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E.D. Lewis

Remarks on the problem of inferring ideology and social structure from the artifacts of human action[1]

To Professor Rodney Needham

Since … only the events and not their inferred relationships are empirically demonstrable in any study dealing with man's past, the difference between the archeological, documentary, and ethnographic records is merely one of degree, not of kind, and the archeologists should not consider that the limitations of their finds impose interpretive strictures upon them any more than upon other students dealing with past actuality. Up to the limits of their evidence, theirs are the same privileges and the same obligations that impinge upon ethnologists and historians.

– Walter W. Taylor, *A Study of Archeology* (1967 [1948])

1. Introduction

In the academic year 1993-1994, Dr. Marek Zvelebil and I found ourselves together as Fellows in the Netherlands Institute for Advanced Study in the Humanities and Social Sciences, the only representatives of archaeology and social anthropology, our respective disciplines, in the NIAS fellowship that year. Perhaps for want of a more qualified person close to hand, I was invited to serve as a discussant for a two-day conference on *Ideology and Social Structure of Hunting, Gathering, and Farming Communities in Stone Age Europe*. With a remark of Prince Charles-Joseph de Ligne (1735-1814) in mind ('This is a subject on which I know absolutely nothing: I should write a book on it') and some misgivings, I accepted the invitation.

I am neither an archaeologist nor prehistorian but, as a social anthropologist, I have carried out long-term fieldwork on the island of Flores in eastern Indonesia where, among a people who call themselves the Ata Tana 'Ai, I have investigated social organization, religion, myth, language, and the human ecology of the Tana 'Ai valley. The topic of the NIAS conference, which encompassed ideology and social structure, was thus attractive to me and I was very curious to learn what my European colleagues in archaeology and prehistory had to say on these subjects. I was not disappointed. Not only were the papers presented in the conference very interesting in themselves, but as the conference unfolded it became clear that its participants shared a common set of problems, approached those problems with a common and coherent assemblage of methods, and were arguing about crucial and non-trivial matters bearing on their main concerns: Mesolithic hunting and gathering, Neolithic farmers, and early Bronze Age society, and how we might determine something fundamental about the people of those ancient societies in Europe who left behind material residues of their activities.

Given the particular and hard-won knowledge and expertise that archaeologists bring to their subject, one can reasonably ask what a social anthropologist might contribute to a discussion of life in the prehistoric societies of Europe. In replying to this question I wish to make it clear at the beginning that most of what I now know about this subject I learned in two days at NIAS. I took away from the NIAS conference the impression that its participants were vitally interested in the ways in which the social structure and ideology, i.e., the culture, of Mesolithic and Neolithic European peoples might be characterized on the basis of data supplied from the study of the material remains of their behavior and activities, and most especially, from the traces of their subsistence activities. These concerns are not too far removed from those of social anthropologists, who investigate behavior and both individual and group activities, albeit among people still alive and communities still extant. The many points at which these papers raise and address problems shared by archaeologists and anthropologists indicate the degree of affinity between the two fields.

The participants in the NIAS conference were assembled to consider three questions:

1. What do we know about the social organization and ideology of the hunter-gatherer and farming communities of stone age Europe?
2. How can we use archaeological evidence and our conceptual frameworks to know of the social domains of the Mesolithic and Neolithic societies?
3. What patterns of change in the social domain can be observed in time during the Mesolithic and Neolithic?

Perhaps the most intractable is the problem of inferring social structure and ideology from the material residues of behavior. This question is also the one of the three that a social anthropologist is best equipped to address, and it is thus to this problem that I will address the remarks in this essay.

In essence, the questions discussed by the NIAS conference reduce to this: with respect to developments in Europe in the late Mesolithic and Neolithic eras, how do we

read the anthropologist's 'culture' from the 'archaeologist's 'culture?' But just what 'culture' is for an anthropologist, that is to ask, what it is that social and cultural anthropologists study, has never been settled satisfactorily. It was a problem in 1952, when Kroeber and Kluckhohn published *Culture: A Critical Review of Concepts and Definitions*, and it remains a problem now. I shall in this essay (perhaps somewhat unsatisfactorily) first say what I think culture is *not* (it is not static, it is not a causal entity, and it cannot be reliably inferred from behavior or the material products of behavior) and from there point to difficulties and, more positively, to possibilities for archaeologists who work from artifacts toward culture as a concept which includes peoples' ideas and patterns of social structure.

2. The material remains of behavior and action

While not ignoring a century of debate about the subject in the social and behavioral sciences, I will here take as *behavior* anything I can observe a person doing. But, as a social anthropologist, I am generally more interested in *action* than in simple behavior. The difference between behavior and action is that action is *intentional behavior*, by which I mean that it has some goal, purpose, or rationale that makes it instrumental or meaningful. Sneezing is behavior, whereas clearing a patch of forest is action. If I ask an informant, 'Why did you sneeze?' he might well reply, 'I don't know.' But any informant will reply, often at length, about why he clears a patch of forest. As an anthropologist I attend most to the latter because I am interested in the patterns and regularities that human action produces, either as an intended goal or as an unintended consequence of action. Distinguishing intended action as a particular kind of behavior from behavior taken generally and further distinguishing the intended goals of action from the unintended consequences of intentional action lead to unsuspected complexities in anthropology.

In the Tana 'Ai region of eastern Flores, for example, people clear forest to make gardens, and they make gardens in which to plant crops, and they plant crops in order to get food. Thus, clearing a forest is an intentional activity. Although action is intentional, it nevertheless can lead to unintended consequences. For example, when looking across the Tana 'Ai valley from its western wall to the valley's eastern slopes, the mosaic of cleared garden land and regrowth forest is the single most obvious feature of the landscape of the region. Without reference of any kind to members of the community, one could map the gardens and forests and, in doing so, could discover an unintended order in the landscape. By unintended, I mean that, should you ask Tana 'Ai horticulturalists about it, they might not immediately recognize what it is you are talking about and, furthermore, might not (indeed, most likely would not) cite bringing about that order in the landscape as an intended consequence of clearing forest and planting gardens the way they do. With or without their intending it to be so, the order is nonetheless there. And it is a worthy object of understanding and explanation.

Before the arrival of Europeans, the valley of the Murray River, Australia's largest catchment and riverine system, was replete with forests of *Eucalyptus*. Eucalypts, or gum trees, are excellent water pumps. Among trees, they excel at drawing ground water through their roots and aspirating it into the atmosphere through their leaves. Mature *Eucalyptus* forests thus regulate ground water levels, as they once did in much of the Murray River basin. Over more than a century, Europeans took advantage of the rich soils of the valley for agriculture. This activity required felling and clearing the valley's forests to make fields and paddocks. With the trees gone, the water table in the valley began to rise. As it rose, the water carried with it dissolved salts. Today, that salt has come to the surface in many parts of the valley, thus making agriculture increasingly difficult. The salting of the Murray River valley is an unintended consequence of intentional human action.

One of the aims of archaeology is to explain artifacts and assemblages of artifacts in terms of the behavior of their makers. Thus, as analysis can reveal an orderliness in artifacts and assemblages, it is reasonable to assume that this order reflects an orderliness in the behavior of those who produced them and who were responsible for their deposition. But if the aims of archaeology include the reconstruction of the culture of the people who made and deposited an assemblage of artifacts, then we require a means for establishing more than a simple correlation of artifact and behavior. To reconstruct a past culture we need, among other things, to be able to infer the ideology and social structure of those who left behind a particular assemblage. And, in order to learn something of ideology (a more or less self-consistent set of ideas about the nature of the universe and appropriate human action in it) and society, we need a way of sorting the intended results of action from the unintended results of action which, by their nature, cannot reflect an ideology. It would, after all, be incorrect to try to explain why Europeans cut down the *Eucalyptus* forests in the Murray River Valley by saying that the ideas they had in mind were to make the valley agriculturally non-productive and that they achieved this goal by actions which caused the soils to salt up.

Both agriculture and the salting of the Murray soils were consequences of the intentional clearing of forests and both would be seen in the archaeological record of human activity in the Valley. But only one of these results (agriculture) would shed light on the ideology of the people who cleared the forests. The problem is this: how can we distinguish the

intended results of action from the unintended results? And, there is a further problem: once we eliminate the unintended results of action from consideration as a purpose of action, we may be only marginally closer to knowledge of the ideology of farmers in the Murray Valley since more than one ideology can accommodate agriculture as a mode of action.

Why is this question important? Let us assume, for the sake of argument, that there is some relationship between some ideologies and some actions (i.e., intentional action, not simple behavior). To the extent that this is the case, then it makes good sense for an archaeologist to trace the relationships between artifacts and action, and between action and ideology. The same can be said of an attempt to trace the relationships between artifacts, action, and social structure.

While artifacts can be taken to be signs of action or behavior, artifacts do not directly index any particular action or any particular form of action. For this reason, the methods employed in establishing a relationship between artifacts (or assemblages of artifacts) and the action that produced them require inference. In this undertaking, an archaeologist can include in the inferential process both artifacts that are the results of intentional action and those that are the unintended results of action. In proceeding from action to ideology, inference is once again required. But, once again, we cannot assume that any particular action is an index of any particular ideology. The reason for this is that, to recall the case of agriculture and the salting of soils in the Murray River Valley, agriculture involves intentional action whereas the salting of soils, while a result of intended action, was not the intended result of that action. Insofar as it is reasonable to assume that, whatever the relationship between ideology and action, ideologies do not encompass the unintended results of action, to infer ideology from action means we first need a means of discriminating intended from unintended artifacts. Thus, if we are to infer the ideology of the Murray River agriculturalists, we will need to keep in mind that the production of food crops was the result of an intended action whereas the salting of soils was an unintended artifact of felling forests for agriculture. The actions and activities of people engaged in agriculture are in accord with some ideologies whereas an action intended to produce soil degradation would be in accord with a different ideology (or ideologies). Furthermore, action that produced as an unintended consequence the salting of soils is consistent with an ideology associated with action that aimed at agriculture, but would not have been intentional action in the sense I am employing the term. Thus, artifacts that are the unintended results of action would not directly tell us much about ideology.

The consistency or compatibility of an idea or set of ideas (an ideology) with an action or actions is not the same thing as causality. Thus, an anthropologist can observe and record textually actions and he can reasonably identify intentional actions as those that his informants themselves explain in terms of a set of ideas (bearing in mind that it is safest to treat the verbal expression of an idea itself as an action). And should he come up with a hypothetical linkage between observed action and a recorded idea, he can refer his hypothesis to informants by way of a query. In other words, an anthropologist can observe the *action* that he tries to link to thought or ideology and he can connect the two in a single inferential step (see Lewis 1997).

In this respect, an archaeologist is at a disadvantage: the action that produces an artifact cannot be directly observed, but must be inferred from the form and (insofar as it can be known) the function of the artifact itself and from the artifact's contextual relationships to other artifacts. Thus, action intervenes between artifact and ideology and, for the archaeologist, a sequence of at least two inferential steps is required to get from artifact to ideology:

Artifact →	behavior:	inference *a*
	behavior → ideology:	inference *b*

Insofar as an inference can carry only a degree of certainty (or a probability of being correct), the sequence of inferences between artifact and ideology is subject to a compounded probability of correctness (or falsity). If the probability of an inference, *a*, from an artifact to the action of its maker ('I am 80% certain that this artifact was produced by technique *x*, or is an element of assemblage *y*, or was employed in process z') is 0.8 and the probability of the subsequent inference, *b*, from the action of its maker to its maker's ideology is 0.6, then the inferential sequence from artifact to action and from action to ideology yields a probability (or certainty) of less than 0.5, which is not very satisfactory.

The archaeologist must frequently imagine the sorts of beliefs, desires, goals, intentions, ideas, and knowledge people might have had that would have led them to act in ways that would have brought about the artifacts observable in an archaeological record. Frequently a number of ideas and goals that led to actions that might account for a single result are possible, but some will be more reasonable than others when direct, indirect, or collateral evidence – or plain common sense – is taken into account. Thus, in studying the Europeans' exploitation of the Murray River Valley, some future archaeologist could discount the following as likely ideas or intentions that led to actions that brought about the salting of soils:

1. the Europeans intended to bring salt to the surface and felled the *Eucalyptus* forests to bring about the degradation of the valley's soils;
2. the eucalypts were abducted by aliens;
3. the Europeans cut down or ring-barked as many trees as possible in the shortest possible time because they hated trees.

Based on comparisons with other contemporary and prehistoric cases of agriculture leading to radical and deleterious alterations to an environment, one or more of the following motivations and ideologies are more likely to be true:

1. the Europeans attempted to recreate an agriculture already familiar to them from their European experience and did not know (or take into account) the peculiarities of the Australian environment;
2. the Europeans viewed the Australian environment as an expendable resource which, coupled with an enthusiasm for capitalistic ideology, inclined them to ignore the salting of the valley in favor of a quick return on a minimal investment of labor;
3. the clearing of the valley was an expression of a European ideology that positively valued man's domination over nature.

In weighing alternative inferences, archaeologists are free to take evidence from anywhere they can find it and to take inspiration from the findings of other archaeological studies of similar assemblages of artifacts and similar situations, from ethnographic reports of contemporary societies, from historical records, and the like.

A concrete example of the differences between archaeology and anthropology in this regard may help clarify my point. In 1980 I observed and recorded in detail (including registration on 16 mm sound-synchronous film) the *gren mahé* rites in Tana Wai Brama.[2] During these rites, the Ata Tana 'Ai slaughtered more than 300 goats and pigs as sacrifices to their deity and ancestral spirits. After decapitation with a ceremonial sword, the severed heads of the goats were then rubbed on the *mahé* altar, which is a branching tree trunk surrounded by monoliths set in a clearing in a small patch of primary forest. The animals' owners then dragged the carcasses off into the forest surrounding the *mahé* altar and butchered them. Except for the jaws of the pigs, the bones of these sacrificial animals were left at the butchering sites. Finding these bones, archaeologists of the future might well associate them with the monoliths, the nearest artifactual feature of the landscape and, if they are lucky, they might also find the remains of the branching altar. They might then infer that the sacrifice of pigs and goats was part of a ritual complex among the ancient people of Tana 'Ai. But could anything in the assemblage of material remains of these rites serve as evidence for the ideational content of the religious beliefs that animated the rites and lent rationality to them? With respect to social structure, would anything in the remains of the sacrifices point to the complex interrelationships between the *mahé* rites and the precedence of the domain's clans, itself encoded in mythic histories of the domain whose narrations were also an essential element in the *gren mahé*? And would the archaeologists guess that the missing pigs' jaws were significant? If so, would there be anything in the archaeological record that would lead them to speculate about the role of the jaws of pigs (and not of goats) in exchanges by which affinal relationships of people in the community were reaffirmed?

As an anthropologist, I was in the relatively fortunate position of witnessing and recording the actions that led to the deposition of what, far in the future, would be discernible as a patterned assemblage of artifacts. An archaeologist of the future would not be in this position and would thus be one, longer step removed from being able to reconstruct the 'ideology' of the people responsible for the depositions.[3]

Archaeologists study artifacts (including buildings) and landscapes that have been altered from their natural state by human behavior. While it might be thought that a social anthropologist has more direct access to the ideology of the people he studies than does an archaeologist, this is an oversimplification with which I would take issue. On reflection, it can be seen that anthropologists also study artifacts of behavior. A social anthropologist in the field can witness behavior, including the actions of individuals and groups of individuals. These actions form part of the data for ethnography. In addition, the anthropologist can interrogate members of a community under study about their intentions and motivations for acting as they do, and can even discuss with them ideas they hold about their actions and their intentions while they were acting. But these intentions and motivations and the states of actors' minds while they acted in certain ways are not directly accessible to an anthropologist. While actors' accounts of their intentions can be recorded after the fact, such accounts are not themselves intentions. The data with which a social anthropologist constructs an ethnographic account of a community and the lives of its members derive in part from such texts. These texts may be field notes, sound recordings, video or film registrations of behavior (that is, people doing things or talking about the things they have done), and, in some cases, the anthropologist's memory of events and what people did when they participated in them.[4] But those texts of informants' recollections, reflections on, and accounts of intentions can be taken to be only indirect evidence for actors' states of mind.

Texts of the last sort are especially interesting because, as descriptions of ideology that might serve as bases for explanations of action, they are related to action in much the same way as archaeological artifacts are residues of action: both are shaped by action, but neither are themselves action. Neither archaeologists nor anthropologists can observe ideology directly. To the extent that an anthropologist draws on records of this sort as a source of data, then the primary sources of data in both anthropology and archaeology are

derivatives at least one ontological step removed from the action that shaped them and a second step removed from ideology. Thus, archaeology and anthropology are similar in terms of the nature of the evidence on which they draw to construct past social life, even when taking into account that the past of the social life studied by the anthropologist may be measured in years whereas the past social life studied by an archaeologist might be measured in centuries or millennia.

3. On backbearing from artifacts, to behavior, to social structure and ideology

For a social anthropologist, society consists of individuals but, more importantly, subsists in the relationships of *groups* of individuals to one another, relationships that are reproduced and thus persist through time. In other words, society can be defined as the patterns of relationships that link groups of people. Groups are categories of people and the groups that maintain on-going relations with one another in society thus constitute a system of social classification. The exchanges of material goods in which individuals and groups engage in social life are indices of the relationships of categories of people and these relationships can be analyzed to chart both the relationships and their nature.

There are two important points about societies of the kind I have studied in eastern Indonesia. The first is that, as in all societies, there are groups that are exogamous, which means that the members of each group must marry out of the group and that each group is dependent upon the others for its own reproduction. In both of the societies of Flores in which I have worked, the members of these groups hold in common control over and rights to certain productive resources such as gardens, plots of forest, and coconut plantations, none of which belong to individuals of the group. In addition to being exogamous, these group are, in anthropological terms, corporate. In exchanging people in marriage, the reproduction of individuals, the reproduction of the groups, and the reproduction of society itself are all achieved simultaneously. If those ancient peoples described in the papers of this volume had societies such as these, then it would be crucial to discover as much as possible about marriage and the material exchanges which accompany it in order to be able to say anything meaningful about the structures of those societies.

A second feature of such societies accords with the first: in small scale societies, those that Lévi-Strauss called 'cold' societies, everyone is related to everyone else as kin. This means that people almost always marry a kinswoman or a kinsman. Here the system of classification upon which society is founded demands consideration. Assuming that reproduction usually occurs within a generation rather than between generations, an individual's choice of a partner in marriage must be made between siblings and cousins. Almost universally in contemporary societies of this kind, siblings (i.e., the children of a particular woman) are classified as unmarriageable. Of the four possible opposite sex cousins which a person might have (FBD/S, FZD/S, MZD/S, MBD/S),[5] one or more might be treated as siblings and classified as unmarriageable whereas the remainder are classified as potential spouses.

By the intersection of these two simple yet elegant ideas, exogamy and the classification of siblings and cousins, the simplest possible society is one made up of two groups who exchange people in marriage and divide more or less equally the offspring of those marriages. In such 'symmetrical' systems, siblings are classificatorily merged with parallel cousins (the children of same-sex siblings) and are classified as prohibited in marriage whereas the children of cross-sex siblings (i.e., cross-cousins) can marry. The interposition of this simple classificatory rule is sufficient to insure both the reproduction of the two groups and of society.

Of greater possibilities are systems that exclude from the category of marriageable cousins one or the other of the cross-cousins while allowing marriage with the remaining cross-cousin. Such systems are found among the world's contemporary societies and can, in principle, accommodate a large number of groups, bringing them all into a single society founded once again upon the ideas that each group is dependent upon another for reproduction and that society subsists in the system of exchange thus created. These societies are known as asymmetric systems and are characterized by generalized exchange.

In these societies, each group, regardless of other functions it may fulfill, is a descent group. A simple way of constituting such a group is to limit membership to people who are related to each other either exclusively through men or exclusively through women. In the former case, we speak of patrilineal descent and in the latter we speak of matrilineal descent. The exchanges in which these unilineal descent groups participate include not only partners in marriage, but a large variety of other exchanges such as bridewealth given by a wife-taking group in exchange for a counter-prestation from the wife-giving group, exchanges at the birth of children, exchanges at the death of a spouse, and many others. Almost universally, people in societies of this kind consider wife-givers to be superior to wife-takers, but as it works out, each group is superior to some other group or groups and each is inferior to yet others. This is a kind of hierarchy, but not one that leads to marked social stratification.[6]

A reasonable question to ask is this: were any of the societies of Mesolithic or early Neolithic Europe of these kinds? A second, equally important question is: what would the archaeological data that would allow us to decide the question look like?

Lineality is about descent, that is, kinship, and, of itself, is about nothing else. Knowing that its descent pattern is matrilineal or patrilineal tells us precious little about any other aspect of a society and does not even provide good cause for inference about other aspects of society. I can cite here as examples two societies in east central Flores which share a language, a common prehistorical origin, and many features of culture including an almost identical system for the classification of cousins. The Ata Tana 'Ai of the mountainous eastern region of the Regency of Sikka on Flores calculate descent through lineally related women and a person belongs to the descent group of his or her mother and not to the group of his or her father. In the village of Sikka, which is only about 50 kilometers away from Tana 'Ai, a person belongs to the descent group of his or her father and descent in this community is thus patrilineal. There are other differences between the two societies, some of which can be summarized as below:

	Ata Tana 'Ai	**Ata Sikka**
1.	matrilineal descent	patrilineal descent
2.	marriage not marked by ritual; child exchange marked by exchange of ceremonial goods	marriage with the exchange of ceremonial goods as bridewealth and counterprestations
3.	shifting cultivators, with some hunting and gathering	intensive cultivators, traders, teachers, government officials, landlords
4.	practice the rites of their local religious tradition	Catholic since the 16th century

The activities associated with features 2, 3 and 4 would undoubtedly leave material residues in the archaeological records of these societies. But all such activities would be compatible with both an ideology of patrilineal descent and an ideology of matrilineal descent. It follows that it would be difficult for an archaeologist to determine the descent principles at work in these societies merely from the material remains of activities relating to marriage, subsistence, and religion.

Rodney Needham was the first anthropologist to point out that elements of culture such as behavior, rules, and categories need not be related logically, causally, or by any other necessity. He came to this conclusion after two decades devoted to the study of alliance systems in Indonesia and elsewhere and by thinking carefully about the nature of prescription.

Prescription is a rule found in many (but not all) of the world's societies by which a person should marry a person of a particular category of kin and to the exclusion of persons of other categories. However, Needham notes that prescription is an ambiguous concept that actually refers to at least three different aspects of culture and realms of social life:

> "In the study of social life there are three main aspects of collective conduct and representations which can usefully be discriminated: (1) behavior, (2) rules, (3) categories. The character of prescription can be attributed to each of these aspects, and anthropologists have in fact done so" (Needham 1973, 171).

Of these three aspects of collective conduct and representation, an anthropologist can directly observe and record (i.e., textualize) behavior. Rules of behavior can be constructed from regularities observed in behavior. These rules have the status of analytical or explanatory constructs. Rules may also be articulated by members of the community under study, in which case they have a different status from analytically derived rules, but in both cases there is a disjunction between rule and behavior: in the first instance, a deduced rule may be found to apply in only some statistically significant number of cases. Thus, from a corpus of genealogical data the anthropologist might find that in 94% of the marriages in a community in three generations, classificatory MBD married classificatory FZS. In the second instance, it may be found that a rule articulated by members of the community themselves is observed in only *n*% of the instances in which it should apply. There is in both cases a measurable discrepancy between behavior and the rules that are thought to govern (or explain) it. Nevertheless, according to Needham, for the anthropologist there are certain advantages to studying rules. Firstly, it is easier to specify what a rule says (that is, what it prescribes or proscribes) than it is to decide whether behavior accords with the rule in a statistically significant number of instances. Secondly, the contents of rules are amenable to comparison. Thirdly, even in those cases in which behavior departs from rules, it is from the rules that the behavior departs and so rules must be taken into account in the description of society (Needham 1973, 172).

Another possibility is to take prescription to be, rather than a matter of rules, a 'formal property of a system of categories of social classification' (Needham 1973, 174). Needham argues that in studying prescription, there is an advantage for the anthropologist in paying attention first to the categories of social classification. Here he notes four pertinent features of behavior, rules, and categories. Firstly, institutions are 'condensed' in categories; secondly, social action varies more than its attendant classifications; thirdly, the comparison of categories is more feasible than is the comparison of variable behaviors; fourthly, forms of

classification are relatively few and simple compared to modes of action (1973, 174). In short:

"By starting from social classification … we need be under no inclination to ignore or to underrate the associated rules and modes of behavior. Indeed, this approach actually brings out more clearly the fact that categories, rules, and behavior are independent variables" (1973, 174).

If categories, rules, and behavior are independent variables, and if there is an advantage for the anthropologist in beginning the study of society with categories, then there follows a methodological imperative:

"Our precept, then, must be that what can vary independently must be analyzed independently (Needham 1970, 255). We should therefore first make a formal analysis of the terminology, then establish the rules framed by this classification, and finally plot the modes of social action; at each stage in the investigation we have to expect disparities" (Needham 1973, 174).

The import of Needham's argument for both anthropologists and archaeologists is that inference from behavior to rules, from rules to categories, and from behavior to categories is very risky. Categories are part and parcel of ideology and, with respect to prescription:

"To prescribe is to lay down a rule, to decree an obligatory mode of conduct; that is, the term refers properly to the jural regulation of social life. But the jural features in question cannot be attributed to a formal structure, and they are not properties of a relationship terminology. Prescribed marriage is logically quite distinct from a prescriptive terminology: *there is no necessary correspondence between categories and social action, and therefore neither can be inferred from the other*" (1973, 177; emphasis added).

I have quoted these points of Needham's argument at length because I am convinced they are worthy of the most careful consideration by any archaeologist who might attempt an inferential backbearing from artifact to ideology or social structure. The argument that 'categories, rules, and behavior are independent variables' [*supra*] is of particular relevance for archaeologists, for it tells us that working from behavior to rules, and thence to categories cannot be done *a priori* because there is no necessary connection between these three aspects of collective conduct and representations.[7]

In addition, Needham proposes that anthropologists should work first from ideology (categories and classifications), then to rules (and institutions), and then to accounts of behavior, which is variable with respect to rules and categories. This is the reverse of the procedure of archaeology, which must begin with artifacts as the material remains of behavior and work then toward social structure and ideology.

An ethnographer can observe and record the behavior of living people but, if Needham is correct, it is not possible to read with certainty rules which presumably govern that behavior from the behavior itself. Normally, where there are rules to be found, an ethnographer discovers them by interrogating living informants. Likewise, the categories of classification systems, including the social categories which make up systems of social classification, are discovered through an examination of language and the actual speech of informants, among whom the ethnographer can make further direct investigations by the simple expedient of discussing with them the identification of categories. Thus, an ethnographer does not (because he cannot) derive rules from behavior, and then further derive the categories of social and cultural systems of classification from rules. In ethnography, each of these is found directly in empirical evidence of different types and thus must be the subject of distinctive investigations. The nature of each aspect of culture is determined individually by reference to living members of the community under study and their actions.

These methods are clearly not available to archaeologists and, because behavior, rules, and categories are independently variable, determining them for people long dead is a very difficult undertaking. I would suggest that the relationships between artifact, behavior (or action), and ideology are similarly indeterminate. There is a further problem: artifacts are not behavior, but the products of behavior (I would say 'intentional action'). The material that archaeologists study is thus a further level removed from the 'culture' (categories and rules or ideology and social structure) they wish to characterize.

4. 'Culture' and inference

James Deetz, who was the leader of a band of enthusiastic students of historical archaeology when I last studied the subject formally[8] once wrote: 'Archaeology seeks to learn about culture from the fragmentary remains of the products of human activity' (1967, 5). This is a worthy goal, but Deetz had trouble defining culture — so much so, that he preferred to make statements about it rather than defining it directly. These statements included: 1. culture is learned behavior; 2. culture is uniquely human; 3. culture is patterned; and 4. society is the vehicle for culture. Thus, 'culture [is] a uniquely human system of habits and customs acquired by man through an extrasomatic process, carried by his society, and used as his primary means of adapting to his environment' (Deetz 1967, 7). But, in addition,

"Culture is highly perishable, and therefore cannot be excavated. No one has ever dug up a political system, a language, a set of religious beliefs, or a people's attitude toward their ancestors. Yet such things as political and religious behavior, language, and social interaction affect what the archaeologist does recover. The patterning which the archaeologist perceives in his material is a reflection of the patterning of the culture which produced it. Pots, arrowheads, house

floors and axes are the products of culture, not culture in themselves, but they are linked to culture in a systematic manner. It is the archaeologist's task to discover how cultural behavior is shown in its products" (Deetz 1967, 7).[9]

It is precisely the evanescence of culture in Deetz's sense of the term, plus certain difficulties in relating various aspects of it to one another, that create problems in the archaeological search for ideology and social structure in the remains of past action.

Deetz's ideas bear a close relationship to those of Taylor (1967 [1948]) who, in arguing for a 'conjunctive approach' (in contradistinction to the comparative and taxonomic approach) in the archaeological study of the past, found it necessary to contrive a definition of culture suited specifically to archaeology. In so doing, Taylor was among the first of many who have noted the ambiguity of the term *culture*. He made three pertinent points. Firstly, he distinguished culture as 'a concept which is holistic and used to distinguish phenomena that are 'cultural' [i.e., 'the product of human activity'] from those that are 'natural' from culture as 'a concept which is on a secondary level of abstraction, which is partitive, which denotes a segment of the holistic concept, i.e., *a* culture.' His second point is that 'culture is a mental phenomenon, consisting of the contents of minds, not of material objects or observable behavior.'[10] Thirdly, 'a 'trait' or unit of culture can be either shared or idiosyncratic, i.e., it can be common to many individual minds or to the mind of a single individual' (Taylor 1967, 96).

In these and two further points, Taylor was ahead of his time (cf. Watson 1995):

"Culture, consisting as it does of mental constructs, is not directly observable. *It cannot, therefore, constitute the empirical data of any discipline*. Culture can be studied only through the instrumentality of observable phenomena, through what have been called the objectifications of culture: cultural behavior and the material and non-material results of such behavior. A tribal dance, the avoidance of a woman by her son-in-law, the identical appellation used between a small child and an old man, the designs of a Navaho sandpainting, a stone axe, all these are observable phenomena. *The culture ideas behind them can only be inferred*" (Taylor 1967, 108-109, emphases added).

The difficulty is the problem of inference from one independent variable (artifact or behavior) to another (social structure or ideology).

While it is not my purpose here to summarize the history of archaeological thinking about culture, the citation of one additional idea from the archaeology of the 1960s and 1970s will help establish my argument. When I last studied archaeology, I learned that the equation of 'assemblage' with 'culture' in archaeology held certain theoretical implications and methodological advantages for archaeologists.[11] Somewhat later, Trigger treated the concept of the assemblage critically and identified it with the 'historical particularist conception, championed by Boas' (Trigger 1989, 190). While I am not competent to fault Trigger's judgment, I will suggest that the equation of an archaeological assemblage with a culture may have some residual value for those archaeologists concerned with the extent to which ideology and social structure can be read from material remains.

My intention in this essay is to voice a warning about the difficulties of backbearing from the material residues of action and behavior to cognition, to patterns of 'social' thought and action, and to 'culture.' I confess to worries about the reliability of such chains of inference because (1) I agree with Taylor's view that, if culture is constructed mentally, then it cannot be apprehended directly and empirically by anthropologists or by anyone else, and (2) I am convinced that what social and cultural anthropologists have long called 'culture' is not a causal entity. That is to say, it is not a force in the universe which acts upon matter or brains and minds. If culture is not causal, then the first question that arises is: can 'culture' be explanatory, i.e., can it be invoked to *explain* human behavior or action? Is it reasonable to say something like: 'The Mumbos (do, say, think, represent, believe) *because* of their culture'? To my mind this leads, at best, to tautology and at worst to metaphysics. There are many complex problems here, but certainly a view commonly revealed in public discourse, and one we find hints of occasionally in anthropology, that culture is causally related to behavior, ignores the difficulties.[12] At a minimum, culture (whatever it might be taken to be) is mediated by individual cognition and choice which, in particular cases, might be linked to action that varies radically from the 'behavioral norms' of a social group.[13]

This is another way of saying that the empirical object of anthropological study is not 'culture' and 'society,' but behavior and action. The fact that patterns of intentional action recur in one human group and may differ from those of other human groups, that is, that these patterns may be 'cultural,' does not mean that culture itself is directly available for study. This leads to the question, if action is the object of study, should archaeology concern itself with discovering the culture (or social structure or ideology) of the people who made an artifact or assemblage of artifacts in the first place?

By this argument, the most important feature of a human being's environment is not culture, but other human beings. This fact arises from the unique biological history of our species, which has produced a creature who cannot survive, either as an infant or as a mature organism, without the care and cooperation of others of its kind. This must be kept in mind when asking questions about the relations between action, the material conditions of social life, and the material

residues of action in a way that assumes a priority of ideology or social structure.

If technologies are indices of ideas, they are also solutions to problems – with room for play and experimentation. The relative complexity of Neolithic technologies can be taken as a movement toward greater eclecticism, a more generally applicable repertoire of tools, techniques, and strategies for the exploitation of environments, which must also include the exploitation of a social environment. If this is so, it raises interesting questions, such as: to what extent, if at all, can a social system be viewed as an element in a technology? While the inclusion of 'social system' in technology might be a matter of definition, considering 'social system' or 'social structure' in this way my allow us to answer questions, such as: can a social system and the ideas held by its members be inferred from an assemblage of artifacts and subsistence strategies, and their material remains? This question returns us to my starting point.

5. Culture as theory

If culture does not cause behavior, and if inference from artifact to behavior and then to social structure or ideology is uncertain, then what in the methods of anthropology might be of use to an archaeologist?

In brief, an anthropologist's data are observations and records of behavior made in the course of field research, including recordings of what his informants tell him about rules and categories. The argument here must take into account the ontologies of these three things. Behavior is physically manifest in the movement (intentional or not) of human bodies. Rules are propositions which are either articulated by informants or constructed analytically by anthropologists. In the first instance, they may be taken as expressed imperatives that are thought by informants to govern behavior; in the second, they are analytical constructs intended to account for behavior. In both cases, they may be found to accord only statistically with observed behavior. Categories are formally specified in language. In other words, categories, rules, and behavior are things of different *types* and it seems unreasonable to me then to say that they make up a thing (*culture*) of a unitary type.

Archaeologists have a similar problem: the material artifacts or residues of behavior studied by archaeologists are not behavior, but are things and phenomena of different logical types from behavior. There may be more than one way to produce an artifact, for example, a stone tool. To the extent that this is so, to the extent that many tools can be put to more than one use, and to the extent that the features of an artifact may not include those that can serve as clues to which one of the possible means of manufacture the maker actually employed or the uses to which it was put, then care must be taken when inferring from the artifact the behaviors that produced it or involved its use.

Ethnography, as the main product of social anthropology, may be cited as a model for the final results of archaeology. But an ethnography is not a description of a culture (or of social structure or ideology); it is a theory of the recorded data an anthropologist accumulates in the course of fieldwork and which are subjected to analysis to reveal their relationships to one another. The analogue in archaeology is the archaeologist's systematic specification of an archaeological site: descriptions of the artifacts it contains, their relationships to one another in space and time, and their relationships to the context within which they were found. It may be useful to think of that assemblage as being associated with or part of a culture, but there is something suspiciously circular in a syllogism that posits culture as a determinant of the production and use of artifacts and then proceeds to discover that culture in the analysis of an assemblage of artifacts. It may be better to conceive of the results of the analysis of an archaeological site as a theory that explains the deposition of the things found there. This is exactly what most archaeologists do.

Just as a symbol is meaningless in isolation, but only takes on meaning in relation to other symbols and to the extent that all are systematically related to one another, so, too, an artifact only takes on meaning in relation to other artifacts and the contexts in which they are deposited. There may yet be a distinct advantage to be found in this view (which was not incompatible with a larger concept of culture). As an assemblage grows and its typology becomes more complex, and as the relationships among artifacts and between artifacts and context become more complex, so the 'culture' changes or gets refined. Here, culture is, in effect, a theory of the assemblage.

The archaeologist's 'culture' is then an explanation – a *theory* – devised by the archaeologist to explain the provenance of the assemblage, the artifacts that make it up, the relationships identifiable among those artifacts (i.e., within the assemblage), the relations of the assemblage to its context, and changes in the assemblage through time (where these can be identified, one can speak of 'cultural evolution' or 'culture change'). Thus culture is not a *thing* an archaeologist infers (or an ethnographer observes). It is, rather, a theory devised to explain what can be observed and described (artifacts, in the most comprehensive sense, for archaeologists; action for ethnographers).

There are thus similarities between archaeology and social anthropology. Both include in their primary data the residues of human action in the world, although anthropologists can include in their data direct observations of action and behavior. In archaeology those data are artifacts, assemblages, and reshaped landscapes. In anthropology they are texts of one form or another. But the fields interrogate these data in similar ways. Both anthropologists and

archaeologists want to know the reasons why a thing was made and what was done with it, and, if possible, the motivations of the people who made and used it; we want to know what the people who made and used them thought about their creations. We both want to know what the things and the things done with the things meant. And we both want to know what can be learned from the origins and evolution of human society because those lessons will tell us also about our potential as individuals and as a species.

A few of the key terms in the discussion of these questions at NIAS were:

Culture
Society
Ideology
Social structure
Hunting-gathering
Agriculture

And we examined, *inter alia*:
Graves
Grave sites
Stone tools
with reference to:
Animals
Plants
Landscapes

These are things of different logical types: some are concrete things in the physical world; some are best described as open and stochastic (i.e. evolving) systems; some have a dual ontology and are both stochastically systematic at one level and logical constructs at another. And, as an ever present danger in anthropology (I will not speak of archaeology), they can be confused ontologically. Thus, a *culture*, which is at best a theory devised to explain ethnographic data, can be mistaken for a phenomenon that has causal efficacy whereby it produces as artifact that (behavior, action) which it explains. This is a double fallacy of misplaced concreteness and the assumption of a conclusion, which many of us anthropologists actually get paid to commit. Depending upon whom you have been reading recently, a *social structure* is an empirically observable configuration of communicating and interacting human beings or either a mechanical or statistical model dreamt up by an anthropologist to explain a corpus of data. If we are to employ these terms, we need to understand the possible relationships between the things for which they stand.

Societies do not make artifacts. Artifacts are made by individual people, sometimes working singly, sometimes cooperatively in groups. If the behavior or action by which an artifact is made is related in some way to an idea or ideology, it is an idea which is in the mind of the individual. Its existence cannot be some metaphysical 'group mind,' nor even Durkheim's *conscience collective*. Similarly, individual thought is not the same thing as collective thought, the *représentations collectives* imputed to the people of a society by Durkheim. The first is cognition, a capacity of the individual mind, whereas the second is social and, as such, can only be inferred with more or less logical legitimacy from the observed representations of individuals. In searching for an 'ideology' that may be inferred from artifacts through behavior, both archaeologists and anthropologists would be well advised to keep clearly in mind whose mind it is to which ideas are attributed or imputed.

Behavior and action may have immediate or efficient causes in brain mechanisms (which are themselves still imperfectly understood). But as explanation for those things that most attract the attention of anthropologists and archaeologists, brain mechanisms are not themselves a sufficient explanation. Nevertheless, the invocation of *culture* (or ideology or social structure) as an explanation of behavior is at least insufficient and is at most ridiculous because culture does not *cause* behavior. It cannot, therefore, be held to explain mechanically the regularities, repetitions, and patterns of action documentable by an anthropologist nor the residues of such action recordable by archaeologists.

If culture is not a causal entity, and if it is not reasonable to say that someone behaves in some way or does something in particular *because* of his culture, then there will be implications for archaeologists who are interested in the links between the artifactual remains of human action and the 'ideology' or 'society' or 'culture' of the people who left those residues behind. Specifically, I am afraid it is not possible to read directly from the former to the latter. If this is the case, then archaeologists must take particular care in the way they *explain* their data: culture as a *thing* (*sui generis* or *Ding an sich*) cannot be invoked as explanation for an archaeological assemblage.

However, this is not to deny that the people who create assemblages of artifacts or who act in patterned ways *possess* a culture. A less satisfying, but much more rational and, I believe, useful implication follows: *culture*, if not a thing or causative force, can be a *theory*. That is to say, one can explain behavior or the remains of it in terms of an explanatory construct that we can call (perhaps for want of a better term) a 'culture,' *so long as the explanation is testable* and correctable in light of new or additional evidence, which is another way of saying that such a theory must contain an heuristic which leads toward further research, and data which may falsify the initial formulation. The argument is, simply, that culture is a theory of things, and not a thing itself. That theory may have the form of a sequence of inferences (strictly speaking, *hypotheses*):

if artifact *x* (and *y*, *z*, …), then intentional action *a*.

if action *a*, then idea (or ideology) *i*.

Each inferential hypothesis, to be of use, ought in principle to be testable, either by the generation of further 'if *m*, then *n*' hypotheses, where *n* can either be sought empirically or can be decided on other reasonable grounds.

The procedure is very much like that by which we solve jigsaw puzzles, only in the case of archaeology and anthropology, pieces are always missing. Each piece which interarticulates with another and then another, to form a mass, ought to invite the fitting of other pieces. If, in the end, more pieces (data) are left out than are incorporated into the puzzle, then another strategy for fitting them together ought to be devised.

A number of years ago I delivered a lecture to a class of undergraduates in which I suggested that the ontology of 'culture' is murkier than most anthropologists generally recognize and that one way of dealing with the concept is to think of a 'culture' as the distillation of a meeting between the ethnographer's culture and the culture of the people he studies. I called this synthesis an 'ethnographic culture' and said that what it really is, is a theory of the ethnographic information an anthropologist has at hand at a given moment and that its most powerful quality was as an heuristic for further research.[14]

Social anthropologists can draw on 'native theories' of structure and employ them as *data*; archaeologists cannot: indeed, it is those 'native theories' that are among the goals of archaeological research. Archaeologists may, therefore, find it useful to know of the problems and difficulties that anthropologists encounter and the methods they bring to bear on them.[15] Culture is, then, neither a thing nor a phenomenon, much less a force that causes anything. But it may still be something that an anthropologist or archaeologist can infer with greater or lesser methodological rigor and then analyze more or less fully. It is most usefully viewed as a cumulative theory of the data to which anthropologists and archaeologists variously attend: behavior and the residues of behavior. An ethnographic description of a community of human beings and the analysis of the patterns of observable behavior and action in which they engage is such a theory. The description and analysis of the individual elements in a material assemblage of artifacts and the context in which they are embedded and their relationships to one another may also lead to such a theory.

notes

1 This essay began life as the notes for a discussant's paper for the Conference on *Ideology and Social Structure of Hunting, Gathering and Farming Communities in Stone Age Europe* organized by Marek Zvelebil and Annelou van Gijn and sponsored by The Netherlands Institute for Advanced Study in the Humanities and Social Sciences, Wassenaar, and the Institute of Prehistory, University of Leiden, The Netherlands, 28-29 April 1994. I am indebted to NIAS for a Fellowship in 1993-94 and for the year of freedom from the quotidian affairs of life in the late twentieth century university it provided me. The notes from which this paper emerged were assembled at NIAS during that year. I would like to thank my colleague, Dr J. Timothy O'Meara, of the Anthropology Programme, The University of Melbourne, for a thoughtful and constructive discussion of the main points of my argument as the draft of the paper neared completion.

2 Tana Wai Brama ('the Domain of Wai Brama') is one of seven ceremonial domains into which the people of Tana 'Ai are divided. See Lewis 1988 for an ethnographic account of Tana Wai Brama, including the *gren mahé* rites, and Lewis, Asch and Asch 1993, a film about the *gren mahé*.

3 A comparison of ethnographic accounts of sacrificial rites in various societies of eastern Indonesia would reveal that ritual complexes which might leave similar archaeological records are attended by quite different ideologies and, conversely, that similar ideologies are associated with ritual practices which would leave quite different archaeological records (as, for example, those of Florenese peoples who sacrifice water buffaloes and those who sacrifice other animals) (see Lewis 1996 and the other essays in Howell [ed.] 1996).

4 I include in the set of such events those which consist of an anthropologist discussing with an informant some question of belief, motivation, intention, or 'ideology.' These events become textualized when the anthropologist records a conversation with an informant or minutes that conversation in fieldnotes.

5 Anthropologists employ a simple short-hand for identifying genealogical relationships: F = father, M = mother, B = brother, Z = sister, D = daughter, S = son.

6 There is yet another major class of societies, those in which kinship (i.e., descent) is determined neither through men nor through women exclusively. Anthropologists refer to the kinship and descent patterns of these societies as *cognatic*. In them, all individuals recognize equally their kinship with their mothers' and their fathers' kin. It is perhaps worth pointing out that almost all of the contemporary societies of Europe are of this class. For that and many other good reasons, an archaeologist seeking to identify a prehistoric society in terms of its kinship and descent patterns must consider the possibility that it was cognatic.

7 In *Belief, Language and Experience* Needham takes up Wittgenstein's proposition: 'An "inner process" stands in need of outward criteria' (Wittgenstein 1953, sec. 580) with respect to belief and argues persuasively that, 'in the case of belief these are just what we cannot discover in any form of action' (Needham 1972: 102). In an argument that bears directly on the problems of anthropology and archaeology, Needham concludes that that 'there is no necessary or general connection between belief and action,' and, because this is so, action cannot be a criterion of belief (1972: 100). Just as all men think, but there is no corresponding bodily phenomenon by which thought may be intuited (Needham 1972: 144), so too with belief, which entails no necessary bodily index. If Needham is correct in his criticism of Wittgenstein and if his argument is extendible to ideology, then the search for ideology in the remains of action will lead to many of the same problems as attempting to determine a person's beliefs from his actions.

8 In the Department of Anthropology, Brown University, in 1973-74.

9 The 'culture concept' has been defined in various ways in archaeology in the U.S.A. Watson (1995) provides a succinct survey of the history of the concept.

10 On this point, Taylor and Deetz apparently disagree, for Deetz says of culture that it is 'learned behavior' (cf. *supra*).

11 Cf. Chard (1969: 23): 'When a[n] … assemblage recurs repeatedly at a number of sites, we are dealing with a *culture*. Each archaeological culture is thought to represent a society and to reflect the patterns of behavior common to the members of such a larger grouping.'

12 Thus, to say 'Joe drinks beer *because* of his culture,' that is, to say that Joe's particular behaviour or actions are *caused by* his culture, is to utter nonsense. This view is distinctly contrary to the popular conception of the dynamics of culture and the ways in which it influences behavior, at least in Australia, a nation whose government policy explicitly promotes 'multiculturalism.' Not too long ago, Radio National, the Australian public radio service, broadcast a discussion of multiculturalism in the law courts. One case cited in the program was that of a man who had migrated to Australia from a middle eastern country and who stood accused of physically abusing his wife. The commentators in the program considered the question of whether or not the man's 'cultural background' (which, it was implied, included wife-beating as a 'culturally sanctioned' form of action) might or might not be a mitigating factor in his case before law. The proposed argument seemed to be: 'Ahmed beat his wife because in Ahmed's culture wife-abuse is normal,' i.e., Ahmed's 'culture' made him beat his wife. Thus Ahmed's 'culture' should be considered a mitigating factor in his behavior and it might be found that Ahmed was not entirely responsible for his action; hence it might not be proper to find him guilty of the charge laid against him.

13 See Freeman (1978, 1981) for an elegant exposition of preferential choice as a defining characteristic of human behavior. See also O'Meara (1997) for an exposition of the causal efficacy of ideas, beliefs, and values.

14 I thought I had lifted the idea directly from Roy Wagner (1981), but in re-reading his book I do not find this phrase, althought the ideas which lead to it are clearly there.

15 See Lewis 1997

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