is by far the simplest and most direct way of transmitting information about how a dig is conducted, and the field school also produces a large quota of unpaid laborers who can potentially make a good contribution to the work at hand. As Judge (1972, 3) has remarked, "The field school student occupies the fairly unique role of being a highly literate slave who pays good hard money to dig holes in the ground." However, "if properly trained and stimulated, the field school students represent an intelligent work force of great potential, eager and willing to contribute to the scientific execution of a legitimate research project" (Ibid.:15).

Though as far as students are concerned, the field situation is a very appropriate medium for acquiring experience in the handling of archeological method and technique, the difficulties of directing field work towards certain research goals are considerably complicated by trying to teach students while conducting a full-scale excavation. Problems with using field school labor to operate an excavation include the loss of data, slowness of excavation progress, and the general lack
of funding available for field school projects, as Judge points out (1972). In a field school situation, collection and analysis of research data is entrusted to students who are inexperienced not only in the processes of data collection, but also in the field archeological situation itself. At the end of an eight-hour day in the sun and dust..., the untrained and inexperienced student is in no shape to make the kind of painstakingly detailed field observations necessary to the success of archeological research. (Judge, 1972:11).

Field school students produce data much more slowly than the experienced excavator or the paid laborer, principally because they need to be instructed in how to do their job, and they demand to know how their particular actions fit into the larger picture.

Once you admit the student's right to question your actions as part of his training, and once you lead him to believe it is the archeologist's duty to record everything in its original context, how do you get him to believe that a legitimate excavation can be undertaken with anything less than a toothbrush and dental pick? (Ibid.: 7).

Field schools are often run on very small budgets. Funding is generally available only from universities sponsoring them, and not from the larger granting agencies. University administrations are typically quite "careful" in their allocation of funds for field schools. The principal problem is that money is very rarely available for analysis (particularly dating, pollen studies, etc.) or publication, thus handicapping the research from the point of view of finishing the job well, and making the results generally available.

Some field schools, particularly in the past, have been especially poor as learning environments for students, largely because they have exploited student labor instead of gradually incorporating the students into the research projects.
One view which seems to have permeated past field schools... is that of the student as a paid laborer, minus the pay. In other words, they were viewed as warm bodies, necessary to move dirt, but not permitted to participate in the excavation in any other fashion. (Judge, 1972:15).

Though the field is an especially appropriate place for the demonstration and transmission of the workings of archeological techniques, methods, and theory, the student can hardly be expected to pick up much of this if he is stuck in a corner with a single menial task, and not let in on where his part fits in with the whole.

The Ideal Field School

In my conception, the ideal archeological field school, if successful in accomplishing its goals, would no longer be a field school but a working research project by its completion. Indeed the "field school" as it most often currently exists is not the smoothest, most efficient or accurate way of completing research, and yet it seems to be the only way of initiating numbers of individuals into the range of technical, methodological and mental skills needed in order for them to function well in future archeological research. Individual apprenticeships and intensive training of a few students at a time for specific jobs in excavation or analysis are more effective and satisfying ways of training students thoroughly and quickly, but these methods are far too costly in time and funding for ubiquitous use. My plan of an ideal field school also requires quite a bit of expenditure of effort on planning and supervision; one way to cut down on this is to have students
teach one another, once some understand a process or technique but
others have not yet got the hang of it,

The principal philosophy behind a field school which hopes
eventually to produce legitimate research is that students must begin
to take responsibility for their own work from the start, and must
participate in the development of the goals and intents of the project
as a whole. Clearly attitude is not all that is necessary in such an
endeavor: a considerate amount of planning must be directed towards
making information and resources of all kinds easily and directly
available to the students. Most effectively, the students should work
in small groups of two or three—so that they may work out ways of
handling problems between themselves before they need to call on a
supervisor—and each of these groups should have complete responsibility
(carefully directed and checked often for accurate documentation) for
some excavation unit (such as a grid or room). Hopefully, that respon­sibility should cover everything from surface reconnaissance to a
"final" report on that unit,

Resources and information available should include reference
collections of all kinds of artifactual and biological materials that
may be encountered in excavation or surface reconnaissance of the
environfs; a small library of relevant works in archeological field
tecniques, methods and theory, and publications from other similar
sites; and especially people (either resident or passing through) with
personal direct experience with this site or other similar sites, or
whose occupations are in the subsidiary technical fields (geology,
ethnobotany, dendrochronology, lithic technology, etc.) that contribute technical information and ideas to archeology. Also as resources I might add people in the latter category above who will be directly involved in some project of investigation and analysis for the site the students are working on. These people should be prepared to incorporate interested students in their projects or offer sound direction for those who want to pursue something on their own. Interest, encouragement and resources should be available in abundance to the students, so that they will have ways and means of pursuing more extended analysis of a site from whatever niche or angle interests them.

Another vital aspect of a field school that is important to its effectiveness is time: time enough to train the students in the minimal methods and techniques necessary to get on and do their own work, time enough to allow students to develop and carry out research projects, time enough to allow frequent discussion and re-evaluation of purposes and methods for the research project, and time enough for completion of analysis and synthesis. A really adequate archeological training program should be spread out over at least four months instead of the usual two. In other words, not only would greatly increased funding be necessary to support a longer session, but the field season would necessarily cut into the regular university schedule.

The goal of the field school should ultimately be to train students to become effective components of a full-scale field operation in archeology, by closely directed, responsible participation in excavation, laboratory analysis, and synthesis of data. "The need to train
students quickly, as well as monitor their progress thoroughly yet unobtrusively, will remain the key to legitimizing the field school" (Judge, 1972:15). The best medium for training is actual research (when the students participate fully and know what is going on), although in the field school as it exists today, the untrained student is often a liability in the field situation.
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The thesis is therefore accepted in partial fulfillment of the requirements for the degree of Master of Arts in anthropology.

18 October 1974
Date

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