

# What is Archaeology, Really?

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## Introduction

It is in the exploration of competing ideas and approaches that the relative values of such ideas and approaches are tested. It is in the exposure of weaknesses and inappropriateness of propositions that advances are made and new questions asked. This is rarely, if ever, accomplished unemotionally and without personal involvement [Binford 1972:451].

Since the mid-1970s there has been a proliferation of approaches to prehistory. Proponents and practitioners of these new research programs implicitly seek answers to the question "What is archaeology?" There are any number of proposals as to the nature of archaeology. It is not archaeology, but archaeography (Deetz 1988). It is neither creative art nor science (Hodder 1986); it is the accumulation of facts (Courbin 1988); it is (or should be) revolutionary practice, but failing that it may be nothing but the practice of ideological dupes (Shanks and Tilley 1987a); it is the history of ideology (Leone 1982:754). And there are many traditional definitions: it is culture history, or human ecology, or cultural anthropology with time-depth. And so on. I suggest that archaeology is all of these things, and probably a great many that have not as yet been considered. But it is surely a mistake to assert, as many have done, that there is only one archaeology or one research program that is appropriate for archaeologists to follow when interpreting the material traces of the past.

The question "What is archaeology?" breaks down into: 1) the proper object of study, i.e. the aims and goals of the discipline, and 2) the best way to achieve those goals. Thus there are definitional and methodological questions. The definitional questions are arguably transitory, as the history of the discipline (and of science in general) demonstrates — they are dependent on the worker's culture or 'paradigm' (after Kuhn 1970), and thus are subject to change (see for example, Binford and Sabloff 1982, Clarke 1973a). The methodological questions, on the other hand, are perennial — they turn on epistemological and ontological matters (i.e. the grounding of knowledge and the nature of being). The question of the nature of archaeology, for me, hinges on what science is (i.e. a method of producing particular kinds of knowledge), and on the nature of the archaeological subject domain (i.e. the real past and its material traces). It is my purpose in this essay to examine the discipline's concern with the truth of claims about the past, and to present the newer research programs (and their aims and goals) as scientific inquiries, albeit having different foci of interest than either the New Archaeology of the 1960s and 70s, or the 'traditional' practice against which Binford and others placed the processual program.

At the end, I hope to be able to convert the overview into a conciliatory statement of sorts. I concur with Wylie, who suggests that "what we need now is not a 'postmodern' or 'post-processual' archaeology, but renewed resolve to come to grips with the problems that modern, processual archaeology and its antecedents have addressed" (1988:11). I conclude that *real* archaeology encompasses all the various approaches taken by archaeologists today, and that terms such as contextual archaeology, symbolic archaeology, ecological, processual and even New Archaeology, are redundant. The discipline should realistically include

considerations of context, history, individual, symbolic practice, unconscious structures, biological imperatives, and ecosystems in the broadest sense, and should rule out none of these, a priori. Instead a deliberate attempt should be made to bolster the empirical justification for any of them. Calling something contextual archaeology (to use just one example), partitions it from what other archaeologists do, allows the namer to possess it, and to present it as the only reasonable way to "do" archaeology. Since the current crop of research programs has ultimately to justify claims of knowledge no less than those that went before, and since it is a philosophical question how to go about doing just that, I will begin with philosophy of science.

## Philosophy of Science

There are a number of reasons why a statement of current trends in archaeological theory should involve philosophy. Debates centering on aims and goals that were begun before the 1940s, as well as those since, have all been concerned with, at their base, verifying knowledge claims. This is ultimately a philosophical question, and one which particularly involves philosophy of science. For, any endeavor which produces knowledge that is not simply personal and idiosyncratic must ultimately appeal to criteria of intersubjectivity, something with which philosophers of science have been concerned for some time. There is, unfortunately for archaeology, no standard version of what science is or what it does. This has led to difficulties, as we will see. However the dominant view of science until mid-century was an empiricist philosophy. My discussion begins with a particularly strident, prescriptive empiricist program — logical positivism.

Since Hume, and until very recently, philosophy of science has been dominated by various forms of empiricism, of which logical positivism and the late logical empiricism of Hempel represent stringent versions. Empiricist philosophy is grounded in three premises. The first of these is that knowledge is ultimately reducible to experience, obtained by observation, which on their account is the only source of sure and certain knowledge. Second, experience itself is "atomistic," meaning it "consist[s] of discrete perceptual elements or events, such that knowledge can be said to be founded on an irreducible substratum of 'basic' ideas which correspond directly to discrete, definable (immediate) impressions of sense or reflection" (Wylie 1982c:145). These are the "basic data" of experience. Thus, for empiricists, there is to be no reasoning beyond experience. And this is where a positivist inspired archaeology encounters severe epistemological indigestion — in the dilemma of coming to "know" something which literally does not exist, i.e. the "human, cultural" past (Wylie 1989a:1), while employing a philosophy that rules out inferences beyond what is observable. The third and final epistemological pillar of empiricism is that knowledge is necessarily restricted to the systematization of observables. It will be seen that this systematization is what New Archaeologists sought to put behind them in calling for an anthropological, explanatory discipline, and what they were ultimately restricted to as a result of their adherence to a logical positivist prescription for 'doing science'.

Positivism, a strict form of empiricist philosophy, relied on formal logic as a primary tool for the analysis of science (Gibbon 1989:14). This form of empiricism held that there was a unity of science, with the natural sciences possessing a privileged model of rationality (not surprising when it is remembered that so many of the proponents of positivism were physicists). All phenomena were explainable in natural terms without reference to spiritual or supernatural or metaphysical events (Gibbon 1989:8). This naturalism saw statements of the

kind, "the unconscious is the first cause of the world" as neither true *nor* false, merely meaningless" (Gibbon 1989:16). Adherence to positivist tenets was supposed to create a "value free and objective" knowledge (Gibbon 1989:26). It can be seen that this is an untenable view of science and knowledge. It is unable, ultimately, to accommodate even much well established physical science and is hopelessly incapable of providing useful insight to archaeologists in its refusal to acknowledge unobservables as the object of study in a scientific inquiry. Despite years of tinkering, and in debates the substance of which are largely outside the scope of this presentation, logical positivism and logical empiricist philosophy of science were ultimately found to be quite incapable of explicating science and scientific knowledge in any realistic fashion, a point to which I will return in a moment.

### The Demise of Positivism

New Archaeology's appeal to positivist tenets usually turned on notions of scientifically sound explanations, as opposed to speculation or unwarranted inference. For this reason, this presentation, like those of Gibbon (1989), Kelley and Hanen (1988), and Wylie (1982c), focuses on the formal characterization of explanation on a positivist account, and the related question (at least for archaeologists) of confirmation of hypotheses. It is perhaps not surprising, but unfortunate, that when Binford and others went looking for models after which to pattern their practice, they went no further than logical positivism, and a vocal adherent, Carl Hempel. For Hempel, an explanation was only valuable as long as it appealed to universal laws of nature that could be seen to include the event to be explained. For a statement to be explanatory, it had to fit in a logical relationship with a "covering law," such that the explanation logically entailed the event(s) for which an explanation was sought. The covering law model of explanation, or the Deductive-Nomological (DN) model, then, provided (or at least aimed at providing) the methodological recipe for secure (i.e. deductively certain) knowledge.

The extremely narrow view of explanation met with criticisms to which it could not respond in any convincing way. For example, the logical form of an explanation modelled on the covering law is conceived as "an argument to the effect that the phenomenon to be explained ... is *just what is to be expected* in view of the explanatory facts cited ..." and that the explanandum (or event to be explained) "follows *deductively* from the explanatory statements" (Kelley and Hanen 1988:168). This account of explanation allows, for example, arguments that fit the model but are not explanations, as well as those that are just not "relevant to" or "explanatory of" the outcome (Wylie 1982c:177). Moreover, some very good explanations do not fit the model (Kelley and Hanen 1988:180). The DN model also allowed "the explanation of any fact by itself" (Kelley and Hanen 1988:175). Thus, it was impossible for positivists to distinguish lawlike relationships from accidental ones in any formal sense that could satisfy empiricist criteria (Wylie 1982c:177). Many good explanations are just not logically entailed in a deductive sense. They are, instead, inductive inferences, often based on probabilities. Adherence to deductive entailment caused trouble for archaeologists who imported positivist notions to their investigations.

For me one of the most telling shortcomings in what the positivists wanted from an explanation — the symmetry of explanation and prediction — is that it rules out a good number of perfectly adequate explanations that are already an accepted part of the natural sciences. For example, we can explain the diversity of life with the fact of evolution, but we can in no way predict the direction it will take, except in the broadest terms. Historical

explanations such as evolution would be ruled out in a strictly positivist science. Thus, there is a real sense in which pragmatic considerations must be included in any model of explanation.

Finally, as Wylie notes, the positivist account misrepresents explanation at a most fundamental level; explanation cannot be characterized adequately in purely formal terms as an argument whose structure establishes a pattern of expectation. Explanation of unique events and the "monster" explanation cases [Those of the kind: Mr. Jones took his wife's birth control pills every day for a year and during that time he did not get pregnant. Thus Mr. Jones did not get pregnant because he took his wife's pills.] suggest, in particular, that this formalistic approach obscures an essential source of explanatory power, viz., background and contextual knowledge (i.e. the content, as opposed to the form of the "argument") about how and why the cited antecedents produced, and hence, explain the explanandum "effects" in question [1982c:185].

It is this contextualist challenge to the basic data thesis of empiricism to which the program was unable to respond in any convincing way. Observation, it can be seen, is ultimately dependent on theory. Data are "theory-laden." One does not meet the world with a naked consciousness, instead the world is met, and infused with meaning, constituted in a real sense, through the background knowledge and in the particular context of the observer. Indeed, "the very possibility of experience depends on the active structuring of a perceptual field" (Wylie 1982c:203). Kuhn's (1970) influential analysis of scientific revolutions turned on an incommensurability thesis that depended on this notion of the context of the worker, and the constituted nature of knowledge, contra empiricists like Hempel. As Wylie has noted (1989b), it is ironic that New Archaeologists such as Binford should have appealed to the Kuhnian notion of 'paradigms', thus legitimizing their new research program, in proclaiming that a paradigm shift had occurred, while at the same time embracing positivist prescriptions for explanation.

Summing up, logical positivist philosophy of science, which privileged experience, and held a concept of knowledge as consisting of particles of experience, contained within it no possibility of determining underlying structures and relationships which would constitute a satisfying explanation of interesting phenomena. It is impossible to maintain, as the empiricists did, that "sensory experience is the (sole) source or basis of knowledge of matters of fact and that this experience consists of a series of basic, immutable impressions (i.e., from which all other ideas or cognitive contents 'arise')" (Wylie 1982c:167). The contextual insight and the constituted nature of knowledge ultimately undermine the empiricist program. Although certain empiricists like Hempel (and to some degree M. Salmon [1975, 1978, 1982a, 1982b] and W. Salmon [1982]) have attempted to 'liberalize' the empiricist framework, they have failed because they include in the description of a satisfying explanation unobservable relationships that are meaningless on an empiricist account. A much richer view of science and of the production of knowledge is available in 'scientific realism', and it is this to which I now turn.

## A Realist Account of Science

In the 1960s philosophy of science began to include "a more focused exploration of the way science actually operates, with explicit attention to the historical, sociological, economic,

and political factors that affect the doing of science" (Kelley and Hanen 1988:19). Recognizing the distinct failure of positivist accounts adequately to characterize explanation, theory or confirmation, and that this failure was grounded in the empiricist notions about knowledge, a more realistic account of science emerged (Wylie 1982c:230).

While retaining the same broad goals for science as those held by positivists (such as intersubjectivity of knowledge, verification, etc.), the realist account makes significant additions to the scope of scientific explanation. Causation is seen as fundamental to explanation, not as an epiphenomenon, and as the thing that makes the world work, not something to be rejected as meaningless because it is unobservable. On a Humean, empiricist account causation is seen merely as the temporal displacement and constant conjunction of two observable events. There is no sense on Hume's account that there is any processual relationship between two events related by cause. This is Hume's 'regularity theory'. When a string is plucked, he would say that the sound follows the vibration in all cases. Thus, a causal relation exists because all such sounds follow all such pluckings. A realist knows that the regular relation between these may provide evidence for the existence of a causal relation. But in saying that there is a causal relation, we mean also that there is some intervening mechanism which, in this instance, is described by means of the theories of sonic physics and neuro-physiology [Keat and Urry 1975:28].

Through "systematic study of more accessible and familiar causal processes and mechanisms" scientists succeed ultimately in "bring[ing] into view underlying causal processes ... without lapsing into pure (empirically unsupportable) speculation" (Wylie 1982c:204-205).

Science achieves success in theorizing about the world mostly through the use of models, analogies to better understood phenomena that help to explain how observed events came about. These are more or less successful because they capture, in essence, causal relationships between observed and unobservable, something which would not have been admitted in an empiricist account. And it is this admission that "causal efficacy and causal necessity [are] knowable features of empirical reality" (Wylie 1982c:281) that fundamentally sets scientific realism apart from empiricism. Including causality as a basic component of explanation expands the kind and number of questions that can be answered. History is allowed back into explanation. On a realist account

answers to why-questions (that is, requests for causal explanations) require answers to how- and what-questions. Thus, if asked *why* something occurs, we must show *how* some event or change brings about a new state of affairs, by describing the way in which the structures and mechanisms that are present respond to the initial change [Keat and Urry 1975:31].

A historical science, like evolutionary biology, includes explanations that are like narratives, where concepts are involved and antecedent events are connected in causal ways to change (Flannery 1986:514). Description becomes a necessary part of explanation, the "how" and "what" of processes are every bit as important as the why.

In summarizing, then, it should be stressed that (at least for the purposes of this presentation) causal relationships, "relations of natural necessity that exist in the physical world" (Keat and Urry 1975:27), understood through the construction of models analogizing known to unknown processes and mechanisms, *are the stuff of science*. This is a far cry from the sterile, formal treatment of knowledge and explanation allowed in an empiricist framework.

## **Crumbling Foundations**

Although the New Archaeology's break with 'traditional' practice is well documented in several monographs (e.g. Courbin 1988; Gibbon 1989; Kelley and Hanen 1988; Wylie 1982c), several points of concordance between empiricist philosophy and archaeological practice prior to the 1960s should be highlighted in order to demonstrate why the New Archaeology program never really got out of the starting blocks as positivist-inspired practice, in spite of its worthwhile goals for the discipline.

In "Some Aspects of North American Archaeology," Dixon fired the first shot in what would be a desultory duel between strict empiricist archaeological practice and an archaeology that had as its goal the explanation of cultural phenomena, when he said:

We are today concerned with the relations of things [not just the things themselves], with the whens and the whys and the hows [1913:565].

Archaeology as it was normally carried out according to his observation had demonstrated "too little indication of a reasoned formulation of definite problems, with the attempt to solve them by logical and systematic methods" (Dixon 1913:563). Clark Wissler's (1917) "The New Archaeology," eerily prefigured both the name and the aim of what was to take hold in archaeology nearly fifty years later. It seemed to Wissler as if archaeology was chronically suffering the empiricist's dilemma, that "any extrapolation beyond 'the data' (as described and systematized by archaeologists) represented pure speculation" (Wylie 1982c:209). The empiricist's dilemma had rendered archaeology a discipline concerned mainly with the accumulation of "minutiae" and the systematization (according, of course, to certain implicit constructs) of the material of the past (Kluckhohn 1939, 1940). In practice a sophisticated antiquarianism, archaeology in the first half of the twentieth century held mainly to a reductivist view of culture as the system of norms shared by the members of the culture (hence 'normative'). This view served to limit the scope of archaeological investigation, because it characterized culture as monolithic, and just amenable to classificatory/historical studies.

There was an almost official skepticism that was finally articulated by Hawkes (1954) as a "ladder of inference" that saw ecological, physical environmental, and typological parameters relatively easy to derive, and which saw social and ideological interpretations as essentially impossible to acquire because too far removed from the phenomenal domain of the archaeologist. Pessimism among the traditionalists was balanced by the official optimism of, for example, Kluckhohn and later Binford, and the tension finally resulted in the "little rebellion" of New Archaeology.

Kluckhohn in 1939 and again in 1940 made it clear that archaeology had problems. And he criticized empiricism and the development of archaeological theory to that time. His feelings were shared by Steward and Setzler, who called on archaeology to "shed some light ... on conditions underlying [different cultures'] origin, development, diffusion, acceptance, and interaction with one another" (1938:7). They recognize that the accumulation of data (however structured) is a finite endeavor, and they, like Binford, realize that it is an essentially finite enterprise, for,

When ... history [has been] ... reconstructed, what task remains for archaeology? ... shall we cease our labors and hope that the future Darwin of Anthropology will interpret the great historical scheme that will have been erected? [Binford 1968a:5].

Others, like Krieger (1944), Brew (1946), Ford (1954), Spaulding (1953), and Hill and Evans (1972) in the typology debate, articulated the failure of the discipline as it was traditionally practiced to accommodate an adequate theoretical awareness. The insight emerged that facts are constructions, and that experience, rather than being the "basic data" of knowledge, is the arbiter of knowledge. Kluckhohn (1939:331) saw no sharp distinction between fact and theory, as indeed there is not. He saw the need to pay heed to the role of ideology, cultural bias, and unconscious presuppositions in the construction of "facts." He characterized all such unreflexive contributions to interpretation as 'enthymematic' (a point also raised by Brew [1946:45], when he says that "a complex theoretical viewpoint is usually implicit in some of the most apparently innocent 'statements of fact' ").

Calls for explanatory aims, some of them even with explicit appeal to hypothesis testing and modelling methodologies, did not, however, result in a turnaround. Archaeologists were still, for the most part, hampered by an empiricist vision of knowledge and science. A vicious circle was entered every time the desire for an explanatory discipline was articulated, since what was desired, i.e. elucidation of unobservable processes resulting in the archaeological record, was ruled out by an empiricist science (and remains a question for empirical inquiry even today [see Wylie 1989a]). It can be seen that "the skeptical entailments of an empiricist framework for archaeology" rendered the knowledge claims of 'traditional' archaeologists simply the "systematization of experience" (Wylie 1982c:216). It was this "stable core of epistemic problems" (Wylie 1988:3), of not being able, methodologically to go beyond the data, that perpetuated inertia in the search for an explanatory science, and that kept archaeology from achieving its goals.

### Archaeology as Anthropology

With the publication in 1962 of "Archaeology as Anthropology," and subsequent polemic statements by Binford (e.g. 1965, 1968a, 1968b), and by his students and followers (e.g. Fritz and Plog 1970; Plog 1974; Watson, LeBlanc and Redman 1971), the gloves were off. The search for archaeological laws, on the positivist model of nomothetic science, was on. It is tempting to employ 20/20 hindsight, and to call the New Archaeologists philosophically naive, and methodological zealots, but it only serves to cast a pall on the real advances in the aims and goals of an anthropological archaeology that were entailed in the movement. Where 'traditional' archaeology had been skeptical of theory, and eschewed explanation in favor of conventional interpretations based on generalizations from archaeological 'data', no such skepticism existed in the program of New Archaeology (Plog 1980:26). Binford's (1962) now famous statement that "*the total extinct cultural system*" is obtainable comes to mind here. And, whereas traditional archaeology had held to a view of culture that was normative and particularizing (i.e. relativist), most anthropologists of the 1950s and 60s, and thus the archaeologists of the 1960s and 70s, sought systemic (i.e. cultural ecological and evolutionary [after Steward and White]) and comparative understanding of their phenomena. Hawkes' infamous "ladder of inference" was deemed scaleable in the new program, although as it has turned out, most New Archaeologists labored at the subsistence/settlement level of inquiry, while social and ideological inferences awaited "strong

linkages" (Binford 1983:17) between the archaeological phenomena and their social determinants. Kluckhohn's critique of 'traditional' archaeology contained the creed for the New Archaeology of the 1960s:

While taking account always of the circumstance that the processes which control events are imbedded in time as well as in space and in the structure of social forms, a primary interest in discovering trends toward uniformity in human behavior under specified conditions will be, I feel sure, the more fruitful [Kluckhohn 1940:84].

I would agree that it is the tendency toward comparability in cultures and in cultural behavior that allows us to apprehend and understand anything at all about others, and specifically to recognize others as cultural and human. In setting the tone for the next forty years of inquiry, Kluckhohn was not advocating a 'dehumanizing' form of naturalism and behaviorism, but rather he was recognizing a fundamental empirical domain of human experience, one he and others after him felt to be worthy of study. I am not sure that the excessively mechanistic eco-determinism of Harris (1979) or Winterhalder (Winterhalder and Smith 1981) are entailed in Kluckhohn's insight, and it is my hope that comparative studies will always form part of the archaeological inquiry.

It is one thing to espouse the ideal of theoretical transcendence of material constraints on knowledge of the past; it is another to put it into practice. By the time of the publication of *Explanation in Archeology* in 1971, post-positivist philosophers and astute archaeological observers as well were heaping mostly unheeded criticisms on the New Archaeology program. Morgan (1973, 1974), Levin (1973), and Johnson (1972) led the attack, but the power base of American archaeology was firm, and many more were convinced by the stand taken by the proponents of the New Archaeology. The (mostly) accurate comments of its critics were largely ignored. The problems perceived by these critics are just those that have brought the New Archaeology program to its present, practically paralytic position, namely those problems delineated in the positivist account of science — the accounts of explanation, and testing on the covering law and hypothetico-deductive model.

Tragically, the goals of Binford, and before him Taylor (1948), Kluckhohn (1939, 1940) and others, have been confounded by a lingering empiricism. So it happens that, when New Archaeology is taken to task, criticism centers on the methodological, rather than the substantive aims. As Wylie notes (1989b), it is only a historical artifact that Binford should have promulgated his theoretical framework (cultural ecology) as the organizing, explanatory framework in the new program. If one ignores the restrictive view of science implicitly held by Binford and others, the substantive goals of Hodder (for example 1982b, 1986) or Shanks and Tilley (for example 1987a, 1987b, 1989) are really no different than those of the New Archaeologists — that archaeology tell us something more about the past than descriptions and chronologies could.

Over and above the external (and internal) criticisms, New Archaeology has shown a surprising ability to be self-critical in spite of its paralyzing empiricist fabric. David Clarke, whose *Analytical Archaeology* (1968) formulated a British variant of New Archaeology, could say that "many elements [of the New Archaeology] are unsound, inaccurate or wrong but that is equally true of much of traditional archaeology" (1973a:12). Flannery, in the same year, recognized that overly zealous New Archaeologists had come up with no laws, other than "Mickey Mouse Laws," but that they and others who adopted less explicitly positivist methods were united in the "dream ... of what archeology could become. ... and the nightmare

... that [certain young traditionalists] will forever keep it the *imprecise pseudoscience* that it is today" (1973:53 [emphasis added]).

Renfrew (1982, 1983), and Wylie (1982c), to name just two, identify the problem. The " 'law-and-order' group (following Flannery's 1973 terminology) put archaeology in a paradoxical situation by prescribing as obligatory an explanatory form so little appropriate that vanishingly few good explanations already accepted in the field could be found to conform to it" (Renfrew 1982: 8). And, from other, less sympathetic voices (e.g. Hodder 1981, 1982b; Miller 1982) the same tune was heard.

In the twenty-five or so years since "Archaeology as Anthropology" Binford and others have moved from heady optimism about the limitless empirical content of the archaeological record to an almost obsessive wallowing in minutiae similar to, but not identical with the traditional archaeology from which they sought to distance themselves. For example, Schiffer gives us *Behavioral Archeology* (1976), his effort to systematize the links between the archaeological record and the behavior that created it, and more recently *Formation Processes of the Archaeological Record* (1988). These are useful examinations of a problem but cannot be said to be the only concern of archaeologists. In Binford's introduction to *For Theory Building in Archaeology* (1977), we are presented to a theme that permeates his writings to the present (for example 1981a, 1981b, 1989a, 1989b; Binford and Sabloff 1982; Sabloff, Binford and McAnany 1987). We need, says Binford, independently derived, 'objectively grounded' means of measuring the variability in the archaeological record — a fool-proof, 'Rosetta Stone' with which to decode the archaeological record. We need to accumulate 'arguments of relevance' to link the static phenomena of the present to the dynamic forces that created the variability in the past. In calling for this, Binford at once demonstrates his fundamentally empiricist underpinnings, and dooms archaeology to the systematization of what are perceived as basic data.

There is, in the work of Flannery, the hope that a systems-oriented, processual program can lead to valuable knowledge of the past. In the evolution of his program, Flannery has included more and more of the considerations that New Archaeology has been criticized for ignoring, such as ideology and belief (see for example, his comment on Sanders work in the Valley of Oaxaca [1988]). It may be for this reason that Flannery seems to have taken a place on the periphery of American archaeology, making his greatest impact in scathing satirizations of practice, such as his "Archeology with a Capital 'S' " (1973), "The Golden Marshalltown" (1982), and "A Visit to the Master" (1986). These criticisms arise, in the main, from the New Archaeology's habit of ignoring culture. There is much of value in Flannery's work, and it is interesting that it has been accomplished under the flag of the New Archaeology, testament, more than likely, to the value of the aims and goals of the program, more than to its methodological prescriptions.

## The Demise of New Archaeology?

The critical insights of New Archaeology that brought about a break with 'traditional' practice have been somewhat overshadowed by the methodological failure of a strictly positivist archaeology. Recognition that it was the empiricist basis of the cultural historians that was hobbling the discipline was a distinct break with the past. Furthermore, the realization that the normative view of culture was severely limited in its ability to understand and explain observed variability in cultures, both archaeologically and ethnographically, was a profoundly important insight. Unfortunately, by replacing the previous skeptical empiricism

with an even more stringent version, in positivism, New Archaeology was incapable of achieving its aims (Wylie 1982c). There is no virtue in being wrong. For their heedless use of positivism, the "New" archaeologists have to bear the brunt of the blame. But this much is certain: archaeologists of the 1960s and 1970s who have been called "New" were ultimately interested in acquiring secure knowledge about the phenomena they studied, and answers to the questions they asked that were not mere untried speculation. Perhaps, as Gibbon (1989) suggests, they were performing an action designed to protect and mystify their position and status in an academic hierarchy; perhaps they simply abducted positivist philosophy of science in a naive effort to produce the secure knowledge they sought. The desire for knowledge and understanding of archaeological phenomena has not waned with the "demise" of the positivist philosophy of science. Questions are turning to the social and ideological, the individual and contextual, yet alongside what Gibbon (1989) would call the Romanticization of scientific enquiry, there is an unwillingness to simply abandon the project that seeks to evaluate claims of knowledge. What form it will take, how it is to be achieved, and what that means for the scientific status of archaeology is the subject of the next sections.

### **Marxist Research Programs**

There is perhaps no more irresistible theoretical framework in the social sciences than that of Marx. In archaeology, as in other social sciences including anthropology before it, Marxist thought represents for its adherents a compelling antithesis to mere ideological reproduction, and 'vulgar' historiography and sociology. There are a number of different Marxist perspectives (to borrow a term from Matthew Spriggs) currently in evidence in archaeology. Each takes what it wants of classic Marxist theory and of more recent tinkering, and each constructs a theoretical explanatory framework within which archaeological investigations are carried out, searching for evidence in the archaeological record that will instantiate theoretical propositions.

Although Harris's (1968, 1979) theory has attracted nothing but invective from strict Marxists, cultural materialism must be considered within the framework of Marxist theory, since its central construct is the relationship between the material and the social/cultural. Its adaptationist foundation, however, intended to replace the 'Hegelian monkey' of dialectical philosophy, renders this program, in the eyes of Freidman, "vulgar materialism" (1974). In the words of Harris, cultural materialism

is the principle of techno-environmental and techno-economic determinism.

This principle holds that similar technologies applied to similar environments tend to produce similar arrangements of labor in production and distribution, and that these in turn call forth similar kinds of social groupings, which justify and coordinate their activities by means of similar systems of values and beliefs. Translated into research strategy, the principle of techno-environmental, techno-economic determinism assigns priority to the study of the material conditions of sociocultural life, much as the principle of natural selection assigns priority to the study of differential reproductive success [Harris 1968:4].

This absurdly reductive program appealed to the New Archaeology, mostly because Steward's cultural ecology and White's cultural evolutionism basically held to similar frameworks. Harris merely made the link to Marx explicit, whereas it had been implicit in the work of Steward and White. Probably because cultural materialism virtually ignores the ideational

realm as having anything whatsoever to do with the evolution of cultural systems, and certainly because of its lock-step determinism, it is a program that could not ultimately survive the criticism of Marxist and for that matter non-Marxist social theory.

The wedding of Lévi-Strauss' structuralism and Marxism came about through a dissatisfaction with the ability of Marxism to make room for the "vertical structures which account for societies as entities" (Friedman 1974:445). As with structuralism, Structural Marxism seeks "knowledge of the fundamental properties of social reproduction which enables us to predict the way a society will behave over time" (Friedman 1974:445). Calling Marx's analytical categories such as 'relations of production' merely "functional distinctions," structural Marxists, like Friedman, see a particular social formation as nothing more than "historically specific" manifestations of unknown structures (Friedman 1974:446). Moreover, Friedman contends that the 'mode of production' is not a technological phenomenon, because the social relations of production cannot be viewed as technical relations. They are, instead the "*social relations* which ... determine the economic rationality of ... the material process of production in given technological conditions" (Friedman 1974:446). The dialectic is seen by Structural Marxists like Friedman as an undefined structure, which should be understood to affect the compatibility of different outward structures in the social formation. Thus "a contradiction is defined as the *limit of functional compatibility* between structures" (Friedman 1974:448) (e.g. between the forces and relations of production). Thus the structural Marxist program, for Friedman, seeks the relationship between forces and relations of production which would lead to an explanation for the social formations that have existed in the past, and that preceded those in the present.

As Gellner (1982) points out, there is a contradiction, even in this avowedly Marxist program, between the determinism that is still retained from the materialist framework, and the notion of historicity in Marxism, which maintains that there are no universal causal mechanisms because of the dialectical nature of existence. Nevertheless, structural Marxism would be accommodated by a realist account of science, both because it appeals to empirical observations, and because it recognizes that there are unobservable causal relationships that must be elucidated for explanations to come to light.

So-called Neo-Marxist programs that have arisen out of the recognition that relations between dominant 'complex' groups and historically dependent 'less complex' groups have probably existed for some time, not simply in the modern, capitalist, world. As a model, Wallerstein's world system provides a much broader scope for the explanation of variability within and between groups, by demonstrating how relationships of domination can arise and be maintained, while always retaining the ability to shift and change as conditions change. A related concept, that of core and periphery, also begins to capture the subtlety which is potentially involved in culture change that a purely Marxist account did not accommodate. Frankenstein and Rowlands (1978) present an account of Early Iron Age society in south-western Germany employing the world systems approach to prehistory, while Dietler (1989) established that a relation existed between the Halstatt culture and Mediterranean groups. The basis for the Neo-Marxist approaches is, again, essentially empirical. It is seeking to establish the existence of relationships that have causal relevancy in the explanation of cultural manifestations.

Recent edited volumes capture the diversity of the Marxist research program as it is being deployed around the world. I have already mentioned *Marxist Perspectives in Archaeology* (Spriggs 1984). It contains a number of thoughtful papers, notably by Kus,

Rowlands, and Pearson, that seek to explore the archaeological possibilities of Marxism. Bate's and Tosi's papers in the same volume are evidence that the archaeological recognition of some of the factors important to Marxian analysis, like quantifications of the productive forces in an archaeological context, or the visibility of craft specialization and labor allocation, are going to be every bit as recalcitrant for archaeologists to monitor as population pressure or niche width have proven for processualists. Miller and Tilley's *Ideology, Power and Prehistory* (1984) specifically views the archaeology of relations of power and domination, focussing on legitimation, by material means, of inequality.

### Problems with Marxist Archaeology

For me, there are several problem areas in Marxist theory and its archaeological application. First of all, there are the definitional problems I see with the dialectic. I would argue that it is used to describe not just one thing, but several. First, it is a description of nature — the dialectic as essence. Second, it is a method of analysis — the dialectic as insight, e.g. unwinding the constituents of a construct. The dialectic, in a special case of the above, is also an explanation of change — the dialectic seen in contradictions and in the class struggle that brings about social change. Third, and finally, it is a model for the way humans think — the dialectic as "an effort to capture and to reduce to communicable discourse ... inventive psychic capacity ... distinct from discursive thought" (Heilbroner 1980:55). If, indeed, the dialectic is all these things, then I would suggest that it is best seen as a convenient term for a slippery and little understood suite of processes (i.e. anything that has more than two dimensions), including structures, of interest to Marxists and others. The dialectic is at best poorly presented as a method, and at worst represents the attribution of change to a universal ineffable cause. Understanding change as the result of something as poorly defined as the dialectic is a little like trying to understand evolution with the concept of variability to hand, but without a notion of selection to give it order. The dialectical view of "changefulness located 'within' things" (Heilbroner 1980:34) is also a convenient shorthand for a complex (in the case of human societies) history of interaction and change, one that is arguably different at every point along the way (or at least open to empirical inquiry of the constituents of change). Neo-Marxists, with a rejection of universal social or cultural laws, recognize this shortcoming of the dialectic as essence, I think. But with all its slipperiness, the model of a dialogue between relational objects or within an object of study is a valuable heuristic device. As Heilbroner puts it: "To use the language of discursive thought ... is to use a language that rules out the very ambiguities, Janus-like meanings, and metaphorical referents that are the *raison d'être* for a dialectical view" (Heilbroner 1980:56). For the time being I would prefer to conceive of the dialectic as an expressive means of describing complicated processes of which we really know very little, at least where social change is concerned.

Marx's hope for capitalism's collapse, and his call for political action to hasten its demise are tied up in his belief in the change inherent and implied in the contradictions he recognized in the capitalist mode of production and in the inequality that he perceived as unjust and unnatural. His fervent hope rests on an assertion that it is the dialectic of contradiction that brings about change, and ultimately the end of the alienation of labor through the class struggle. By ignoring other determinants of change, or by relegating them to inferior roles, however, Marx was ultimately restricted in his ability to explain change. It is not clear if his

aspirations will ever be fulfilled. It is also not clear if, as he would assert, there is one right way for human groups ultimately to be organized (i.e. along socialist lines).

Many commentators on Marx have noted that there is no account of change in non-capitalist societies in Marx's original formulations (Heilbroner 1980:71, Kristiansen 1984). Kristiansen and others are attempting to fill in the missing pieces using general principles drawn from Marxist-oriented works, especially the process of legitimation. This does not erase the need, as I see it, to examine how Marx's overall theory was affected by his arguably naive perceptions about "primitive" societies. His stage-evolutionism, for instance, critiqued by Bloch (1983:64), presents a lock-step relationship between technology and political systems that has to a large degree been discredited in the intervening years. Add to this the proposal of a pristine human condition, that of primitive communism, where "class divisions ... are not to be found ... [property] is almost non-existent, save for minor personal belongings ... [and] the economic basis of society ... is seamlessly woven into its social and political functions" (Heilbroner 1980:70). The naive notion of a golden age of hunter-gatherer cooperation must surely give way to the recent critiques such as Schrire (1984) and Cashdan (1980). Indeed, the habit of distilling to an average case all that was known and has since come to be known about groups organized in less complex fashion than our own has carried on to the present, and is not unique to Marx, but I think there may be plenty of room for adjustment. In viewing such groups as in some sense politically stable, egalitarian (except of course for gender and age inequality), and based on a stable mode of production such as hunting and gathering, Marx may inadvertently have stacked the deck in his critique of Capitalism. What this means to his whole theory is unclear at present.

The rather static view of "primitive" humanity, at the mercy of nature, having no surplus nor any need for surplus, persisting, but not evolving, "gives us no special explanation as to why or how [they] are disrupted" (Heilbroner 1980:71). The lack of a theory of historical change for 'primitive' societies and Marx's unworkable classification of modes of production have been critiqued by Meillassoux, Godelier and Althusser, structural Marxists who have refined and indeed invented new ways of looking at 'pre-capitalist' modes of production (Bloch 1983, chapter 6). A richer understanding of the variability of human social structure is emerging as a result.

## **The Contribution of Marxist Theory**

It will become clear as this exposition proceeds how fundamental are Marxist notions to the present variety of research programs in archaeology. So for the moment I will restrict myself to the direct contribution of Marxism to archaeological theory.

On the application of Marxist principles to the archaeological situation and in the development of theories to account for change in past social systems there are already too many examples to allow anything approaching a comprehensive review in a paper of this size. Bender (1978) reminds us for instance, that social relations of production are as important in charting the transition from hunter-gatherer to farmer, in a paper that seeks to disturb the complacency of the eco-determinist workers of the 1960s and 70s. Bradley's (1984) excursion through the prehistory of Britain presents a coherent view of changing ideologies and their material manifestations, ultimately leading to a non-idealist, non-normative point of view that still manages to demonstrate the importance of social meaning in things. Self-interest and larceny are salient in Gilman's (1981) treatment of the emergence of Bronze Age culture in the Mediterranean. Blackmail and the "Might makes right" mentality were apparently responsible

for complexity in Gilman's view. Kristiansen's (1984) *tour de force* on ideology and material culture in the Neolithic and Bronze Age of Denmark illustrates how a regional view and erudition, combined with a Marxist perspective, can enrich and enliven the past. Critical theory, a valuable off-shoot of Marx's unity of 'theory and practice' thesis, will be dealt with in a later section, but it too has had a recent profound impact on archaeological theory.

Perhaps the most important spin-off of these Marxist approaches is the archaeological potential of the role of material culture (the traditional focus of archaeology) in ideological reproduction and legitimation, a point I raised earlier. This will lead archaeologists more and more to examine the structures as well as the symbolic systems that are expressed in material culture, as I will develop in a moment, and will more and more involve historians in the archaeological enterprise. As Shennan writes,

The concept of ideology then links an approach to material culture [i.e. structural and symbolic] to the understanding of past societies and economies [i.e. Neo-Marxist] ... it actually does more ... to some extent it dissolves the distinction between the two since the ideology is actually part of social and economic reality, inasmuch as it presents aims and ideals and categories in terms of which people act [Shennan 1986:332].

Previously, archaeologists have dealt with certain categories of material culture, such as "ceremonial" or "prestige goods" as epiphenomena (Kus 1984:102). As Pearson notes, "...artifacts are a product of human action and are also used to carry out actions ... it follows that their meaning derives from their relationship with beliefs" (1984:161). The meaningfully constituted nature of material culture is also a theme that will be picked up by Hodder, as I explore in a short while.

Material culture is thus no longer to be viewed as a passive reflection of the beliefs or norms of a given group. Instead, because of the relationship between ideology and material culture, we can directly perceive human products that had an active role in structuring social relations in the past. The introduction of ideology and its role in legitimation and mystification "opens up ... the search for conditions that govern the cultural manifestations of material functions and hence determine the interpretation and explanation of a major part of the archaeological record, whose explanatory potential has hitherto been much neglected" (Meltzer 1981:77). Thus we enter a new domain. As Shennan puts it, archaeology becomes important, by giving us the ability to describe and explain the things that define and change social relations (1986:333).

In all this, the role of history, and the roles of individuals begin to assert themselves. With society viewed not as a monolith, but as composed of individuals each negotiating social roles (at times using material culture), the products that archaeologists study become more meaningful as reflections of the social actions of individuals. New Archaeology assigned no role to individuals and treated social actions as if it were a given that they conformed to overarching natural principles, selected for by some unknown evolutionary agent. At least now, the agent can be conceptualized. Individuals are generators of cultural variability through their ideologically informed actions, and individuals are also forces of selection (to borrow a term from evolutionary biology), in that they actively promote change. Change, therefore, is situated within, not external to culture, as many archaeologists have asserted in the past. As a corollary to all of this, although the tendency is to view cultures and societies as essentially stable, with periods of abrupt change marking transitions to 'higher' states, we should rather

extrapolate as Pearson (1984:61) does: "We should understand society as continually changing with the constant reproduction of practices creating an 'illusion' of stasis."

### **Structuralist Programs**

It is perhaps not surprising that there should be such antipathy between what Hodder (1985) has called the "postprocessual" archaeologies and the New and traditional archaeologies. Although they are not (necessarily) anti-empirical, these more recent research programs really put empiricist science behind them. Structuralism especially defies positivist practice in its search for unobservable causal mechanisms for human behavior, and no doubt has attracted the vituperation of New and Old archaeologists because (at least as I read it) investigations are based on a new ideational concept of culture. This view, much more complex and less normative than the traditional archaeologists held to, includes the claim that culture is meaningfully constituted, and reflexive — acting upon and being acted upon by individuals.

In an archaeological context ... where the richness and variability of the material record is too great to be explicable solely in terms of response to environmental constraints or stimuli, factors internal to the cultural system must be considered [Wylie 1982b:41].

Here, Wylie is referring to the discovery of empirically observable regularities in many aspects of human social production that can only be explained by reference to rules. While the real nature of the rules that govern behavior is as yet not well understood (and need not be a single source), it is possible to point to many examples of the presence of rule-governed behavior. The success of some structural analyses in elucidating the patterning resulting from unobservable structuring mechanisms gives support to Wylie's (and others) confidence in the empirical veracity of the research program. A sampling of the literature yields the following examples.

Perhaps the most successful structural analyses have been undertaken within the context of historic archaeology. Deetz (1977) gives a good account of how colonial America's worldview changed through time, through a comparison of structural changes in ceramics, vernacular architecture, gravestones, grave pits, refuse, cuts of meat, furniture and cutlery. Between 1760 and 1800 the American mental landscape was transformed from a communal state where nature is included to a personal and private state that was separate from nature. Deetz's is a convincing view, only because it marshalls evidence from so many ostensibly unconnected cultural manifestations. This had to have occurred, says Deetz, "at a deep level of the Anglo-American mind," for it to have had so profound an effect on material culture (Deetz 1977:127).

Even though there is no corroborating written text against which to understand generative structures in prehistory, material evidence exists that can potentially illuminate the cognitive structures of archaeological cultures. Only the method is lacking with which to 'decode' the meaning present in the material. Of course, meaning for the structuralist equates with the deep structures that generate observed patterns, and indeed meaning is the structure, as previously mentioned. The search in prehistory, then, is for patterns that could not have come about randomly, and which represent the transformation of structure into material. Probably the earliest and most rigorous attempt at deriving a prehistoric structure was the work of Leroi-Gourhan (1967, 1986). His long study of the painted images from Upper Paleolithic caves of Europe is not without its problems, nevertheless he was able to recognize clear associations of depictions of certain animals with each other and their location within caves.

Furthermore, he describes a 'mythogram' (1986) which defines a deep cognitive structure relating women, men, and certain animals. This 'mythogram' consists of bipolar oppositions such as male:female, bison:horse, (1967:150). Leroi-Gourhan, before his death, had backed off from his conclusions about the meaning of the associations he recognized in the images, preferring instead to stress the relations (Conkey 1989). Nevertheless, his is tantalizing evidence that some as yet little understood cognitive mechanism permits humans to make meaning.

Conkey (1980) was able to identify a structured set of design elements on incised bone from the early Magdalenian of Cantabrian Spain. Their presence together at the site of Altamira was used as evidence of its possible use as an aggregation site. Criticisms of her study raise the question of contemporaneity, suggesting that a lack of temporal control invalidates her conclusions. I would say that it is an empirical question whether or not such a conflation is warranted. Clarke (1973b:808), using information from excavations at Glastonbury many years previously, reconstructs a "building model" that he hoped would converge with a "conceptual planning model of the individual builders, suggesting something of the cultural and functional priorities and rules of construction they may have tacitly followed." He was successful in describing "at least two different categories of linked structures — pairs of interlinked substantial houses and smaller huts linked to subrectangular structures" (Clarke 1973b:811-812). These paired structures and internal divisions were tentatively identified with the male:female opposition. Although studies such as Clarke's do not set out to achieve an understanding of the reason for such sets of rules, they do demonstrate that with further methodological refinement of the sort now championed by Hodder, the retrieval of meaning will be increasingly made possible in future investigations of the archaeological record.

By way of summarizing, the structuralist program can be seen as a "theoretical shift in how archaeological data — especially human material culture — are conceptualized" (Conkey 1989:150). It opens the door to a "reconceptualization ... [of] ... processes whereby humans construct meaning from experience" (Conkey 1989:151). This is undoubtedly true. There are problems, both empirical and epistemological, with the structuralist program, and I deal with a sample of them now.

## Problems with Structuralism

Structuralism has been likened to "killing a person to examine more conveniently the circulation of the blood," and has been described as "hair-raisingly unhistorical" (Eagleton 1983:109). These criticisms come from within the structuralist school, and will be seen to arise from the 'post-structuralist' concern with how meaning is constructed, and from the Marxist concern with historical materialism. All social formations have a history, and it is this history which has importance to individuals, not necessarily the structure that gave rise to the social formation. To use an analogy, it is not that we speak, but what is spoken that is important. Thus, a structuralism that does not consider the meaning manifest in the observable transformation of a structure is both anti-humanist and (probably) only serves to reproduce an ideology, not to critique it. From a processual viewpoint, too, it is desirable to explain change, not just to document it, and not to explain it away by universals that are so sweeping as to have no explanatory power. In the search for deep structures there is much that can be learned, but there is much more to be learned from the understanding of surface meaning, and of the reasons for change.

Wylie (1982b) has discussed the linguistic analogy and the scientific status of the structuralist research program in archaeology. She concludes that structural analysis contains very fruitful insights and should not be excluded from archaeological inquiry. The linguistic analogy is one way of making sense of what is clearly a system of symbolic meaning, and that it is an empirical question to what degree the analogy holds and what it means for the study of archaeology. The success of Chomsky's formulation of a deep structure in language, most likely having a genetic basis, has spurred the structuralist program in anthropology and latterly, in archaeology. As a means of (tentatively) modelling cultural variability the linguistic analogy has proved useful. Wylie (1982b:40) observes that "however much [cultural constructs] express and reinforce a world view [and thus resemble a language], they cannot be said to have distinctive, unambiguous 'meaning effects' in the same sense that a linguistic expression of a world view would have." She continues,

non-linguistic constructs ... may not be intended to produce 'meaning effects' of the specificity of the messages conveyed by linguistic constructs ... The archaeological structuralist ... cannot expect that models of specifically linguistic articulatory mechanisms will apply directly to their field [Wylie 1982b:40].

The fact remains, however, that there is a real sense in which the meaningful cultural constructs represent "a definable tradition whose distinctive structures of articulation ... embody a set of intuitions about what constitutes a well-formed construct comparable to the intuitions identified in linguistics as governing competence or body of structuring principles" (Wylie 1982b:40). There seems to be no reason then, to reject the linguistic analogy, but there is good reason to be aware that, for the moment (and until there is better evidence for it), there is no strict correspondence between language and material culture.

Critics have observed that quite often the elucidation of structures in cultures involves complete immersion over long periods in the material within which structure is sought. This has led to doubt about what might be called replicability in the inquiry. As Eagleton (1983:112) observes, "Structuralism appeared to constitute a scientific [elite], equipped with an esoteric knowledge far removed from the 'ordinary' reader." Indeed, when binary oppositions are inferred from observations made of the material under study (again along the linguistic analogy), often no criteria are offered as to how the determination of polarities is made. They are either presented as self-evident (a dangerous position to take) or "somehow explanatory of the world that the users of the said text live in" (Gellner 1982:119), but again no criteria are given as to "how one is to identify the crucial polarities [other than genius]." The tenuousness of the appeal to binary opposites may result from the ease with which "any finite set of distinctions can be presented as a series of nested binary ones" (Gellner 1982:119). This is not a problem with structuralism *per se*, any more than strict loyalty to a linguistic correspondence is necessarily linked with the structuralist program, but because oppositions are discovered and appealed to so often as descriptions of generating structure, wariness is here prescribed.

### Contribution to Archaeological Theory

Implicit in this discussion of structuralism has been the new way in which culture is conceived, as a meaningful, rather than an environmentally determined complex of human interactions. Material culture is no longer seen as merely reflective of human social relations, but rather constitutive of them as well. The insight that there are systems of relationships,

with observable consequences that are open to empirical enquiry, finally ushers in a real investigation of the causes of variability in the archaeological record. Whereas the empiricist-based traditional and New Archaeology espoused the goal of explanations that appealed to causal mechanisms, they could never untie themselves from the (perceived) need to stick to what was observable. This proved untenable. In a very real sense, structural analysis represents an intuitive appeal to a 'realist' philosophical account of science, by seeking the unobservable structures generating human cultural behavior. As a result, structural analysis leaves open the possibility for a truly explanatory archaeology, in a way that the New Archaeology never could. All this optimism, however, must be tempered.

### Post-Structural Programs

The logical next step after one begins to think in terms of a linguistic analogy is to consider the process of making meaning and of constructing it and being constructed by it. This was the 'post-structuralist' step taken by literary interpretative theory which has been appropriated by the architects of current archaeology, like Hodder (1982a, 1982b, 1984, 1985, 1986, 1988, 1989a, 1989b, 1989c). The break with traditional structuralism came when it was realized that signs, like words, did not just represent some thing; instead, they only represented that thing in terms of the history of the sign, all the other signs to which it might have reference, and the particular negotiation of that sign by a particular individual. Thus the wraith of radical contextualism had arrived in the guise of post-structuralist hermeneutics (interpretative theory). Looking at the archaeological record as a text, rather than as a fossil record, has far-reaching implications for the inquiry (Patrik 1985), involving a number of analogies that may take the practice beyond its current limitations. Look at how sophisticated are the concepts with which we are dealing, and consider the implications for archaeology and archaeologists, who have now to make sense out of a meaningful domain, with no appeal to informants, no surety of its commensurability with their own experience-near concepts, and no, as yet, developed methodology for dealing with any of it. A model of culture is being developed that can deal once and for all in a realistic way with its complexity, on a textual analogy, but we know neither the plot, nor the syntax, nor the vocabulary for the text. What and how can archaeology do with this?

Hodder (1982b, 1984, 1985, 1986) has presented archaeologists with a new set of aims to elaborate those espoused by New Archaeology. Most important for the "postprocessual" programs is the realization that individuals are active in structuring their social environment, not passive components of an ecologically determined system as was maintained by New Archaeologists. Individuals negotiate social roles, actively create and transform social structures. There is "no human nature independent of culture." There is no division between fact and theory. And there is an inescapable historical dimension to social change (Hodder 1985:2). Clearly, these derive from Marxist, structural, and hermeneutic insights. There is a reflexive relationship between individual and culture, and an active role for material culture. Meaning in prehistory is only derivable through a rigorous examination of context, because meaning is ultimately context-dependent (although as will be seen not radically so).

Shanks and Tilley summarize this approach to material culture, melding structuralist and post-structuralist, Marxist and other approaches, in this manner: material culture should be thought of as

- 1) being subject to multiple transformations in form and meaning content;

- 2) that its meaning must be regarded as contingent and contextually (i.e. historically and socially) dependent;
- 3) that it does not necessarily reflect social reality. There may be various relations of ideological inversion. Material culture is charged with power relations;
- 4) that it forms a framing and communicative medium in, for, and of social practice;
- 5) that like a text it requires interpretation but that such an interpretative process can never end: there are no final answers;
- 6) that it forms a channel of reified and objectified 'expression', both being structured and structuring in a manner analogous to a language;
- 7) that it is a social, not an individual production. The individual agent should be regarded as being structured through language and material culture;
- 8) that the meaning of the archaeological record is irreducible to the elements that go to make it up;
- 9) that the primary importance of material culture is not so much its practical functions (to say that a chair is for sitting on tells us virtually nothing about it) but its symbolic exchange values as part of the social constructions of reality; and
- 10) that it is polysemous: the meaning of an artefact alters according to (i) who uses it; (ii) where it is used, its social and material location; (iii) where and in what circumstances the interpretation takes place; (iv) who does the interpretation; (v) why they are bothering to interpret in the first place and in relation to what expected audience [Shanks and Tilley 1989:3-4].

If the fruitfulness of a research program can be measured in terms of the number of interesting studies undertaken in its infancy (as might be argued for the New Archaeology), then contextual, hermeneutical programs show promise indeed. Recently Hodder has edited a number of volumes of papers exploring the dimensions of the new domain of inquiry. In *Archaeology as Long-Term history* (1987a), *The Archaeology of Contextual Meanings* (1987b) and *The Meaning of Things* (1989c) (but especially in the latter) we see that the influence of post-structuralist interpretative theory is already vibrant.

### Archaeological Applications

But how put into practice a contextual research program? As Hodder pursues it, the key is the elucidation of similarities and differences in the contexts of objects, design motifs, or structures (1986, 1988, 1989a). As his program evolves we see Hodder becoming more sophisticated in his application of 'post-structuralist' reasoning. In "This is Not an Article About Material Culture as Text," he illustrates why we might think of certain changes in material culture as representing meaning transformed from one social context to another (e.g. of metals used at one time in dominance display, and at another time in utilitarian contexts [1988]). This, says Hodder, suggests that, as with meaning in language, meaning in material culture depends on the history of meaning. Each use of a sign refers to its historical use, its intended use in the present context, and ultimately prefigures its future use. There will be difficulties perceived in Hodder's new program. His optimism for success, no less than Binford's for the New Archaeology program, must be tempered somewhat and the epistemological limits examined as Wylie (1989b) suggests. There is an empirical realm and

there are constraints operating on those who would seek to derive meaning from archaeological contexts. The suggestion that material culture can only be understood in terms of textual meanings is open to empirical verification. I would want to remind Hodder that artifacts can have utilitarian functions that may be important perhaps before social meaning is applied (at least, this too is an empirically verifiable assertion). I can see, however, that much of the explanation of cultural variability and cultural change in the past must be built up from the meaning of classes of artifacts and their relationships in space and time.

Archaeological inferences, whether based on a framework of cultural ecology or post-modern textual analogies, are only as good as the premises on which they are based, and from which their arguments proceed. A case in point is Botscharow's "Sites as Texts: an Exploration of Mousterian Traces" (1989). Based on the assumption that flowers buried in the vicinity of the Shanidar IV Neandertal were deliberately placed there during a burial ritual, she concludes that this denotes social differentiation. I might be accused of special interest here, but as I have pointed out (Gargett 1989), there may be no justification for the assumption that flowers were placed with the corpse. The problem for contextual archaeology, no less than for other research programs, is that it will depend on received knowledge built up by other researchers. And inferences can be mistaken. This does not radically affect the value of Hodder's and other's contextual program. It just makes it that much more important to take account of the kinds of insights produced by the more mundane investigations of site formation processes and the bridging arguments championed by Binford, Schiffer and others. In assessing competing hypotheses as part of the process of building relatively accurate knowledge (in a realist sense), all the available data need to be constantly evaluated for their usefulness.

The introduction of post-structuralist interpretative theory to the study of archaeology represents a significant addition to the culture concept, and as such can only enrich the discipline. Still, there are those for whom these novel insights do not represent a significant break with past archaeological practice; archaeology will only (they say) be a valuable social practice when it becomes self-critical, and even revolutionary; when it becomes ideological critique, not simply ideological reproduction. I now turn to these criticisms of archaeological practice.

## Socio-Political Critiques

Let's be clear: there is no discrimination against women in archaeology. They are to be found in classrooms, in summer field schools, and as wives of archaeologists. (The phrase male archaeologist is redundant.) Successful female archaeologists (*read* married to an archaeologist) are employed in small colleges, preferably female ones; in historical, classical, and even in archaeological museums and laboratories. Frequently they do ethnohistorical research. Usually, they help their husbands in the field.

Few females without predilections for marrying archaeologists are attracted to field work crews. Mixed crews will continue to pose problems for supervisors concerned with decorum until the status of Female Archaeologist is redefined, although most geared-for-success female archaeology students need little supervision in the field. They display domestic traits such as washing, sorting, reconstructing, and cataloguing artifacts. To call these girls "camp followers" as many informants do is unjust. They are fulfilling important

ecological niches in the profession and, without them on summer digs and in classrooms, it is doubtful that many archaeologists would get married — or remarried. They would have to change their current wife-securing procedures. It is important here to recognize the functional-structural significance of the summer feel cruise [Sellers 1973:146].

Though tongue-in-cheek, this excerpt from Sellers's guardedly critical 'ethnography' of archaeology in the early 1970s serves as a paradigm for a genre of self-reflexive archaeological literature that has as its purpose the drawing-together of empirical data to support a claim that economics, politics and society subliminally influence the practice at all levels and in all ways. This is an extension of the contextualist critique of science mentioned above, which was one nail in the coffin of empiricism. When directed at a specific science, like archaeology, a picture emerges of a discipline that, in naive unreflexive style, has been shaped by forces that many would have thought (on an empiricist account once again) could not have had such an effect. As one might expect, the critique originates in the disenfranchized segments of the community, and there is no more peripheralized group in archaeology, especially in British/North American practice, than women. Gero (1983, 1985) demonstrates that Sellers's exposition has more than a grain of truth in it. Men do significantly more of the fieldwork, and are more successful in getting grant money, than women. Men, she says, take their data 'raw', while women 'cook' their data. Women spend more of their time in museum study than men. There is more than coincidence involved in this close parallel between the traditional role of women in archaeology and in North American society at large. And, indeed, it seems as if there is more than the usual bias against women at work in archaeology. Conkey and Spector (1984) are more sweeping in their analysis than Gero. The litany of gender bias that they expose is, for those of us who happen to be male, embarrassing to say the least. I would add shameful. What is missing, the authors contend, is an explicit theory of human social life, which would include a theory of how gender is constructed, maintained, and acquired. This lack of a very sophisticated social theory in archaeology will be brought up again in the next section. For my part, a gendered archaeology would be a more realistic archaeology, and the removal of a focus on 'man [sic] the hunter' to the exclusion of the other half of humanity would restore a little integrity to a discipline shamelessly rife with misogyny.

Of course, women are not the only members of the society to be injured by a non-reflexive inquiry. Most archaeologists are white males. It's as simple as that. Non-whites have traditionally been ignored or misrepresented in American 'historical' reconstructions (see for example, Trigger 1980, 1989), and worldwide, from time to time, prehistoric reconstructions have served to reinforce the dominant racist ideology (e.g. Hall 1984). Even European groups such as the Shakers have been painted with a presentist brush, in the naive assumption that what occurs today must necessarily have obtained in the past (this is an essentially Marxist insight, and one that lends support to the thesis that theory of any kind is radically ideological; for interesting case studies, see Leone 1981; Leone, Potter and Shackel 1987; Meltzer 1981 [for a modern example], and Handsman 1980, 1981). As if that weren't enough, Wobst and Keene (1983) point out an interesting artifact of "origins research" (of human origins and of the origins of agriculture) and of regional typologies. By their very nature, they can be 'owned' by practitioners. This sets up quite a competition in the search for origins, and creates lucrative power positions for the owners of the first anything. Those who 'own' the biggest sites in a region own the typologies, and the rights of access, because their sites can always be expected to contain the greatest diversity. I don't know if Wobst and Keene's rather

cynical (though unfortunately true) account needs necessarily to be the case. A change of focus could always undermine the framework (although I suspect the authors would argue that someone would always work out a way to turn the situation to their advantage, and to acquire and wield power as a result).

All this need not be the cause of a persistent pessimism about the ability of the discipline to transcend these situations. Archaeologists until now have been noticeably unreflexive, even while their siblings in anthropology have become almost cripplingly reflexive. That the socio-political critique exists at all is a healthy sign, and if archaeologists could get over the fear that they represent evidence that our production of knowledge is radically relative, we might get beyond. Archaeology may be "fundamentally conditioned by the larger society that supports it" (Gero 1985:342), and may be susceptible to the naturalization of the present by imposing it on the past (Leone 1981:309), but as Wylie (1985a) argues, by including in our source-side models for the past the reflexivity found previously lacking, we should be able to avoid some of the pitfalls pinpointed by the socio-political critique.

## The Radical Critique

Total objectivity is not to be expected in human judgement, and the best we can do is recognize and account for those subjective biases that we carry with us [Deetz 1977:160].

A final break with empiricism, which was not forthcoming in the New Archaeology, comes to archaeology through Marxist theory, structuralism and post-structuralism. The full-blown, many-faceted explorations of archaeological practice represented by Shanks and Tilley's *Re-Constructing Archaeology* (1987a) and *Social Theory and Archaeology* (1987b) were presaged by other works by the pair (notably 1982), and in the work of anthropologists and sociologists from the continent. Critical theory, the application of Marxist insights to practice in academe, had been an active part of intellectual life on the continent since the 1920s. Shanks and Tilley, however, have brought the full force of Critical theory to bear on archaeology as it has been practiced by Westerners, at least, since before the break with tradition represented in the New Archaeology. It is impossible either to synopsise their wide-ranging criticisms, or to even begin to be critical of them in this presentation. But perhaps I can devote some time to their motivation and to what they see as a worthwhile archaeology.

Shanks and Tilley (1989) encapsulating their two monographs in "Archaeology into the 1990s" wanted to criticize, first of all, naturalism, scientism, phenomenism and empiricism. They wanted to counterpose naturalism (i.e. making social life a second nature) with meaning in culture and individual agency. They wanted to stress the creation of 'facts', to characterize theory as practice (and archaeology as theoretical practice). And, contra positivism, they introduce Critical theory, and interpretative theory (hermeneutics). They wanted to examine 'meta-theoretical' issues such as value-freedom and the politics of theory. Finally, they were engaged in pointing out "the object of archaeology — the relationship between contextually situated social practice and material culture" (Shanks and Tilley 1989:3). This study of the past would require "self-reflexive discourse," and this would be "a process in which [signs from the past] ... are written into the present" (Shanks and Tilley 1989:4). This would be an empirically based enterprise "involving both resistance (the material record does constrain what we can write in various ways) and transformation (the movement from things to words) ... [by moving] from a material culture 'text' to an archaeological text backing up our arguments and

statements by 'quoting' with artefacts" (Shanks and Tilley 1989:4). Shanks and Tilley (1989:7) advocate "the full realization of archaeology as a strategic intervention in the present through a focus on (i) archaeology itself as a micropolitical field; (ii) an adequate theorization of the relation between material culture and social structures both within contemporary society and in the past; (iii) using the *difference* of the past to challenge established economic and social strategies, categorizations, epistemologies, rationalities, modes of living, and relating to others." This is calculated to move archaeology from the category of ideological reproduction and into the realm of *praxis*.

Reviews of Shanks and Tilley range from acceptance through qualified approval to unmitigated disapproval. But from reactions to their work will come, I think, a better understanding of what they intended in their highly stylized and polemic brace of monographs. Already, having been asked to summarize their work in the *Norwegian Archaeological Review*, Shanks and Tilley have toned down their provocative language, and in more succinct style presented the outlines of their case (1989). Reactions from Kristiansen (1988), and the commentators in the *Norwegian Archaeological Review* raise many important points that will generate further clarification of their program. Traditionalists will have their say, though I daresay, not with anything like the reasoned, seasoned clarity or balance of someone who is already steeped in the traditions out of which Shanks and Tilley emerged. As an example, Earle and Preucel (1987) took the Radical Critique to task and argued that (as seems in some sense to be the case) the Radical Critique of archaeology was a critique of the New Archaeology. The authors preferred, in the end, to name a new archaeology as "behavioral" but, unlike the behavioral archaeology of Schiffer (1976), theirs would include structuralism, decision-making, and would somehow better account for material culture patterning. It does not, however, include a discussion of post-structuralism which, I am afraid, renders almost any discussion of aims and goals, epistemologies and ontologies that lack such consideration, passé.

This has been an, admittedly, curt treatment of the Radical Critique of archaeology. Thus far it has been conducted in the main by two committed and vocal archaeologists who see as their mission the re-construction of archaeology. I would rather give them space to do so, since they have marshalled so much evidence that archaeology is in need of a refit. Their imprint on the field has only just been felt. And no doubt further reflection and study, combined with attempts to make their program applicable to the real material of the past (as represented in the present), will render archaeology a more interesting, and in the words of Shennan (1986), more important, endeavor than heretofore.

## Real Archaeology

In the preceding I have attempted to present the flavor of current archaeological research programs, by way of illuminating their essentially scientific nature. None appeal to unempirical domains, and in seeking the unobservable processes structuring the observable they are not eschewing an empirically based science, only one that adheres to a narrow empiricist account of knowledge and its production. Nothing about these programs is inherently unscientific, and although scientism has justifiably been criticized (as for example in Harding [1986]), science has not, and I think should not be seen as pernicious. Our ability to pierce the mystifications of ideology by reference to empirical observation (in the Marxist and socio-political critique) is one proof, for me, of the value of intersubjectivity and verification of knowledge claims, the foundation of scientific reasoning. In what follows, I

hope to present what archaeologists, as scientists, really do when they attempt to produce knowledge of the past. I will assume that a realist account of science is the best that we have at the moment, and I will address the twin issues of objectivity and verification that have so plagued the discipline of late. Much of the polemical debate about the value of various research programs has centered on the ability of practitioners to achieve a level of objectivity in what they do. The shading of these debates suggests that there is some worth in the ambition of objectivity, but what it is and how it is to be achieved are not straightforward, to say the least.

In "Data, Relativism, and Archaeological Science" (1987) Binford is at his most arrogant and his most philosophically naive. His argument, drawn out, is that empirical claims for cultural relativism, which inform the textual/contextual research program, render these programs radically unscientific. As he sees it, 'empiricism' is at fault, because it privileges experience, and experience is hopelessly grounded in an individual's cultural experience. If anything, however, it is Binford's own empiricism that disallows him from sorting out what is an empirical question from an epistemological one. As Wylie (1989a) notes, whether, and to what extent human groups can be seen as manifesting universal processes (as opposed to radically idiosyncratic ones) is an empirical problem, not something to be asserted or assumed. The epistemological question that Binford seeks, rightly, to answer, but fails to come to realize, is one of obtaining his hoped-for 'objectivity'. On his narrow account of 'objectivity' (basically an agreed-upon observation language [1987:392]), he will always be frustrated, since on his account humans are doomed to experience-near understanding, and are unable therefore, to understand alien cultures or the past in anything but the experience-near understanding of the "other's" culture or the past. This is clearly not the case, or there could be no shared understanding between people or between cultures, and there would be no opportunity for the success that science has had in understanding human behavior or physical phenomena. We are not mired in our own experience, as Binford asserts. In recognizing that experience shades our understanding, we do not preclude science or scientific knowledge (just those things that Binford cherishes). Indeed, the recognition that culture is meaningfully constituted, and that knowledge is to a great extent contextually based imposes constraints on scientific knowledge that simply cannot be argued away.

Binford's argument thus fails at a very basic level, because he misunderstands the problem. It is not the problem of science versus non-science (or 'empiricism' versus Binford's 'objectivism') that plagues him — that through a recognition of the culturally constructed nature of meaning all knowledge is rendered radically context-dependent and therefore unscientific, and the textual or contextual program anarchic. Rather, it is Binford's mistaking the empirical question for an epistemological one that haunts him; it is a phantom of his philosophical naïveté, coupled with his zeal for secure knowledge that makes him see problems where really none exist. If not radically relativist, nor ideally objective, what, then, is a science like archaeology? How does it manage to produce, tentatively, a view of the past that seems to approximate what actually took place? I follow Wylie's (1989a) analysis of archaeological practice. She describes a process of reasoning that is every bit as complicated as Shanks and Tilley's "four-fold hermeneutic" (1987a:107), but she manages to make it stand as a model of practice that actually demonstrates why archaeology, and social science in general, can escape accusations of radical relativism which would call into question any and all accounts of the past.

Wylie's analogy of cables and tacking captures some of the complexity of reasoning involved when archaeological phenomena are made sensible. It is a far cry from the formal logic of the positivists, or from the characterization of hypothesis testing as a linear process of instantiation of a theoretical construct. For Bernstein scientific arguments are "more like cables than chains," in which

researchers ... do not (indeed, cannot) proceed by 'a linear movement from premises to conclusions or from individual "facts" to generalizations'; they must exploit 'multiple strands and diverse types of evidence, data, hunches, and arguments to [assess and, ultimately, to] support a scientific hypotheses [sic] or theory' (Bernstein 1983:69). ... even where there is no single commensurating ground for judgement, 'the cumulative weight of [disparate, multidimensional considerations of] evidence, data, reasons, and arguments can be rationally decisive' (1983:74). Extreme relativism in which there are no grounds for choice between alternatives — in which 'anything goes', taken on its own terms — does not follow from the fact that no one set of considerations is fundamental across the board, no one strand of argument conclusive [Wylie 1989a:7].

Where the possibility of incommensurability arises, as it can in coming to understand an alien culture (living, or archaeological), Wylie, after Bernstein again (and remembering Geertz), shows "how anthropologists actually do (or can) avoid the pitfalls of taking either their own framework or that of their subjects as foundational" (Wylie 1989a:8). Geertz advises that "anthropologists must grasp the system of 'experience-near' concepts in terms of which members of a culture ordinarily understand and represent their own actions, beliefs, and concepts, concepts that are not necessarily familiar to the people being studied but that ... make intelligible the symbolic forms [of their culture]' (Bernstein 1983:95)" (Wylie 1989a:8).

Understanding of one's own and the other's culture is to be accomplished through a 'process of dialectical tacking back and forth'.

On Geertz's account, tacking operates between 'our' distant — theoretical, abstract — concepts and 'their' [meaning the 'other'] concrete, experience-embedded concepts. ... a movement between ' "parts" and the "whole" ' (Bernstein 1983:133), and requires a 'dialectical interplay between our own preunderstandings and the forms of life that we are seeking to understand' (1983:173). Taken together, cross-context understanding is conceived as, in effect, a diagonal tack, where the dimensions traversed are, on one hand, abstract:concrete and, on the other, familiar: alien [Wylie 1989a:8].

The complexity does not end there, however. Wylie adds "some form of 'dialectical tacking' ... on a vertical axis within our own context" to produce "our own experience-distant concepts" (Wylie 1989a:8). These make our own experience intelligible, and to be understood, we need to know more about our "experience-near concepts and practices, the 'preunderstandings', that constitute [our] context" [Wylie 1989a:9].

These are vitally important analogs of practice for archaeology, since we are dealing, in the main, with a subject that literally does not exist, one of which we cannot be sure if it is commensurate with our own context. As Wylie adds

Whatever its specific aims, archaeological interpretation depends on background knowledge of contemporary contexts; usually it proceeds by means of ethnographic analogy. It is therefore explicitly and heavily dependent on

vertical tack arguments within the source context (broadly construed) which produce both experience-distant concepts — generally theories about cultural development, differentiation, interaction, and adaptation — and experience-near models of specific past contexts. ... Archaeological inquiry also exploits a diagonal tack from our interpretive concepts to the observable, material consequences of past practices [Wylie 1989a:11].

In the remainder of the analysis, Wylie shows how archaeologists have managed to accomplish all of this through the use of examples drawn from the literature.

The foregoing is not intended as a blueprint of practice [by me or by Wylie]; it is, however, intended to illustrate just how complex scientific reasoning is, *contra* Binford (1987) and others, whose construals of science are usually unidimensional, involving the simple confrontation of hypotheses with data. In other works, to demonstrate the heretofore unacknowledged complexity of archaeological reasoning, Wylie has examined the use of analogy, its increasingly sophisticated use as a source of explanatory hypotheses, and its epistemic legitimacy on a realist account of science (Wylie 1982a, 1985b). These were undertaken to right the erroneous 'rejection' of analogical argument in archaeology, based on a (once more) misunderstanding of the real form of reasoning employed by science.

Testing, on Wylie's model, involves a Collingwood style 'question and answer' where "evidence is sought that is specifically relevant to the question whether it is likely that a particular past context instantiated the reconstructive models archaeologists bring to it" (1989a:14). This, she goes on to demonstrate, can be (has been) successful in limiting what can be claimed about the past context.

In all cases, however, interpretive conclusions depend on various lines of argument developed on vertical and horizontal tacks in both source and subject contexts. And, in this, their strength derives not just from the diversity of their support but, more specifically, from the fact that the constituent strands draw on different ranges of background knowledge in the interpretation of different dimensions of the archaeological record; they are compelling taken together because it is highly implausible that they could all incorporate compensatory errors [Wylie 1989a:15].

Thus, against a "nihilist relativism" Wylie places her "mitigated" objectivism.

The message to be taken home in all of this should be comforting to adherents of the current diverse archaeological research programs, New Archaeologists, Marxists and contextualists alike. First off, Wylie's argument concludes that

theoretical commitments [like eco-determinism and Marxism] do not monolithically control both the interpretation of archaeological data as evidence and the generation of reconstructive hypotheses which the data as evidence might be expected to test. In any given reconstructive-evaluative argument, it will be necessary to exploit a range of different, *independent* sources to accomplish these diverse tasks [tacking on four dimensions]. It is the independence of sources, and therefore of the constituent arguments about evidential significance, which ensure that the strands of the resulting cables are not just mutually reinforcing but are also, and crucially, mutually constraining [Wylie 1989a:16].

Secondly, although our experience teaches us to see what "our theoretical commitments prepare us to see," that need not render radically relative all that we know or can know of the archaeological past.

our presuppositions, theoretical or otherwise, are not all-pervasive. We frequently find out that we were wrong, that the data resist any interpretation that will make them consistent with our expectations, and that we are dealing with a subject (cultural or otherwise) which is very different from anything with which we are familiar. We are then forced *by the evidence* to consider completely different interpretive possibilities than we have entertained in the past, and even to rethink deep-seated orienting presuppositions about the nature of cultural phenomena [Wylie 1989a:16].

The empiricist yearning for "context-independent, 'objective' knowledge" is asserted to be unattainable. But as Wylie has shown, "it is also not the case that data are entirely plastic, that they are so theory-permeated that facts can be constituted at will in whatever form a contextually appealing theory requires." Instead,

researchers routinely exploit this capacity for resistance on a number of dimensions: in the processes of vertical tacking within our source context(s) by which the theories and background knowledge about determining structures are built up and tested, and in those of diagonal tacking which seek evidence that these structures were (or were not) instantiated in particular past contexts" [Wylie 1989a:16].

Wylie's argument would also seem to undermine the Marxist contention that all theory is "shaped by society and ... [influences] society in particular directions (the 'materialist' thesis)" (Saitta 1983:301). According to this, theoretical truths are relative truths, grounded in the dominant class of a society, and only serve to reinforce the dominant ideology. If this is so, there would (on a realist account) be no scientific knowledge, nor any knowledge of the real world that more or less accurately describes it. Our understanding of the world would be no more sophisticated than it was in the Middle Ages, when the sun was believed to orbit the earth, and the earth itself was flat. To argue, as Marx and others have, that theory is ideology, is to, once again, misrepresent the actual practice of scientific reasoning and the hope for a 'mitigated objectivity' in the production of knowledge (it also, ironically, undermines [as does any claim of a radical relativism] their own thesis). At least, if one accepts Wylie's account of anthropological and archaeological science, there is reason to doubt that knowledge is radically context-dependent, and that knowledge is purely produced in the interest of a dominant class.

## The Future

By presenting Wylie's philosophical analysis of the science of archaeology in such detail, I have highlighted the complexity of reasoning involved when we 'do archaeology'. Old notions of science as an essentially linear process of hypothesis, test, and reformulation of hypothesis were overly simple, and obscured what really happens when we go about constructing the past from the present phenomenon of the archaeological record. As I stated at the beginning, theoretical frameworks and research programs are ultimately context-dependent, and thus are subject to change. But what really constitutes scientific practice is perennial, and for the moment at least, Wylie has provided us with a much more realistic view of what comprises the scientific endeavor in archaeology than that to which we have previously had access.

So for the future, I would expect that archaeologists will continue to struggle with their arguments of relevancy (middle range research) and with the difficulty of multi-dimensional tacking to acquire knowledge of past cultural contexts that are, if nothing else, temporally distant and difficult to apprehend using the material remains and their spatial and temporal relationships (all we really have to work with). I am optimistic that the disagreements over what science is and how we do it will subside, as more and more archaeologists are brought to an understanding of scientific practice that at least matches Wylie's account in its complexity. And, as Trigger (1989) implies, emphasis on extreme relativist and extreme objectivist positions will wane, and the insights of Marxism will continue to yield fruit in studies of human history.

I said at the outset that the habit of calling each research program a different kind of archaeology is damaging. I hope I have brought out, in the discussions of 'postprocessual' research programs, the notion that *there are many aims and goals in archaeology, not many archaeologies*. This is not just semantic quibbling on my part. The practice of naming 'other' archaeologies is, at bottom, political or at least polemical. And in so naming each program as 'this' or 'that' archaeology, the practitioners are acting to distance themselves from one another; a kind of intellectual quarantine. This divisiveness is not only visible in the debate between the old-line empiricists like Binford and the contextualists like Hodder and Tilley, it can be seen in arguments between different kinds of materialists, and between different kinds of structuralists. This demonstrates, probably more than we would like to admit, a characteristic in most of us, that of taking a position and defending it. This aspect of the social context of archaeological science, more than any disagreement over what happens to be (or not to be) the 'proper study of humankind', more than any dispute about what is or is not scientific, threatens to turn the field into a cacophony of shrill voices, and will inevitably lead to a disaffection of the majority for the vocal minority. I have to hope that there are enough earnest people in archaeology today with the energy and the motivation to eschew intellectual posturing and examine the numerous research programs for what they are — worthwhile sources for explanatory models and analogies to enable theory-building, employing the complex reasoning alluded to by Wylie (1989a). And what is needed more than anything else, it seems to me, is a conciliatory attitude among the various proponents — a desire to really improve, rather than simply to tear down, what archaeology has thus far contributed to the understanding of the past and of ourselves. This will not be an easy task.

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