

THEORETICAL PARADIGMS AND PRACTICAL RESEARCH

A view from the Paleolithic

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RESUMO:

O aparecimento e desenvolvimento dos diferentes paradigmas que se têm vindo a suceder como dominantes no campo científico da Arqueologia é um processo histórico que está também intimamente relacionado com os problemas da investigação prática e não resulta portanto apenas das transformações ocorridas nos ambientes intelectuais em que a disciplina foi crescendo. No decurso deste processo, os resultados obtidos pelas gerações anteriores no quadro de paradigmas diferentes dos que actualmente orientam a investigação são integrados, e não simplesmente negados, nas construções teóricas do presente. Dadas as limitações do registo arqueológico, a informação que dele podemos extrair é diferente da que se obtém em História -- há níveis da realidade que apenas podem ser abordados a um nível elevado de abstracção e através de modelos adaptados da Antropologia. Tal como em todas as outras disciplinas históricas, o processo de conhecimento em Arqueologia é acumulativo, e o seu produto é uma imagem do passado real composta de reflexos correctos do objecto, distorcidos pelo condicionamento histórico e social do sujeito. É pela superação desses condicionamentos que um conhecimento verdadeiro e objectivo do passado pode ser construído. A afirmação da possibilidade de obtenção de um tal conhecimento implica considerar a validação das conclusões como questão de importância central para uma Arqueologia que se pretenda responsável para com a sociedade.

ABSTRACT:

The appearance and development of the different paradigms that have successively dominated the field of Archeology is a historical process that is also intimately connected with practical research problems and not the just the result of changing intellectual environments. This process can be seen as one where the results obtained by previous generations in the framework of paradigms other than those currently dominant are integrated, not just negated in any simple manner, in the theoretical constructions of the present. Given the limitations of the archeological record, the information that

in GAMITO, Teresa Júdice (ed.) -- *Arqueologia Hoje*,
I, Faro, Universidade do Algarve, 1990, p. 220-241.

can be extracted from it is different from the one obtained in History -- certain levels of reality can only be approached at a high level of abstraction and through the use of models derived from Anthropology. As in all other historical sciences, the process of knowledge in Archeology is cumulative, and its product is an image of the real past composed of accurate reflections of the object distorted by the social and-historical conditioning of the subject. It is by superseding this conditioning that an objective and true knowledge of the past can be constructed. Since such a knowledge is conceived as possible, the testing of conclusions becomes a central issue for a socially responsible Archeology.

Recent reviews of the main theoretical paradigms that currently co-exist in archeology (Gallay 1986, Hodder 1986, for instance) have developed a presentation of the subject whereby these paradigms are compared from the point of view of their philosophical and logical foundations. This kind of presentation has an obvious pedagogic advantage: the one of making it clear to the reader where exactly lie the essential differences between the competing approaches, and therefore making it easier to understand them.

However, this also has some inconveniences. For instance, from Gallay's discussion of the issues at stake one might be lead to the idea that the history of archeological thought is simply one of competition between two main phyla of ideas -- the humanistic approach and the natural sciences or evolutionary approach -- with alternating periods of predominance. The developments that have been taking place since the early 60's could for instance be regarded, in this light, as a full historical cycle: the culture history paradigm of the first half of the century gave way to the new archeology in the 60's and 70's, while the "reactionary view" of Hodder and his colleagues would mean nothing more than a comeback to the old "normative" or "paleopsychologist" approach so vehemently attacked by Binford and associates.

The danger of conveying an idea of cyclicity, which, in my opinion, would be a far simplistic way of understanding the theoretical developments of the last 30 years, is not however the only one. From Hodder's discussion one might, on the other hand, be lead to the also far too simplistic conclusion that the several competing approaches arose only as the result of an importation into archeology of bodies of theory generated outside the field, in total independence of the ways our discipline has been evolving, and of the nature of the concrete practical research problems it has needed to face since its birth. Also, one might be lead to the conclusion that each individual archeologist's adherence

to a specific paradigm could be solely the result of his or hers philosophical preferences, bearing little or no connection at all with his or hers own practical research, and the overall state of affairs in the discipline as a whole.

I am sure that none of the mentioned authors comparts such simplistic conclusions. On the contrary, Hodder (1982a:214), for instance, has posited the relationship of his approach with the "normative" and "processual" archeologies that developed before him as one that could be described as of thesis-antithesis-synthesis. That is, one that builds upon the work of its predecessors and that, while standing for a comeback to the historical concerns of the generation of Gordon Childe, also integrates, while criticizing them, the acquisitions of the new archeology, instead of simply rejecting it as a whole. But, on the other hand, it is certainly not just a coincidence that Binford and his followers have been mainly concerned with early man and the paleolithic, while "post-processual" archeologists have dealt mainly with recent prehistory or with historical archeology.

My purpose in introducing the problem from this angle is not, therefore, the one of criticizing the valuable work done by both authors, but the one of emphasizing the need to put the development of new paradigms in historical perspective, that is, in the context of the development of the concrete practical archeological research. Work along these lines has been done by Willey and Sabloff (1980), and also by Klejn (1980), but it seems to me that our discipline still has as one of the tasks lying ahead of its current development the establishment of a genetical epistemology of the field. My own largely insufficient knowledge of the subject does not allow me to define as my goal in this paper the offering of significant contributions along these lines. What I will attempt is rather the presentation of some reflections on the relationship of "normative", "processual" and "contextual" approaches with the concrete archeological record with which we have to deal, especially in what concerns my own research field, the paleolithic. These reflections will revolve around a more general issue, the one of the relationship between theory and practice, and, hopefully, will not be completely commonplace, or useless, to the discussion of how new theoretical principles arose, and ultimately became dominant, in our particular scientific field and at a particular point in time.

1. "Normative" and "processual" approaches

The first problem one has to deal with in this regard is the one of why the "normative" approach, for so long dominant (and still dominant in most of Western Europe), was ultimately questioned, and why did the new archeology develop at all. It could be argued, using its own jargon, that the whole business of the "processual"

approach was nothing more than a successful adaptation of archeology to the positivist environment of american science. However, Willey and Sabloff (1980:184) suggest, and I think one can easily agree with them on this, that the most important reason for change came from within archeology itself. The slow accumulation of culture-historical data made it almost a necessity that, at a certain point, questions concerning evolution, that is, non-random change through time, had to be addressed, and this was indeed to become one of the main tenets of the new archeology. And in what regards the paleolithic, the concern with establishing the origins of modern human behavior is certainly behind most of Binford's work (Binford 1983). In continental Europe, or at least in France and Iberia, however, the same slow accumulation of data gathered under the culture-historical paradigm has failed to produce a similar development. Instead, what seems to have happened is that traditional approaches have continued to be dominant among aboriginal paleolithic archeologists, while research inspired by the new archeology was developed in a parallel way by foreigners (mainly from across the ocean). As a spanish colleague once told me, the americans do their thing and we do ours.

Many explanations can be offered for this state of affairs: for instance, the close relationship that archeology has with history instead of anthropology in european universities; or the fact that evolutionary concerns were always an integral part of the european approach. The latter is a particularity that may stem, as suggested by Leone (1982), from the fact that, unlike in America, in Europe the past was assumed by Childe and his generation to be directly tied to the present, and therefore contributing to its elucidation. However, in what regards the paleolithic of western Europe, it seems to me that a deeper reason may exist, a reason that has to do with the relationship of french and iberian archeologists both with past research and with the past they researched. That is, while in America one could say that the new archeology built upon the established culture-history, accepting it to go beyond it, in Europe the adoption of Binford's point of view on the nature of variability in lithic artifacts had as its direct consequence the need to reject (at least in their more elaborate form) the traditional culture-historical schemes of the paleolithic as developed in France and Spain. It is only natural that such a rejection was a difficult step to take for those who developed the scheme in the first place, and was in turn made difficult by them to the generations they trained.

The acceptance of the points raised by Binford against the "normative" interpretation of variability that still prevails among continental Europe paleolithic archeologists will therefore probably come only through the slow accumulation of anomalies, in the sense of Kuhn (1983). The refutation of the validity of the finer sub-

divisions of the upper paleolithic (such as the periodization of the aurignacian or of the so-called perigordian V), the recognition that the distribution of some index fossils (such as the solutrean shouldered points) is determined by function and raw-material economy more than by chronology, the refutation of Laville's chrono-stratigraphy of the old Wurm by TL absolute dating, the realization that mousterian industries go back to 300 000 BP and can therefore be contemporaneous with the acheulean, are examples of such anomalies. Their resolution demands a critique of the theoretical principles that make them appear as such, and one can therefore expect that, in the near future, the "normative" view of the paleolithic of Europe will be finally superseded also among local archeologists.

Another situation that can contribute towards change from within the discipline is the fact that research in western Europe is now fastly expanding into areas outside of the "classical regions" of Cantabria and the Périgord. In some of these areas, the archeological record is structured in ways that differ significantly from the patterns observed in those two classical regions, and the new problems this different structuration poses to its investigation may also contribute to a questioning of the prevailing paradigm. As an example, I must confess that when, ten years ago, I began my research on the upper paleolithic of Portugal, I was moved by culture-historical concerns, and the only aim I could conceive, at the time, for such a research, was the one of, in the long term, establishing a sequence comparable to the one of those two better known areas. And it was in the first place for very pragmatistical reasons that I was forced to move away from such a theoretical framework. As a matter of fact, such an aim demanded the excavation of rich, stratified sites, spanning long time ranges, such as southwestern France's rock-shelters. Detailed periodization and chronology could not be established otherwise, and without them questions about origins, influences, and diffusion, were impossible to answer. Now, such sites simply do not exist in Portugal (or have not been found so far). Instead, the available database is made up mainly of non-stratified open air sites, with a few caves having been sporadically used for logistical purposes during the solutrean. Approaching this database from a culture-historical point of view was leading me to a dead end (as it had led my predecessors), and this is what made me look for other ways of dealing with the available evidence, and to concentrate on the issues of settlement and subsistence in the framework of the concept of culture as adaptation. I was however well aware of the concept's limitations, for instance to explain stylistical variability **within** ecological boundaries, on one hand, or stylistical uniformity **across** ecological boundaries, on the other. Therefore, in my recent synthesis on the portuguese solutrean (Zilhão 1987), these limitations were not

forgotten, and I tried to combine the new approach to the data with the discussion of some more traditional culture-historical problems.

2. "Processual" and "post-processual" approaches

From the discussion above one is easily led to the conclusion that I value the developments I predicted -- that is, the acceptance of the points raised by Binford against the "normative" view of variability -- as welcome and necessary if european paleolithic archeology is to advance beyond its present state. The recent development of the several "post-processual" approaches, however, makes it necessary to discuss whether this is indeed so. "Post-processual" archeologies have in common their understanding of material culture as "meaningfully constituted", that is, as being of a symbolic nature. They criticize the dichotomy between form and function characteristic of both traditional and new archeologies, and sustain a structuralist (sometimes lévi-straussian, sometimes marxist) approach of society whereby the decoding of material culture is thought to need an understanding of the ideological realm. Each society is in turn conceived as the result of a particular historical development, the explanation of many aspects of its material culture being irreducible, and possible to understand only in terms of an historical enquiry. The simple fact that a society exists means that it is adapted, the concept of adaptation having thus little explanatory value, serving only to determine the conditions of existence or non-existence. Because of this, the explanation of change cannot be restricted to external, environmental, influences, as results from the functionalist-systemic approach of the new archeology, but, instead, should be mainly concerned with the search for the conditions under which and the processes through which individuals and social groups adopt new social strategies that question the prevailing order (Hodder 1982b, 1986; Tilley 1981, 1982). At first glance, one can hardly disagree with these points, especially if one, as is the case with most portuguese archeologists, was primarily trained as a historian. However, a closer analysis of the issues at stake indicates that some prudence is needed here, and that the possibility of accepting these views as a programme for practical research is still faced, in the present state of archeology, with some important epistemological obstacles.

One of the most important ones is undoubtedly the patchy nature of the current knowledge of the archeological record. A "contextual" archeology as defined by Hodder is only conceivable where there is context, that is, detailed empirical knowledge of all aspects of a specific society's material culture. This is, however, a rather limited view of what context should mean. No matter how well we know a specific past society, we will only be able to evaluate its distinctive

particularity if we are able to confront it with its contemporaneous world as well as with the societies that preceded and followed it. Things are what they are only through their opposition with what they are not, a logical statement that it seems to me "post-processual" studies of particular prehistoric situations have tended to overlook, Hodder's study of dutch neolithic pottery (Hodder 1982c) being a good example. To be fair, one must acknowledge that he does state that "it is necessary to carry out a broad comparative study of contexts in western and northern Europe where megaliths occur in order to identify further factors which may be relevant for the explanation of the particular correlations observed in the Netherlands" (p. 176). The point, however, is that it is the very significance of those correlations that cannot be evaluated outside the framework of such a comparative study.

The importance of context in this sense is perhaps better highlighted by the fate of the interpretation of upper paleolithic art as a basically european phenomenon, arising out of the need to promote the mutual identification of the participants in the long-distance information exchange systems made necessary by the harsh peri-glacial environment (Gamble 1983). One may consider that it could be said right from the beginning that the functionalist approach producing the interpretation did not augur for its success. But the fact that the paleolithic age of rock art is now well established in such geographically distant and ecologically different places as Brazil, Africa and Australia, certainly weighs in a more decisive way towards suggesting that the model should be abandoned or, at least, revised. The rather uneven state of development that characterizes archeological studies today, not only at a world-wide scale but even within its birth place, the european continent, is therefore a critical limitation to the potential of "contextual" approaches.

A more important problem, and one that deals with the core of these approaches, is that one has to bear in mind that the symbolic nature of material culture cannot be taken as a **given**, but rather as a **product**. That is, that it is not until late in the history of Homo sapiens that evidence of symbolic behavior (such as burial or art) is seen in the archeological record. This means that, for most of the paleolithic, that is to say, for most of the history of humankind, the only general theoretical framework that can be used to interpret the archeological record is the one supplied by the ecological sciences, and, more specifically, evolutionary ecology and the socioecology of living primates. This amounts to the same thing as saying that archeological interpretation will have to revolve around the concept of adaptation, and that "Archeology as Human Ecology" (Butzer 1982) will be, at least in the current state of our knowledge of the subject, the only way of doing a rigorous scientific archeology of early man. Indeed, it

is no wonder that it is precisely in the field of lower and middle paleolithic studies that the search for the phylogenetic reconstruction of "archeological cultures" has proved the most misleading as a strategy for knowledge. A failure that is better highlighted by the success of recent studies of variability in early paleolithic artifacts (for instance in the mousterian) as a function of raw-material economy (Dibble 1988). On the other hand, the current debate over the issue of early man as a "noble hunter" or a "carrion robber" (Selvaggio 1987) is a good example of some of the key issues that cannot be settled without recourse to ecological principle, as shown by Tooby and De Vore (1987).

Even when dealing with not so remote time periods, that is, even when symbolic behavior can be assumed, one also has to bear in mind that the degree of freedom from ecological constraints, that is, the number of equally viable ways of organizing a society, depends to a large extent on the technological level attained and is certainly not unlimited. This is obviously especially true for hunter-gatherers, and has as a consequence that for upper paleolithic and mesolithic societies the study of adaptations is still of great importance. This may not be the case any more when neolithic and proto-historical agricultural societies are concerned, but, even in what regards societies where the degree of freedom from environmental constraints is larger, one has to bear in mind that "we are dealing with thermodynamic systems, not simply with cost-free symbolic codes" (Binford 1986:468). However, a remark against "processual" approaches is also necessary here. As a matter of fact, and as one should expect given the predominance of positivist ideas and of the "covering-law" model among new archeologists, studies of adaptation tend to be mostly concerned with showing how a specific set of behaviors was necessary in the given environmental conditions. Instead, the mapping out of the whole range of possibilities from which such a set came to be selected, taking in full consideration, as an important variable, the range of possible social relations through which the environment had to be exploited, would surely produce results of a much higher explanatory value.

This position assumes of course that the food residues and the utilitarian artifacts upon which it is possible to reconstruct past adaptations, past settlement-subsistence strategies, are discarded by hunter-gatherers in a way that does not radically distort what was done with them in real life. That is, to give just an example, that people did not burn or throw into the rivers the bones of the animals they used for food, while discarding around the camp the bones of their dead pets. Symbolic attitudes towards refuse that result in such distortions have indeed been identified and presented (Hodder 1982d), but one can hardly see how arguments derived from african peasants, european gypsies, american hippies, or London punks, can be

considered "contextual" to the discussion of hunter-gatherers. Although these examples are important as cautionary tales, what seems to be necessary is to discuss, in so far as paleolithic and mesolithic hunter-gatherers are concerned, whether there are any indications that similar behavior might have occurred in the past. To my knowledge, no such indications exist, and, at least in this instance, the assumption that middle-range theory and actualistic studies (Binford 1977) are a possible and viable way of deriving past dynamic behaviors from present static patterns of discard therefore seems justified, at least until proven otherwise. The question of whether middle-range laws can be considered universal is a totally different problem, and certainly one cannot expect that the same patterns of carcass exploitation will be valid for Nunamiut reindeer hunters as well as for medieval cattle butchers. The question, however, is whether they are valid as an independent means of measuring the archeological situations that inspired Binford's ethnoarcheological investigations of the Nunamiut: that is, the sites left behind by paleolithic reindeer hunters.

The positive answer that this question in my opinion deserves is obviously based in a point of view regarding the nature of human societies that has been underlying all the above remarks and that, therefore, at this point of the discussion needs to be made fully explicit. That is, it stems from the position that the realm of ideas is not independent from the material base of a society, but, on the contrary, it is conditioned, and in the last instance, determined, by that same material basis. Which means that, therefore, given a specific material base, the corresponding ideology can to a certain extent, and only at a high level of abstraction, be predicted from it. In as much as archeology as a restricted scientific field is concerned, these are of course assumptions that cannot be evaluated from within the discipline, and whose validity can only be argued from the point of view of general social theory. Regardless of whether one agrees or disagrees with these assumptions, the fact remains, therefore, that symbolic approaches, in as much as they aim at dealing with the social and ideological realms, are necessarily heavily dependent on the theoretical production of the social sciences. Archeology is not, and cannot be, self-sufficient in this regard. What archeology can and should do is to utilize the evidence from ethnographic and historical studies in order to establish criteria of "contextual" relevance that can be used when considering the pre-historic record. This in turn means assuming the uniformitarian principle that past social formations do not differ radically from those documented in the historical and in the ethnographical records (Dalton 1981).

In the light of this, the optimism of some "post-processual" archeologies should therefore be considerably

moderated. That is, it is only through the anthropologically grounded mediation of the ideological predictions stemming from the analysis of the material base that meaning can be assigned to the past, not through the internal analysis of the archeological residues themselves, no matter how careful, exhaustive, and "contextual", such an analysis can be. This criticism should not, however, be used to obscure the fact that post-processualists do deserve all the credit we can possibly give them for the emphasis they put on the need to move away from the doomed to failure search for universal laws of social behavior that characterized most "processual" archeology. This was indeed a necessary step in the way towards the development of studies aimed at generating those "social formation specific" laws that the interpretation of the archeological record badly needs. By "social formation specific" laws I mean the laws of relationship between base and superstructure that characterize different modes of production and different social formations, as implied by the principle of the specific validity of categories (Korsch 1971).

Although some work along these lines has been developed by ethnologists (Godelier s.d.), the still underdeveloped state of this kind of research is therefore one of the main limitations to the viability of symbolic approaches in the archeology of today. In so far as the paleolithic is concerned, however, there is also another important problem: the low level of chronological resolution that characterizes our data. As Hodder (1986:10) put it, the examination of where things come from, the problem of the origins of cultural traits and their diffusion, is an important one. As a matter of fact, once the adaptationist program of most "processual" approaches, which tends to reduce the explanation of all culture to issues of survival (and thus sometimes originates what Gould and Lewontin [1982] have called "panglossian reasoning"), is superseded, the explanation of particular cultural traits can only be historical, that is, found in the past events that shaped the society under study. Enquiries into such events may however prove difficult if the minimum amounts of time that can be discriminated as different moments of the chronological scale have durations in the order of five hundred to one thousand years, as is the case in the upper paleolithic. Events or processes of diffusion that presumably can take place within a much shorter time range therefore appear as occurring simultaneously across wide areas. This is for instance the case with the appearance of the solutrean. Assemblages where the use of the abrupt retouch was replaced by the use of the flat retouch to produce lithic points are known or presumed to date to ca. 21,000 BP in France as well as in Spain and in Portugal. The new technological fashion probably originated in a particular region of this large area and subsequently diffused to the other regions that compose it. The establishment of how this happened would

certainly be an important contribution to the history of the period. The lack of chronological resolution, however, means that one can predict right from the beginning that the search for such an epicenter as an archeological problem worthy of substantial research will most certainly be a rather sterile enterprise.

This is not, however, the only way our "geological myopia" affects the study of the pre-historic past. The concern of "post-processual" archeologists with explaining change through the consideration of the ways individuals and social groups act in the concrete events that shape the transformation of societies can only make sense if one is able to discriminate time periods at the scale of the individual's life span or of the generation. For most of the situations we have to deal with in prehistoric studies, however, such resolution is just not possible. This means that, in general, social reality can only be attained at a different level: the level of the structured long-term results of the complex interaction of the different individual and social forces that once acted within societies. This is a level of reality of which the participants in such interactions could hardly have been conscious, and is the level of reality with which prehistorians should be concerned. This point was already raised by Binford (1986:474-5), in a paper where the question of the participant's perspective is, in my opinion, dealt with in very adequate terms:

"We have a chance to understand humankind in a way that no participant, or no social scientist addressing the quick-time events of direct social experience, could ever imagine. To fail to recognize this potential, to fail to grasp a new understanding of humankind from this different perspective -- the perspective of macroforces that condition and modify lifeways in contexts unappreciated by the participants within complex thermodynamic systems -- is quite literally to 'abandon our right of birth'".

And also:

"For a long time archeologists have had an inferiority complex relative to ethnologists and cultural anthropologists. We were convinced that the participant perspective and its personalized scale of experience was the only reality. The archeological record was viewed as a poor distorted reflection of this assumed unitary reality. Surely we need to develop links between the varying scales of perception [...]; but more importantly we must realize that we have the opportunity to study scales of reality that are experientially denied to the ethnographer. The appropriate action for us is not to lament the 'limitations' of the archeological record but to appreciate the limitations of the ethnographic experience and the records and ideas that arise in the ethnographer's brief touch with a circumscribed reality. The archeological record documents a broader and potentially more fascinating reality."

In subscribing to Binford's point of view in this regard, I do not wish to state that causal explanation is the only type of explanation valid, or sufficient, in historical studies. As pointed out by Schaff (1977), understanding and reconstructing the intentions behind the actions of individuals and social groups, that is, searching for finalist explanations, is also necessary. Actually, it is this need that constitutes one of the main differences between social sciences and natural sciences. This is a point where Hodder (1986:23-25) rightly insists, convincingly showing that the notion of function assumes the notion of end. For instance, in what regards megaliths, "the theory about the social functions (competition, legitimation) is based on a theory about what the tombs meant (ancestors, the past)". However, when dealing with the remote past, the prehistorian is somewhat close to the situation (hypothesized by Schaff as a way of showing the importance of understanding motives, ideals and aims as an important part of explanation) of the terrestrial historian who would have to deal with an extraterrestrial culture. Given the absence of a common psychosomatic structure that might serve as the material basis on which an attempt at empathy with such a culture might be grounded, it would be impossible to translate its language into our own, and, therefore, it would be impossible to understand it, even if it could be shown that the participants in that culture acted in a conscious way and pursued well-defined aims. The position of the pre-historian is of course not as desperate. We do share the same biology with the megalith builders, and the question of which were the motivations (regardless of our evaluation of them as false or true consciousness) of the individuals involved in their erection is not an entirely hopeless one because we can find out, through enquiries into societies we can observe, about the attitudes behind such behaviors that exist in similar social formations. However, the fact that we lack the materials used by historians to answer such questions -- the written documents -- has as its consequence that they can only be addressed, as discussed above, at a high level of abstraction, in a cross-cultural way, and through the mediation of the ethnographic record and correlative assumptions.

Binford's statement that questions of **why**, and not of **what for**, are those we are best equipped to deal with, is, therefore, an adequate assessment of the current potential of pre-historic archeology, and, at the same time, illustrates one of the main differences between history and pre-history. It is perhaps worth noting, in this regard, that, in the one example of the assigning of meaning through the "associational, contextual" analysis of the archeological materials considered by Hodder (1986:45-46) as most convincing (McGhee's analysis of the arctic Thule culture), we are dealing with a situation where historical information is available. The dichotomies land/sea, man/woman,

winter/summer, said to be behind the antler/ivory dichotomy observed in the Thule material culture were, as a matter of fact, known to exist in historic Inuit mythology. Hodder comments on the fact that "such evidence (...) simply supplies more contextual information concerning the hypothesized structure and its meaning". However, it seems obvious that what is going on in this instance is exactly the opposite: not the deduction from the archeological materials of an interpretation that is confirmed by ethno-historical information, but an illustration in the realm of material culture of a mythological theme which was already known to exist. From Hodder's account, then, one is thus led to the conclusion that McGhee's work is a good example of the potential of a very traditional methodology (the "direct historical approach"), not a demonstration of the validity of the assignment of meaning through careful "contextual", structural, analysis.

3. Validation in archeology

The fact that one agrees with Hodder's criticism of the imposition from the outside, without adequate consideration, of supposedly universal meanings that are often implicit, undiscussed, and biased by a western view of the world, does not necessarily mean, therefore, that one agrees with his solution to the problem. On the contrary, the analysis of structured sets of differences, the discovery of generative rules, the assimilation of different spheres of human activity to the same underlying scheme, that is, the internal, structural and "contextual" analysis of particular situations **without reference to any trans-cultural generalizations**, inevitably raises the issue of verification, as Hodder (1986:49-53) does acknowledge. How can it be demonstrated that the meaning assigned by the archeologist is correct, once Lévi-Strauss's assumption that all variation can be reduced to variation of unconscious, immanent, logical rules produced inside the human mind, is abandoned?

This is a problem that is not exclusive of Hodder's approach. Indeed, most "post-processual" studies have failed to formulate adequate practical tests for their conclusions. This lack of concern with testing may, as Leone (1986) thinks, arise out of the critique of processualists as being so concerned with the degree of certainty over conclusions that they tended to reduce them to subsistence and numbers of things, and to ignore social and symbolic relations and the role of humans and of tradition, therefore opening a chasm between archeology and society. That this was a real problem is perhaps highlighted by the new archeologists' use of the material culture of american Indians as a way to substantiate general anthropological laws, instead of a way of reconstructing their past history (Trigger 1986) - an attitude that certainly has something to do with the conflict that currently opposes the american

archeological establishment to the aboriginal communities.

However, the question of whether our reconstructions are accurate or not is still an important one, even from the point of view of the relationship between archeology and society. By showing that modern society is the contingent product of certain developments that might as well have turned out otherwise, archeology, as is also the case with history, undermines the prevailing teleological ideology. Therefore, it contributes to the task of making humanity conscious that present day society can and will change, as did change past societies. That is, that modern bourgeois society is not the "final cause" of mankind's history. Of course, the credibility of this message will increase with the capacity of archeologists to show that their reconstructions of past events are accurate, that they are not mere phantasy, or just another way, as good as myth, folk-tale, or teleology, of accounting for that past. Thus, the validation of interpretations is a step of the archeological reconstruction of the past that cannot be avoided, if our discipline is to be a socially responsible one.

Trigger (1983) and Gallay (1986) have suggested what seems to me the only way this can be done: the verification of *retrodictions*, that is, the confrontation of predictions derived from the models, and regarding still unknown aspects of the archeological record, with newly investigated parcels of that same archeological record. A practical example at hand is Straus's Abri Dufaure project, which aimed at checking whether the findings made at the nearby Abri Duruthy were replicated at the new site. Such a replication, if verified, would contribute to the validation of the interpretation of the Pastou cliff, where both shelters were located, as part of the regular, repeated pattern of human settlement and subsistence activities related to the structured availability of major food sources (reindeer, horse, bison and salmon) that was derived from the analysis of the Duruthy materials. Excavations were therefore carried out at Dufaure in order to test the prediction that it would contain magdalenian and azilian occupations with the same structural properties of those recognized at Duruthy (Straus 1988).

Hodder's position in this regard is however a particularly dangerous one, especially when analysed in the context of his flirt with Collingwood's ideas (Hodder 1986:93-102), and therefore needs to be discussed in some detail. The stance adopted by Collingwood that the past cannot be known with any certainty is echoed by Hodder in a few passages of his book, and helps to explain his lack of concern with verification. In the introduction, for instance, he clearly states that "there can be no 'testing' of theory against data, no independent measuring devices and no secure knowledge about the past" [1986:16]). What Collingwood concludes from these

premisses that Hodder seems to share is that, therefore, the ultimate aim of historical studies can only be self-knowledge ("in projecting ourselves into the past we come to know ourselves better"). This, however, is an obviously self-contradictory statement. If the past cannot be known, how can we know ourselves through it? Psychoanalysis, social psychology or sociology surely would seem to be much more effective ways of achieving such an ultimate aim, and therefore the question of why should we do history or archeology at all necessarily arises. The only logical answer to this question that gives a meaning to the doing of history or archeology is that historical studies are nothing more than a way of justifying through the past the present day political or ideological aims of individuals or social groups.

This open relativism is however rejected by Hodder as too extreme a solution. In other passages of his book he does state that "it is possible critically to evaluate past and present contexts in relation to each other, so as to achieve a better understanding of both", and that "there is a human mental ability to conceive of more than one subjective context and critically to examine the relationship between varied perspectives" (1986:169-170). In following Collingwood, however, he suggests that such examination, that is, the discussion of the validity of the interpretations, can only be done through discussing their coherence. By coherence it is meant both external coherence, that is, correspondence with the evidence, the real data, and internal coherence, that is, "internal coherence of the argument" and compatibility with "the archeologist's picture of the world" (Hodder 1986:95). Since, as it is obvious, no one in his sound mind will propose a reconstruction that is not in the first place coherent with the available data, it remains that it is the internal coherence that in the last instance validates the reconstruction. However, the demonstration of the internal coherence of an argument is a method of testing that is valid only in the field of the formal sciences, such as logic or mathematics, but that can only lead to speculation in the field of the objective sciences. For these, truth can only be evaluated through practice, that is, through the confrontation of the propositions with the empirical realm from which they were inferred or deduced, for instance along the already mentioned lines suggested by Trigger and Gallay.

If (as I believe it should) a theory's success is to be measured as a function of its usefulness as a guide for action, in our case as a guide for acting upon the archeological record, then "post-processual" archeologies, and particularly Hodder's "contextual" approach, seem to be at a crossroads. For the time being, the emphasis on finding about the "participant's perspective" seems to have had a result opposite to the one intended, that is, the one of moving archeology two steps back into speculative story-telling, instead of one

step ahead into becoming a fully scientific historical discipline. The future of these approaches therefore largely depends on their ability to provide the practical tests for interpretations that "processual" archeologists do have the merit of having tried to develop. They were not, however, the first ones to have done it. Testing retrodictions is a behavior that has been associated with archeology ever since it established itself as a scientific discipline. For instance, it was not by showing the internal coherence of his argument, but by testing predictions derived from it against the archeological record, that Schliemann was able to demonstrate the existence of Troy. Conversely, despite displaying an internally coherent argument, on one hand, and being to a large extent externally coherent with the data, on the other, Clan of the Cave Bear can hardly be considered an archeological interpretation of the past. I suggest that the essential difference between the work of Schliemann and that of Jean Auel's is that the former's formulations were verifiable, which is something that cannot be said about many "post-processual" studies.

4. Objectivity and truth in historical studies

In stressing the issue of validation I am of course assuming that an objective knowledge of the past is possible, a statement that needs to be confronted with the fact that professional historians and pre-historians often present different, if not opposed, interpretations of the same data, and that history is being constantly rewritten as generations go by. This is an issue that has been extensively dealt with by Schaff (1977), to whose positions on the matter, superseding the debate between positivists and presentists (a debate that is to a large extent echoed in the discussions between "processual" and "contextual" archeologists), I subscribe. Schaff showed that the issue of objectivity in history is in the first place a philosophical one, one that has to do with ontology and gnoseology. It is from a materialist and realist point of view -- history exists as fact, not only as discourse, knowledge is a construction of the subject, but one that reflects, although not as a faithful copy, the external object, true knowledge is the knowledge that is in conformity with the external object -- that he addresses the problem of the limitations which derive from the active role played by the subject in the process of knowing. These result from the fact that the subject of knowledge is a historically and socially determined being. The product of the act of knowledge is therefore an image of the real past composed of accurate reflections of the known object (the actual events and processes that once took place), distorted by the social and historical conditioning of the knowing subject, and knowing is a process, the process of approaching absolute total truth through the developmental accumulation of successive partial, relative, but objective, truths, constantly superseded as research progresses.

In this framework, then, it becomes possible to understand the existence of different interpretations of the same past, without denying, at the same time, the possibility that it can be known. That is, from the nature of the process of knowledge as sketched above, it necessarily arises that: all interpretations will share common elements; as research advances the amount of common elements accepted by all interpretations will increase; in well defined restricted areas of the process of knowledge it will be possible to arrive at exhaustive conclusions, unchangeable by future work. The validation of this model of knowledge in what regards our scientific field therefore demands the development of a genetical epistemology of pre-historical archeology, and, thus, the reasons why its great necessity was stressed at the beginning of this paper should now be clearly understood. It is only in order to show how this model provides, in my opinion, a good explanation of the actual development of our discipline, that a concrete example -- the history of the solutrean as a concept (Zilhão 1987) -- will now be briefly discussed.

The paradigm that prevailed in nineteenth century archeology viewed the archeological record as containing the demonstration of the validity of the Law of Universal Progress (Mortillet 1867), the role of scientific research being the one of elucidating the concrete stages (conceived in the manner of geological periods) of humankind's unfolding. In the framework of this paradigm, then, the only questions that made sense were those of whether the laurel-leaves and associated index fossils defined just a local facies or a truly separate period and, if so, which was their chronostratigraphical position inside the paleolithic. Breuil's demonstration that the solutrean's correct position was between the upper aurignacian and the magdalenian (Breuil 1907, 1913) settled this controversy, but at a time when the shift towards the culture-history paradigm was already under way. Under the new paradigm, the universal nature of the solutrean underlying its definition as a period was rejected, a result that came also through the development of research not only in other european regions outside of the Périgord, but also in other continents outside of Europe. However, this new understanding of the solutrean as a regional archeological culture was built upon the core of its definition as outlined in the context of the earlier paradigm. This culture was still viewed as characterized by the manufacture of the several foliate types previously recognized, and its chronostratigraphic position between the gravettian and the magdalenian, in the restricted area of western Europe (Iberia and France, to the south of the Loire and to the west of the Rhône) where it developed, was also maintained.

In the framework of the new paradigm, however, new questions were also raised. The identification of the solutrean as an ethnic entity led to research aimed at establishing the geographical origins of these people,

the routes of their invasion of western Europe, and the chronology of their establishment and further development in the different areas where they replaced the local gravettian peoples (Smith 1966). Explicit historical analogies, such as the arab occupation of Iberia in early medieval times, were used as models for such a replacement process (Jordá 1955). Recent research, basically oriented by a new archeology perspective, has rejected most of the schemes produced by these authors, as well as the well-founding of the issues (origins, influences, migrations) they aimed at solving (Straus 1983; Zilhão 1987). This was a consequence not only of paradigm shift, but also of the production of fresh archeological evidence, which made clear, for instance, that the solutrean began at approximately the same time throughout the entire area where it developed, a result that clashed with one of the basic foundations of those schemes: the identification of the Périgord as an epicenter from which, in later stages of its development, the solutrean had expanded into Iberia. However, these approaches also retained one basic definitional aspect of the culture-historical view of the solutrean: the space boundaries it had established, on which was grounded its characterization as a **technocomplex** (Clarke 1968), not as an ethnic culture. The analyses of adaptation and of inter-regional contact addressed in these two syntheses were therefore built not only upon the rejection of important parts of previous interpretations, but also upon the maintenance of no less important parts of those same interpretations. Namely, the time (acquired in the framework of the universal progress paradigm) and space (acquired in the framework of the culture-historical paradigm) boundaries of the phenomenon, which can be considered as one of those objective partial truths, acquired by exhaustive research of a restricted area of scientific knowledge, shared by all interpretations and unchangeable by future work, whose existence was predicted by the model of the process of knowledge outlined above.

5. Conclusions

To conclude these somewhat disparate reflections, I would like to summarize, under the form of short answers to complicated questions, the way I view the problems at stake:

a) **On the nature of archeology** -- Archeology is a historical discipline with both the particularizing aim of reconstructing past societies in their concrete totality from their material culture residues, and the generalizing aim of elucidating the ways and processes that ruled their change across time and space.

b) **On the relation of archeology with anthropology and the other social sciences** -- Archeology uses laws of general or "social formation specific" behavior derived from the social sciences, in order to reconstruct, from the material culture residues, the

missing realms of past societies. At the same time, archeology, as is the case with history, contributes to the social sciences by studying long-term processes that lie outside the empirical domain of the latter.

c) On the need of a rigorous, rational, methodology, as opposed to speculative, uncontrolled by practice, manipulations of the data -- Archeology can contribute to the undermining of the finalist ideology of modern bourgeois society only if its conclusions have credibility. Therefore, the development of tools (both theoretical and practical) that can help us in getting at better and more rigorous interpretations of the past is of the greatest necessity, and need not be considered antagonistic with a humanistic, politically active, understanding of the discipline. Regardless of whether our statements are or are not socially acceptable at a certain moment, we still need to be able to assess the extent to which they are right or wrong, if we want people to believe in what we have to say instead of believing in the word of God, not to mention other less heavenly competitors such as Von Daniken and his like.

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