

THE
LIMITATIONS
OF
ARCHAEOLOGICAL
KNOWLEDGE

Edited by
Talia SHAY
and
Jean CLOTTE



Légende couverture :

"Grotte des Trois-Frères (Montesquieu-Avantès, Ariège, France). Personnage appelé "Sorcier à l'arc musical", relevé par l'Abbé H. Breuil. Cette gravure d'un être aux jambes humaines et au reste du corps animal (bison), tenant un objet énigmatique, interprété à tort ou à raison comme un arc musical, illustre bien les limites de nos connaissances".

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Etudes et Recherches Archéologiques de l'Université de Liège, n° 49

1992

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Dépôt : D/1992/0480/4

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FOREWORD AND AFTERTHOUGHT

by

Talia SHAY and Jean CLOTES

A volume whose title proclaims the limitations of the discipline of archaeology naturally challenges the reader to examine such a view. The purpose of this paper is to justify the title of this book by making explicit what some of the limitations of archaeology are, and how in view of these limitations archaeology should continue to acquire its knowledge.

G.B. Collingwood, a philosopher and archaeologist, argued in his 1926 lecture on the problem of historical consciousness that "the only possible object of knowledge is something that is real now. Of the past as a past ... we can have only conjecture, better or worse grounded" (Collingwood 1975 : 568). Over the years, a number of other archaeologists have also recognized the past as a temporal realm distinct from the present (Piggot 1950; 1965 : Daniel 1962); and it was Binford (1938 : 18-20) who asserted that "the observations we make about archaeology are here and now, contemporary with ourselves". The very same idea is expressed by Lowenthal (1985) in his poetic book *The Past is a Foreign Country*. Drawing on all the arts, humanities and social sciences, the author examines the ever-changing role of the past in shaping our lives, as well as the continuous reshaping of the legacy of the past by each generation in line with current needs. Every act of recognition or protection of a relic, Lowenthal suggests, affects its form and our impressions (Lowenthal 1985 : 263).

To recognize that historical knowledge is invariably changeable may seem self-evident for some of us today. It is also consistent with recent studies in other sciences that consider the non-epistemological influences that shape the processes and products of research (Kühn 1962; Feyerabend 1975; Jackson and Willmott 1987). As a result of this atmosphere scholars have come to consider themselves as part of the cultural tradition that they wish to interpret, and call for criticism of the presuppositions underlying their theories and moral norms (Hesse 1988 : 332).

The philosophical notion that truth is relativistic has been carried over to archaeology. Western archaeology is now concerned not only with a search for facts but also with the examination of the non-epistemological effects on its theories and practice. Trigger (1984, 1986), for instance, relates traditional archaeology to nationalistic or colonial interests. Nationalistic interests, Trigger asserts, tend to focus on the predominant culture rather than on smaller communities; and to concentrate attention on periods when the major culture was in power. In regard to national archaeology a recent review by Litvak King (1985) of the archaeology in Mesoamerica presents it as provincial and chauvinistic (see also Shay 1989). Colonialistic

orientations, on the other hand, are associated by Trigger with the diffusional view of traditional archaeology, by which cultural changes are explained in order to denigrate native societies under colonial rule which are regarded to lack initiative to develop on their own culture (Trigger 1984 : 363). According to Trigger, the background assumptions of the new archaeology are different from, and are related to, imperialistic interests. New Archaeology emphasizes universalism and denies the validity of studying national traditions that stand in the way of American economic and political activity.

Background assumptions are related today not only to theories but also to archaeological practice. For instance Tilley (1989 : 279) associates excavations primarily with the socio-political interests of the present; on the other hand Leone (1981 : 5) asserts that the display of archaeological materials in museums turns them into a piece of modern material culture, to be analyzed for what it tells of the culture that created it, but not about the past *per se*. Not everyone accepts Leone's attitude, and the liberties taken by modern museology in interpreting and presenting the past are denounced by Bronowski (*Ha'aretz* 14.9.90) as being a deception which leaves nothing of the magic and authenticity of the past. The deception to which Bronowski refers is that of Lascaux II, a copy of the "Sistine Chapel" of French prehistory executed by modern artists for the presentation of the 17,000 year-old paintings found in the cave.

The moral implications of the archaeological activity are also being discussed at length today primarily by examining the conflict between traditional archaeological values that dictate its responsibility to acquire knowledge and other value systems in the modern world (Green 1984; see also Bahn, Siegel and Shay in this volume).

As we might expect, scientific language betokens the emergence of these new trends, since those scientists who deliberately insert traces of the present into their research also use less formal language as well. Thus, while in the 1950s Daniel, in his book *A Hundred Years of Archaeology* (Daniel 1950 : 10) apologized for presenting to his readers what seemed to him important rather than a catalogue of discoveries, the archaeological language of the 1980s is denounced by Hodder (1989 : 271) for being impersonal and objective as though its descriptions were self-evident.

The awareness that the past is ever changing is only recent. Many archaeologists still believe that their knowledge of the past cannot be acquired by way of a historical and cultural plurality of endeavors, but that it is rather a unitary (i.e. non-pluralistic) and technical product which derives its authority from adherence to an impersonal method. As an example in kind of this approach, which in other sciences is termed absolutist (Jackson and Willmott 1987 : 364), we should like to cite a recent popular article by Ben Dov (*Ha'aretz* 16.11.90), who rejects the criticism of being politically biased in his discoveries at the Temple Mount, by arguing that his excavation was founded on the conviction that an archaeologist can present objective facts relating to earlier periods regardless of the political issues of the day.

As this approach to knowledge of the past is also founded on the conviction that bias can be eliminated by the application of proper scientific method, it allows for only limited self-reflection. One might mention in this regard the puzzling silence of Israeli archaeologists, who adhere to material facts but have thus far ignored the values which underlie their theory and practice, as well as the ethical implications of both (Shay 1989).

The absolutist approach to historical knowledge almost universally espouses analogy as an appropriate procedure for making inferences about the past. "Archaeology as a whole is analogy" says Chang (1967 : 109), "for to claim any knowledge other than the objects themselves is to assume knowledge of patterns in culture and history and to apply these patterns to the facts". Contextual archaeology of the 1980s also depends on analogy for its interpretation of the past. Hodder, for instance, asserts that "the meaning of objects is derived from the totality of its similarities and differences, associations and contrasts... cultural meanings (within the systematic approach of new archaeology) are imposed... from the outside" (Hodder 1986 : 25, 138; also see Hesse below). Accordingly, inferences concerning the past take place by analogy with similarities and differences between particular cases, and not by law-like explanations which may affect the objectivity of the research.

Science, too, rests essentially on analogical inference from the concrete, says Hesse (see below). But in contrast to the humanities, science goes beyond analogy and seeks law-like relations in its pursuit of the scientific "objectivity" (see also Hesse 1988 : 331). The use of analogical inference by archaeologists has been questioned by Binford (1972 : 14-21) and Gould (Gould and Watson 1982 : 372) who pointed out the difficulties of validating such inference and of producing an interpretative principle from the particular data upon which analogy is founded.

In our view the application of analogy by traditional archaeology may be associated with the conviction that the physical phenomena of the universe have inherent and immutable features (Fitting 1973 : 288) whose similarities and differences are self-evident to the observer who can thus compare them in order to make inferences concerning the past. As for the use of analogical procedure by contextual archaeology, it was pointed out by Binford (1986 : 401-402) that there is a paradox in the adherents of this approach appealing to cultural relativism (Shank and Tilley 1987 : 2).

In assessing the two archaeological approaches toward knowledge of the past, it is questionable whether the epistemological assumptions underlying traditional archaeology can be reconciled, as was recently hoped (Trigger 1986 : 1), with the conflicting assumptions that non-epistemological influences shape the processes and product of research. One would rather think that there is only very limited dialogue possible between those who are convinced that they can uncover and preserve the past as it was and those who believe that the past is continuously altered.

Although traditional archaeology is hardly concerned with self-reflection, it has nevertheless given thoughts to the limitations on our knowledge of the past. These limitations have been attributed to the incomplete state of the archaeological record, as well as to the assumption that material remains have in the course of time lost those non-material features which pertain to the sociocultural system under study (Binford 1972 : 21).

In the late 1960s, Binford took issue with this view of the matter and argued that the limits of inference about the past from the archaeological record are not set by the nature of the record, but depend on our theoretical assumptions and our methodological ingenuity (Binford 1972 : 22-23; Shenan 1989 : 832). However Binford's approach has been criticized lately as being partial, and ignoring the complexity and creativity of cultural systems (Hodder 1982 : 4, 5; Trigger 1986 : 11; Shenan 1989 : 832).

Traditional efforts to construct and evaluate knowledge of the past in relation to the purely epistemological, timeless conception of scientific method are today seen as being illusory. The new criteria offered by new archaeology (Binford 1972 : 17) for the accuracy of our knowledge of the past are also linked to a certain extent to the absolutist conception of knowledge (Trigger 1986 : 2).

In contrast to the approaches set out above, the self-reflection of today's archaeology on the limitations of the discipline derives principally from the conviction that past and present are not exclusive, and that our knowledge of earlier times is contemporary with ourselves and is thus changeable.

These changes of paradigms in archaeology involving a new definition of scientific truth conflict with the traditional absolutist concept of perfect knowledge, but are congruent with the recent relativistic approach toward knowledge of the past. According to the latter, scientific progress results from the concentration of each generation on different goals and its defining anew of the limitations of the discipline (Kühn 1970; see also Shenan 1989 : 832).

In this regard we would disagree with our friend Bernard Siegel (see his article below), who claims that the new archaeology has a short history. New archaeology is criticized today in the light of recent tendencies which have emerged in the discipline (Trigger 1986 : 10-11). Nevertheless, there is no doubt that it has enormously widened and altered the range of factors considered to be relevant to comprehending the variability in the archaeological record to date. As a consequence today's archaeology is no longer bound by the limitations set by traditional archaeology – a case in point being contextual archaeology which emphasizes the role of ideas and beliefs (Hodder 1982 : 11, 12; Trigger 1986 : 5).

Recognizing that our knowledge of the past has been altered continuously, we confront the question of value. Thus if no objective facts of the past can be found by archaeologists, what then should the value and goals of our discipline be ?

Yet to see why and how scientific knowledge of the past is changed helps free us from past notions of perfect knowledge that are no longer relevant to the modern scientific quest, and to accept that any historical knowledge which we may find is no more final than that previously found. In replacing the traditional search for absolute historical truth with the pursuit of tentative truth, the relativist approach encourages a continuous re-examination of historical knowledge as well as an openness to assessing different claims and strategies of research in archaeology.

Most archaeologists would agree that archaeology should be related to other sciences (Steward and Seizer 1938 : 5-7; Trigger 1986 : 6; Meyers 1984). Moreover, in a symposium on related subjects (see below), Funkenstein has suggested that unless vital relationships are maintained with other disciplines, a discipline could no longer be regarded as scientific. It is our view that archaeology should favorably consider replacing its absolutist standpoint with a more relativistic approach; otherwise it may be excluded from the debate taking place in other sciences (Jackson and Willmott 1987 : 361).

In broad outline, our argument is that since the attempts to discover perfect historical knowledge and to purge archaeology of its non-epistemological influences have proven to be

illusory, archaeology should direct its concerns toward the development of a theoretical framework of pluralistic approaches for the comprehension of human behavior in the past. The traditional claim, therefore, for the study of "archaeology as archaeology" and Binford's argument in behalf of considering "archaeology as anthropology" should now be extended to embrace as well the notions of "archaeology as religion" and "archaeology as art and aesthetics" and so forth. Such pluralistic efforts to adopt the viewpoint of various disciplines would doubtless broaden the spectrum of our knowledge of the complexity of human activity in the past.

Our conviction regarding this kind of archaeology has resulted in Talia Shay's efforts to organize a symposium entitled "The Objectivity/Subjectivity of Science" for the annual meeting of the Israeli Anthropological Association in Tel Aviv University in 1989. Some of the articles appearing in our volume were discussed for the first time during the 1989 meeting. This symposium represented a counterbalance to the official position of Israeli archaeology which has no interest at present in studying the sources and limitations of its knowledge, and rather prefers to adhere to Biblical and traditional studies of the region's past (Shay 1989).

Although this introduction has outlined some avenues of explorations in archaeology, it would be incorrect to consider this volume as representing a single viewpoint.

The papers collected in this book were written by a variety of authors from different parts of the world : Africa (Ghana, North Africa), Europe (England, Belgium, France, Spain), the Middle East (Israel), North America. They illustrate the plurality of approaches to archaeology that we find so necessary and they can be arranged under two main labels :

1. *Those dealing with politics and ethics :*

Gonzalez Morales gives a first-hand account of the wide-ranging influence that local authorities have been exercising over research in Cantabria since the political system in Spain changed from a centralised government to local "autonomias".

Braun acknowledges the strong bias existing in Israel in favour of Biblical Archaeology and its detrimental effect upon the study of non-Biblical periods. However he argues that it is mitigated by the consequences of a policy of salvage archaeology, presumed to be more objective.

About the same country, Syon gallantly presents "the other side", that of a nationalist archaeologist, whose position rests mainly on the facts that on the one hand it is doubtful whether "anybody can escape from his ideologies in any of the roles he plays in life", and on the other hand that "no evidence is deliberately withheld or distorted."

Agorsah's is a case study of archaeological research in Ghana, where he shows how preconceived notions, in particular diffusionism under the influence of Eurocentric ideas, for some decades obfuscated reality.

Shay describes the conflicting positions in Israel of the religious circles that believe in the sacredness of the dead, and, on the other hand, of the archaeologists who, contrary to some

modern Western tendencies (see Bahn below), adhere to the traditional idea of the capital importance of Science, rising above personal and religious feelings.

Bahn admits as self-evident the limitations of archaeological knowledge and addresses the problem of excavating the human remains of foreign native societies and of what to do with them; i.e. he is ready to accept some more limitations because of the ethical issues involved.

A well-known anthropologist, Siegel, analyses the different kinds of constraints put on archaeology, whether they be politically motivated or result from theories and paradigms. In view of those constraints Siegel tries to foresee the future of archaeology.

2. Theories, methods and their biases :

Hesse remarks that archaeology is beset by two main problems : – theory must be about unobservable events; – a dichotomy exists between a scientific rationalisation of archaeological thought and the symbolic way of thinking of traditional societies with which archaeology deals. In order to overcome these obstacles, archaeologists should make speculative hypotheses on the basis of analogies with comparable systems. This method is not in conflict with other scientific methods as it only emphasizes the local and the concrete and makes no demands upon the universality of law-like relations and theories.

Three other papers deal with Palaeolithic art in Europe.

Lorblanchet, as an example of some unconscious a-prioris that shape and warp research, writes about the triumph (which he deplores and condemns) of naturalism in art studies.

Ucko emphasizes the intimate relationship between the investigator's own preconceptions and the research methodology adopted in the study of Palaeolithic art.

Marshack challenges the so-called "objective" categories and methods so useful to and so favoured by researchers. He thinks that we should recognize the subjectivity of the "maker", i.e. analyse the "processes involved in the creation" of symbols and their different "levels of cultural and individual relevance" rather than the material end products. In so doing he too points to the inherent subjectivity of interpretations.

Camps' paper exposes the limitations of our documentation when it stems from one single archaeological source, as is so often the case. This can be proved when different documentary sources exist, each with its own limitations, but which can then be checked against each other, as is the case with the Neolithic and protohistoric fauna in North Africa.

Clottes deals with "fashions" in Archaeology, which entail an unreasoned rejection or adoption of methods and/or concepts and set up a distorting prism between archaeologists and their work, all the more dangerous as it is mostly unperceived.

Otte and Keeley denounce the excesses of post-diffusionism in Palaeolithic European studies and the present-day tendency to a jingoistic attitude which prevents researchers from perceiving the full scope of cultural and social archaeological problems.

Finally, Heimann tells the romantic and strangely beautiful tale of a contemporary archaeologist whose love for the desert and his home land drove to a sort of wild and mystic quest, to a borderline form of archaeology.¹

This book thus includes many different opinions and examples concerning the development and practice of archaeology. Notwithstanding this divergence of views, and maybe because of it, there is a common thread through it all, which is the search to the question we put at the beginning – to wit : what and where are the limitations on our archaeological knowledge ?²

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¹ Pr. S. Heimann's paper calls for a special comment. I met her a few years ago in the faculty club of Haifa University. As we were the only people in the restaurant, we talked and she told me that she was writing, at the moment, about her experience in the desert, where she had participated in an archaeological survey. While listening to her I could not help thinking of the "Visit to the Master", the final chapter of Flannery's book "Guila Marquits" (1986), which goes into the origins of agriculture in Meso-America. Flannery put to the Master, who is a kind of "Holy Man from the Orient", different questions regarding his explanations of early agriculture. "A Portrait of an Archaeologist", by S. Heimann, similarly inquires into the boundaries between archaeology and reality. It thus provides a fitting ending to this book (T. Shay).

² The work of Talia Shay was supported by the Memorial Foundation for Jewish Culture.

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**ARCHAEOLOGY, POLITICS
AND ETHICS**

RACINES : LA JUSTIFICATION ARCHÉOLOGIQUE DES ORIGINES RÉGIONALES DANS L'ESPAGNE DES COMMUNAUTÉS AUTONOMES

par

M.R. GONZALEZ MORALES *

Même si l'on fait abstraction des biais introduits dans la recherche archéologique par les énoncés théoriques préalables, les *a priori* idéologiques de l'étude ou la perspective du chercheur, il est évident que l'apparition de certaines tendances ou modes de recherche est souvent en rapport avec les conditions matérielles du travail. Dans ce sens, le cas que nous examinerons ici entend démontrer comment le cadre des Etats européens actuels peut poser d'importants problèmes pratiques à la recherche archéologique et entraîner la soumission de cette dernière à des critères strictement politiques. Cette situation se retrouve aussi sur l'autre versant de la recherche, celui de sa publication : de graves distorsions affectent la conception même de la discipline archéologique ou de l'ensemble de la recherche scientifique et les travaux subissent des manipulations dans leur réalisation ou leurs résultats.

Un des éléments négatifs qui jouent à ce propos est le présupposé de l'objectivité de la recherche scientifique ou de l'indépendance relative des chercheurs dans le cadre des conditions établies par le pouvoir politique de régimes démocratiques. Il est pourtant évident – et cela devient un classique du répertoire de la recherche – que nul ne mettrait en doute cette action déterminante du pouvoir politique sous des régimes autoritaires. Nous négligeons pourtant l'existence, dans les circonstances actuelles des démocraties occidentales, de contraintes non explicites – donc souvent ignorées –, et nous oublions de nous interroger à propos du caractère des procédés de la recherche ou de ses manifestations plus pratiques, comme la diffusion de certains résultats.

Une première analyse sur le développement de la politique des Communautés autonomes en Espagne nous mène à la conclusion, extrêmement grave, qu'il existe, dans certains cas, des signes alarmants de ce type de limitation de l'objectivité de la recherche de la part du pouvoir politique. Elle nous révèle aussi jusqu'à quel point ce pouvoir assume, par des moyens indirects, la direction des travaux scientifiques ou la diffusion sélective de leurs résultats.

A titre personnel, nous considérons que ce second aspect est essentiel, dans la mesure où la plupart des chercheurs ne sont pas conscients de l'effet que peuvent avoir les résultats de leurs

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travaux sur l'opinion publique, estimant que ceux-ci s'adressent à des cercles restreints. Pourtant, dans tout processus de formation d'une conscience populaire ou de communauté, ce sont souvent ces résultats externes qui comportent la plus grande transcendance historique, dans le sens strict du terme. On ne peut donc pas établir une séparation tranchée entre une activité de recherche pure et la diffusion de ses résultats, étant donné l'effet transformateur que la recherche historique peut avoir, à travers eux, sur les mentalités ou sur la société.

Un autre élément doit être examiné dans le cadre de la recherche archéologique et nous y porterons une partie de notre attention; le rôle que joue la recherche même sur le patrimoine commun. On a souvent voulu séparer la recherche "scientifique" de l'étude des problèmes pratiques de conservation et de protection du patrimoine archéologique. Pourtant – et c'est encore le point de vue personnel exposé dans cet article – il y a une liaison évidente entre les deux. Dans une certaine mesure, certaines positions politiques adoptées par les pouvoirs publics agissent de façon sélective, en fonction de critères idéologiques, sur les problèmes matériels de conservation, en affichant des préférences pour des vestiges archéologiques déterminés. Ces préférences ne pourraient être justifiées si le patrimoine archéologique était uniquement considéré comme un document historique.

Notre analyse aura pour cadre la région cantabrique, zone classique depuis les premières études préhistoriques, qui compte une longue tradition de recherche. D'ailleurs, la diversité des systèmes d'autonomie politique en vigueur dans les Communautés qui la forment nous permet de mieux établir les contrastes auxquels nous avons fait référence plus haut.

LES DIFFÉRENCES ENTRE LES TRADITIONS DE RECHERCHE

Même si nous pouvons considérer l'oeuvre de Manuel de Góngora, *Antigüedades prehistóricas de Andalucía* (1868), comme le premier travail de recherche préhistorique – *sensu stricto* – de la Péninsule Ibérique, les premières études archéologiques sur les périodes préhistoriques plus anciennes eurent lieu dans la zone nord de la Péninsule. Cantabria avait connu les efforts pionniers de Marcelino Sanz de Sautuola, célèbre par sa découverte exceptionnelle de la grotte d'Altamira et qui avait aussi effectué la première série de fouilles systématiques sur différents gisements (Sanz de Sautuola 1880). Dans son entourage, se trouvaient bon nombre d'amateurs érudits appartenant aux différentes branches des sciences naturelles, comme c'était le cas de Sautuola lui-même et de quelques autres auteurs de l'époque. L'activité scientifique de ces années se concrétisa avec la fondation de la *Estación Biológica* de Santander par Augusto González de Linares, et les fouilles que ce dernier effectua en collaboration avec Salvador Calderón y Arana (tous deux membres de la *Sociedad Española de Historia Natural*) (Madariaga de la Campa 1972), ainsi qu'avec les travaux de collecte archéologique réalisés par Eduardo de La Pedraja. Tout cela fait partie de ce premier élan régional, encore ancré dans une perspective "antiquaire-naturaliste" de la Préhistoire.

Au Pays-Basque, la tradition de recherche remonte à la moitié du XIX^e siècle, avec les travaux de Velasco, Becerro de Bengoa o Baraibar, qui faisaient partie du courant "celtique", cher à cette époque, comme il apparaît dans plusieurs études de I. Barandiarán Maestu ¹ ou dans

¹ I. Barandiarán Maestu (1967) reprend les aspects de base de l'histoire des recherches; ses monographies ultérieures (1985, 1987, 1989) sont aussi fondamentales.

celle, bien connue, d'E. Vallespí Pérez (1970). Les travaux du Comte de Lersundi, de P.M. de Soraluze et de G. de Reparaz dans le gisement de la grotte d'Aitzbitarte semblent traduire les premières préoccupations relatives aux étapes les plus anciennes de la Préhistoire régionale (I. Barandiarán Maestu 1967).

Dans le cas d'Asturias, nous trouvons des éléments semblables dans les premiers travaux archéologiques, épars, de la fin du XIXe siècle, comme les fouilles de Justo del Castillo dans la grotte de Collubil (Amieva). Mais c'est au début du XXe siècle, à partir de 1910, que nous assistons à une véritable première de la recherche préhistorique, avec les travaux et fouilles réalisées par E. Hernández Pacheco.

Un peu avant, avaient eu lieu les premières activités systématiques de quelques grands chercheurs de la science préhistorique continentale, comme E. Cartailhac et H. Breuil, en particulier à Altamira, qui eurent un rôle de catalyseur pour ce type de recherches en Cantabria. Dans les premières années de ce siècle, nous assistons à une floraison d'études et de travaux réalisés par des groupes de recherche appartenant à des institutions extérieures à la région cantabrique elle-même. Dans le cas de Santander, l'Institut de Paléontologie Humaine (IPH), récemment créé, soutint les travaux menés à Altamira à partir de 1902. Avec la collaboration de H. Alcalde del Río comme prospecteur local (dans un rôle secondaire, malgré les études hagiographiques postérieures), le poids de la recherche retomba sur les grandes figures de l'Abbé Breuil et, plus tard, de H. Obermaier.

Si les travaux d'E. Hernández Pacheco dans les zones de Ribadesella et de Las Regueras avaient constitué un point de départ déterminant pour la recherche en Asturias, ils trouvèrent une continuité non moins importante dans l'oeuvre du Comte de la Vega del Sella. Ces chercheurs étaient attachés à une institution espagnole, la *Junta de Ampliación de Estudios e Investigaciones científicas* (prédécesseur de l'actuel organisme centralisé de la Recherche, le *Consejo Superior de Investigaciones Científicas* - CSIC). La création, en 1912, de la *Comisión de Investigaciones Paleontológicas y Prehistóricas* dépendante de la *Junta*, marqua la constitution du premier groupe formel de recherche sur la Préhistoire et la Paléontologie quaternaire dans la Péninsule.

Au Pays-Basque, la situation se présentait différemment; il n'y existait pas de présence systématique et organisée d'entités centralisées ou étrangères. Ce sont les chercheurs basques eux-mêmes, groupés dans des institutions régionales ou locales, qui poussèrent plus avant les travaux de recherche. L'action de l'Institut de Paléontologie Humaine se limita à quelques visites de H. Breuil et E. Harlé et au projet (jamais réalisé) d'une fouille dirigée par H. Obermaier et J. Bouyssonie à Aitzbitarte.

Le développement de la recherche pendant la deuxième décennie de ce siècle devient, à notre avis, d'une importance cruciale pour la compréhension des divergences qui se produisirent à des époques postérieures. Il ne faut pas oublier que les premiers travaux en Cantabria, déjà mentionnés, étaient dirigés essentiellement par H. Breuil, et H. Obermaier, sous le patronage de l'Institut de Paléontologie Humaine. Malgré sa présence, H. Alcalde del Río n'était pas rattaché ou ne représentait aucun organisme supérieur de recherche scientifique en Espagne. Par conséquent, la responsabilité ultime des travaux de recherche et de l'étude ainsi que les décisions sur le matériel incombaient aux chercheurs étrangers.

Cet état de choses constitua une source de conflits lors des fouilles de la grotte d'El Castillo, comme on peut l'observer sur les contrats établis entre H. Alcalde del Río et le Prince Albert de Monaco, afin de définir la participation du premier cité aux travaux ². L'équipe qui participa aux fouilles de cet immense gisement, à partir de 1910, était constituée par des personnalités de différents pays. C'est précisément ce caractère international qui provoqua la séparation de ses participants en 1914, au moment de la première guerre mondiale. Evidemment, l'appartenance des deux principaux chercheurs à des nations antagonistes provoqua la dissolution du groupe et la paralysie des fouilles. Cela entraîna la perte d'une bonne partie de l'information et la disparition des collections, ainsi que l'interruption des études stratigraphiques.

En effet, non moins important que cette dépendance d'une institution étrangère fut l'arrêt des activités de recherche dans d'autres zones de la région, puisqu'elles avaient été animées par le même groupe de chercheurs. Parmi les travaux fondamentaux de l'époque, on peut compter l'étude des peintures et gravures de la grotte de la Pasiëga, les fouilles d'El Valle et de Hornos de la Peña et la publication de la grande monographie sur l'art pariétal quaternaire, *Les Cavernes de la Région Cantabrique*. Le manque de liens professionnels avec des institutions scientifiques espagnoles de la part des chercheurs locaux inclus dans le groupe (Alcalde del Río et Lorenzo Sierra) occasionna les pertes déjà mentionnées et la rupture du cours des recherches, l'appui théorique et matériel ayant été fourni par une institution qui cessait ses activités. L'on sait qu'après la guerre le groupe ne fut jamais reconstitué et qu'il fallut attendre de nombreuses années avant que l'on puisse récupérer et systématiser une partie de l'ancienne documentation provenant de la grotte d'El Castillo (travail effectué récemment par V. Cabrera).

L'exemple d'Asturias est bien différent. Dans la deuxième décennie du siècle, nous assistons à un transfert des recherches : des études initiales de Hernández Pacheco à celles du Comte de la Vega del Sella. Ce dernier n'était pas un scientifique professionnel au moment de ses premiers travaux, mais il fut rattaché à la *Junta de Ampliación de Estudios* à travers la CIPP, comme professeur honoraire du *Museo Nacional de Ciencias Naturales*. Ce statut lui permit d'entreprendre, parallèlement et sans entraves, plusieurs travaux de fouille et l'étude de gisements de la région avec Hernández Pacheco. En même temps, il entretint une étroite collaboration avec H. Obermaier, avec lequel il publia quelques travaux. C'est ainsi que, en Asturias, les recherches entreprises sous le contrôle et avec la collaboration des équipes du *Museo Nacional de Ciencias Naturales* ne s'interrompirent pas en 1914; Vega del Sella les poursuivit pendant la décennie, et au-delà, avec la collaboration sporadique d'H. Obermaier. Une grande partie des résultats a été ponctuellement publiée dans la série *Memorias de la Comisión de Investigaciones Paleontológicas y Prehistóricas*, ainsi que d'autres travaux des scientifiques espagnols du moment, et les collections publiées provenant de ces fouilles sont parties du fonds du *Museo Nacional de Ciencias Naturales* à Madrid, siège de la *Comisión* (Marquez Uría 1974).

En Cantabria, par contre, le vide laissé par l'équipe de chercheurs de l'Institut de Paléontologie Humaine ne fut pas comblé par des institutions de recherche formellement constituées. Après l'étude de La Pasiëga, Alcalde del Río arrêta pratiquement ses travaux sur le terrain (il publia un petit article sur la grotte de Cudón, quelques années plus tard) et c'est l'abbé Carballo qui prit le relais; ce qui eut pour conséquence un véritable recul vers le type de recherche naturaliste de la fin du XIX^e siècle. Carballo se trouva totalement isolé des groupes de recherche espagnols ou étrangers de l'époque. Pourvu d'une formation de terrain en sciences naturelles et

² Ces problèmes et conflits ont été publiés dans les travaux de B. Madariaga de la Campa (1972 : 49-51).

d'un bagage de lectures assez limité, il entreprit de nombreuses fouilles dans certains des gisements importants qui commençaient à être connus. Ses descriptions pittoresques et ses efforts pour changer la nomenclature furent le reflet évident de son isolement par rapport à l'avant-garde scientifique du moment.

Le groupe constitué par Telesforo de Aranzadi, José Miguel de Barandiarán et Enrique Eguren, en 1917, et le début de leurs activités sur le terrain, sont considérés par les historiens comme le point de départ d'un véritable "âge d'or" de la Préhistoire basque. Leur solide base pluridisciplinaire, leur rigueur méthodologique et leur incroyable capacité de travail marquèrent une différence radicale avec leurs prédécesseurs. Pendant vingt ans de travail continu, dans des gisements appartenant à des époques préhistoriques différentes, ils établirent les bases archéologiques pour la connaissance du plus ancien passé régional. Cette étape, considérée comme la "consolidation" de la Préhistoire basque (I. Barandiarán Maestu 1967) marqua la pleine maturité de cette discipline scientifique dans la zone et un apport singulier à l'ensemble des études péninsulaires. Par ailleurs, elle servit à définir ce que l'on considérait comme les caractères différentiels des communautés préhistoriques de la région.

La guerre civile espagnole (1936-1939) provoqua le démantèlement de la structure de recherche scientifique du pays. En 1939, la *Junta de Ampliación de Estudios e Investigaciones Científicas*, très ébranlée à cause de l'exil et de l'ostracisme subis par la plupart de ses chercheurs, fut remplacée par le *Consejo superior de Investigaciones Científicas*, dont l'orientation fut sensiblement opposée à l'esprit de travail de la première. Ce changement perturba de façon particulière la tâche du groupe de chercheurs appartenant à l'ancienne *Comisión de Investigaciones Paleontológicas y Prehistóricas* et au *Museo de Ciencias Naturales*, parce qu'il entraîna la rupture d'une ligne de recherche dans la région cantabrique. La situation se détériora davantage en Asturias, après la mort de Vega del Sella en 1941. Au cours des années 40, on remarque une absence évidente de recherches dans la région; elle ne seront reprises qu'au début de la décennie suivante par F. Jordá et déjà sur des postulats très différents. Pour la plupart, les travaux de cet auteur ont mis en valeur les caractères différentiels du "Paléolithique cantabrique", face au modèle français proposé par H. Breuil, ce qui suppose une unité interne, même s'il signale des différences, qu'il considère comme secondaires, entre les divers gisements et les mobiliers des régions septentrionales.

A Santander, Carballo continua à faire figure solitaire dans la recherche au cours des années 40 et 50. Ses points de vue personnels sur le Paléolithique régional furent exposés dans plusieurs articles. En même temps, il effectua des fouilles dans la grotte d'El Pendo et organisa, avec J. Martinez Santaolalla, des chantiers internationaux dont la finalité patriotique était explicite dans des textes de l'époque. Le résultat final de ces travaux, presque aussi triste que celui des fouilles d'El Castillo en 1914, comporta la dissolution de l'équipe de travail et l'éparpillement des notes et des matériaux pendant de longues années. Par conséquent, quand les travaux sur le terrain démarrèrent, quelques années plus tard, sous la direction de nouveaux groupes qui adoptèrent des techniques et des problématiques différentes, il ne se produisit pas une révision critique des idées traditionnelles, mais des adaptations méthodologiques partielles.

Au Pays-Basque, la mort d'Aranzadi et d'Eguren pendant la seconde guerre mondiale entraîna la disparition du principal noyau de la recherche préhistorique. Par ailleurs, l'exil de J.-M. de Barandiarán détermina l'interruption des fouilles en cours, qui ne reprirent qu'à son

retour, en 1953. Cette même année fut publiée sa synthèse, *El Hombre Prehistórico en el País Vasco*, où il exposa son concept des caractères différentiels des populations préhistoriques basques.

Les années 60 apportèrent une série de nouveautés importantes, en même temps qu'une réactivation remarquable dans le cadre général qui nous occupe. Les activités du *Patronato de las Cuevas Prehistóricas* et la création du *Seminario Sautuola de Prehistoria y Arqueología*, à Santander, le développement de la SCN Aranzadi et la fondation de l'*Instituto Alavés de Arqueología*, au Pays-Basque, marquèrent ce nouvel élan, très lié à des entités de caractère local. Par ailleurs, l'arrivée de chercheurs nord-américains dans la zone cantabrique, au cours de cette même décennie, occasionna une réaction face aux méthodes traditionnelles et introduisit de nouveaux concepts théoriques et des techniques de travail modernes. Ce dernier aspect eut une incidence particulière en Cantabria, à partir des fouilles de Cueva Morin en 1966. Dans la décennie suivante, cette présence fut encore importante en Asturias, où se développa le *Proyecto Paleocológico de la Cueva de La Riera* (1976-1979), et, en Cantabria, avec le début des fouilles de la grotte d'El Juyo en 1978. Plusieurs équipes, dirigées par des chercheurs provenant d'Universités d'autres zones, travaillèrent aussi de façon régulière sur des gisements des deux régions. Par contre, le Pays-Basque resta en marge de ces influences et ce sont les groupes locaux qui assurèrent les travaux sur le terrain.

Au cours de cette période, se produisit ce que I. Barandiarán a défini comme "la consolidation et la régularisation des activités des centres universitaires". En Asturias, l'Université d'Oviedo avait déjà une longue tradition et une grande influence sociale et scientifique, mais sa section d'Histoire naît précisément dans les années 60 et se consolide comme centre de recherche dans les années qui suivent. A la même époque, le Séminaire d'Archéologie de l'Université privée de Deusto, au Pays-Basque, voit le jour. En 1972, se crée l'Université de Cantabria, mais la naissance de la Faculté de Philosophie et Lettres, avec son Département de Préhistoire et d'Archéologie, date de 1978. Ce même département est constitué, dans la toute récente Université du Pays-Basque, en 1983.

Il est évident que ces créations plus ou moins précoces ont induit un effet de décalage important en ce qui concerne l'influence relative de l'Université dans chacune de ces régions. La longue tradition et le rôle social que l'Université détient en Asturias ont déterminé l'attribution presque exclusivement universitaire de la recherche pendant les vingt dernières années. Par contre, ce sont les organisations locales qui ont assuré cette fonction au Pays-Basque et en Cantabria, en l'absence d'institutions universitaires. La tardive création de ces dernières années a provoqué un véritable effet de concurrence et de lutte pour les moyens et le prestige, avec une réelle incidence sur les aspects politiques et sociaux de la recherche.

Arrivés à ce point, il nous semble nécessaire d'expliquer que, si le tableau dressé jusqu'ici révèle une insistance particulière sur les époques anciennes de la Préhistoire de la zone, ce n'est pas un hasard. En effet, ce sont ces époques qui ont fait l'objet des interventions les plus systématiques, à l'exception des recherches mentionnées au Pays-Basque, tandis qu'on négligeait la Préhistoire récente, avec pour conséquence une tradition de recherches moins importante pour cette dernière.

Pour résumer, les différences dans l'histoire des recherches, dues aux trajectoires particulières des chercheurs ou des groupes et aux diverses circonstances politiques du moment, ont eu pour résultat une évolution inégale de la Préhistoire dans chacune des régions étudiées. Sur cette réalité non homogène se superposent les divergences politiques et administratives qui sont le produit de l'application du nouveau système des Communautés autonomes.

L'IMPACT DES ADMINISTRATIONS AUTONOMES

Le développement du processus de décentralisation administrative de l'état espagnol, à partir de la Constitution de 1978, a provoqué certains changements fondamentaux dans le paysage de la recherche archéologique. A cause du transfert presque total des compétences en matière de patrimoine historique aux Communautés autonomes, les Administrations régionales ont acquis un pouvoir de décision exclusif ou prioritaire sur les différents aspects de l'archéologie, parmi lesquels :

- a. l'autorisation des différents types de recherche archéologique (fouilles, prospections, études d'art pariétal, révision des mobiliers...);
- b. le financement des recherches;
- c. la définition des orientations officielles de la programmation archéologique;
- d. les procédures d'urgence et l'archéologie préventive;
- e. la création et le soutien des publications d'archéologie, en particulier celles des résultats de fouilles;
- f. le dépôt des collections dans les musées.

Le modèle centralisé antérieur est donc remplacé par un contrôle presque complet de la part des gouvernements autonomes. La loi de 1985 sur le patrimoine historique espagnol introduit seulement des éléments correcteurs en cas de négligence manifeste de la part des Administrations régionales. Il ne surprendra personne que cette loi soit discutée au cours d'un procès en appel devant le tribunal constitutionnel, pour cause de conflit de compétences suscité par les gouvernements régionaux.

Par conséquent, les décisions politiques se trouvent davantage influencées par des facteurs locaux, parmi lesquels les aspects régionalistes ou nationalistes, qui semblent particulièrement sensibles à tout ce qui concerne le passé de la Communauté. Par ailleurs, les institutions de recherche qui n'appartiennent pas à la région ou qui conservent, de par leur statut, une relative autonomie de décision – c'est le cas de quelques universités –, sont l'objet d'une certaine méfiance qui peut provoquer, dans le pire des cas, un refus d'autorisations et de crédits, soit de façon directe, soit par des entraves ou des contrôles imposés dans leur travail.

Les raisons sous-jacentes à ce type de situation ont une explication assez simple : les revendications autonomistes ont souvent exigé une enquête sur les éléments d'identité justifiant politiquement de telles aspirations, sur la constitution d'un territoire et d'un groupe humain déterminés sous forme d'une entité à part. La langue, la tradition commune et l'existence préalable de structures administratives et politiques autonomes, dans un passé récent ou lointain, figurent parmi les arguments les plus souvent évoqués. Dans ce contexte, l'Histoire devient explicitement une arme politique; elle sert à fournir des arguments, mais elle peut aussi être

manipulée. L'histoire locale devient prioritaire face à l'Histoire globale et aux problèmes de recherche approfondie, et elle monopolise tous les efforts de financement et de planification.

La Préhistoire – l'Histoire la plus ancienne, en fin de compte – est privilégiée dans cette dynamique. La quête des racines d'un peuple conduit inévitablement à celle de ses limites avant la première présence impérialiste (cette dernière étant souvent identifiée, de façon quelque peu optimiste de la part de certaines Communautés autonomes, au monde romain). La revendication autonomiste plonge ses racines, surtout littéraires, dans les populations pré-romaines, en tant que source non contaminée de l'entité régionale primaire. Cette approche du passé est, par définition, partielle : elle obéit à des raisons politiques, dans le sens restrictif du terme, et cherche surtout une justification pour des actions concrètes dans le présent.

Le problème pratique pour le chercheur surgit au moment où son programme concret de travail scientifique (donc critique) entre en conflit avec l'Administration chargée de l'autoriser, et souvent de le financer. Il se trouve souvent en concurrence avec d'autres projets ayant une orientation plus proche de l'idéologie officielle, ou confronté à une programmation générale qui favorise explicitement les lignes de recherche de ce dernier type. Une deuxième facette du même problème se manifeste dans la concurrence entre chercheurs qualifiés, appartenant à des entités de recherche indépendantes (Universités, CSIC, etc.) et présentant des programmes de travail également indépendants (et critiques), et les chercheurs locaux, dépendant administrativement du pouvoir régional ou proches de lui.

La troisième question est en rapport avec un aspect-clé de la recherche : la publication des travaux. Nous trouvons ici la même opposition entre des critères d'intérêt scientifique et les lignes éditoriales des Administrations régionales, intéressées surtout par la publication de ce qui est considéré comme "prestigieux" ou "profitable". Il en va de même pour les maisons d'édition locales, dont le critère de rentabilité profite aux produits régionaux et à des travaux – parfois douteux – à caractère spectaculaire, voire ésotérique.

Ce dernier problème peut revêtir une grande importance, puisqu'il entraîne la formation d'une mentalité régionaliste ou nationaliste à travers des publications didactiques où la manipulation de la Préhistoire et de l'Antiquité devient une pratique courante, ainsi que la promotion de trouvailles et de découvertes, sans aucun esprit critique, sur la base de leur caractère spectaculaire ou exceptionnel. Les polémiques générées par des situations de ce type transmettent à l'opinion publique l'impression que tout est sujet à remise en question dans le domaine de la recherche archéologique et que la "raison" est le fait des personnages les plus connus ou les plus proches des "intelligentsias" locales.

Dans le cadre du premier sujet évoqué ci-dessus, il est évident que tout projet de recherche concernant plus d'une Communauté ou qui adopte une perspective théorique "anti-régionaliste" – le simple refus de faire remonter à l'Antiquité l'origine des frontières, langues ou groupes humains actuels – peut entraîner de sérieuses difficultés de financement ou même d'autorisation. Il semble communément accepté que chaque Administration ne doit pas subventionner des travaux qui se déroulent hors de son territoire spécifique, ce qui suppose une vision assez particulière de la recherche et de la gestion du patrimoine archéologique. Par contre, les avis sont unanimes lorsqu'il s'agit d'appuyer certains travaux mettant en valeur les aspects les plus sensibles de la mythologie régionale (populations pré-romaines, les premières villes et leurs

vestiges, etc.), sans prendre en considération leurs qualités scientifiques. Dans quelques Communautés autonomes du Nord, cette procédure a autorisé l'investissement de sommes démesurées sur des fouilles uniquement destinées à utiliser le témoignage tangible du passé à des fins politiques.

Le même problème concerne les organismes administratifs chargés de décider de la planification générale, des autorisations et du financement, d'un point de vue technique. Les trois Communautés qui nous servent d'exemple disposent de Commissions techniques d'archéologie, à caractère consultatif. Le seul exemple positif se trouve en Asturias, où la Commission intègre quelques représentants de l'Université d'Oviedo, tous des professionnels de la Préhistoire et de l'Archéologie. Cela procure à ces chercheurs indépendants un certain poids dans les discussions. En Cantabria, la Commission est composée de cinq membres de Cantabria. Au Pays-Basque, la Commission est composée de trois représentants de l'Administration régionale et de trois autres membres, désignés par la *Diputación Provincial* de chacun des *Territorios Históricos*. L'université n'a pas de représentation institutionnelle. Dans ce dernier cas, l'affaire se complique davantage depuis une décision récente des *Territorios Provinciales* allant dans le sens d'une gestion totalement indépendante de l'archéologie dans chacun d'entre eux.

Le deuxième sujet évoqué plus haut est directement en rapport avec les personnes. En partant d'une optique de rationalisation des moyens matériels et humains, on pourrait attendre des différentes régions une politique de collaboration des équipes de chercheurs, ce qui permettrait d'éliminer les difficultés dues à la fragmentation, consécutive à l'autonomie. Tout au contraire, il existe un conflit, plus ou moins explicite, entre les chercheurs professionnels des Universités ou des centres de recherches nationaux et ceux, professionnels ou amateurs, attachés à des organismes régionaux ou locaux. Cela se traduit par une concurrence, face aux crédits et aux publications, qui favorise l'interférence des éléments politiques sur l'objectivité des recherches.

Concrètement, les problèmes varient, bien entendu, d'une région à une autre. Comme nous l'avons déjà mentionné, la tradition universitaire d'Asturias marque des différences avec ce qui se passe au Pays-Basque et en Cantabria. Dans ces dernières régions, la tardive création de l'Université publique et des départements d'Histoire a permis la consolidation des noyaux traditionnels de recherche dépendant des organismes régionaux ou locaux.

Les critères de sélection et de recrutement du personnel dans ce type d'institution sont établis par les organismes régionaux eux-mêmes et, pour la plupart, ne sont pas soumis aux formes d'évaluation des corps de fonctionnaires de l'Université, du CSIC ou des conservateurs de musées. Souvent, les postes de direction, à caractère représentatif ou politique, ne sont pas occupés par des chercheurs, mais par des personnes appartenant à des professions ou des formations très diverses. Leur situation d'étroite dépendance administrative avec le pouvoir local rend plus faciles leurs demandes de crédits; mais, en même temps, ils deviennent plus perméables aux exigences d'orientations précises pour leurs recherches. Le résultat se traduit par une symbiose : le pouvoir politique, en échange d'une stabilité de travail et du financement d'institutions et de personnes, reçoit le produit des recherches faites à la mesure de ses intérêts. Ceux-ci, comme nous l'avons déjà dit, visent souvent tout ce qui fait ressortir les caractères individuels ou l'exaltation d'un passé glorieux, sujets considérés appropriés pour être offerts à l'opinion publique.

Cela nous mène au troisième sujet proposé dans notre introduction – que nous avons considéré comme un aspect relevant de la recherche : la diffusion des résultats. Entre 1979 et 1985, le Ministère de la Culture a publié une série de volumes annuels consacrés à des comptes rendus et à des notes préliminaires des travaux de terrain. La série *Noticiario Arqueológico Hispánico* a accueilli, jusqu'à une date récente, des rapports brefs, préliminaires ou définitifs. Finalement, la collection *Excavaciones Arqueológicas en España*, encore en activité, publie de façon monographique les mémoires définitifs de fouilles qui exigent un volume important.

Le transfert des compétences aux différentes régions entraîna la disparition de la première série, qui avait pour fonction d'informer la communauté scientifique et de justifier des activités menées avec des fonds publics. La Communauté autonome basque publie, depuis 1982, la série annuelle *Arkeoikuska*, consacrée à ce même type d'information sur les activités archéologiques de la région. Asturias a récemment publié (janvier 1990) le premier numéro de la série *Excavaciones Arqueológicas en Asturias* 1983-1986, orientée vers l'accueil de mémoires préliminaires brefs plutôt que de nouvelles annuelles. Cantabria ne dispose, jusqu'à présent, d'aucune série institutionnelle de publication, que ce soit des monographies ou des nouvelles. Aucune des trois Communautés ne compte sur une ligne régulière de publication de mémoires définitifs de fouilles. Ceux-ci apparaissent souvent dans la série nationale mentionnée plus haut ou sont édités grâce à l'intervention ponctuelle de l'Administration, des organismes de recherche, ainsi que sous forme de numéros monographiques dans des revues spécialisées. Dans ce sens, nous considérons remarquables les exemples basques de la *SCN Aranzadi* et de la *Sociedad de Estudios Vascos*.

En résumé, à part les exceptions mentionnées, les Administrations régionales n'ont pas un programme défini de publication des résultats des travaux archéologiques sur la base de critères indépendants à caractère scientifique (contrôle de qualité assuré par des rapporteurs extérieurs, à la façon des revues scientifiques de prestige). La diffusion *in extenso* des résultats des recherches est soumise, au cas par cas, aux intérêts de l'Administration centrale ou autonome, ou bien à l'appui obtenu auprès des organismes de recherche régionaux ou locaux. Une fois de plus, cette situation entraîne l'absence de garanties pour la diffusion des résultats d'une recherche idéologiquement indépendante. Il est évident que les publications strictement scientifiques ne représentent pas un marché qui pousse les maisons d'éditions commerciales à entreprendre l'aventure de les financer. Par ailleurs, la publication condensée des résultats dans des revues spécialisées extérieures restreint de façon importante l'impact de tels résultats sur la collectivité elle-même, parce que leur diffusion se limite à une communauté scientifique déterminée.

Au Pays-Basque, l'existence de publications périodiques régionales qui consacrent une bonne partie de leur espace à l'archéologie (*Munibe*, *Veleia*, et *Kobie*) permet de rapprocher l'information des lecteurs locaux, même si leurs critères de sélection et la qualité moyenne des collaborations sont extrêmement variables. En Asturias, le *Boletín del Instituto de Estudios Asturianos* a joué un rôle similaire, mais à une moindre échelle et avec des critères assez larges de contrôle de qualité. Au risque d'être polémique, nous considérons que Cantabria ne dispose pas, jusqu'à présent, d'une véritable publication périodique de qualité moyenne reconnue, mais plutôt d'une série d'articles archéologiques disséminés dans de nombreuses publications liées à l'érudition locale ou à la spéléologie.

La diffusion des résultats à l'échelle populaire pose aussi des problèmes de "haute sensibilité" politique. L'impact des généralisations sur le passé archéologique de la Communauté peut être beaucoup plus direct quand il atteint les niveaux éducatifs de base et moyens, ou quand il s'adresse à l'ensemble de la collectivité à travers les médias. Dans le cas des publications de caractère scolaire, les critères qui priment sont commerciaux, quand il s'agit d'initiative privée, ou politiques si elles ont été subventionnées par les organismes régionaux. En ce qui concerne les médias, la nouveauté et le sensationnel l'emportent sur la rigueur scientifique, et le fait exceptionnel sur le quotidien.

Le problème de fond réside dans le fait que la manipulation et la sélection opérées *de facto* sur le matériel de publication ont pour résultat de façonner une mentalité collective. Cela peut aboutir à l'incorporation au *substratum culturel* commun de concepts, implicites dans certains discours tels que : "il est possible que les Néolithiques aient déjà parlé l'*euskera*"³, ou "le culte naturaliste des druides eut lieu dans sa première étape en Cantabria"⁴ et autres perles de cet acabit. La manipulation du document historique (archéologique), sa falsification, tout semble justifié par l'affirmation du passé local comme différent et exceptionnel. Le préhistorien parvient difficilement à faire accepter que les évidences matérielles ne nous permettent pas de dater les *castros* asturiens, par exemple, au-delà de l'époque romaine, ou que les seuls établissements cantabriques archéologiquement documentés se trouvent hors des limites administratives actuelles de la région. Tout cela se heurte à une prétendue et confuse variété de "celtisme" que l'on veut donner comme base historique à la revendication régionaliste. Dans ce contexte, le travail essentiellement critique du chercheur indépendant peut être presque considéré comme subversif et entraîner, de plus en plus fréquemment, la condamnation à l'ostracisme.

LA PRÉSERVATION DES TÉMOINS DU PASSÉ : COMMUNAUTÉS AUTONOMES, RECHERCHE ARCHÉOLOGIQUE ET PROTECTION DU PATRIMOINE.

Un dernier aspect de ce sujet – suffisamment important pour mériter une étude à lui tout seul – concerne les rapports entre les Communautés autonomes, la recherche archéologique et la protection du patrimoine. On a déjà précisé que les Autorités régionales détiennent la responsabilité totale de la conservation du patrimoine archéologique et historique de leur Communauté respective. Cela inclut, théoriquement, tout ce qui est en rapport avec l'archéologie préventive, l'inventaire du patrimoine, les fouilles de sauvetage, l'inspection de l'état de conservation et, le cas échéant, la prise de décisions sur la restauration, les mesures de conservation ou la fermeture d'un gisement. L'absence de moyens suffisants pour répondre à un si vaste éventail de besoins potentiels oblige à réaliser une sélection des interventions. C'est ici que l'objectivité des décisions à prendre peut se trouver sérieusement malmenée par des *a priori* politiques visant la poursuite des racines autonomes. Très souvent, les critères utilisés à l'heure de décider des investissements pour des projets de conservation ou de restauration doivent davantage au caractère spectaculaire d'un gisement, ou à la possibilité de l'afficher publiquement

³ Texte d'un panneau du *Museo Arqueológico, Histórico y Etnográfico de Bilbao*.

⁴ Présentation officielle d'une supposée trouvaille celtique, lors d'une conférence de presse organisée par la *Consejería de Cultura, Educación y Deporte de la Diputación Regional de Cantabria* (*El Diario Montañés*, 6 février 1990).

comme paradigme ethnique qu'à son importance objective comme document historique devant être conservé et protégé.

Dans le pire des cas, l'activité des groupes de pression politique et économique sur les pouvoirs régionaux est plus effective qu'elle ne l'était auparavant sur l'État central. Lors de conflits entre des intérêts immobiliers ou industriels et le patrimoine archéologique, celles des Communautés qui ont une conscience régionale plus développée pratiquent une politique de prévention plus énergique que les autres. En Cantabria, la faible valorisation publique du patrimoine archéologique comme base de l'identification régionale (fondée plutôt sur la littérature) est en train de favoriser sa destruction accélérée.

Autre aspect du problème : la sélection résultant de la limitation des moyens déjà évoquée provoque des déséquilibres entre la recherche théorique et celle directement appliquée à des questions de conservation. On observe actuellement une tendance des Administrations publiques en Espagne – exception faite de celle de Cantabria – à augmenter les crédits et le personnel consacrés à ce que l'on appelle “l'archéologie de gestion”, au détriment de “l'archéologie de recherche”. Nous sommes confrontés au danger de voir cette archéologie de gestion, contrôlée par les Administrations régionales, assumer un rôle exclusif au grand détriment des ressources encore destinées à subventionner les travaux des Universités et d'autres centres de recherche.

CONCLUSION

Nous ne voudrions pas être trop pessimistes, mais l'expérience que nous avons, jusqu'à présent, de l'attitude des Gouvernements régionaux des Communautés autonomes de la zone cantabrique en matière de recherche archéologique se révèle, de façon globale, assez décourageante. L'objectivité des recherches semble menacée par des facteurs de type politique ou économique, et la manipulation de l'Histoire – surtout la plus ancienne – devient trop séduisante et profitable pour le pouvoir local. Nous pensons que les professionnels de la recherche doivent dénoncer ce danger et contribuer à le conjurer par un dialogue avec les Administrations et la Société en général. Il nous appartient de montrer à la collectivité que la critique et la rigueur historiques ne sont pas en contradiction avec les aspirations à l'autonomie, mais que, au contraire, elle les dépouille de leurs composantes mythiques et les replacent dans leurs termes réels. Et nous sommes aussi obligés de promouvoir la protection du patrimoine archéologique en tant que document historique, bien au-delà de l'opportunisme du moment ou d'intérêts qui lui sont étrangers.

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OBJECTIVITY AND SALVAGE EXCAVATION POLICY IN MANDATE PALESTINE AND THE STATE OF ISRAEL : AN APPRAISAL OF ITS EFFECTS ON UNDERSTANDING THE ARCHAEOLOGICAL RECORD

by

Eliot BRAUN *

ABSTRACT

Archaeological research in the Holy Land grew out of the discipline of Biblical Archaeology, an approach which has, to date, directed the lion's share of field activity towards sites related to Biblical History. This selective approach has been responsible for the creation of a large and important, but extremely subjective primary archaeological data base; one which is inherently biased away from non-Biblical periods. The result has strongly influenced all understanding of the archaeological record, often detrimentally. However, since the time of the British Mandate for Palestine until the present, the implementation of a determined, objective policy of salvage archaeology in the territory of the State of Israel has acted as a curb on this tendency by adding a healthy, non-biased increment to this data base. It is a policy which makes no distinctions between Biblical related and non related sites and it has been responsible for major discoveries which have, in certain instances, radically altered scholarly comprehension of the archaeological record.

When first approached to express my views on objectivity in salvage archaeology at the annual meeting of Israel Anthropological Association conference my immediate reaction was that most archaeological activity and especially salvage work has a primary motivation which is intrinsically subjective. It is initiated by a conscious desire to elucidate obscure, buried aspects of the past; an approach which takes for granted that such an activity is worthwhile, even desirable. The alternative would be to limit archaeological inquiry to those ruins and artifacts which we literally and figuratively trip over.

To dig or not to dig is never a question of human survival and archaeological excavation is one exercise from which society will rarely reap material benefits. Indeed most excavation is an

* Israel Antiquities Authority.

activity requiring considerable expenditure of time, effort and expense in search of the obscure. Yet for all this, archaeological exploration is accepted practice and even prescribed by law in many parts of the world. What then does objectivity have to do with salvage archaeology ?

Before addressing this question it is perhaps best to briefly explain what is meant by the term "salvage" and how it differs from "rescue" work. The latter is understood to be excavation with the end result the preservation, *in situ*, of at least the major remains of a site. Salvage archaeology is the recording of archaeological data through excavation prior to a site's destruction with the consequent loss of all data not recorded.

The question of objectivity in "salvage" archaeology is one which I would like to explore briefly in the pages below. It can involve a multiplicity of answers on different levels in what Gallay [1989:29] has described as a "hierarchy of explanations".

In Gallay's scheme archaeological research is understood as having a structure, the base of which is data that is descriptive of material remains. Using these data as a foundation the archaeologist, by means of analyses and syntheses, offers explanations [*i.e.* interpretations] which progress in complexity in a hierarchical system in which each level is based upon the explanations established in the step below, until finally the "meaning" of the research is set forth. This paper is concerned with one aspect of the lowest level in this hierarchy, a primary element in the construction of a data base, the choice of sites for excavation.

The selectivity of "directed" approaches to field work has, traditionally, introduced strong biases in the basic data available, especially in favor of the excavation of large tells associated or identified with Biblical history. In Israel this tendency continues till the present as may be witnessed by a list of excavations in Israel in the summer of 1990 [Menahem 1990]. The sites include : Ashkelon, Tel Miqne [Biblical Ekron of the Philistines], Tel Jezreel, Caesaria maritima, Tiberias, Beth Shean, Tel Hazor, Tel Bet Saida and Biblical Geshur [The Golan Heights].

Salvage archaeology in Israel, on the other hand, is uniquely free of preferences inherent in "directed" strategies [*i.e.* biases which confine their contributions to highly selected portions of the archaeological record]. Obviously this has important bearing on the very nature of the primary data base, here understood to be the sum total of archaeological information acquired from excavation, duly recorded for posterity.

Salvage archaeology is derived from a venerable tradition of archaeological practice. An understanding of its history is instructive as to how, as the result of a subjective, benevolent attitude towards all antiquities, a modicum of objectivity has been infiltrated into the ground floor of the discipline where otherwise it would be absent. The discussion below focuses on the objective nature of the decision as to which sites will be excavated and some of the more outstanding fruits yielded by this approach.

The practice of salvage archaeology in Israel today is an indirect result of the inception, in the 19th century, of the discipline of Biblical Archaeology. The idea of systematic excavation in the Middle East in general and in Turkish Palestine in particular, in search of physical remains associated with Biblical events, was one which aroused a great deal of interest in European circles of the last century and which continues to the present. Nowhere was there more

enthusiasm than in Britain and with the granting of the mandate of Palestine by the League of Nations, His Majesty's government became the legal custodian of the Holy Land and the keeper of all its antiquities.

To the everlasting credit of the colonial administration this was understood as a sacred trust, one which called for the institutionalization of archaeological activities and which made no distinction between antiquities related to the Bible and those lacking such significance. This judgement, which gave a positive value to archaeological activity, was to create a law and a climate which, at least in theory, gave blanket protection to all antiquities.

By 1920 the first Antiquities ordinance was promulgated, creating a Director, the custodian of this trust, in charge of a department for implementing the law. Thus all legal excavation was regulated and what was more important, all antiquities, especially archaeological sites, published in an official list, came under its protection. The destruction of sites was permitted only with permission of the director and then usually only after excavation. The Department, by virtue of its trusteeship of buried sites, then became the executor and sponsor of salvage excavation when such was deemed necessary. Just how seriously this trust was taken may be noted by the subsequent behavior of the British authorities. The archaeological treasures of Palestine were never removed to Britain to enhance the collections of the British Museum. Instead a local authority became the repository of the major finds. Eventually the imposing edifice of the Palestine Archaeological ["Rockefeller"] Museum was constructed to house it.

The policy of salvage excavation, inherited from the British Mandate administration, was continued from the time when the State of Israel gained its independence, by The Department of Antiquities and Museums of the Ministry of Education and Culture. It remains in effect to the present in the government regulated Israel Antiquities Authority which, in August 1989, replaced the Department as the institution responsible for the implementation of the Antiquities Law.

The original Antiquities Ordinance of the British Mandate government in 1920 [A. O. 1920], later additions of 1929 [A. O. 1929] and its two restructurings in 1960 [A. O. 1960] and in the Law of 1978 [A.L. of 1978], by the State of Israel are, amongst other things, clear statements of an "objective" approach towards archaeology within the subjective framework referred to above. The statutes provide for the protection of all antiquities sites with no regard to religious, sentimental or other considerations.

Antiquities sites are defined as areas which contain antiquities and appear in "Reshumot" [A.L. 1978, Ch. 7 : 28] or schedules which originated in the mandate period. Additions to these schedules are published by the government from time to time at the instigation of the Israel Antiquities Authority. Sites not officially recognized in "Reshumot" can, by decree of the Director of the Antiquities Authority, be declared as such [A.L. 1978, ch. 7 : 28, a] and consequently may come under the protection of the law.

The definition of an "antiquity" in Israel's 1978 law [A.L. 1978, paragraph 1], still in effect, is of interest in this context :

1. any object, whether detached or fixed which was made by man before the year 1700 of the general era, and includes anything subsequently added thereto which forms an integral part thereof;
2. any object referred to in paragraph [1] which was made by man in or after the year 1700 of the general era, which is of historical value and which the Minister has declared to be an antiquity;
3. zoological or botanical remains from before the year 1300 of the general era.

This definition, derived from the original British concept, is remarkably free from bias and it has introduced a modicum of objectivity into the general data base, since 1920, by the demand for excavation of upwards of 2000 sites or portions thereof, in danger of destruction. This does not mean that every single site which has been obliterated has been excavated in its entirety in this period; merely that major efforts have been made to glean the most amount of information from the majority of sites to be destroyed. Decisions as to which sites deserved the most attention seem to be relatively free of some of the more obvious forms of political and religious bias.

Certainly a degree of bias exists even in this system but I suggest that it is by far the most objective approach to archaeological excavation shown by any institution involved in the archaeology of Israel. A list of salvage projects sponsored by the Department in its 70 year history would be indicative of this objective policy. It would include virtually every type of site from every period in the archaeological record. I myself, in 14 years of having been engaged almost exclusively in salvage work, have excavated sites related to virtually all major periods from late prehistoric to almost modern times; some of them attributed to the major ethnic and religious groups still inhabiting the country.

The decision-making process of "to dig or not to dig" is one related to the projected degree of loss [of a site and the data base which can be derived from it] measured against resources and outside pressures. To ensure against hasty decisions, when the size and state of a site are completely unknown, surveys and sondages are methods most frequently employed. They have been standard practice, for the period in which I have been involved in salvage work, and the instruments by which momentous discoveries have been made and major errors in decision making have been avoided.

This policy of salvage excavation has been responsible for the creation of an enormous body of knowledge, an addition to the primary data base which otherwise would have passed out of human cognizance. Some of this material is published, much of it remains in raw form in the Department's archives. Despite all the severe limitations of the quality of this data it remains a completely random sample of those sites, known to have ceased to be part of the archaeological record, in those regions of the country which have undergone modern development.

This "system of sampling" is not, however, without its own negative bias. The size of the data base available for any one region is dependent upon and in direct proportion to the scale of modern development. Thus those portions of the country which did not undergo major development are not represented faithfully in the primary data base.

This was especially true of the Negev until recent years when major re-deployment of the Israel Defense Forces prompted intensive archaeological surveying with upwards of 11,000 sites discovered and an enormous spurt in salvage excavations [R. Cohen, chairman Archaeological

Survey of Israel; personal communication]. Of these, hundreds were "salvaged" and many more were « rescued ».

Additional subjectivity in the primary data base has been imposed by research designs which choose sites by methods other than random encounters. No criticism of problem-oriented research is intended here, merely an admonition to scholars to be aware of the bias it introduces into the primary archaeological data base. This bias was, and still is, a serious impediment to research because it is intensified in the literature; relatively few salvage excavations have been published and access to the archives mentioned above is either restricted or denied to many researchers.

Indeed, were research to rely exclusively on published excavation reports, the distinct impression gained would be that the Biblical periods were the most populous, in antiquity and, necessarily, the most prominent, archaeologically; a circumstance which is open to question.

Major efforts are, however, now being made by the Israel Antiquities Authority to rectify this situation. The publication of long overdue excavation reports is greatly contributing to the primary data base as is the recent policy of enforcing minimal reportage of excavations in the periodical, *Hadashot Archaeologiyot* [News in Archaeology] to the Authority. The institution of systematic surveying [an increasingly significant aspect of field archaeology] and gazetteer works such as that by F. J. de Cree [1990] are, more and more, making available to researchers primary information on the number and general nature of sites. Similarly growing interests in prehistoric and post Biblical periods also have contributed to correcting the imbalances created by the selected retrieval and perception of data. Still biases exist and I suspect they will always be a factor to be considered in archaeological research.

When the data base derived from only salvage operations is compared with that originating in research oriented excavations which are directed at one or, at most, only several specific aspects of the archaeological record, one can begin to appreciate the significance of the fruit of 70 years of this policy of salvage work. Following are several examples of bias, briefly reviewed, which exist in the perception of the archaeological record in Israel; a condition which, to some extent, can be or has been corrected by the contribution of salvage work.

A significant bias in our perception of the archaeological record is associated with the Chalcolithic culture. The major occupation sites of this period presently known are in the Golan Heights [Epstein 1978], the Southern Jordan Valley [Hennessy 1969; Porath 1985] and the Negev [Perrot 1984]. There is little hard evidence for sedentary settlement in the coastal plain in this period; what is known is mainly from surveys and soundings of limited size [Gophna 1974]. Few also are the permanent occupations known in the hilly part of the country or in its north. Indeed the little substantive information that is available on Chalcolithic settlement in these regions comes from fortuitous discoveries in salvage operations.

This dearth of information on Chalcolithic settlement in the well watered heartland of the modern State of Israel and the West Bank territories is virtually impossible to understand. The "lack" of large permanent settlements in these regions is quite clearly a function of an insufficient [i. e. biased] data base. It is manifest in a recent synthetic work on the Chalcolithic period in The Levant [Gilead 1988 : 412-413] in which there is an inordinate reliance on sites of the "Southern

Levant" [*i.e.* The Negev]. The researcher is obviously aware of extensive references in the literature to Chalcolithic deposits in central and northern Israel (e.g. Zori 1977; Gophna 1974; Raban 1982, VII), many of them probably representing permanent settlements, but is forced to virtually ignore this region because most sites remain either unexcavated or have not been exposed to any appreciable degree.

This is neither the result of a conspiracy nor an attempt to mislead. It is, on the one hand, merely the consequence of non-development or inadequate inspection in certain regions of the country where these sites are located and, on the other, the fruit of years of excavations almost exclusively "directed" towards other regions, principally the Northern Negev, where sites are eminently visible and easily unearthed. The resultant bias in the primary data base is of major proportions.

Salvage archaeology has done only some small service in correcting this obviously distorted data base. Recent excavation at the site of Tel Teo [Eisenberg 1989] has shown evidence of a portion of a sedentary, well organized Chalcolithic community in the Huleh Valley in the far north of Israel. Another Chalcolithic settlement, of unknown proportions, has been noted at Sataf near Jerusalem [Gibson 1988] on a steep hillside overlooking the Vale of Sorek. Buried beneath later terracing, this early occupation was encountered during a rescue operation in a region where it was little suspected. Numerous other sites of this same horizon have been noted in formal and even casual surface surveying. Undoubtedly continued development of the central and northern regions of Israel will radically alter current theories of settlement patterns for the Chalcolithic period.

A more positive example of salvage archaeology's considerable contribution towards the rectification of a particular bias which seriously impaired scholars' understanding of the archaeological record is that concerning the EB I period. As recently as 1978 the late Kathleen Kenyon [1979, Chapter IV], in her interpretation of the primary data base, declared that the period saw several groups of invading tribesmen entering the country, undergoing a process of gradual sedentarization and development which led, eventually, to the floruit of an urban [Early Bronze II] culture; hence her term for this formative period : Proto-urban [= EB I].

Such a characterization as this was, with some reservations, still possible at the time of her writing. Relatively few settlements of the EB I were then known and fewer had been excavated. Of the latter group of sites, most were exposed only on a small scale and published only minimally. Indeed they were explored mainly because of their presence in the basal deposits of tells of interest to Biblical students rather than for the interest they generated of themselves. The literature on the subject was disproportionately biased in favor of tombs rather than settlements; a state of affairs clearly reflected in Kenyon's [1979] chapter on the Proto-Urban period. Indeed her use of the term Proto-Urban seems to have been based more on intuition than on hard evidence.

At the same time that Kenyon was developing her synthetic treatment of EB I society she could draw on a far greater and more representative fund of knowledge concerning the following, EB II culture, enabling her to characterize it with greater accuracy. There was then a positive bias for this period; one derived from much published, accurate and quite detailed information on numerous sites of the EB II. This was not the result of any special design but

merely a set of fortuitous circumstances. These urban centers happened to have been located on sites later occupied by towns of Biblical fame which first drew the attention of archaeologists. Continued interest in these urban centers can be understood from the impressive nature of their remains.

The implications were, for Kenyon, obvious. She perceived, somewhat understandably, in her synthetic study [Kenyon 1979] a far greater gap between the levels of development of the societies represented at EB I and EB II sites than was actually warranted by the evidence [Braun 1989 b, 1]. Today with major increments in our knowledge [the contribution of salvage archaeology] it is abundantly clear archaeological research has only just begun to comprehend the intensity, diversity and importance of the EB I [Kenyon's Proto-Urban] period; one distinguished by many and sizable sedentary communities displaying evidence of a long and complicated history of settlement and development [Braun 1985a, 73-76; 1989a; 1989].

With the notable exceptions of Hartuv [Mazar & de Miroschedji 1989], Jawa [Helms 1981] and sites of Biblical significance [e.g. Megiddo, Beth Shean and Jericho], the bulk of our information on the EB I period is derived from salvage projects : Meser [Dothan 1957; 1959], En Shadud [Braun 1985a], Yiftahel II [Braun 1984; 1985b], Beth Yerah [Amiran & Cohen 1977; Yoyev 1985, 14], Tel Shalem [Eisenberg 1987], Tel Kittan [Hadashot 1976], Tel Teo [Eisenberg 1989], Kabri [Scheftelowitz 1990], "Small" Tel Malhata [Negev] [Hadashot 1979] and Palmahim Quarry [Braun 1990]. What indeed would be the state of knowledge of the EB I without the information derived from these last sites ?

Biases of this sort undoubtedly remain in the primary data base and will probably continue, with only some modifications, to plague researchers for a long time to come. Similar biases can, no doubt, be cited for other periods equally neglected in field work. Gaps will remain in our knowledge of the archaeological record and there will continue to be "lesser known periods" despite the relative importance, in the entire buried archaeological record, of sites of those time spans.

Only the fortuitous unearthing of archaeological sites of all descriptions, combined with a systematic, continued implementation of an objective policy of salvage work is likely to persist in correcting for biases of this sort. The reliability of the primary data base, as a true reflection of the archaeological record, can then be expected to increase in direct proportion to the scope of salvage excavations, creating an ever more sound basis for the discipline.

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CONFESSION OF A NATIONALIST ARCHAEOLOGIST

by

Danny SYON

ABSTRACT

Criticism on the nationalistic tendencies of Israeli archaeologists is analyzed and classified into three types : ethical, political and ideological. Based on the experience of the writer, it is suggested that ideological subjectivity in archaeological work is inescapable, and that it does not necessarily detract from the value of the work.

INTRODUCTION

In a recent article Shay (1989) criticized the nationalistic tendencies of Israeli archaeologists. Other criticism came from Trigger (1984) and Bowersock (1984).

Reflecting upon an article which I am preparing on Gamla for a popular archaeological publication, I learned that I too would be classified as 'nationalistic', but also that I am in the generally respectable company of most Israeli archaeologists (Bar Yosef & Mazar 1982 : 310; 322).

Three types of criticism are aimed at nationalistic archaeology; that it is unethical, that it involves politics, and that it does not conform to the supposed universal objectivity of science. As for the first two – I side with the critics, with some reservations which are to follow. My aim in this paper is to elaborate on the third kind. I suggest that nationalistic archaeology in Israel can be free of the first two vices and rest only on ideology, a form of non-subjectivity, and that this need not detract from the scientific value of the work done.

DEFINITION

Loosely defined, the function of nationalistic archaeology is to "bolster the pride and morale of nations and ethnic groups" (Trigger 1984 : 360). In Israel, this generally applies to the study of the Biblical period (roughly the second millenium B.C. until the fall of the First Temple in 586 B.C.) and Second Temple period (from about 540 B.C. until the end of the Bar-Kochba revolt in 135 A.D.) which is called "the focus of Israeli national pride" (Shay 1989 : 769; Shavit 1987 : 54).

Admittedly, probably more Israeli archaeologists study these periods than any others. A contrary opinion is voiced by King (1983 : 211), who thinks that the archaeology of Early Judaism (ca. second century B.C. to the fourth century A.D.) is a "relatively neglected era in Palestinian Archaeology". I believe that nowadays there is a reasonable balance in the study of other periods as well (Broshi 1987 : 32; Ussishkin 1982 : 95), and that whoever deals with non – 'mainstream' periods is scarcely "made to feel very much alone" as Bowersock (1984 : 134) claims.

THE FIRST TWO TYPES OF CRITICISM

"How you look at the evidence depends on *what* you want to know and *why* you think it may be important" (Dever 1980 : 42).

The criticism is sometimes in the ethical domain, over the deliberate misinterpretation of the archaeological record or the use of questionable methodology to further some national aspirations. Some archaeologists were (and are) accused of such manipulations, but on the whole Israeli methodology is accepted as properly scientific in the sense that no evidence is deliberately withheld or distorted. The *value* of Israeli methodology, compared to New Archaeology methods is, however, a different question (Dever 1973; Meyers 1987 : 24*). As far as I know no one has been accused of absurd excesses, such as those of Kossinna (1911; 1912; quoted in Trigger 1984 : 360) who tried to prove the supremacy of the German race and that Germany was the homeland of Indo-European peoples. Indeed, even the late Yadin, possibly the greatest exponent of nationalistic archaeology in Israel, is generally thought to have used blameless methodology (Shay 1989 : 770). Yadin did take too far perhaps the grand phrases he habitually used to describe his discoveries (*ibid*) and actually entered politics. Others perhaps stress their feelings a bit too much (e.g. Ben-Dov 1982 : 18-19).

Sometimes the criticism is over the politicization of archaeology. This was put very bluntly by Bowersock (1984 : 134) who claims to quote an unnamed Israeli archaeologist who simply equated archaeology with politics. Olsen (1986 : 36) claims that a scholar is a political person and that "archaeology is, if anything, politics". To this I agree only to the extent that because of the exposure archaeology receives in Israel, the politicization of it is done mostly by politicians and not archaeologists. An extreme case, involving religion, politics and archaeology, was the "City of David incident" (Shanks 1981). Gamla is also a case in point; with the best of intentions, how can I keep a political group from using Gamla as a tool for claiming the legitimacy of annexing the Golan Heights, for instance (see also Olsen 1986 : 34).

THE SOURCE OF CRITICISM

Shay (1989 : 769), following up on Meyers' (1987) claim to the relative isolation of Israeli archaeologists, believes that this isolation is reinforced by the fact that modern archaeology (that is anthropologically oriented New Archaeology) differs from Israeli archaeology in focusing less on national traditions. It appears to me that even European archaeology is still largely nationally oriented – not to mention third world countries (Trigger 1984 : 358-9; Olsen 1986) – so that

Shay's argument best serves to point to the source of most of the criticism : the home of New Archaeology.

Most criticism seems to come from American scholars, who, to put it in a simplistic way, do not have the possibility to search for their own ancient past in their country (Bowersock 1984 : 141). It is instructing to note that there is no direct reference to this 'branch' of archaeology in the Code of Ethics or in the Standards of Research Performance of the Society of Professional Archaeologists in the U.S.A. (Greene 1984 : 22-27). These deal mainly with the archaeologist's legal responsibilities to the public, employers and peers, not stealing your fellow researcher's data, preparing well for a project, publishing promptly, etc. The closest reference to nationalistic archaeology is found in "Four Statements for Archaeology » by the Society for American Archaeology which states : "The archaeologist does not discard classes of information in favor of a special interest" and "Wilful destruction, *distortion*, or concealment of the data of archeology is censured..." (Champe *et al.* 1961) (*emphasis mine*).

BACKGROUND

Some words on Gamla and its excavator, Shmarya Gutmann, are in order, to better understand my views on the matter. Being a self-made historical archaeologist, Gutmann is a somewhat controversial figure in Israeli archaeology, though a well respected and liked public figure. He has been a major activist in various organizations of the Zionist movement since the Thirties. He was a messenger to the Jewish communities of Eastern Europe before the war, to try and make them immigrate to Israel before it was too late; before and after the establishment of the State of Israel he held high positions in the military intelligence; he conducted confidential diplomatic transactions for the State.

Between all these he found the time to instill the love of the land into thousands of young people by leading trips of the youth movements to all areas of the country. Especially dear to him were the Negev and the Judean Desert, where he led groups at the time Palestine was under the British mandate, without permits and under difficult logistic and safety conditions. At this time his interest in the history of the country was aroused, and especially in the late Second Temple period (from the Hasmonean revolt in 165 B.C. – to the destruction of the Second Temple in 70 A.D.). His trips took him to Masada, the famous desert fortress where a group of Zealots committed mass suicide rather than fall into Roman hands after a long siege in the year 73 A.D. After studying Josephus' accounts of it he undertook a major survey of the mountain, which he conducted intermittently for over ten years. His work on Masada resulted in a book in which he offered not only the results of the survey, but also insights into the importance of the mountain to both ancient Jews and Romans and to the modern state (Gutmann 1965). He played a major part in the drive to excavate Masada, a project finally undertaken by Yadin. Not surprisingly perhaps, in this age of a renewing Jewish settlement in the country, the first Jewish researchers took interest in Jewish subjects almost exclusively (Broshi 1987 : 26; King 1983 : 134-135; Shavit 1987 : 46, 54). Gutmann was also preoccupied by the whereabouts of Gamla, believing it a missing link in the history of the Great Revolt of the Jews against the Romans (66-73 A.D.) and a site of no less significance than Masada. He was not satisfied with Dalman's (1967) identification of the site and after the war of 1967 tried to identify it on the Golan.

Gamla was one of the first cities to fall to Vespasian on his march from the Galilee to Jerusalem in late 67 A.D.. Its key location on the route where help was expected from Babylonian Jewry made it imperative for the Romans to take it. Having been abandoned after its fall in October, 67 A.D., Gamla offers an almost unprecedented opportunity to study a Roman battle site as it was left, the development of a Jewish city which existed from about the middle of the second century B.C. until its fall, and a huge Early Bronze Age settlement (ca. 3200-2500 B.C.). The wall of this Early Bronze Age settlement may be responsible for the tradition in the Jewish literary sources that Gamla is a walled city from the time of Biblical Joshua (Tosefta; 'Arachin 5 : 37). Josephus is, however, the main source on the city – its location, topography and the very vivid description of the battle (War, IV 1).

The description of the final moments of Gamla is similar in many ways to that of Masada. This created the commonly held belief that five thousand of Gamla's defenders and citizens committed a mass suicide by jumping off the cliff, rather than fall in Roman hands. Bolstering this belief would be in our nationalistic interest. The evidence however does not support this. Based on topographical and demographical considerations, as well as on the fact that at Gamla there was an actual battle fought, contrary to Masada, where the defenders had time for deliberation, Gutmann believes that people were actually trying to flee down the steep slope in sheer panic with the inevitable result of many dying.

The one find that more than any other supports the claim for national importance of the site is a very crude coin, probably minted locally, which bears the inscription : "For the redemption of Jerusalem the H(oly)" (Meshorer 1982 : 129).

SUBJECTIVITY IN SCIENCE

Archaeology operates in a social context and not in a 'scientific' vacuum, and therefore we should resign to the fact that it is subjective to a certain degree. If indeed one of archaeology's aims is to help social scientists to better understand the nature and direction of current and future human activity, then the archaeologist cannot detach himself from this activity – he is part of it (Trigger 1984 : 357; Olsen 1986 : 37; McGimsey 1984 : 172). Perhaps the following quote makes the point : "... we will be able to be more objective only if we learn to conceal our subjectivity less" (Scharfstein, quoted in Broshi 1987 : 32).

Shay (1989 : 771) would like to see Israeli archaeology "reappraise its traditional assumptions and respond to the new trends, which have emerged in response to the Western ideals of objectivity and universalism". Kemeny (1959), Maquet (1964), Nash (1976) and Winter (1984 : 42) agree that we cannot escape from our own cultural values and personal biases and that the quest for a totally impersonal objectivity in science should be abandoned. This is not to say though that one should not strive for separating value judgments from scientific approach whenever possible (Winter 1984 : 45). To borrow from Anthropology again :

"To define adequately an Anthropological study, is not enough to indicate its object ... but one should add : as seen by an Anthropologist belonging to [certain socioeconomic class in a certain social environment] (Maquet 1964 : 51)

IDEOLOGY

My view is that nationalistic overtones creep into archaeology when the importance of certain finds or interpretations is emphasized over others, according to the convictions and ideology of the archaeologist.

Being a member of modern Israeli society I am doing what other archaeologists in other countries do when excavating sites connected with their own history (see for example Olsen 1986) : I am looking in the record for links between the present and the past and gladly show it when I find them. Trigger calls this "affirming the links between an intrusive population and its own ancient past" (1984 : 358), "intrusive" being an adjective which is, to say the least, controversial. This is not to say that I shall *ignore* other evidence (such as the Early Bronze Age settlement) nor that I shall *create* it if not there. Only if by international consensus all archaeologists were to practice only in foreign countries and not their own, would we (perhaps) be rid of nationalistic archaeology – a suggestion neither practical nor necessary. After analyzing the data I will stress what I believe to be more important to me – namely the finds that relate to the Jewish city and to the war. Whatever I choose to stress, however, the data is there for anyone else to interpret.

The following statement by Ussishkin, substituting 'Second Temple' for 'Old Testament' and 'Historical sources' for 'Biblical sources', reflects my feelings when I approach archaeology :

"To Israeli Archaeology, this connection with the Old Testament, with the Biblical sources, is deeply emotional, and it gives us a special satisfaction, perhaps even a special happiness, when working in the profession of Archaeology. I think that this is responsible for a large part for the motivation of Israeli Archaeologists" (1982 : 95).

I arrived at Gamla as an amateur archaeologist and volunteer in 1977, and have been literally trained in the field for the past thirteen years. It is clear to me that working there under Gutmann has had a profound effect on my thinking. Having listened for years to his stories about his exploits as a younger man and his dedication to his work, I naturally came to see in Gamla more than just a site to excavate, and my interest too focused on the historical period of the Second Temple.

An archaeologist's first interest in his subject can be aroused in many ways : be it reading about it, hearing a lecture, a field trip. My introduction to Gamla is just another way, and it did not change my political or ideological convictions. But I doubt if a person can escape from his ideologies in any of the roles he plays in life (see Meltzer 1981 : 116-117). My Zionist ideology may have affected my choice of coming to Gamla in the first place, my interest focusing on a certain historical period and my wish to further the knowledge about this period by excavating at Gamla (see also Shavit 1987 : 54; Olsen 1986 : 26). It has not affected my choice of methodology nor my interpreting the record in the light of well established rules and commonly held concepts – to the best of my ability.

My nationalistic archaeology is therefore taking pride in the achievements and regretting the omissions of my distant forebears, as these appear in the archaeological – and historical – record, and sharing this with the scientific community and the public.

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OBJECTIVITY IN THE INTERPRETATION OF THE ARCHAEOLOGY OF GHANA

by

E. Kofi AGORSAH *

One of the desires of the majority of archaeologists is to obtain from their analyses an objective knowledge of the societies they study. However, the mere desire to obtain objectivity does not ensure its attainment especially where biases cannot be controlled. Added to this difficulty of obtaining objectivity is that not only is the subject matter of archaeology vast and varied, but also archaeologists disagree on directions of study and interpretations, and in many cases very few scholars attempt to put themselves at a standpoint from which to measure the inadequacies and unevenness of their interpretations. Fundamental to interpretations in archaeology is the definition of cultural entities as well as of their constituent elements and such interpretations are not acceptable by fiat.

Ghana, the country formerly known as the Gold Coast, is centrally located on the southern coast of the great bulge of West Africa, sharing borders with Togo, Ivory Coast and Burkina Faso on the east, west and north respectively. These borders were the result of very recent accidents of history and political expediency rather than geographic or cultural considerations. This situation is not peculiar to Ghana or to West Africa. The point of emphasis, however, is that the focus of this paper, which is the geographic area known as Ghana, is discussed as an artificial or "an incomplete" cultural entity with a record of a past that cuts across its modern political boundaries.

Roughly rectangular in shape, Ghana measures approximately 680 km (420 miles) from south to north and averages about 430 km in width, thus providing a total area of approximately 238,000 sq.km (92,100 sq. mls), roughly the size of Great Britain. Archaeological activity within these borders developed only within the last century and, until recently, only as an appendage to amateur activities and European Archaeology (Posnansky 1982), although there had been an earlier awareness among the local societies of the significance of cultural material (Anquandah 1982). However, with a very limited number of professionals, many of them just passing through or spending brief periods in Ghana, considerable work has been undertaken that, put together, constitutes a good basis for reflecting upon interpretations of its cultural past.

This chapter, therefore, is an attempt to examine some selected crucial issues emerging from interpretations of the archaeological evidence relating to various stages of cultural development in Ghana and adjoining areas. The question is : do the interpretations of the prehistory and history

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of Ghana provide us with an objective knowledge of the societies of the time and their ways of life ? If so where do we go from there ? If not, what are the weaknesses or unevenness in the evidence or interpretations that need to be redressed ?

These and related issues will be examined in the context of the archaeology of the general area as well as methodological dimensions gained by the discipline in recent years. Although there have been attempts by some scholars to recount the history of the development of Archaeology in Ghana (Anquandah 1982, Myles 1986, York Mathewson, Calvocoressi & Flight 1967), no critical assessment of the *objectivity in interpretation* of the archaeology of the area, as examined in this paper, has ever been undertaken. In addition, with the increase in the pace of research activity in the area as more qualified professionals are emerging, there is the ardent need to begin to bring together the various ramifications in theory, method and techniques that have been and continue to be utilized in the archaeology of the area. It is expected that this paper will identify issues the redressing of which will lead to a better understanding of the cultural development within the general geographical zone around Ghana.

EARLIEST TIMES AND INTERPRETATIONS

The archaeological interpretation of the cultural past of Ghana dates back to the end of the 17th century by European travellers such as Barbot (1732), who described what is known in Ghana as *nyame akuma* [polished stone axes], Bowdich (1819), who wrote about the spectacular *aggrey beads* of Ghana and John Lubbock in the 1880's (Anquandah 1982). But much of earliest works that touched on archaeology came from such geologists as Kitson (1916) and Junner (1940), both Directors of the Geological Survey of Ghana in the early 1900's. The former described objects now referred to as "cigars" and known to be associated with early food-producing societies of the Late Stone Age of Ghana, as well as grinding hollows and cisterns or wells of the same period and cultural traditions. Activities of scholars such as Junner and the successor of Kitson, Braunholtz of the British Museum, who visited West Africa in the 1930's, Captain P. Wild (1927) Inspector of Mines in Ghana also in the thirties, and Thurstan Shaw (1944), an archaeologist, first appointed curator of the then only museum in Ghana situated at Achimota near the present university at Legon are well outlined elsewhere (Anquandah 1982) and clearly indicate that long before systematic archaeological research in Ghana scholars had already begun to make speculations about its past using all kinds of evidence. Names that also count in these early efforts include A.W. Lawrence who published a book on European forts and castles along the coasts of Ghana (C. Lawrence 1963), H.D. Collings and R.B. Nunoo, associated with the establishment of the National Museum, the latter being its first Ghanaian Director, Peter Shinnie who initiated the teaching of archaeology at the University of Ghana, Paul Ozanne who first established a chronological scheme for Ghanaian archaeology using smoking pipes as a basis for his classification (Ozanne 1971), and last but not least, Oliver, Davies, a geologist who has the credit of having conducted the most extensive ever archaeological survey of Ghana, particularly of the Volta Basin. Davies was also the first person to attempt a reconstruction of the cultural development in the general West African region (Davies, 1964, 1967; York Mathewson, Calvocoressi & Flight 1967).

Although the Volta Basin rescue project generated considerable archaeological research (location of sites) the distribution of researched sites was geographically uneven and there were gaps in the chronological schemes afforded by the available evidence.

Scholars in other disciplines such as Linguistics, History, Museology and the natural sciences have also contributed considerably to early research efforts in the archaeological reconstruction in Ghana. Linguists have for example used the growth or development distribution of languages and their relationships to speculate about the movements of language or ethnic groups (Painter 1966, Dakubu 1976, 1982, Hall 1983, Steward 1960). Geological studies as well as those by botanists and allied subjects have similarly contributed to the archaeology of Ghana.

Discussion of objectivity in interpretation of archaeological material could be conducted in three ways. The first approach is to examine individual scholars, their particular studies and how their interpretations relate to those of other scholars in the same field. This obviously means the discussion would be limited to a few selected archaeologists. This is because it would be practically impossible to examine the numerous scholars that have been involved in one way or another with archaeological research in Ghana and adjoining areas. The second approach is the one that could discuss emerging issues or themes that are crucial to the interpretation of the archaeology of Ghana. In this approach only some crucial issues would be selected. Besides, there is the possibility of a discussion that could easily ignore the temporal and spatial contexts of the archaeological evidence under discussion. The third approach is one that could discuss the material and studies related to specific periods beginning from the earliest to the more recent. Resort to this chronological approach will not only permit a discussion that would involve such considerations as environmental changes, cultural adaptation and transformation over time but would also afford comparative analysis that could allow cross-cultural considerations, and analysis of recent advances in archaeological method and theory in the area. Although this third scheme may not initially emphasise specific themes, it is expected that some crucial related issues will emerge that would then form the basis of the dialogue that this paper is expected to generate.

INTERPRETATION OF THE EARLY STONE AGE

In Ghana and indeed the whole of West Africa there is a dearth of evidence regarding early man. What is known about man's origins is based on limited evidence and speculations about cultural material. If we are to accept the stone tool finds designated as belonging to the general African type described as Oldewan, Acheulean and Sangoan, some dating to as early as 50,000 B.C. (Davies 1967), then we can speculate that human societies were adapting to and exploiting the environment and resources of Ghana in those early times. In spite of the paucity of evidence scholars in various fields have attempted studies that somehow provide a picture of those earliest times in Ghana and adjoining areas that provide an interpretative framework.

However, much of the interpretative framework lacked objectivity. For example, Davies (1964, 1967) attempted a reconstruction of the Stone Age in Ghana dating as far back as 50,000 years ago in the context of the general West Africa area. Using evidence of changes based on European climatic nomenclature and geological sequences based on raised beaches, past river terraces and soil sections in Ghana, Davies speculated about very early stone age traditions

though not as early as those of Eastern Africa. Many of the Stone Age sites discussed by Davies (1964) appear to be located in the northern Volta Basin. Although features of the period are noted in coastal areas particularly in valleys around the south of the modern power station about 100 km north of Accra. The weaknesses in Davies' speculation stem firstly from the application of a European terminology and climatic classification to an African situation and secondly because the speculations were based mainly on surface material.

Andah (1979) has strongly criticized the philosophy behind Davies' chronological scheme considering it as inappropriate. Further, Andah argues, and quite rightly, that the use of such Kamasian pluvials dating in Europe to around 50,000 years ago was too arbitrary to be acceptable as a scientific analysis. Studies conducted at the coastal site of Asokrochona near Accra also reveal more of the lack of objectivity in Davies' interpretations. For example it has been argued (Andah 1979, Nygaard and Talbot 1976) that much of what Davies describes as Sangoan (Middle Stone Age) tools may in fact have been a variant of Acheulian (Early Stone Age) material. The excavations at Asokrochona constituted the initial reaction to and the curiosity of scholars towards ascertaining not only the subjective interpretations of Davies but also his diffusionist propositions.

Considerable debate on the suggestion that the earliest cultural traditions in Ghana came from the north, outside its modern territory, had until recently dominated the interpretation of the archaeology of the area. The main reason was that not much had been studied in the south that suggested the antiquity that Davies had assigned to the northern cultural traditions. In order to reassess the confusion that Davies propositions had created Andah conducted a detailed analysis of the Asokrochona material and presented a chronological framework that was more objective. Owing to its significance for the Stone Age in Ghana a brief summary of it is provided.

Andah and Nygaard set out to ascertain the stratigraphic picture claimed by Davies and "to delimit the character of the industries at Asokrochona and their environmental setting" (Andah 1979). Two cultural levels, identified and designated as Asokrochona I and II, provided a lead to the establishment of a general chronological scheme for the site. Andah adopted an approach in his artifact analysis that was a departure from those formally dependent upon systems worked out by use of European terminology for Eastern Africa. Andah referred to his Sakumo I as Oldowan chronologically equivalent to what is known in Eastern Africa. One significant point to note that makes Andah's approach much more objective is his acceptance of the typology as a framework or model for interpretation. Secondly, also objective is his basis of overall identification of morphological differences and methods of manufacture of tools. However, he needs to demonstrate in a much more convincing way the relevance of his metrical analyses because they do not demonstrate how the differences or similarities can be related to variations in related activities. Would one need to take into consideration consistent availability of raw material over time in a continuous fashion, how do we relate Sakumo I to II in terms of the wider variation observed with Sakumo II? One tends to see Sakumo II as indicating ecological adaptation geared towards the exploitation of a wider variety of resources over a possibly wider area. It thus probably represents a transformation to a later period somehow similar to the range of equipment used by the early users of Bosumpra Cave excavated by C.T. Shaw (Shaw 1949) although in a different environmental resource area. A final issue with Andah's study of the Early Stone Age, in Ghana by way of the Asokrochona excavation and analysis is its cross-cultural applicability. This appears to be the most outstanding achievement over Davies' study. The

importance of the cross-cultural implications of the Asokrochona study unfortunately has been the only one known since Davies. Other studies have only examined the peripherals of the issue working on the dying period of the Stone Age, particularly the transitional period between the Stone and Iron Ages in Ghana. Despite the fact that Acheulean and later microlithic material from other sites such as Asokrochona (Andah 1979), Bosumpra (Shaw 1949), Apreku and Tetewabuor (Anquandah 1982) suggest a date around 50,000 years ago, the date cannot be generalized to depict similar cultural materials of the basin. Apart from the Asokrochona material there is the need to examine evidence from other sites researched in the basin. These include rolled pebble tools (choppers) from the site of Kadengben, Mamata and Yapei; quartz pebbles from Akroso Amanfoso; tortoise cores from Asukawkaw and broken Acheulean handaxes from Otisu and Apasu. Many so-called Middle Stone Age sites have also been recorded near Kete-Krachi. An examination of the range of material recovered from the basin and adjoining areas should provide some clues to the debate generated by Davies' interpretation.

From the site of Rim adjoining Burkina Faso, have for example been identified sites that flank the main stretches of the valley of the upper portion of the Volta. Of the three phases identified by Andah, the first one was characterized by a late Stone Age lithic industry, dated by radiocarbon and geomorphological features to between 12,000 and 5,000 B.P. This first phase contained artifacts similar to the types found at some of the sites located by Davies in the Volta Basin. The second phase has been dated to between 3,600 and 2,900 B.P., while the third is dated to between 1,500 and 1,000 B.P. These last two phases contain lithic material that indicate continuity with the Iron Age that followed. The transition to food production explains the transformation observed in the changes in the tool kit in response to changes in the ecology. The evidence from Rim and those from various parts of the Volta basin thus seem to be what can be used at present to speculate that there certainly were human occupations in parts of Ghana and adjoining areas in Quaternary times.

THE LATE STONE AGE

Available evidence indicates that by at least 4000 years ago, or most probably earlier, some parts of the Volta Basin and coastal areas of Ghana were inhabited by Late Stone Age food-production societies. The site of Ntereso in northern Ghana and Chukoto on the west bank of the Black Volta are the best known in the basin. The former dates to the early part of the second millennium B.C., and evinces early food production that may have involved fishing and farming. Bones of dwarf goats or sheep as well as polished stone axes, bone harpoons and fish-hooks and terracotta objects referred to as "cigars" were recovered (Agorsah 1976, 1985). A main feature was wattle and daub house structures (Davies 1973). The site of Chukoto contained pottery with heavy rolled rims.

The late Stone Age cultural tradition of Ghana has been referred to as Kintampo after the village in northern Ghana where it was first recognized and where several sites are located (Fig. 2). Davies at first referred to the tradition as Neolithic (Davies 1962). The Kintampo cultural tradition is associated with the earliest known houses. They are therefore considered the pioneers of village life in Ghana. Evidence from such sites as Nsesrekeseso and Mumute Bonoase, Boyase and Kintampo include terra-cotta figurines, grinding hollows, bifacially ground stone axes and stone beads, as well as faunal and floral remains (Stahl 1985) which indicate activities

related to food production and settlement (Agorsah 1976, Dombrowski 1976, Newton and Woodal 1976, Stahl 1985).

Recent settlement pattern analysis in the Volta basin seems to provide evidence that enhances our knowledge of late Stone Age societies (Agorsah 1988 a & b). It is observed that settlement patterning within the Volta basin operated within a framework of environmental and culturally determined parameters. For example evidence suggests that the preferred areas of habitation were terraces and local hill lands or slightly raised areas overlooking valleys. Further it is observed that the location and distribution of settlements indicate that many of the early settlements in the northern Volta basin developed in valleys, flood plains, fans, confluences, and pediments with soils that are periodically revived by alluvial and colluvial deposits. Sites on higher grounds may have been good in time of flooding and for game. The sites of Ntereso and Kintaupo mentioned above indicate that early settlers within the basin took advantage of such locations from the beginning of the Stone Age (Davies 1973). Flight (1967) doubts the objectivity in this interpretation of the presence of human habitation of the floodable zones. Evidence from Ntereso also clearly suggests dependence on aquatic floods (Carter and Flight 1972).

Evidence for reconstructing the Late Stone Age in Ghana and its interpretation has seen much more resolution in recent years. The controversy about the use of the term "Neolithic" seems to be dying out as the term has somehow disappeared from the literature in recent years. Still unresolved, however, are the questions of the actual distribution of late Stone Age cultural traditions as well as whether they were indigenous or derived from outside as Davies claimed. In relation to this claim is the question of human movements in the general area of Ghana and neighbouring territories. Linguistic studies have generally enhanced the objectivity in interpretation and the quality of archaeological reconstruction in Ghana as is the case in other parts of Africa (Ehret & Posnansky 1982). The chronological dimensions of the settlement history of the Volta Basin as indicated by archaeological and ethnographic evidence (Agorsah 1986, 1988, Andah 1979, Davies 1967, 1971), as well as linguistic speculations (Dakubu 1982, Painter 1966, Stewar 1960), certainly extend human colonization of the basin into the Stone Age. Both types of evidence indicate that within the past 10 to 12 millennia there has been a gradual increase in the occupation of the basin right from its sources in Burkina Faso. It is now established that the first wave of settlements in Ghana probably occurred in the Stone Age within the area of the confluence of the White and Black Volta. This area coincides with the area of the greatest concentration of Stone Age sites (Agorsah 1988). To enhance the adequacy of explanation of the early traditions, this first wave of settlement has been considered alongside evidence for environmental change that occurred in the whole Sahel-Sahara belt of West Africa some 10-12 millennia ago (Andah 1979) which may have caused the selection of sites overlooking streams and river valleys. If similarity in cultural equipment means anything to the archaeologists, then the evidence can be considered as pointing to connections with cultural traditions outside the general area of Ghana as Davies claims. In addition to this, the linguistic evidence regarding the break-up of the Niger-Congo language family supports the claim of an outside connection, even if it did not come from the Sahara as Davies claims (Davies 1961).

Another unresolved issue in the archaeology of the late Stone Age in Ghana is the significance of the artifacts referred to as "cigars" or "rasps". Although it is one of the diagnostic artifacts of the Kintampo cultural tradition there is a lack of agreement on explanations given for given for their use, or their significance in the tradition as a whole. In fact, "cigars" remain one

of the most controversial artifacts in African archaeology. The cigars are generally cylindrical, oval in section, some measuring about 15-22 cm long and 3-5 cm wide in the middle section. The majority have geometric scorings and have been observed to have been worn down through use (Agorsah 1986). The interpretation of "cigars" is one of the least understood aspects in the archaeology of Ghana. This lack of explanation of the cigars demonstrates how archaeologists are limited in achieving an objective interpretation without ethnographic data.

The use of ethnographic data to build models for the interpretation of archaeological data has continued to attract a large following mainly because of the central role occupied by the logic of analogy. Ethnoarchaeology, as this approach has come to be referred to, has its roots in the need to identify a body of generalizations or inferences beyond mere date. Also ethnographic data has provided a means for initiating an approach towards objective interpretation, that is based on well articulated theoretical and methodological frameworks (Phillipson, 1985, Brown 1963). The use of ethnographic research as espoused in Ethnoarchaeology should, however, be considered as a means to an end (Agorsah 1990).

THE IRON AGE

Like speculations about the Stone Age traditions, interpretation of the Iron Age has lacked objectivity because of its connection with diffusionist concepts. In the whole of the West African region metal working has been considered to have marked an important advance in technology but its beginnings have been attributed to outside cultural traditions. These diffusionist ideas have usually been linked to the general West African area. Scholars such as Mauny (1952), Tylecote (1975), Arkell (1953) and Shinnie (1971) suggested that iron technology was derived from Carthage, Meroe, Egypt or some other area outside the West African area. The background to all these speculations was the notion that no technological development of the magnitude known for the Iron Age could develop from West Africa. While some unobjectively based their speculations on furnace types (Williams 1969) others based them on known dates and chronologies as well as on Eurocentric ideas. Recently, Andah in his analysis of these subjective speculations has emphasized the uncritical way in which the question of the origin and the development of iron has been examined. Andah states that "their conclusions have also been based on the untested assumption that where the conditions expected are not in fact met with, then such an area must have been backward, and not capable of independent discovery" (Andah 1979 : 140). Lack of objectivity in these early speculations which in the early history of the archaeology of Ghana plagued interpretations is demonstrated by later evidence.

As indicated in the early part of this paper, the chronological divisions of Ghana's past into Stone Age – Iron Age are only for analytical purposes and convenience. Evidence for the transition from the late Stone Age food-producing traditions to the introduction of metal technology is fragmentary and unevenly distributed in Ghana as a whole. Where the evidence exists it is sometimes flimsy and unreliable. From the site of Ntereso for example, iron was present in levels the temporal association of which is not definitely known. Buipe, the area currently occupied by the Gonja ethnic group, provides archaeological evidence of the transition. The first of three phases at New Buipe (York & Calvocoressi 1967) manifests an early Kintampo food-producing tradition C¹⁴ dated to 915 ± 27 B.C. Following closely is an eighth century

A.D. date which marks the second cultural phase which also contains evidence of an iron technology (Anquandah 1982).

The onset of metal technology in Ghana is best discussed in the context of the General West African area (Davies 1967, Andah 1979). The determination of how early the technology was in West Africa as well as lack of data on the process of its development are problems that are related to early attempts to reconstruct the history of the development of the technology. Many of the dates are not only misleading but have gaps in reason. Evidence from Nok in Nigeria dates to 500 B.C. (Shinnie 1967), while the Chad area north of Nigeria provides dates of 2000 years ago (Andah 1979). Clearly the dates from Ghana do not fall within a close range of these dates.

One significant development in the archaeology of the Iron Age, in West Africa, even in the face of the fragmentary evidence is the demise of diffusionist theories. Considerable ethnological evidence exists in Ghana that explains variation in the technological practice in recent times (Pole 1974, Andah 1979, Wertime & Muhly 1980).

Evidence from the Volta Basin of Ghana seems to confirm that the distribution of iron sites suggest that the greater part of the basin had been occupied by the last centuries before the Christian era, and that some were comparatively large settlements. According to Davies (1971), population within the gallery forest areas of the Volta Basin was quite dense. Unfortunately, however, the evidence of an increase in the number of settlements during the Iron Age is not complemented by chronological data that can be used to demonstrate the developmental stages of the settlements. Another area that has seen much study is the reconstruction of the iron working process mainly using furnace types and the ethnographic evidence (Pole 1974, Goucher 1981). Recently studies carried out by Goucher demonstrate the impact of metal working traditions on the environment (Goucher 1984). The study draws attention to the fact that many studies have ignored the examination of the "intermediary refinery (foundry) operation" (Goucher 1989). This draws attention to the fact that each process of metal technology should be studied as a system in itself because that is the only way to obtain an objective view of the process as a whole. This also applies to the study of other metal technological traditions such as those of gold, copper and brass industries. But with the accompanying rise of states and kingdoms speculations have seen much more objectivity with availability of evidence.

One of the most significant contributions of archaeological research in Ghana has been the provision of evidence that explains the origin and development of towns, kingdoms and civilizations in the area. The Volta Basin, particularly the northern section, has been identified as one of the crucial areas in the development of early towns and traditions such as those of the Guang, Dagomba and Gonja (Agorsah 1986, 1988, Aquandah 1982, Kense 1985). Owing to the good environmental conditions and the trans-Saharan trade that linked such trade centres as Damongo, Buipe, Yeji, Salaga (Anquandah 1982) and one of the largest trade centers of the day, Begho (Posnansky 1971, 1975, 1976), the period, mainly around the beginning of the second millennium A.D. seems to have witnessed not only increased trade activities (Posnansky 1973) but global movements of populations, generated by the rise of large political entities (Daaku & van Dantzig 1966, Effah-Gyamfi 1979, Boachie-Ansah 1985, Fy 1975). Using oral traditions and archaeological evidence Effah-Gyamfi (1979) has established the rise of the state of Bnomanso as a strong force in West central Ghana and even compared it to the greatness of the ancient capital of the Sudanic empire of Ghana (Effah-Gyamfi 1979). The gold trade centred

on the Begho area (Posnansky 1973, 1975), as well as viable local mining, pottery making, ivory carving, textile, brass and iron manufacturing industries as depicted by archaeological evidence, seem to have provided support to the development of the Bonomanso and adjoining areas in the later part of the Iron Age. Oral tradition, Arabic documents and limited archaeological evidence also reveal the rise of the Gonja Kingdom in Northern Ghana as well as the Dagomba, Mamprusi Wala, Mossi and others, many of whom developed through Islamic connections.

In the southern portions of Ghana, many Kingdoms sprang up with those of the Akan standing out prominently. The Akyem, Asante and Adansi were the more prominent ones in the forest zones while nearer the coast developed the Ga and Dangme who are said to have migrated into the area. Our knowledge of the later kingdoms comes more from historical than archaeological evidence and has been summarized by Anquandah (1982). The kingdoms of Efutu and Ahanta, Elmina, Kommenda and others also sprang up along the coast and were still viable up to and including the arrival of the first Europeans on the coast of Ghana. Excavations have been conducted at Efutu (Agorsah 1986), Elmina (Decorse 1987a & b, 1988), Fort Ruychaver (Posnansky & Van Dantzig 1976) and various studies carried out on the Forts and Castles along the coast (Lawrence 1963, Van Dantzig 1978, 1980). These afford us evidence on pre-and European context, but illustrate the limited Historical Archaeology that has been carried out in Ghana. Although evidence of the period involved has documentary support there still exist areas of controversy that cast doubts on objectivity in interpretations which need to be examined in the same way as those of the preceding periods.

The picture that we see about Ghana's past generally is that the archaeological evidence covers a period that spans some 50,000 years with gaps in it. Although a sequence can be proposed for this period of time it cannot be strongly suggested that it provides a basis for making strong generalizations coherent enough to be considered objective. However, the fact of this lack of a clear objectivity does not mean a complete lack of meaning in the interpretations. Discussion of certain issues may help to elaborate further on these states of interpretation of the archaeology of Ghana.

EMERGING ISSUES

1. Chronology

One of the most important issues that come up for discussion is that of the chronological framework used for interpretations of cultural traditions. The earliest real attempt at a chronological scheme comes from extensive studies carried out by Oliver Davies (1964, 1967). The first problem of Davies' chronology is the fact that a considerable portion of his evidence was derived from surface collection, thus lacking stratigraphic correlations. Analyses of his framework, which gives the impression of a diffusion of much of the earliest cultural traditions in Ghana, by the later scholars such as Andah (1979), Sutton (1980), Nygaard and Talbot (1976), have streamlined the subjective aspects of his scheme. For example as Sutton (1980) has pointed out the unclear dating of geological features as well as relating the sequences to the affinities of the artifacts, suggests that faulty typologies are used which suggest traditions from other parts of Africa that do not properly apply to the material from Ghana. This lack of objectivity has also been emphasized by Andah (1979) and Talbot who have, like Sutton,

suggested the abandonment of the use of Stone Age terminologies, such as "Aterian", "Kalinian", "Lupemban" and similar ones that apply to regional traditions of Northern and Eastern Africa, as well as alien concepts and typologies in the Archaeology of Ghana. The chronological framework seems to go back some 50,000 years for the earliest known and dated Stone Age material from Ghana and continues through a series of microlithic industries to a Late Stone Age early food-producing cultural tradition dating to about 4000 years ago. This latter period merges into the Iron Age beginning about two thousand years ago and leading into the historical period over half a millennium ago when the first Europeans set foot on the coast of Ghana. Prior to these existed the Trans-Saharan trade contact. The above chronology is quite generalized. It is the details that raise several issues of terminology, analyses, and methodology. For more recent periods chronological schemes have been based on certain dated artifacts such as smoking pipes (Ozanne 1971). The issue of chronology thus becomes directly linked to methodology.

2. Methodology

The issue of methods of approach is one that has been crucial in the achievement of objectivity in the interpretation of the archaeology of Ghana. Instances of projects arising from chance factors are recorded now and again in the archaeology of Ghana and have constituted the determining factor of many significant studies. But the majority of archaeological studies have been determined by traditions within particular institutions or departments, grant requirements, a researcher's individual training and interest, as well as master-student relationships, the last two being the major factors in recent decades. A feature that seems to have characterized the archaeology of Ghana is the fact that the development of cultural traditions has been better explained only in the context of the general region. The origin of agriculture, for example, has been better explained in the context of evidence of the ecological zone of Ghana. Consequently in a new approach the earliest developments of the cultural traditions of the area have been linked with geomorphological and related conditions (Andah 1979). This approach has been recognized as much more objective because it takes cognizance of the fact that the boundaries of modern Ghana are only recent and did not exist in prehistoric times. It is for this reason that propositions such as the "invasion of Ghana" by early agricultural people as was proposed by Davies (1964, 1967) should never again arise at all. Other issues such as trade, rise of kingdoms and later historical developments have all been better understood in a wider ecological or geographical context. The Daboya Project (Kense 1983, 1985), the Volta Basin Archaeological Research Project (Agorsah 1983, 1986), the West African Trade Project (Posnansky 1973, 1975, 1976) are examples of projects that deal with zones rather than individual sites or areas. This is the strategy, whether intentional or mere accident, that has helped streamline the chronology and its related interpretations in the archaeology of Ghana.

3. Ethnoarchaeology

One concept of archaeological research in Ghana had been ethnoarchaeology, although only a few can boast of such studies in the true sense of the sub-discipline. The practice of referring to ethnographic study as ethnoarchaeological is quite common (Anquandah 1985) and has been one of the obstacles in archaeological interpretation in recent years. On record, however, are useful ethnoarchaeological studies such as those by Fletcher (1977) on the development of structural features in the historic village of Haui (McIntosh 1977) on decay patterns of mud-walls in the

Begho area. To this list may be added my own study of the relationship between construction processes and decay patterns of mud-wall structures in Ghana (McIntosh 1977, Agorsah 1985). These above-mentioned studies and a very few others somehow utilize the methods of ethnoarchaeological research procedure. The general practice among many other studies is to infer past archaeological behaviour directly from ethnographic or ethnohistorical data. This means that although the concept of "analogy" occupies a central position in archaeological interpretations in Ghana, it has been misused or totally ignored in several studies. As stated earlier ethnographic evidence should only be taken into account to formulate models for use in interpretations. They are means to an end. It is becoming clear in the Archaeology of Ghana that mere accumulation of ethnographic data and the provision of half-baked generalizations, which often unfortunately appeal to public hearing, with a cautionary note that "more needs to be done on this subject", are in effect obstructing objectivity. Frankly speaking, in the whole of sub-Saharan Africa, ethnoarchaeology has its roots in ethnographic research but its use does not seem to have gone much farther than a mere accumulation of ethnographic data. Consequently many scholars working in Ghana who do ethnographic research and are able to make the most tenuous link between their material and archaeology, have enjoyed the liberty of claiming to have undertaken an ethnoarchaeological study. Hypothesis formulation also goes with assumptions and arguments of relevance and the proof of a hypothesis does not come by fiat or traditional practice but by controlling the study by use of test implications and testing the prediction to confirm or disconfirm them. This is basically how the relationship between modern behaviour patterns and prehistoric archaeological data should be. As has been pointed out, ethnoarchaeology, whether by "analogy" or "anomaly", should be considered as a means to an end in social scientific analysis in which it is considered that explanation, not mere description, is the highest form of knowledge.

4. Identifying populations

One area where archaeological research in Ghana has often run into difficulties regards ethnic identification. Even though traditions can be generally assigned to archaeological data it is difficult to establish clear continuities. It is here that ethnoarchaeology has been of use but also has often been misused by several scholars, especially by assuming direct connection between nuclear and prehistoric populations, forgetting that in very recent years populations have moved from place to place or become mixed.

The concept of ethnicity has been in the literature for the past half-century but the term remains elusive (du Toit 1978). Ethnic groups are normally considered as named, bonded units of people who identify themselves or others as different and may have similar or identifiable social behaviour patterns and value systems such as language and religious affiliations. The ethnic group may also display more or less distinctive patterns of economic and political organization. Linguistic evidence has been of considerable use in collaboration with archaeological evidence in speculations about the known ethnic groups of Ghana (Dakubu 1982, Painter 1966, Steward 1960, Agorsah 1988). The reconstruction of the settlement history of ethnic groups in the Volta Basin is one of the most recent such attempts at linking evidence from archaeology with defined populations. As such studies spread out into other parts of Ghana a new picture that will be obtained could shed light on the cultural past of specific groups beyond historical times.

5. Other related aspects

There are several areas of archaeological study in Ghana that have added to our understanding of its cultural past. These include settlement pattern analysis (Agorsah 1986, 1988), historical archaeology (Decorse 1986, 1988) and art history (Bellis 1972, Calvocoressi 1970, Nunoo 1970), as well as palaeoenvironmental studies (Talbot, 1983, Talbot & Delibraiss 1980). These cannot be discussed here. In fact the discussion of this paper cannot be considered as exhaustive of all the various features of the archaeology of Ghana that could have been examined. Although one may call for continuity in the efforts, it appears that what needs to be done now is a reorganization of the available data in order that further data collected may not leave doubts about their use in obtaining an objective knowledge of the cultural past. As has been remarked at the beginning of this paper a desire to obtain objectivity does not necessarily ensure its attainment.

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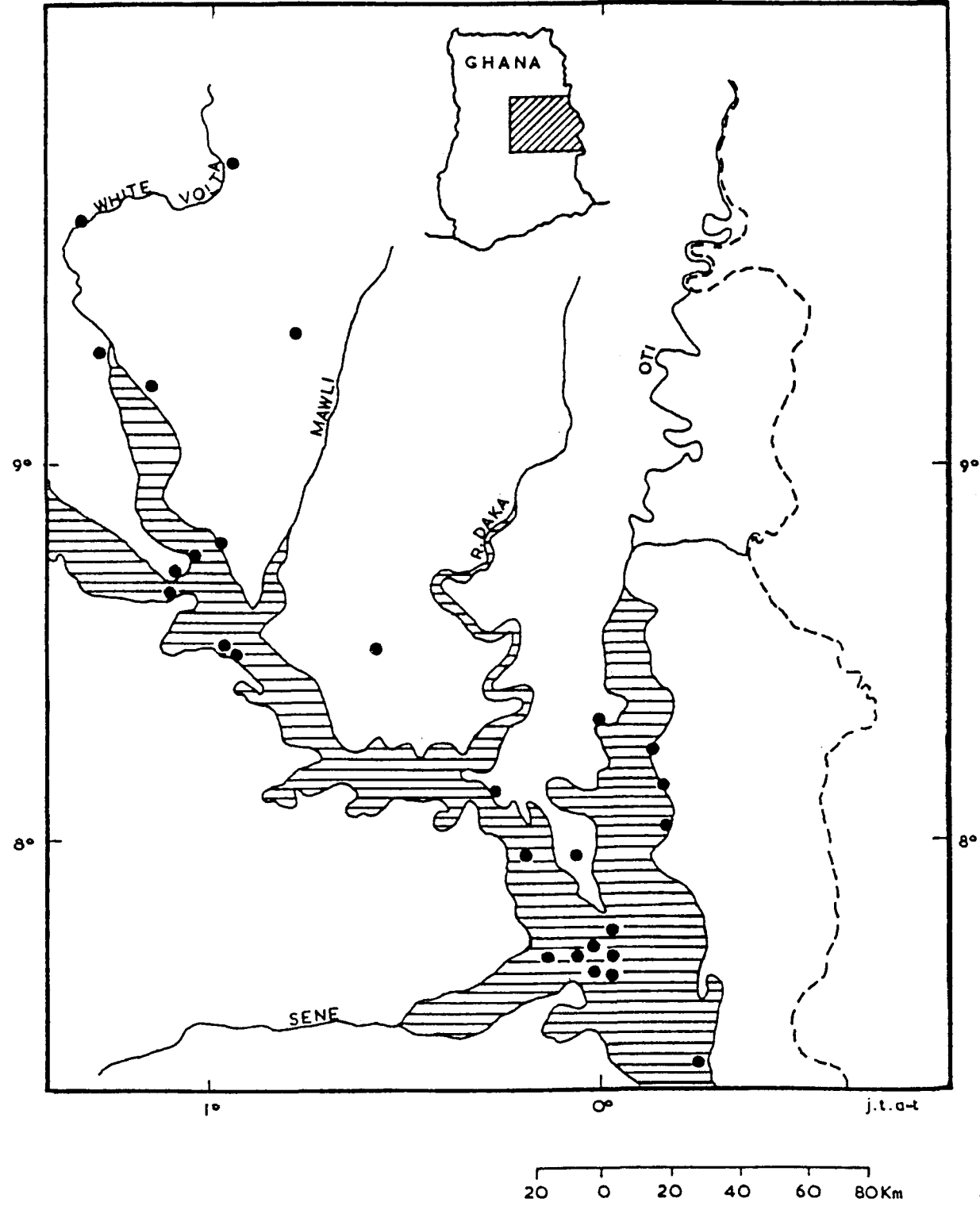


Fig. 1
Distribution of excavated archaeological sites in Ghana 1933-1990
(Calvocoressi, D.A. & York, R. 1972).

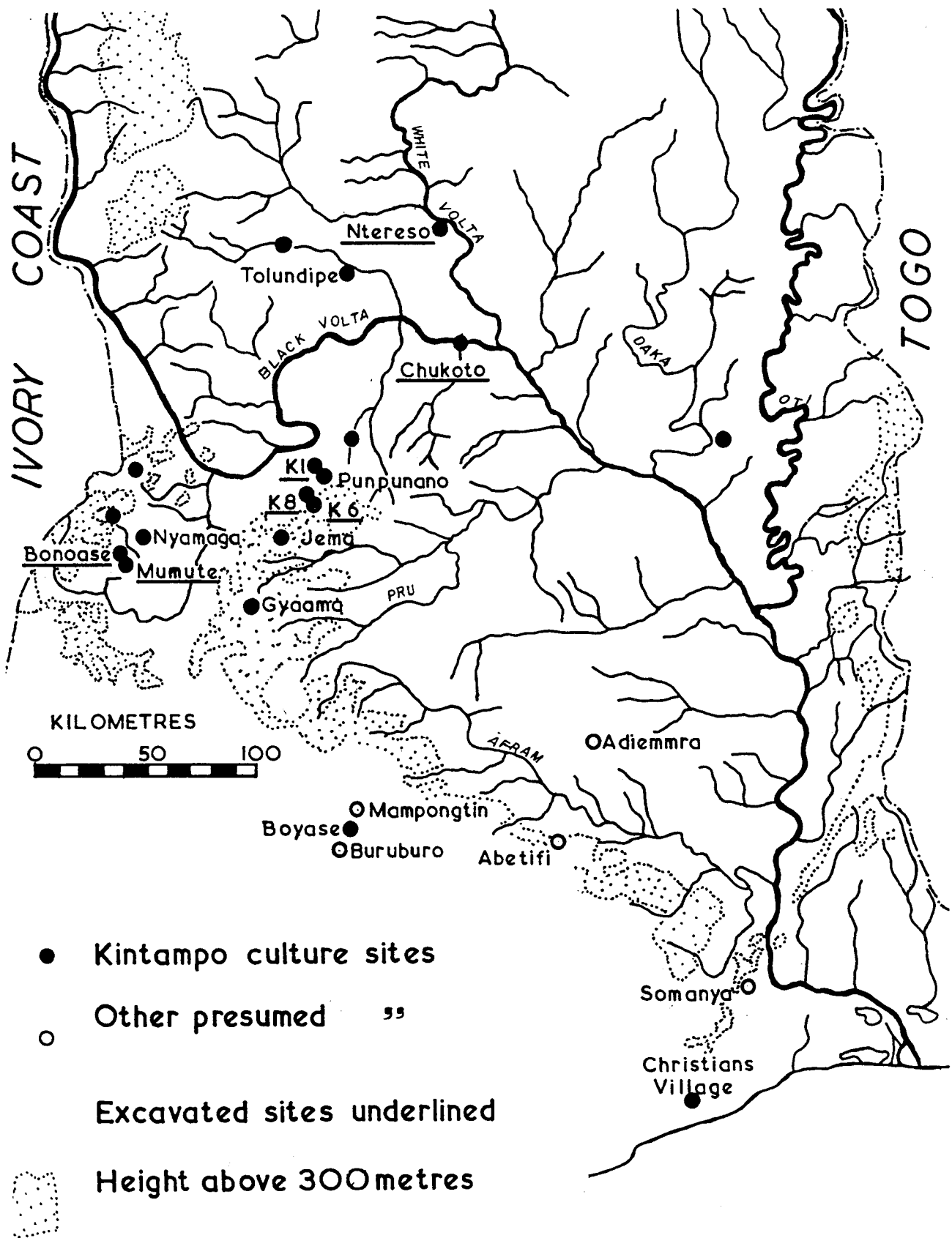


Fig. 2
Sites of Kintampo cultural traditions.

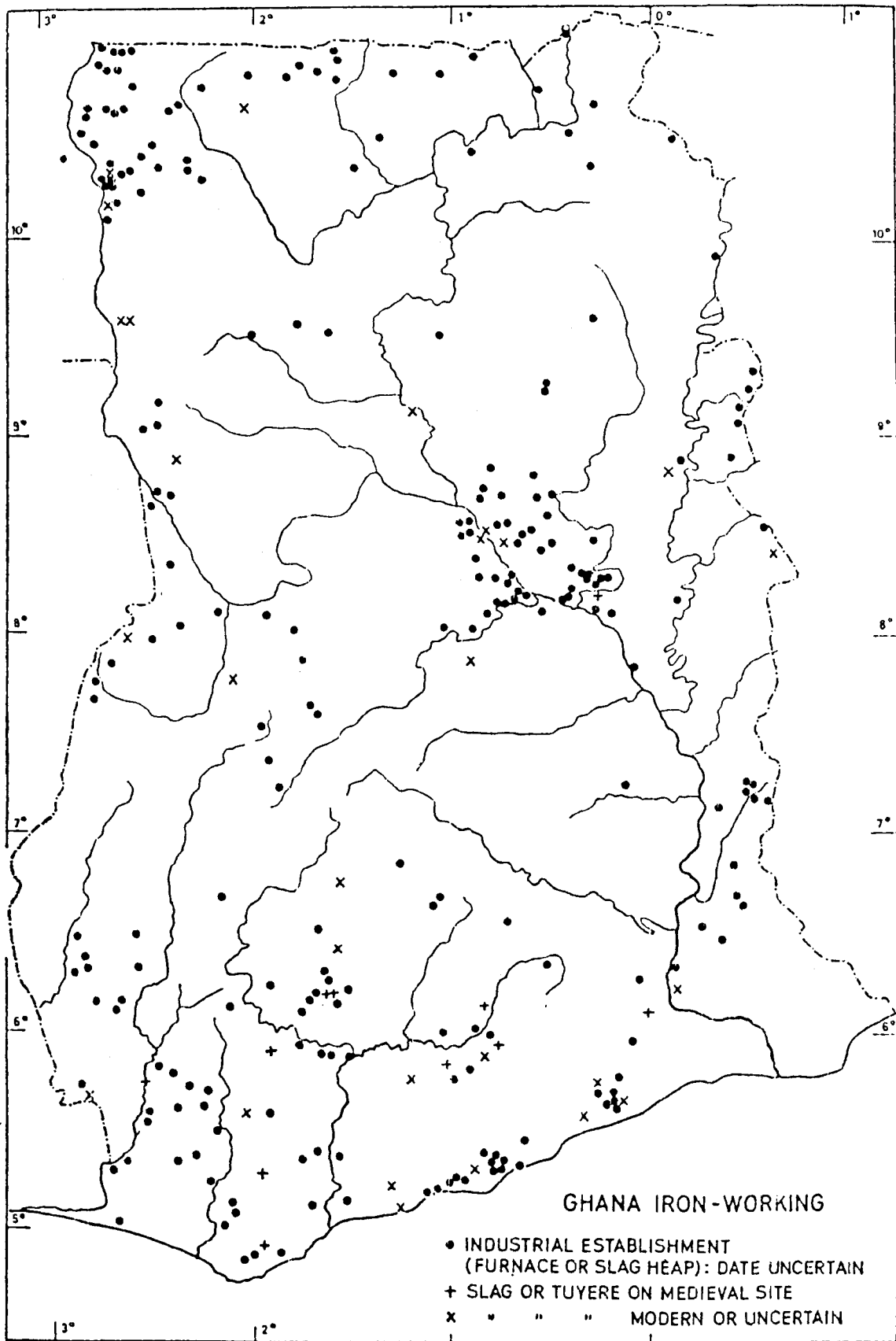


Fig. 3
Distribution of Iron Age sites (O. Davies, 1967).

THE LIVING AND THE DEAD : THE DILEMMA OF EXCAVATING GRAVES IN ISRAEL

by

Talía SHAY

In 1983, one of the organizers of the World Anthropological Congress asked me to present the subject of the public debate, raging in Israel, over the excavation of graves (Shay 1983). At the time, Israel was in turmoil over the archaeological excavations being conducted in Jerusalem (Shank 1981); an attempt was made in the Knesset to enact, in the thick of the night (27.8.1983), a law "protection burial grounds", whose aim was to actually turn the Ministry of Religious Affairs into the custodian of archaeological excavations in which human bones were disinterred (Apine 1983:19). It was only during the preparation of my lecture for the Congress that I first systematically pondered over the many implications of archaeological activity in Israel, whose influence is sometimes felt beyond the scientific domain.

In his book "*The Idea of the Holy*", Rudolf Otto (1923) describes the religious experience as attached to feelings aroused by the supernatural, the holy which is both awe-inspiring as well as fascinating and in the presence of which man feels to be but "dust and ashes" (Genesis XVIII : 27). It has been seventy years now since Otto's book was published, still his discrimination regarding the quality of the religious experience is valid even at the close of our century. This diagnosis, whose essence is the perception of reality pertaining to a religious person as opposed to that of the non-believer, is the subject of this paper which compares the various approaches of the religious circles to those of the archaeologists regarding the subject of burial.

The subject of death occupies a highly significant position in all human societies, embracing contradictory feelings of terror together with a deep love for and kinship with the deceased (Malinowski 1957 : 47-53). The form that the attitude to the deceased evinces, differs from one society to another. Nevertheless, Levi-Strauss (1974 : 232ff.) has advanced that as far as the rights-duties relation between the living and the dead are concerned societies divide into two categories : the first consists of those that allow the deceased to rest in peace, in exchange for which the deceased refrain from harassing the living, as long as the departed are treated with due respect; the second category, on the other hand, contains societies in which the survivors utilize the power of the deceased and demand that the latter come to their assistance on different occasions. The societies are harassed by the dead who bedevil the living descendants for their manipulation of the dead. Besides these two contradictory approaches, there are additional societies whose attitude to the deceased lies within the range extending between these two extreme approaches.

Furthermore, there is an essential difference between the respective attitudes of the religious and the secular to the dead (Eliade 1959 : 185). For the believer whose world consists of both the holy and the profane, death is considered as a sacred domain attended by intricate religious rites. On the other hand, where the secular person is concerned, religious rituals have shed their import, and the bonds between the living and the deceased, persist, mainly, at the level of the individual and his family; to some extent this is also true in respect of the status of the deceased in the society to which he belonged.

This fundamental difference between the Religious and the Secular regarding death characterizes the subject of the excavation of graves. Where archaeologists and anthropologists are concerned, the subject of burial is considered in view of this intricacy as an essential source of knowledge about various socio-cultural and economic aspects of ancient societies (Alekschin 1983 : 137). As to the believer, unlike the agnostic, he feels an actual or spiritual bond with the burials which the archaeologists dig; this scientific matter, belonging to the province of burial is, often, apprehended by the believer as a violation and a desecration of the holy. This feeling of violation is sometimes accompanied by an antagonism to archaeological activities; this hostility finds expression in political marshaling, airing the subject in public debates, especially by the Indian minority in the U.S.A. (Pastron 1973; Winter 1980), or even – at times – violent struggles against the digging of graves, for instance in Latin America (Conteras 1973) and in Hawaii (Linnekin 1983 : 246; Saxe 1983).

The large collection of mummies at the Cairo Museum was also closed to the public at large, by order from the President of Egypt Anwar Sa'adat, for the very same reason : outrage to the dead (*Time* 21.12.81).

Following the hostility to their scientific activity and subsequent developments in other sciences (see *supra* Shay and Clottes), archaeologists in the West have begun, in the generation, to study the moral implications embedded in their scientific activities (Sprigg 1986). An abundance of publications on this subject has seen light, especially in the U.S. and in other countries as well, in which the ethical values deriving from the past are studied, namely : the scientific responsibility of archaeologists seeking to obtain information as against their present day moral commitments to minority groups who feel outraged by the scientific activity of the archaeologists (McGimsy and Hester 1977; Rosen 1980; Green 1984; Bahn and Paterson 1986; Webb 1987; Layton 1989).

Unlike archaeologists in the West, their Israeli colleagues have not, as yet, discussed the moral implications of their activities and their effects upon sundry minority groups; their main occupation involves the historical and methodological aspects of their profession (Bar Yosef and Mazar 1982). This different approach of the Israeli School of Archaeology subsumes a variety of reasons which are partly entailed by the national implications of archaeology in Israel; but, as this article deals with the subject of burial, the above reasons cannot be cited here (Shay 1989).

According to Jewish Halachah, the mutual relations between the living and the dead are founded on the duty to bury the deceased (JE 1902 : 432), to respect him (Judaica 5 : 1425), and the interdiction to profit from him in any manner or form (Preisler 1985 : 765). The status of the deceased, after death, is unclear; Rabbinical literature contains opinions cutting both ways (Judaica 2 : 337-338). Anyhow, the deceased possess no harmful demonic powers. The dead are

called feeble, i.e. frail (Urbach 1969 : 191) and they may, as some assume, even feel pain (Bavli 13 : 2). Although there may, at times, exist appeals for assistance made to the dead (JE 1903 : 484), this is not done as a rule, since praying to the dead is prohibited (Peldman 1971). The attitude displayed towards the deceased in Jewish tradition enables them to rest in peace; this is the source of the antagonism of the Religious Circles to the archaeological activity attached to the excavation of graves. This antagonism has already been known since the last century, when the Jewish Community declared a fast, a day of mourning and activities to save the bones remaining as a result of excavations of the Tombs of the Kings in Jerusalem (Ben Asher 1974 : 297-302). This hostility survives to this day. With the establishment of the State of Israel, attempts were made to determine the relations between the Religious Circles and the archaeologists; an agreement was signed which stipulated that the excavation of graves was subject to the re-burial of excavated bone remains, in accord with the precepts of Jewish Halachah. Nevertheless, not all the Religious Circles are satisfied with this agreement.

In order to attempt and understand the view-point of the Religious individual I have interviewed a number of personalities belonging to the Religious Circles. I was presented with sundry views all of which relied on Jewish tradition. My first objective was to meet one of the leaders of the "Holy Site" (Athra Kadisha) group, which was founded at the turn of the fifties, in connection with the Beth She'arim excavations, and which aimed at preventing the desecration of ancient graves. After numerous difficulties, I managed – with the assistance of Mr. Tom Segev, a journalist with the "Ha'aretz" daily – to meet, at Bnei Be'rak, inhabited by a high percentage of Orthodox Jews, with Rabbi D. Schmiedel, the leader of the group. The Rabbi, whom I met in the company of his wife, displayed great expertise regarding the development of the religious opposition to archaeological excavations, in Israel as well as in other countries – since the last century to this day. He was of the opinion that antagonism had increased lately in Israel, not because of political reasons (Agudath Israel had joined the coalition government in 1977), but rather because of the institutionalization and the development of archaeological research which had set itself the goal of excavating graves, contrary to the above mentioned agreement, concerned with the fortuitous excavation of graves. This aroused the Ultra Orthodox, who organized in opposition to the excavation of graves, precipitating a public controversy over the subject.

Religious man, Rabbi Schmiedel explained, is first and foremost, in duty bound to obey the precepts of religion which like universal laws supersede all other precepts; these precepts prohibit the disturbance of the repose of the deceased (Ben Asher 1874 : 297-302), not in view of the resurrection of the dead, as the profane are used to thinking, but principally because of the distress of the deceased who cannot defend themselves and require the protection of the living. The desecration of graves injures not only the dead but also distresses their living kin, but this is a human consideration not a religious precept. The members of the "Holy Site" are prepared in the name of their convictions, to bodily prevent any outrage to the dead.

Rabbi Schmiedel is aware that the archaeological finds in Israel serve to bring home the sources of the Bible as well as the national significance of archaeology. Nevertheless, as a religious person he finds the secularization of burials unacceptable, and appeals to archaeologists to respect religious sentiments and look for alternative sources of knowledge.

The religious public in Israel consists of a wide range of opinions, partly antithetical and inadmissible by members of the "Holy Site". Such is the opinion held by professor J. Leibowitz, a believer and scholar, who regards feelings about sacredness and concern for bones as idolatrous elements alien to the spirit of Judaism. In my interview with him, he argued that in spite of a variety of references to the dead in Rabbinical Literature, only two of these are fundamental and determining, and these are; burying the deceased, and the prohibition to profit by him. Leibowitz doubts whether scientific research, for instance archaeological excavations, may be considered as exploiting the dead; hence he argues that as long as the archaeologist abides by the first rule of burying the bones, the excavation of graves does not seem to him an offence to the values of Judaism.

Between these two extreme views lies the opinion of the chief Rabbinate of Israel, which represents the mainstream of Judaism. This position was represented, in the early eighties by the chief Rabbi of Haifa, Rabbi Sha'ar Yashuv and his assistants. The chief Rabbinate maintains that it generally views with favour the persistence of archaeological activity, due to the national significance it exercises in the country. As to the contradiction existing, in their opinion, between the stance of the Jewish Halachah and archaeology, regarding the excavation of graves, their spokesmen advanced that the problem would be solved only by means of mutual respect and moderation, exercised by both sides.

Countering the protest of the Religious Circles, who claim that they are entitled to decide what is to be done with the remains of their forefathers, the archaeologists put forward scientific and reasonable considerations for the continuation of the excavation of graves. Naturally it is impossible to compare a system of considerations maintained by the Religious Circles which stems from their religious "*weltanschauung*", to the secular arguments advanced by the archaeologists. I have turned to several prominent archaeologists in Israel and asked for their opinion regarding the dilemma of the excavation of graves in Israel. These persons have submitted, to me, diverse reasons for the pursuance of excavations; they also informed me of sundry personal ways they had of coping with the religious opposition to the excavation of graves. All those interviewed shared the view that if a law protecting burial sites were to be enacted, it would halt the pursuit of archaeological activity in Israel; they are, therefore, resolved to oppose most energetically the passing of this law.

Professor Mazar, dean of Israeli archaeologists, analyzed in an interview we had the values of Jewish religion contrasted with those of science; he argued that whereas the origins of the former lay in medieval times and were impractical nowadays, in a country under construction and all of which was strewn with graves and remains from the past, it was therefore the scientific responsibility of the archaeologists to obtain information about the past. This responsibility transcended religious laws. Mazar, nevertheless, suggested that since Religious Circles feel offended by the excavation of graves, attempts should be made to reach an understanding with them, which would take into account the reinterment of the excavated bones in the spirit of Jewish Halachic laws.

Professor Yadin, who dug up Masada and deciphered the Dead Sea scrolls, submitted logical arguments and maintained that religious belief was relative – prohibiting today the transfer of bones, whereas in the past during the period studied by the archaeologists – it was an accepted matter and in accord with the Faith of the interred. Yadin claims, therefore, that it is unwarranted

to enact a law which would prohibit archaeologists to excavate erstwhile Jewish graves. Yadin was prepared to uphold his ideas about the necessity to persist in excavating graves, at all costs, including his going to jail – as he claimed – for infringing the archaeology law, if it was enacted. Other archaeologists, with whom I have discussed the subject, have raised arguments similar to those of Mazar and Yadin; in addition, they pointed out that, in their opinion, the dispute over the excavation of graves had gone beyond the question of the values of religion contrasted with those of science; this dispute was kept alive mainly by political power – struggles between secular and religious parties.

To conclude – the concern for the spiritual well-being and the sacredness of the dead by the Religious Circles in Israel confronts the ethical, logical and political arguments of Israeli archaeologists who claim their prerogative to excavate graves. Similar arguments, which equate the objection to excavating graves with ignorance, a change of religious beliefs or a desire for a political gain have often been mentioned by Western archaeologists (Cheek and Keel 1984 : 206-207; Bahn and Paterson 1986 : 257-269; Layton 1987 : 1-2; McGuire 1987 : 172, 180). Nevertheless, the general tendencies of Western archaeology to-day are to revise its theoretical premises in view of their practical implications on minority groups (Layton 1987 : 18). While these tendencies to modify archaeological goals have not yet affected Israeli archaeologists, it is hard to think that the latter will be able to adhere to their traditional goals in a world where the rights of individuals and minorities – in particular – are increasingly respected.

ACKNOWLEDGEMENTS

This work was supported by the Memorial Foundation for Jewish Culture.

The author would also like to thank Dr A. Greenbaum for the advice and the invaluable information provided as regards the treatment of the dead in Jewish Sources.

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ARCHAEOLOGICAL ETHICS AND THE TREATMENT OF THE DEAD

by

Paul G. BAHN

*"Science is not an absolute to which all things
have to be subordinated and eventually
sacrificed, even the dignity of man"*

Cardinal Joseph Ratzinger, 1987.

ABSTRACT

Focusing on the issue of ethics, and the question of the return and reburial of human remains and grave-goods, this paper examines archaeology's previous attitude of indifference to the wishes of outsiders and its current obligation to face up to responsibility for its actions, both past and present. Compromises are rapidly being reached by many museums and institutions, but some areas of uncertainty are outlined that remain to be settled. It is argued that while not a vital discipline in itself, archaeology does have some noteworthy contributions to make to the good of humankind, and in particular it can serve to strengthen the cause of the very groups who are at present opposing some of its practices.

INTRODUCTION

The above statement, made in a debate concerning test-tube babies and surrogate mothers, is equally relevant to the question of archaeological ethics and the limitations of archaeological knowledge.

Despite the wishful thinking of some archaeologists (particularly those still attached to the so-called "New Archeology", that brave new world of the 1960s) who cannot bear to admit that there are things we shall never be able to find out about the past, archaeology is not a true science. Certainly it draws on many scientific techniques, and uses many of the same procedures of logic and interpretation as the hard sciences, but it is nevertheless inevitable that the subject remains closer to history : we simply cannot prove anything much about past events, and the best we can aim for is a very simplified model with some degree of probability based on inference, analogy, many assumptions and extremely patchy evidence.

Should archaeology's inherent limitations, therefore, impose limitations on its practice? In other words, since the discipline cannot aspire to "truth", or the "real past" or to anything more than a temporary set of hypotheses and a wide consensus, should it be accorded any overriding significance?

As I have mentioned elsewhere (Bahn 1989), when I first began to study archaeology over 18 years ago it was a self-important and self-satisfied discipline, carried out predominantly by people from the wealthiest nations. They seemed to feel free to investigate whatever and wherever they wished, with only warfare, natural hazards or economic difficulties (lack of funds, equipment or personnel) to set limits to their activities. I cannot recall ever encountering a single word about ethics in lectures or textbooks at that time (see also Zimmerman 1989), except for one paper on "Ethics in Archaeology" (Thomas 1971) which dealt primarily with the moral duty to seek fresh data.

Where the dead were concerned, there were numerous accounts in print about how to excavate graves or skeletons, with an intense concentration on funerary rites, orientation of grave/body, presence/absence/type of grave-goods; but not a word about whether one had the right to disturb the dead in this way, let alone any suggestion of sounding out the indigenous people's feelings on the matter. When asked about disturbing the dead, in a 1973 TV interview, Sir Mortimer Wheeler merely claimed this was a sentimental tradition, and the excavation of graves did no harm (quoted in Bahn 1984:128).

The whole purpose of archaeology seemed to involve obtaining "knowledge about the past" so that it could be passed on first to the archaeological community (for purposes of career advancement and peer respect), then to the educated public, and lastly (though rarely, if ever) to the rest of humanity. To a large degree this is still true in many parts of the world; but in a few key areas, archaeologists have been rudely awakened from their idyllic reverie.

It was in the late 1970s that the first rumblings began to reach Britain about the anger of native peoples in North America and Australia, and of ultra-orthodox Jews in Israel, towards the disturbance, analysis and display of ancestral human remains. I was eventually prompted to put together what was, I believe, the first paper to appear in Europe on this topic (Bahn 1984), which concentrated on the historical aspects of the problem. I followed this with a second (Bahn & Paterson 1986) which examined the position of moral philosophy, and which is still, as far as I am aware, the only attempt to do so despite the importance of morality to this issue.

In the countries concerned – above all Canada and the USA, Australia and New Zealand – the 1980s have seen a radical transformation of the situation. What began as a curiosity in the press, an excuse for editors to print witty titles about "Bones of Contention" or "Skeletons Rattling", has quickly become big news, covered at length in publications such as *National Geographic*, *Newsweek* and countless newspapers, and the subject of television documentaries (completed or forthcoming) in several countries.

But its greatest impact has been not so much on the media as on the academics, with special committees formed by archaeological and anthropological associations and by museums to discuss the issue, and with an international congress held in 1989 at Vermillion, South Dakota. The aim of the committees has been to thrash out policies which will go some way towards

settling the demands of the objectors while limiting the damage to existing collections and the threat to future excavation and research.

Inevitably, it is the extremists whose views are heard most often, because they are the most active and vociferous, and because the press is far more interested in conflicts than in agreements. On the one hand there are those academics who oppose any interference with their work, any return of skeletal material or artifacts, and any reburial. On the other, there are indigenous groups, often self-appointed representatives for entire nations, who insist on the return and reburial of all such material, and who inevitably use the issue as a political football, since skeletons and museum collections are a highly visible (and, dare one say, convenient) symbol of past oppression and of current struggles for rights of all kinds. Human remains are an emotive issue, and it is all too easy to cast the academics as unfeeling racists or patronizing colonists who treat the remains as mere data.

It should always be borne in mind that by no means all past disturbance of the dead was carried out by archaeologists, and it was not limited to the remains of foreign native peoples (see Bahn 1984; 1989). Moreover, although it is archaeologists who actually unearth much of the material, it is physical anthropologists who tend to study and curate it, the archaeologists being more interested in a grave's layout and artifacts.

One American museum curator informed me recently that in the past twenty years fewer than half a dozen archaeologists had come to examine any of his museum's collection of human skeletal material, whereas literally hundreds of physical anthropologists (both students and professionals) and medics had done so. Yet some of the committees and decision-making bodies involved with the reburial issue have failed to consult, let alone include, any physical anthropologists.

Consequently, while it is archaeologists who have borne the brunt of the attack from objectors so far, it needs to be realized that a trinity of characters has equal involvement in this issue; the archaeologist (disturbance and removal), the anthropologist (analysis) and the museum curator (storage and display).

This is not to imply that archaeologists are apathetic to the questions of study and curation : indeed, many of them are vehemently opposed to the return and reburial of skeletal remains. Their view may sometimes spring from a particular interest in this material per se – though, as mentioned above, few of them ever bother to look at it, and very little is taught in archaeology classes about human remains. Yet one might expect those who study the record of past human activities to be interested above all in the remains of the very people who produced that record. In fact, several reviewers of a forthcoming textbook on archaeological methods (Renfrew & Bahn 1991) have expressed surprise (and some satisfaction) that, unlike other similar texts currently available, it devotes a whole chapter to the wide range of methods which can extract a wealth of information from human remains.

Rather, most archaeological objections to the return and reburial of skeletal material tend to be of the "thin end of the wedge" variety : that is, if the skeletons go, can the artifacts be far behind? Some native groups have already begun to include grave goods in their demands for the return of material, and archaeological alarm is growing.

While few archaeologists, other than extremists, can have any great objections to the return of recent skeletal remains, or of those collected under horrific circumstances (such as the Crowther collection of Tasmanian remains), there is considerable disquiet about the reburial of bones that display important clues to cultural behaviour : some of Australia's Coobool Creek skulls, for example, had been artificially deformed during infancy (see Mulvaney 1989), and therefore could be argued to be, in a sense, "artifacts" as well as human remains.

The main argument put forward by anthropologists against reburial of human remains is that no analysis is ever definite, and that new techniques are constantly arising which enable new questions to be asked and which help to improve the information already acquired. Exactly the same can be said by archaeologists of their artifacts, and the argument is indisputable; this was brought home to me quite forcefully when compiling the above-mentioned textbook which, had it been written when I started my studies, would have been a fraction of the size, and which never ceases to grow : several new methods had already appeared between the writing of the text and the arrival of proofs! New analyses of all kinds in archaeology and anthropology are proliferating, with isotopic analysis and genetic studies in particular offering remarkable and unexpected breakthroughs.

On the other hand, it is equally true that, in tandem with these new techniques, there is a tendency for ever smaller amounts of material to be analyzed; radiocarbon dating can now be done on minute fragments, and the genetic techniques also need very tiny quantities of material.

Human remains and burials have been of major importance in the history of archaeology, but they are still only a small part of what archaeologists study. Since we know of far more archaeological sites today than could be investigated by all living archaeologists in several lifetimes, and since there is an immense backlog of unpublished excavations and finds in museums and institutions, there is really no excuse for research excavation of burials at present, and in fact they have largely ceased in many parts of the world. Most archaeological encounters with the dead now come from salvage excavation : so the basic questions are how to deal with "salvaged" graves, and what to do with the remains already unearthed (whether by grave-robbers, archaeologists or bulldozers) and curated.

With regard to what has already been removed from the ground, we should consider whether we really need to hold on to everything for ever. Our museums overflow with material that nobody ever looks at, and even the most eminent institutions face ever-growing problems of disintegrating objects and of insufficient funds and storage-space. Quite apart from ethics, therefore, there are good practical reasons for relieving some of this pressure.

This could be achieved in part by returning much material to those who have petitioned for it and who have solid and sincere reasons for their demands. The floodgates are unlikely to open, and visions of empty museums are a fantasy; there will always be thousands of skeletons available for study, which are preserved in museums around the world, and which nobody wishes to rebury. Even in the most sensitive areas such as America and Australia, indigenous views on the issue vary widely, and a growing number of local communities are in favour of some analysis of remains, provided that their permission is sought and that the resulting information is explained to them. Supplies of new specimens are not going to dry up, since

salvage excavations will certainly continue and graves will go on popping up unexpectedly – indeed, the pace will increase in the face of intensifying development and construction.

In other words, the reburial of some collections is probably far less of a blow to “science” than it might at first appear. We have the means to make excellent casts of all material to be returned (to cater for future analyses involving external features) while the extraction of a small fragment from each skeleton (say, a tooth or bit of bone) should suffice for other new techniques which, as shown above, require ever smaller quantities of material to work on.

There remain five areas of uncertainty which need to be considered in any discussions aimed at drawing up a code of ethics on the issue of archaeological treatment of the dead (Bahn 1989).

- 1) Natural disturbance -v- human disturbance (construction, salvage, looting); some indigenous groups see natural disturbance (and sometimes even disturbance by the impersonal bulldozer) as part of the process of decay, and object to archaeologists interfering or “rescuing” human remains that have come to light in this way.
- 2) Buried remains (marked or unmarked) -v- unburied remains. There is a considerable difference between disturbing a real grave (whether purposely or not), into which someone was carefully laid, and exposing a corpse (such as the many bogbodies of northern Europe) that was tossed into a pool. It is possible that this rite was as religious as an elaborate entombment, involving a sacrifice (like the many people thrown into the cenote of Chichen Itza), but the bogbodies usually seem to have met with violent and gruesome deaths, and so could have been executed or murdered. This uncertainty is tied to
- 3) Burials of people whose beliefs are known -v- those with unknown beliefs. It goes without saying that we know nothing definite about prehistoric beliefs, while so far there have been no objections by descendants to disturbance of many of those whose beliefs are well known to us (e.g. Egyptians, Greeks, Romans - including their intense dread of being disturbed) (Bahn 1984). This leads us to
- 4) Communities which object to disturbance and/or analysis -v- those which do not. This is an issue that needs to be decided at the local level, because among Aborigines or Amerindians there are widely differing views about what should be permitted.
- 5) Finally – and most critical in the eyes of many archaeologists and anthropologists – the problem of recent remains -v- ancient remains. As mentioned earlier, there can be few objections to the return of recent material. But the problem of a “cut-off date” is a thorny one, especially as it is often only through exhuming the dead that one can discover how old the remains are. Some native groups refuse to accept any cut-off date, seeing all pre-colonial remains in their land as being necessarily those of their direct ancestors, however remote. Archaeologists, however, argue with some justification that where Pleistocene skeletons, for example, are concerned, they should be seen as a “world heritage” rather than as local ancestors : it goes without saying that nobody knows anything at all about the beliefs and wishes of people many hundreds of generations before us, and to consider them ancestors at all is symbolism rather than fact; it is reckoned, for instance, that of the French people living in 1789, no more than 14 % have any descendants alive today after only two centuries!

CONCLUSION

In the past, archaeologists consistently failed to consult local people about their beliefs and their wishes concerning the disturbance of the dead. Nowadays, somewhat late, they are finally facing the task of explaining clearly to concerned outsiders why they feel it useful, necessary and beneficial to do work of this kind. But there is a more fundamental point to be faced : is there really any point in doing archaeology at all ? Is it necessary ?

Inevitably, the immediate answer is negative. As Glyn Daniel (1976) reminded us, archaeology is a "back-looking curiosity" – it is an intellectual game, a luxury, even a parasite (Bahn 1989). It is also a newcomer, having been around for only a couple of hundred years. The world did not start turning when archaeology began, and could get along very well without it.

The same, however, could be said of most disciplines. And archaeology does have its good points; for a start, it is enjoyable and brings pleasure to ever-increasing numbers of people who share that curiosity about the remote past or their roots. There are also some important practical benefits (other than employment for archaeologists!): these may be financial (some regions, and even whole countries depend very heavily on archaeological tourism); medical (information about leprosy, of value to the diagnosis and treatment of 12 million lepers today, can be obtained from ancient leper-cemeteries); or economic (there are projects underway to reintroduce efficient ancient methods of irrigating desert areas and of farming raised-fields in South America).

Ironically, however, in view of the political overtones of some extremist demands for the return and reburial of material, one of archaeology's major contributions has been political; it is archaeology which can prove that indigenous peoples have been present in Australia or America for tens of thousands of years. It is archaeology which brings out the lost evidence of cultural complexity and sophistication of the indigenous peoples' forebears, of the great antiquity of their traditional rituals, and which provides insights into their cultural identity.

I have argued elsewhere (1989) that mutual respect is the key to future understanding in this area, and it is highly probable that increasing numbers of indigenous communities will cooperate and even encourage archaeological endeavours once they come to perceive the benefits of the knowledge that can be obtained this way. As Mulvaney (1989:72) has aptly summed it up, "It is worth reflecting that, despite terrible violence to their graves, ancestral spirits may approve that knowledge was conveyed through centuries to present generations searching for greater cultural identity".

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EXTERNAL FACTORS AND ARCHAEOLOGY *

by

Bernard J. SIEGEL **

By external factors that affect archaeological research I think of circumstances that constrain in one way or another the nature of the archaeological enterprise by putting limits on access to data and kinds of problems to be studied. Three issues suggest themselves to me in this regard, two of them politically motivated. The first of these, demands for reburial by ethnic or religious groups, I shall discuss from discourse concerning material remains from aboriginal (pre-Colonial) populations in the United States. The second, in my estimation very real but for which I do not know of any literature, concerns what we may call Mainstream Archaeology. By this I mean the set of problems that primarily engage the research interests and direct the attention of archaeologists working in a given country of the world. I suspect that for new nation-states in today's world, governments may have a stake in determining which kind or kinds of research shall receive financial support. In Israel, for example, Biblical Archaeology might constitute Mainstream Archaeology. Such concentration of effort does not preclude other kinds of research but the former are most likely to receive internal government support.

And, finally another consideration we can think of as external to doing archaeology in its broadest sense is the paradigms and theories that may exclude certain kinds of investigations as they emphasize new ones and demote the old, just as the emergence of molecular biology has overshadowed other kinds of once predominant biological research.

One might think of other, political, factors that affect access to data – where and how archaeologists can work. For a time American ethnologists, for example, found it difficult if not impossible to undertake research in Colombia because that government required funding agencies in the United States (primarily affecting grants from the National Science Foundation) to provide financial support for one or more Colombian anthropologists as co-participants in project applications. While this policy was in place the NSF refused to agree to it. It is possible that in some cases the demands of nation-states to retain or have returned any remains of excavations might affect the work of foreign archaeologists, but this is probably of lesser concern. Paleoosteologists, on the other hand, find it difficult to assemble for study in any one center the skeletal remains of early man. Working from casts is not exactly the same thing.

* I am indebted to Barbara Bocek and John Rick, Stanford archaeologists, for guiding me to bibliographic sources on the reburial issue and recent developments in bioarchaeology (a term used synonymously with paleoosteology).

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THE REBURIAL ISSUE

In the early 1970s, American archaeologists, who had for long sought to recover and interpret material evidence of this country's past, began to encounter an obstacle to their research they had heretofore never thought about. And the problem has continued to escalate to the present time. It concerns the demands by Native Americans for the return and reburial of skeletal remains of purported ancestors from museums and research centers where they are curated. The moral and political issues involved are by no means unique to American archaeology. Canadian, Israeli, and Australian archaeologists, among others, are agonizing over similar claims to their data bases, what some consider an indispensable part of their field notes¹. The American case, however, is distinguished by its complexity – the operation of federal laws and constitutional rights, fifty separate State jurisdictions, the special relation of Native American peoples to the federal government – to identify some of its most prominent features. Furthermore the controversy has generated a large number of articles that argue pro and con over the scientific versus ethnic importance of bones and other grave materials from different perspectives. A central point that emerges from these arguments is : who owns them ? And by implication, who has the right to decide what to do about them ? There are no simple answers to these questions, which go to the heart of how objectively prehistorians may carry on their research².

To understand why "Reburial quite obviously threatens the future of American archaeology" (Buikstra 1983) we must first examine the kinds of information that burials provide. Indeed they produce such a diverse and important body of evidence for an analysis of prehistoric and, in the case of skeletal material, of living societies that they may be said to constitute virtually a sub-field of archaeology.

Let us consider first what bio-archaeologists learn from bones and the case for long curation. For this task we have several sources that summarize the rather voluminous literature on topics commonly addressed by human osteologists which are of central concern to the archaeologist (Aleshin 1983; Buikstra 1983; Buikstra and C.C. Gordon 1981; Owsley, Douglas W. 1984; Ubelaker, D.H. 1989; National Museum of Natural History Newsletter for Teachers 1989; Huss-Ashmore, R., A.H. Goodman, and G.J. Armelagos 1982; Larsen, C.S. 1987; and Cheek, A.L. and B.C. Keel 1984). The types of information that can be obtained from human remains briefly fall into the following categories : (1) physical characteristics of the population being studied (e.g. stature and body build); (2) demographic factors, such as sex and age, sex ratios, longevity, death rates, and migration; (3) biological or genetic elements that enable determination of degree of relatedness in given prehistoric populations; (4) pathological evidence that yields information on diseases, accidents and cause of death; (5) diet and nutrition that tell us about quality of life and changes that occur over time, for example, from evolutionary processes

¹ For the reader's information, I am the only author addressing the issues in this volume who is a social/cultural anthropologist, albeit one with an abiding interest in archaeology. Undertaking this essay provided a welcome opportunity to review and contemplate the debates – and the sources of these debates – over proposed new directions in this field from approximately 1960 to the present. I am especially impressed by the considerable parallel development in this respect with my own field, a not too surprising occurrence, perhaps, given the more broadly anthropological interests of archaeologists during this period.

² The fear of some archaeologists extends well beyond that of bioarchaeologists. The return of skeletal remains, they assert, is simply the first demand. Already some Native Americans lay claim on religious grounds to repatriation of artifacts. When burials are encountered in future excavations what role will Native Americans play in supervising the disposition of recovered objects, even of the photographs and notes of the archaeologists *in situ* ? In a few cases this may not be an idle question.

associated with the emergence of agriculture as a dominant mode of food production among once food gatherers and hunters; and, finally, (6) mortuary practices that may reveal aspects of e.g. social organization, division of labour, and religious life.

Various authors have illustrated these topics with different case materials. Apart from burial artifacts, genetic features of populations now recoverable from skeletal data may provide evidence of the origins, movements, biological relationships and cultural histories of Native American prehistoric ancestors, for example. Elsewhere, physical evidence from teeth and skulls from prehistoric peoples in Japan and other parts of Asia has helped to resolve conflicting interpretations about the ancestry of contemporary Japanese, their derivation from one or another of two major prehistoric traditions. At the same time it has also clarified the problematic relationship of a numerically small and marginal ethnic group in the island of Hokkaido, the Ainu, to social classes of the dominant population. Whatever their ultimate origins – are they the remnants of an upper paleolithic population that first settled the islands of modern Japan ? – they now appear to have left some of their genes in an earlier day through interbreeding or intermarriage with the Japanese upper classes (Museum of Natural History 1989).

Extrapolating from what we know and continue to learn from bones in living populations has enabled bioarchaeologists to make detailed analyses of diet and nutrition, pathology and behavior (e.g. preparation of food) among prehistoric peoples. Adaptive behavior and nutritional deficiencies are reflected variously in growth curves, bone deformities, reduced amounts of normal bone (osteoporosis), other diagnostic skeletal markers for certain vitamin deficiencies and iron-deficient anemia, dental wear and wear patterns, damage to enamel, and a number of other tooth marks. Certain infections, whose causes may be unknown, also show up in bone. Larsen (1987 : 380-81) comments that a study at Dickson Mounds in Illinois, comparing low intensity agriculturalists, also showed that mean age at death was lower in individuals with infections than those without.

Bones also give both specific and (more often) non-specific infectious diseases, like tuberculosis, endemic syphilis, and yaws. The feedback relation investigators have determined between undernutrition and lowered resistance to infection among living agricultural populations of the Third World (Scrimshaw *et al.* 1968; Taylor 1985) suggests that infection processes we see today are probably the end result of nutritional deprivations experienced in past populations.

Aside from severe trauma, injuries that would be caused by weapons for example, specific causes of death are extremely difficult to determine from skeletal material. On the other hand, life table methods for assessing mortality differences within or between groups can provide evidence for inferring the effects of differential nutritional stress. In another prehistoric Illinois population mortality increased in those age groups predicted to be more mal-nourished. In general osteological reconstructions of diet, demography, growth and disease patterns reveal a good deal about the health costs of increased dependence on agriculture. Elsewhere similar differences are reflected in the quality of life in ancient stratified societies.

From this brief summary of what bioarchaeologists can elicit from prehistoric skeletal remains it is not difficult to understand why they tend to view reburial (and hence the permanent loss of these data) with dismay if not alarm. Some have also addressed the query often made by Native Americans: why must you retain the bones of our ancestors indefinitely in the dust bins of

your museums ? Is there not some reasonable period of time for curation and study after which they may be laid to rest in their graves ? Buikstra and Gordon (1981) respond by asking hypothetically what impact reburial would have had on archaeology in archaeology if, say, the issue had occurred in 1952 and a decision at that time specifying reburial were made "after 'complete' study in the fashion of the time". They point out, for example, that archaeologists would have had to forego: (1) attempts to measure status differences and social complexity; (2) benefits of newly refined techniques for estimating age and diagnosing sex for demographic analysis; (3) evidence of juvenile patterns and infant health status that are sensitive indicators of quality of life; (4) genetic data on origins and biological relations of different Native American tribes, based on individual units rather than group averages; (5) commonly unreported remains of e.g. females; and (6) observation of Harris Lines as an indicator of feast-famine cycles or of other environmental themes.

Because the concerns of the time led bioarchaeologists to concentrate on constructing typologies and racial differences they paid scant attention to any of these features that bones can reveal. Since then theoretical interests of archaeologists shifted largely to the study of evolutionary processes and yet other problems in the 1980s to which osteological evidence can make significant contributions as independent tests of interpretations from artifacts alone. Furthermore, in the past few decades technological advances and applications of dietary evidence from bones by paleonutritionists have led to e.g the measurement of protein content and carbon isotopes from hard tissues. We can now measure, for example, the percent of maize in the diet of prehistoric peoples and the effect of diet on skeletal pathology.

The point of all this is simply that new paradigms and directions of research, as well as new and more sophisticated technology continually fuel what can be learned from material remains. In Buikstra's view, and that of a number of other archaeologists, these observations argue for indefinite curation rather than a "reasonable period" of, say, one or two years for analysis before reburial. So far as very large existing collections are concerned the opposing argument is that the cost of restudy and the small number of scientists available to do so make it unlikely that any but a very few of these assemblages will ever be subject to review. There is merit to both positions. The advances in reinterpretation and new understandings from re-analyses that have been made, plus the ever likely possibility for the reduction or elimination of ambiguities in the future lend support to long term, if not indefinite curation. On the other hand, in the light of restricted financial support and available manpower, attention must be paid to the fact that large repositories of skeletal remains may never be studied for these reasons, and suggests that some reasonable compromise should be made on this aspect of the reburial issue.

Archaeologists and museum curators, who either oppose or have strong reservations about reburial in general, also cite demands for the return of artifacts and other recovered materials for reinterment. In extreme cases they may even request field notes. If the worst case were to prevail, they have argued, archaeology as a discipline would cease to be viable. The fact, is however, that the worst case has not been, and is not likely to be, realized, except possibly on some reservation lands where the tribe has greater control over access to excavations.

The protagonists over conflicts about reburial essentially represent two competing interests; the scientific community of anthropologists, paleo-osteologists and museums, on the one hand,

and the cultural and religious interests of Native Americans, on the other ³. In their concluding chapter on *Reconstructing Archaeology Today* Shanks and Tilley (1987a : 246) remark in this context that "...the conflicts of concerns between American Indian groups and archaeologists... has its roots no only in the issue of whether or not archaeologists have the right to uncover Indian remains but also in the images created of the constructors of those remains... The white American having dispossessed the indigenous inhabitants of their land and possessions and virtually destroyed their culture now requires that the Indians respect his or her 'right' to reconstruct their past and if this involves the violation of sacred remains the type of empiricist science subscribed to ensures that this action is eminently justifiable... Archaeological discourse may or may not have truth value. It certainly can have power effects operating to reproduce the relationship between the dominant and the dominated in contemporary society. It is this which must be opened to critique." This critique builds upon the proposition that "... there can be no completely objective account of the past. The 'truth' of the past can never be known for certain; objects are locked into their time, archaeologists into theirs" (p. 12). The past, in short, is part of the present in that it is known now, not then, and that archaeologists bring current ideological and conceptual perceptions to bear upon their interpretations of these objects. They are also embedded in their own relations of dominance and subordination, and by insisting upon the pursuit of their own interests they perpetuate the powerlessness of descendants of past peoples.

From these arguments we can conclude that, for the most part, native groups and archaeologists not only have disparate views but have largely talked past one another in terms of their interests over the reburial issue. Archaeologists argue for the value of scientific knowledge, for its usefulness and benefits for mankind generally, as we have summarized above, and have also sought to persuade native peoples of its value to them in seeking to recover the history of their past. In their view archaeologists have the responsibility for tending to the natives' best interests, which would be counteracted by reburial and thereby destruction of the records of the past. Legally they also assert they must attend to and serve the public interest in its broadest sense.

If some archaeologists wonder why native peoples today should be so concerned about the treatment of their deceased ancestors when (1) they have left no record to tell us exactly what meanings the latter ascribed to their practices in the unearthed record and (2) practices themselves have changed over time, one can again take refuge in the above argument. Not only have archaeologists documented changes in native burial practices, e.g. from cremation to burial in central California where the Ohlone Indians now live, but also substantial changes in cultural beliefs and activities are known to have occurred in Europe and North America. The English who once included grave goods in their burial now include none; from uniformly burying their dead they may now cremate them, from burial of the privileged in the church they now bury them outside, and from burial in graveyards within the town they now bury the dead in cemeteries (Hubert 1989 : 134ff.). Similarly R.H. McGuire (1989) summarizes changing practices and beliefs in the New World from the colonial period to the present, and he describes corresponding changes in white attitudes toward the proper treatment of deceased Indians. The questions remains then : if Whites treat their ancestors as well as their recent dead in accordance with

³ Neither scientists nor Native Americans uniformly support these contrasting views. Among the latter there are some trained as archaeologists, who take the scientists' position ; certain tribes, like the Seminoles in Florida, have also stated they do not care about skeletal remains. Among the former there are archaeologists who support a moral argument for reburial over their presumed self-interest.

beliefs they hold today, why should we expect Native Americans to do otherwise ? What difference does it make that some native peoples in Northwestern Canada once exposed their dead to be consumed by scavenging animals or that others who now bury their dead in cemeteries once practiced cremation ? For Indians today, McGuire asserts, "the degree of intensity of concern certainly varies..., but the sanctity of the grave is clearly of greater religious, emotional and political interest to (them) than Whites", for whom the desire to leave graves undisturbed is a secular concern primarily out of respect for surviving family members. In his view also "the idea of communal ancestral relations being expressed by the Indians is foreign to the American public, who are primarily interested only with their direct blood relatives" whose connections they can document with genealogical evidence. This last point, as we shall see, enters into efforts to adjudicate cases that arise over conflicting claims.

On other grounds certain Native Americans may not see any particular value to them of the archaeologists' labors. Those who are most nativistic do not share the westerner's concern for history and comment that they learn what they want or need from the past through accounts by their relatives and ancestors. The archaeologist's interpretations of the past may be ideologically motivated in ways that do not necessarily serve the native's concerns for his past. Other natives complain that archaeologists have compromised their claim to scientific value of future digs by the indefinite storage of past excavated remains. These are the emotionally supported views that partisans for each position are loath to relinquish. Having said this, we need to bear in mind that as I write an increasing number of both groups find themselves somewhere between these polar positions. Indeed a few archaeologists, overwhelmed by the guilt they have come to feel about the past treatment of American natives, have converted to the latter's position, and seem to be paralyzed from deciding how they should carry on their professional activities (see, for example, Zimmerman 1989). On the other side there are some young Native Americans who have become professional archaeologists, and in this role tend to support their scientific concerns but not everywhere without mixed feelings (e.g. Jo Mangi 1989). In the main, however, the dispute, while often heated, has not reached the emotional level of, say, that over the abortion issue with its polarized, no-compromise positions that will only respond to legislation or the courts.

A question remains : why now ? Why did groups not make claims much earlier, or even in the 1960s ? The answer probably lies in two aspects of recent American history. The first derives from those forces that unleashed struggles for ethnic rights and identities, especially the Black Civil Rights Movement that culminated in far-reaching legislation in the mid-1960s. This dramatic struggle subsequently led other ethnic groups to seek greater empowerment, political in their own right, and a discrediting of assimilationist policies. Cultural pluralism has become both a fact and a value that should move in tandem with a variety of means for pursuing personal and economic well-being.

Second, as the most dispossessed and culturally degraded peoples after Blacks, Native Americans in recent times have found leadership voices among the newest generations. Some with higher education and knowledge of rights and opportunities through access to dominant institutions have successfully introduced entrepreneurial activities with government support. Others as professionally trained lawyers have come to serve their tribes, often with White colleagues, in the protection and pursuit of rights. Reburial is a convenient symbolic issue that, like resource disputes, lends itself to demands for reappropriation of cultural and political rights that for long had been depreciated or ignored. By continuing demands for reburial, sometimes

winning them or forcing some acceptable compromises, they draw national attention to their rightful indignation over generations of maltreatment that might serve as a springboard for political action in other directions.

Patricia Robertone (1989) has argued very recently that the archaeology of the colonial period, examined more from the material remains of native peoples rather than from the everyday life of colonists in the contact equation, would reveal resistance against domination as early as the mid-17th century in New England. She bases her conclusion on a re-examination of remains from two excavated sites of Narragansett Indian, paying particular attention to quantities of wampum grave offerings. To her they suggest that by taking "... quantities of wampum demanded as tribute by the colonial government out of circulation" [it was] "... an unwritten statement of political resistance (and) symbolically upheld Narragansett tribal authority" (Robertone : 42). If further investigations from this perspective were to confirm her views they would challenge the stereotyped rationalizations in support of continuing territorial expansion that assumed acculturation of native peoples without struggle. This historical perspective suggests that the current forms of protest by Native Americans are expressions in another guise of frustrations over abuses, injustices, and unequal treatment thrust upon them over centuries. They follow upon prior guerilla warfare, open battles, or political-religious movements like the Ghost Dance.

The paths to the resolution of these disputes have affected the disposition of remains from past excavations now curated in museums of one kind or another, but they most immediately affect archaeological research in progress and in the future. Rosen (1980) and Bowman (1988) have analyzed this conflict essentially in terms of four alternative approaches that have been or are being pursued in various situations: (1) discussion between the parties to the dispute to arrive at accommodation and compromise; (2) statements of policies by governmental agencies and professional associations in formulating guidelines for relating to Native Americans; (3) judicial action; and (4) legislative action. As Rosen asserts (p. 6) : "... more is at issue here than some arcane features of archaeology and the law. What is really at issue is the way in which anthropologists must think their way through the difficult ethical problems that arise from their studies of other human beings – problems that pose analogous complexities in every domain of the social sciences."

The second, third, and fourth of the above approaches are each burdened with many pitfalls, for a more detailed discussion of which the reader is referred to Rosen and Bowman among others. In this essay I can only briefly summarize the complexity of the problems they pose as extrapolated from the many cases that native groups individually or nationally have contested. On the basis of these analyses I am inclined to agree with Margaret Bowman that "the most straightforward approach, and perhaps the one with the most promise in resulting in ongoing positive relations between the Native American and scientific communities, is discussion of the conflict between the interested parties, directed toward an understanding that is mutually agreeable" (Bowman 1988 : 147).

Natives can take unresolved conflicts to both fifty State court systems and to Federal courts and appeal for reburial on the basis of their constitutional rights in all of these contexts. To date they have done so in a number of cases, in only a few of which they have received favorable verdicts. Their cause is greatly jeopardized by a number of factors. A not inconsiderable barrier

at the outset is the need to translate among three languages : from ways of thinking as expressed in their own language to English and from English to arcane legalese. When do they have Standing to Sue ? When is a burial site part of a cemetery and when is a cemetery by law considered to have been abandoned ? These and some other considerations (for example, pleading on the basis of rights of privacy on behalf of the deceased or the descendants of the deceased) are both matters of translation and varying interpretations. For native groups the older the remains and the greater the distance from the burial site the more difficult to claim the bones by recourse to judicial guidelines whose meaning is derived from Euro-American culture. The onus is placed upon the individual or group to demonstrate a reasonably close connection to individuals in uncovered graves. In American law a private owner may also give an archaeologist the right to excavate on his property. Can he then allow the archaeologists to claim the remains of burials the latter may have recovered ? The owner as well as the native has the right to sue on his own behalf and the outcome is never certain.

What constitutes a cemetery or a gravesite when it is considered to have been abandoned are equally thorny problems. In the recent past it has sometimes gone to the very heart of the legal definition of what is a religion – to what extent can Native American spiritual beliefs and practices be equated with the Great Religions of West and East – and when is a burial supported by religious belief to be protected by law. Several tribes have had great difficulty in justifying their claims to land over and against those of eminent domain, by recourse to arguments based on their spiritual importance. More immediately relevant, the notion of gravesites established near a church or of bounded cemeteries with grave markers situated at some remove from a community have no counterpart in Indian cultures; they are products of Euro-American social and political history. Native American burial customs differed widely and interments were often scattered in different sites known only by oral tradition or not remembered at all (Rosen 1980 : 7). Some groups were involuntarily moved by the government and still others were mixed with the bones of their enemies whom they confronted in face of the Western Movement of White settlers. To document genealogical or even less stringent criteria of connection with the deceased under these circumstances obviously poses great problems in court. In one state a decision has gone against a tribe because individual burials were said not to be easily identifiable and natives had the custom of never returning to visit the dead who were thus deemed to have been abandoned. In another state a court ruled that the mere act of burial indicates that survivors do not intend to abandon the dead. When the right of ownership and access to remains have been complicated by early treaties native arguments about what they intended at that time are open to further dispute.

Suits have been argued on constitutional grounds : freedom to exercise their religions, equal treatment of human remains, and rights not specifically enumerated in the constitution, like the reburial of ancestral remains. Occasionally native plaintiffs have been successful, but more often than not they have failed to win their case. The judicial approach remains a viable approach in the future. It is however very expensive and often frustrating.

In addition to suing in court Native Americans can also seek to have existing state and federal legislation enforced and, as an increasingly visible interest group, lobby for new laws regulating archaeological research and protecting their resources and disinterment of dead bodies. There exists legislation on all these matters, including the reburial of human remains. This has the advantage of moving beyond judgements based on a meagre body of existing case law. Legislatures have already enacted laws that affect the status of ancient remains as well as that

dealing with cemeteries and recently dead bodies. Although Congress in 1979 passed the Archaeological Resources Protection Act that increases native participation in the control of material resources on reservation lands, it does not provide protection for off-reservation sites nor does it require re-burial. In the same year Congress passed the American Indian Religious Freedom Act that requires federal agencies to "examine their regulations and procedures with an eye toward preserving the ceremonies, sites, and objects necessary for the exercise of Native American religions" (Rosen 1980 : 14). The efficacy of this and similar acts will depend upon how these statutes are interpreted in practice. Other legislation has dealt with the preservation of historic sites, the environmental impact of federally assisted projects, etc.

Federal laws and regulations pertain mostly to federal or Indian land. Each of the fifty states, when confronted with reburial issues and the perceived need to regulate archaeological field research, may therefore have to enact legislation of its own. Several have done so in recent years and others are likely to follow suit. A few give broad protection resulting in beneficial cooperation between archaeologists, establishing a Commission on Indian Affairs (where present) and local Indians, others little at all. Bowman (1984) cites four states (California, North Carolina, Massachusetts, and Iowa) that, although variable in their approaches to the recovery of human remains, have succeeded "in creating an atmosphere of cooperation between Native Americans, archaeologists, and state officials" (206). Of these only California exempts scientific merit from considerations of reburial. In practice, however, all parties to conflicts of interest have resorted to compromise in arriving at a resolution of these conflicts. The recent decision by Stanford University (*N.Y. Times*, June 14, 1989) to return all disinterred curated remains of neighboring Ohlone Indians after an agreed upon and presumably short remaining period of study is a case in point. A committee constituted of the chairman, Department of Anthropology, a university archaeologist, a political representative of the Ohlone and university administration representatives met on a regular basis for almost two years to arrive at this decision. Even so it faced the strong opposition of other archaeologists both within and outside the university (Gross 1989). On the other side the university also received strong commendation for its action.

The Stanford case reflects the great ambivalence and anxiety that archaeologists in the United States have about constraints they must face about the merit and pursuit of scientific research. Since 1985 their professional societies have conducted several conferences with the participation of Native Americans, anthropologists, museum curators, and lawyers that might seek mutually acceptable legislation. They have also established an ethical code to serve as a guideline for respecting the legitimate interests of those whose cultural history they are researching. This and other positions, especially on the reburial issue, have met with opposition.

However much such stated policies and guidelines have improved communication and cooperation between the interested parties they are not enforceable and therefore do not in themselves resolve the reburial conflict. In my estimation that will depend upon the weight that certain prestigious institutions, that have made nationally advertised decisions, will carry for the disposition of future cases. Stanford is one possible model that other universities could emulate. Policy set by the Smithsonian Museum, with by far the largest number of curated remains, is likely to have a greater influence in this regard. Its present director, and himself a distinguished archaeologist (Adams 1989) states that Smithsonian policy will respect "... the right of descendants to reclaim the remains of their ancestors... Beginning with clear cases and working toward more difficult ones, we are seeking to establish analytical and ethical principles that will

cohere into a consistent repatriation policy. Such a policy must be sensitive both to the role that living tradition of ancestral continuity play in contemporary Indian communities and also to the part that science plays in deepening our understanding of the whole aboriginal past." How broadly the demand for proof of descent will be interpreted at this moment is still undetermined.

At present, in light of the immense difficulties in establishing an agreed upon national legal policy reconciling respective interpretations of inevitable compromise, it seems to me that a case by case or group by group resolution of the disposition of human remains, such as enunciated by the Smithsonian and as embraced in the *Statement Concerning the Treatment of Human Remains* by the Society of American Anthropology (*SAA Bulletin*, June 1986), is likely to carry the day in the foreseeable future. Some cases, however, will probably serve as models for many others.

I have dwelt on the reburial issue at some length – even so, in summary fashion – for two reasons. First, similar controversies have taken place almost simultaneously, as we have seen, in other first world countries, notably Canada, Australia, and Great Britain. It is a burning issue in Israel and may become so in other new nations of Africa, the Pacific Islands and elsewhere. And, second, the debate questions the objectivity of science expressed by many archaeologists by raising moral, political and ideological problems inherent in the interpretation of their evidence. Aside from the ethical issue of dismissing native positions in the process of doing science, it addresses the problem of whether or not archaeologists correctly interpret the past when they ignore native views about their own past. Robert Layton (1989 : 2), in his introduction to a published set of papers presented at a recent conference on international archaeology, asks : "Can indigenous peoples contribute to a reassessment of their own past, or does Western culture have a monopoly on scientific method ? » I rather think the question should be put : can indigenous peoples provide new evidence of the past that would favor alternative scientific explanations ? Science contains a subjective dimension – creative imagination – as well as an objective one. As Layton concludes (p. 18) : "If people from other cultural traditions question the archaeologist's models of stability, change, and discontinuity, or the association of cultures and genetic populations, their criticisms should not too hastily be dismissed as unscientific." For some, perhaps increasingly, the present controversies support this opinion; others will seek to defend their view of science and arrive at the best possible compromise with native groups. In either case archaeologists will continue to articulate and to some extent must alter both their aims and procedures. However much they may have preferred to think of science as neutral, they operate in fact in an interactive world.

MAINSTREAM ARCHAEOLOGY

What I refer to as mainstream archaeology concerns the kinds of problems the study of which nation-states seek to promote for its own purposes. They might do this through selective financial support of proposed projects, the creation of government bureaus for archaeological research, the establishment of museums and cultural centers, and the hiring of archaeologists with topical specialization– or some combination of these. If they come from other countries archaeologists may work on problems outside the mainstream by bringing external funds and securing government permission. In this sense, perhaps, archaeology as practiced in countries with a long and well established scientific tradition does not have a mainstream, although even this is open to question. It may be more characteristic of new nations with long histories of

relatively independent histories before the advent of European colonialism. At present I know of very few cases, the best known being Israel (see Shay, 1989, for a recent and very useful discussion of this issue). Perhaps the request by e. g. African states for the return of valued art objects from museums in Western countries signals some such tendency in the near future. The two most common purposes we can think of for mainstreaming are : validation of claims to boundaries and national interests of any other kind. Mangi (1989), a Papua New Guinea archaeologist, asserts that given the great diversity of local groups, of western Christian churches and political immaturity, there is little basis for people in this new nation for a shared sense of political nationalism. In this light he sees the role of archaeology as contributing to nation building by revealing a common ancestry and a common past overlaid by the legacy of a colonial past and alien political institutions imposed at the time of independence. Does Chinese (possibly Indian) archaeology reflect an overacting national concern for its ancient origins and long continuity, with pride in esthetic achievements or with some other evidence from the past that would reinforce a shared sense of national consciousness ? If there were a mainstream archaeology in the United States or elsewhere today it would probably be identified by the kinds of projects funded by the National Science Foundation or some comparable agency, in line with what good science should be. It is to these orientations, past and recent, as well as to an emerging critique and suggestions for new directions in the archaeological enterprise that I turn to finally as constraints on its objectivity.

COMPETING INTERPRETATIONS OF ARCHAEOLOGICAL DATA

Strictly speaking the various approaches to the interpretation of data in archaeology are internal to the discipline; archaeologists are doing the interpreting. To date, however, the theories, and concepts all come from elsewhere, from economics, social and cultural anthropology, ecology, sociology, and so forth. It is in this sense that I think of them as external to how archaeologists ply their craft. With the professionalization of the discipline archaeologists continued for long to display an interest in the more spectacular remains of prehistoric communities. Spaulding (1987 : 263) could say that archaeologists, among other things, could use their methods to provide ancient art objects for esthetic contemplation. This was especially true for research into ancient civilizations, whether in Mesopotamia, Egypt, Greece, Persia (modern Iran) or in the New World civilizations of Aztecs, Toltecs, Mayans, and Incas. Until recently archaeologists of ancient Mesopotamia, for example, concentrated their efforts on the excavation of urban centers, monumental architecture, hoards of great art, royal tombs, temples and palaces, the succession of dynasties, and as much of the dynamics of production as could be recovered, with the aid of written records.

Even where excavations revealed the historical succession of communities from simple neolithic villages to complex cities little attention was paid to the processes by which this occurred until the 1960s and thereafter. Until then concentration on such matters of power structures, the nature and relations between temple and state institutions, dynastic successions, craft specializations, trade, changes in regional power relations, and other understandings to be gained from the study of urban communities left unanswered questions of how ancient cities arose and they dynamic interplay between cities and their rural hinterlands. The emergence of cities in Mesopotamia was largely recovered through stratigraphic excavations of prominent sites (see Wooley 1939, for example). Adams (1965; 1981) and Nissen (1972) were the first to

emphasize the importance of regional analysis to bear upon these problems in Mesopotamia. In the major ecological zone of Uruk they attended to such matters as new technology, labor and craft specialization, and changing settlement patterns recovered from survey analysis. Among other things they asked the question : why did one village site rather than others become first a town and then a city and where did its population come from ? From these analyses and the work of many other archaeologists in that part of the Near East, ranging from Lower Mesopotamia to the Zagros mountains, we have begun to get a picture of zonal development and inter-zonal connections - the role of climate, soil characteristics, salinization, changing channels of the Euphrates river, and other environmental factors; the significance and expansion of trade, colonization, and conquest in the historical dynamics of this part of the ancient world (Nissen 1988).

Sanders *et al* (1965) in the Valley of Mexico had undertaken a similar investigation in the New World, and Mayan archaeologists by turning their attention to the nature of agrarian production as well as the more spectacular urban centers have revealed not only the historically changing relations between urban communities but also the dynamic interplay between rural and city sites as well as the historically changing relations between urban communities themselves. In the process they have also altered our understanding about the historical population dynamics of these cities, and have provided a much fuller and more accurate conception of Maya civilization, its origins, larger context and development.

Archaeologists who began to attribute special importance to regional analysis (see Johnson 1977) could draw upon a large body of surface survey studies of settlement patterns that had accumulated over two decades (Ammermon 1981). The examples from Mesopotamia and Middle America, cited above, are cases in point. These and other kinds of regional analyses began to draw upon geographic theories of spatial behavior that led away from exclusive concentration on specific site excavation to a broader contextual analysis of site relationships and long term social change, for which the archaeological record is especially useful. It is one example of how earlier preeminent attention to one body of data precluded research in others; how theory external to archaeology governed the nature of questions asked, relevant data sought, and analytical methods.

A similar development occurred in social anthropology with the publication of Skinner's pathbreaking publication of regional analysis in historical China (Skinner 1964/65). In fact the burgeoning of various archaeologies in the past thirty years has closely paralleled that of anthropology, which in turn drew upon internal revisions of culture theory and other social science theory. Proponents of one view or another heatedly sought to define the proper goal of anthropological research and in the process have nearly fractionated the enterprise. Ortner (1984) sounded one note of optimism in her not altogether successful attempt to suggest the complementary contributions they make to a more or less unitary discipline.

Archaeology also has its optimist in Trigger who suggests that acrimonious debates over the theoretical propositions relating to human behavior are "yielding to profitable dialogues, while archaeology as a whole is coming to appear less sectarian within the broader context of anthropology" (Trigger 1984 : 275).

The struggle with diverse realities of the factual record led Shanks and Tilley (1987b : 23) to query whether a "repressive pluralism" would prevail in archaeology or whether "different archaeologies are simply different approaches to the same past". Some social anthropologists and archaeologists alike have been almost paralyzed by similar competing claims to theoretical legitimacy. These developments have prompted various historical surveys and critiques, ultimately favoring one position or another, that reflect much intellectual sophistication, soul searching, and substantial scholarship. Rather than traverse at some length the content of these reviews [see especially Trigger 1984; Hodder 1986; Earl and Preucel 1987, Shanks and Tilley 1987a and b; and Gibbon 1989] I intend briefly to consider the principal forms of research they entail. These are:

- The new archaeology and Systems theory
- Structuralist archaeology
- Marxism and its several interpretations
- Symbolic archaeology
- Behavioral archaeology

The *new archaeology*, first enunciated by Binford (1962), was based on a positivist philosophy of science that privileged the collection of data within a theoretical and hypothetical-deductive framework that rejected traditional archaeology as normative in its conception of culture, descriptive, and speculative. It emphasized process and sought to test propositions in evolutionary terms as a people's adaptation to environmental and ecological circumstances. It emphasized the construction of models and experimented with different kinds of quantitative analysis. This exclusive view of science rejected not only historical studies in the absence of documentary evidence – the cultural history of traditional archaeology – but also inductive approaches to science.

In the first blush of enthusiasm for the power of scientific analysis the new archaeologists claimed to have established a new paradigm for doing prehistory which would supercede an outmoded view of culture and methods of doing prehistoric ethnography. Subsequent critiques of earlier studies in this vein – a reliance upon structural functional theory that emphasized equilibrium and stasis with change induced by external factors – toned down the missionary aspect of this claim. When it moved beyond specific site and regional analysis to test cross-cultural propositions (the correlation between style and distribution with the presence or absence of lineage structures, for example) it was censured for taking traits or features out of context. In this respect it resembles cross-cultural studies in social anthropology which, despite methodological criticisms, continues to constitute a sub-field of that discipline.

The short history of the new archaeology also reminds me of ethnoscience and componential analysis in cultural anthropology. This is a cognitive approach to the search for native principles underlying various domains of culture and has made interesting and important contributions. The flurry of early ethnoscience research was also thought by its practitioners to replace more conventional methods of ethnographic analysis. It too had its critics, since it was shown that more than one model could explain a given array of facts and therefore one could never be sure which of these correctly reflected thinking about them. Nevertheless ethnoscience today constitutes one among a number of methods of ethnographic data collection and analysis. So, it seems to me, is the future likelihood for the new archaeology, especially with its reforms and

emphasis upon middle level theory (although symbolic archaeologists, like Hodder [1986 : 116-117], assert the falsity of this approach, as well). Insofar as they train and influence future practitioners in this mode the new archaeologists impose constraints on the statement of goals and unity of the discipline, as indeed the practitioners of other archaeologies we shall consider. Spaulding (1988 : 268), who conceives of archaeology as science in the broadest sense attempts to reconcile even the most contradictory theories as simply "urging us on... to better science".

Systems research, as reflected in the work of Clark, Renfrew and other British archaeologists, is integrally related to the new archaeology. It places emphasis upon processes of intercommunication and interaction between a series of entities or subsystems that constitute a society. Drawing upon systems theory it uses concepts of negative and positive feedback as internal sources of stability and change, the latter as instigated by external forces (environment, population, settlement pattern). Like the new archaeology, or perhaps as part of it, the systems theory approach to the interpretation of material culture thinks of stability as the natural state of society and culture and change as something to be explained.

While recognizing the several advantages of systems analysis over traditional archaeology (e.g. Shanks and Tilley 1987b : 33-34) critics have raised the same objections to it as to the new archaeology. To quote Hodder, for example (1986 : 25): "Within the... systemic approach cultural meanings are imposed, but always from the outside, without adequate consideration. The assignment of cultural meanings is normally based on Western attitudes, which are implicit and undiscussed. It is assumed that burials, rituals, headgear and pot decoration have universal social functions, linked to their universal meanings [and to social-cultural stability]; objects are wrenched out of their context and explained cross-culturally". A functionalist approach to the interpretation of fact, in this view, not only fails to allow for alternative interpretations of the same facts but also imposes our own attitudes and ignores those of the people who made the objects, buried the dead in a certain way, etc.

Earl and Preucel (1987) refer to the remaining departures in archaeological research during the past two decades – contextual, structuralist, Marxist – as Radical Archaeology. Marxism in whatever mode has a distinct advantage over new archaeology and systems research in that it invokes a dynamic sociology that attends to internal sources of change, contradictions and competing social interests, and also power relations that govern access to resources. It therefore is to be commended for assuming variability of meanings assigned to cultural laws, behavioral patterns, and the like by different social categories (e.g. by gender, age, status), noted in living populations. In both attends to history and promotes in-depth studies of particular histories within a general theoretical framework that enables cross-cultural generalizations.

Structural and contextual archaeologies stress the role of human agency, the practices of individuals in everyday activities or events that have recursive relations with the structuring of these activities. Drawing upon the theory of Giddens and Bourdieu they seek to show how pattern is generated and, in the case of Bourdieu, how it may change. Individuals matter; they play an active rather than passive role in creating both stability and change. For Hodder the insights of Bourdieu are exciting for the archaeologist because he includes "the mundane items in the material world, of the type excavated by archaeologists – pots, bones, pins and door frames – [as playing] a part in the process of [early] enculturation, in forming the social world [habitus]... Material culture in particular plays a highly active role, creating society and creating continual

change" (Hodder 1986 : 73-74). In these respects the links of structuralism to Marxism constitute an improvement over the applications of materialist Marxism to the archaeological record.

Hodder proceeds, however, to criticize structuralist, together with processual and Marxist analyses, as "... limited in their ability adequately to explain the past, because they refuse to grapple with the *content of historical meanings* (italics mine) and with the question of where style, structure, or ideology comes from" (p. 75). The context of actions and the reconstructions of meanings ascribed to the physical remains that prehistoric peoples left behind, Hodder asserts, should constitute the legitimate goal of archaeological endeavors. Even Bourdieu's structuralism, which incorporates culture (*habitus* = early enculturation with deep personal significance) as a mediating element between theory and practice, fails in this perspective to deal adequately with contemporary culture theory.

In brief, symbolic archaeologists applaud Marxist research – indeed, they may often think of themselves as Marxists – for its concern with history and internal sources of change and structuralist research for its emphasis on human agency and the activities of everyday life. They find fault with the former, and Giddens's structuralism, for its presumed exclusive emphasis on the social and the latter for its failure to attend to the contexts and meanings embedded in the material objects of the prehistoric record (material things are part of a material *culture*). As regards meaning, Hodder goes farther than Shanks and Tilley (1987b), who accept that the archaeologist cannot with complete accuracy determine past meanings but must be content with translating the past in the language of the present. They liken this process to the anthropologist faced with the problem of analyzing an alien culture in his/her language rather than in that of the original. Their concern, however, continues to be the construction of as accurate a model as possible of that past culture and, insofar as variation in style permits, with variation in meaning within the "positioned social situation of the individual" (pp. 115-117). Like the ethnoscientist they must recognize the possibility of other, perhaps more accurate, models but without the advantage of living actors to observe in context and discourse.

Some neo-Marxists (e.g. Shanks and Tilley 1982, 1987a; Miller and Tilley 1984) seem to turn Marxism on its head. To quote from Earl and Preucel : "This neo-Marxism shares with other Marxist approaches the conviction that contradiction and conflict provide the fundamental basis for an understanding of social domination, legitimization, and change. It differs from other Marxist approaches in its emphasis on ideology and structure rather than economy as the prime determinant" (1987 : 507).

To take one example we may cite Shanks and Tilley (1987a : 155-171) who work through a data set consisting of 70 completely restored or restorable vessels attributable to the southern Swedish middle neolithic funnel neck beaker culture. From a detailed analysis of the distribution of graphic styles and skeletal remains in and at the mouth of a communal tomb they arrive at a series of oppositions (e.g. individual/group, culture/nature/bounded/unbounded, disarticulated/articulated basic body symmetries), "involving social strategies arising from opposed structuring principles of social control by individual lineage heads or elders in contradiction with collective production, and direct, unmediated reciprocity and exchange relations between kin groups". And they conclude (p. 170) that : "the generative principles governing the sequences of bounded and unbounded primary design forms on the pots deposited outside the tomb during phases II and III can be seen as an attempt to resolve on an imaginary (because graphically displaced) plane the

contradiction entailed by an assertion of social boundedness [we/them] and non-boundedness [all such groups are part of a communal society] at the same time... The style becomes a material form of ideology attempting to transform the relationship between oppositional elements into a spontaneous whole with the overall aesthetic effect of unity rather than opposition. »

This interpretation and the lengthy, informative discussion of style that preceded it, based on the assumptions they make, is reasonable but is certainly open to criticism on the basis of how much material remains can reveal – or perhaps on its own terms, to alternative interpretations. Given their opposition to functional analysis in other archaeologies one might also question whether this is not an example of functional analysis in other terms : when an ideology attempts to transform the relationship between oppositional elements, is this not its function ?

Finally I mention behavioral archaeology, particularly as espoused by Earl and Preucel (1987 : 510-512). Their position on the future of archaeology is to accommodate elements of the radical critique within a revised framework, one that focuses on more recent decision making models at the level of the individual. They suggest that radical archaeology does not so much constitute a new paradigm as refinements and improvements on it, a position taken by Spaulding, as we have seen. What they are unconvinced of in the work of the radicals is their ability to penetrate the mind of the prehistoric individual, and what further troubles them is the “apparent rejection of theory and the disregard of a replicable and verifiable methodology” (p. 509). If, for example, extensive irrigation works (an irrigation-based economy) has evolved into resource control and stratification in one place, why is it not appropriate to test for the generalization of this process elsewhere ? And if ideology operates in a certain way in a given context, is there a methodology by which it can be observed to operate in other, similar, contexts ?

Behavioral archaeology from this perspective “incorporates the description and explanation of spatial patterning... within a positivist framework...” (Earle and Preucel : 511). They envisage that its theoretical development will be most productive in its derivation from decision making theory and processual Marxism. The data base of archaeology does not readily lend itself, in their view, to cognitive and deep studies of culture, which is problematic enough for cultural anthropologists.

CONCLUDING REMARKS

In this essay I have attempted to point out different kinds of constraints archaeologists have encountered in pursuing their research goals in recent decades. Some have been labelled “imperialists” by two camps : by oppressed peoples who claim that archaeologists desecrate their ancestors’ graves by digging them up and sequestering their remains more or less indefinitely in museums; and by cultural relativists who assert that those who subject their data to comparative and generalizing analysis do violence to the uniqueness of prehistoric cultures and the meanings embedded in material things.

The reburial issue, wherever it arises, will require compromise on how archaeologists conduct field research, construct data banks, or return to and rethink about evidence recovered from earlier excavations. At the same time archaeologists increasingly see themselves as involved in the present and its political manifestations. The clamor of contemporary inter-ethnic discourse

requires them to attend more to the living peoples, whose territories they invade and who also assert a claim to the past and seek to curb what appear to them as insensitivity to, and disrespect for, their cultural values as well as an assault on their civil rights.

On another level, the channeling of research activity by overriding concerns for the uses of the past by governments and peoples, especially in new nations, begins to assert itself in the form of financial support, the training of indigenous archaeologists, and perhaps an increasing scrutiny of projects designed and managed by foreigners. At this point in time it is difficult for a non-practitioner to assess just what role such a political force will play in future research.

By contrast with the controversies that have been swirling around the reburial issue those among advocates of diverse theoretical orientations in archaeology seem to be very sedate. However contentious they may be, the authors of more general reviews fairly (and similarly) describe the central attributes of these positions, but raise different criticisms and ultimately propose and defend their respective views about what the archaeology of the future should look like : what theories, concepts, methods, and goals the material record of the past can reveal when stretched to the limit. The wide range of external scholarly resources examined, assimilated, and thought about is impressive.

The debate (discourse ?) about these issues appears to revolve about a number of dichotomies. Those that stand out to me are : objectivity/subjectivity, society/culture, history (relativism)/generalizing science, the individual/the collective, and common understandings/cultural variability. They are not necessarily exclusive, in that critics of one body of work may attack it from more than one perspective. Symbolic archaeologists, for example, sharply criticize the exclusive emphasis on objectivity and empiricism of scientific generalizers; there is after all subjective imagination in the construction of theory. But they also attack their inattention to the role of the individual as an intentional actor, hence to intra-cultural variability. The prehistoric evidence and its distribution in space is only known through the senses; obviously it is thus empirical. But stress upon the social – groups, social systems – fails to take into account cultural meanings, values, or ideologies situated in material things, as incorporating cultural ideas.

Hodder (1986 : 89) even finds merit in the explanatory powers of diffusional studies by traditional archaeologists, especially in the application of Kroeber's concept of stimulus diffusion, thought of as "an active social process as working on systems of meaning which develop over the long term". He also credits mainline Marxist theory for its concern with historical dynamics and internal variability and conflict, but, with the exception of Bourdieu's contribution, he finds fault with its omission of human agency and individual interpretations of shared meanings. In this he makes common ground with the rethinking of culture theory by many anthropologists in these same decades.

Perhaps, therefore, it is the social/cultural dichotomy that lies at the heart of these controversies. Those archaeologists who take society as the basic, indeed the only knowable, unit of analysis believe they can accommodate the individual through economic and psychological decision-making theory, as well as retain the contributions of evolutionary theory, systems theory, and spatial analysis without assuming a unilinear model, and by examining cases in their particularity over time. The problem with post-processualism is then reduced to the

possibility of cultural explanation from archaeological evidence, given the polysemic and arbitrary nature of symbols (in the case of prehistory, of objects and events as symbols).

So far those who favor cultural analysis, in my view, have yet to make as convincing studies as those who do science in one form or another, including certain processual and generalizing analyses, like the comparative study of the evolution of ancient states. Even so, they are to be commended for raising important questions about the validity of other archaeologies and directing attention to new possibilities for interpreting the prehistoric record. Like anthropologists they will probably continue to struggle for some time with goal definitions and attempts to achieve some unified conceptions of the field. At the same time archaeologists will get on with their work, from whatever perspective they find useful or insightful. The pluralism we observe will lead either to better science, as Spaulding suggests, or to complementarity of effort and building from conceptual omissions of the past. As some archaeologists have asserted, the problems they study and the explanations for interpreting the nature of prehistoric societies are, at least in part, conditioned by the political and economic realities of the present. Social science in this sense is not neutral. For them the recovered evidence about the past constitutes "texts" to be read in context, and, like ethnographies, are subject to continual reinterpretation.

It is an exciting time for archaeology, as well as for anthropology generally. Instead of dismay some may perceive in the present fractional tendencies of the discipline, archaeologists of all persuasions should pursue their various lines of inquiry, improve upon them as possible, and only then recognize dead ends when they become evident.

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**THEORIES, METHODS AND
THEIR BIASES**

ARCHAEOLOGY AND THE SCIENCE OF THE CONCRETE

by

Mary HESSE

THE METHODOLOGICAL DEBATE

Like most of the human sciences, archaeology has passed in the last few decades through most of the phases of methodological debate that have characterized the natural sciences. There was a positivist and descriptive phase in the mid-20th century, which rejected earlier speculative and romantic tales of migrations, invasions and diffusions of culture having little conceptual or historical basis. Archaeology understood itself as what can be dug up and the immediate inferences therefrom. With the more sophisticated hypothetico-deductive method delineated in the mid-century for natural science (by Popper, Hempel, Nagel), the so-called "New Archaeology" of the 1960's and '70's came to take a more relaxed view of the possibility of theories about the unobservable, so long as deductions from these could be empirically tested. In consequence, systematic, theory-led collections of test-data flourished. Statistical methods were ingeniously exploited to test hypotheses by means of apparently non-random distributions of objects, properties, and spatial and temporal arrangements. Such statistical studies often re-worked old data and derived illuminating new correlations.

The 1960's and '70' were, however, also the period in which philosophers of the natural sciences were coming to realise that empirical data are not simply given, but are theory-laden, that is, collected and interpreted in the light of some theoretical framework or other, and that if this is true in the natural sciences, it is true a fortiori in the human sciences. The process of observation-theory-prediction-test is circular, not linear ; change of theoretical framework can lead to rejection of previously interpreted observations as well as observations leading to rejection of theory. Since archaeology is a human science, its data are always laden with interpretations involving human intentionality – flint flakes are related to tool-using, ditches to draining or defence, holes to foundations of buildings. Archaeology is also a historical science, and as in history it is misleading to expect rigorous law-like correlations between types of human action and its social and natural environment. Inference typically takes place not to laws but to theories about the particular causes and antecedents of particular events, and takes place partly by analogy from similarities and differences between particular cases.

So far the methodological debate has followed the same lines as that common to all the human, or hermeneutic, sciences. But archaeology has the special feature that its theories must be about unobservable events that are inaccessible in time, and about social relations and mental beliefs that are inaccessible from mere material non-literate remains. Thus the methodology of

inference from analogy with more accessible societies, whether documented or ethnographically describable, becomes crucial, and the problems of analogical inference become fundamental problems for all philosophy of archaeology. There is an added difficulty, shared by archaeology with the anthropology of extant societies, but to a greater degree. This is the problem of the dichotomy between so-called traditional or symbolic thought-forms, and the scientific rationalizing presuppositions of western thought, including of course the study of archaeology and anthropology themselves. To understand and reproduce the symbol-systems pervading non-western cultures that can be visited is a hard enough task ; to discover it for societies distant in time is so daunting that positivist exclusion of any such attempt from a scientific archaeology might seem to follow necessarily.

The problem is this : how to infer from material remains and their natural environments to the socio-cultural features of the peoples who left them, a problem exacerbated by the fact that everything we know about prehistory and pre-literate peoples tells us that their conceptions of themselves and their societies were structurally very different from ours. How, in other words, do we infer from things to symbols and what they symbolize ?

SYMBOLIC AND STRUCTURAL ARCHAEOLOGY

It may be well to begin with some examples of studies which explicitly address this question. A model investigation of this kind (Von Gernet & Timmins 1987) concerns a discovery in a Canadian Iroquois grave of the 12th century AD of two significant items : some bones of the Carolina parakeet, which had never been found alive north of the Great Lakes and is now extinct, and the stone bowl of a pipe. The context suggested the association was not fortuitous, and the author traces similar associations between birds and pipe-smoking through ever-widening circles of analogy and generalization, including 17th century literary and artefact evidence of pipes ornamented by bird skins and bird engravings, the Indian peace-pipe ritual, the smoking of entheogens (mind-altering plants), analogous modern Indian collections of significant artefacts, to the bird effigies and symbolisms associated with North American and Asian shamanism. Thus the original find of grave goods is put into a context of meanings linking time and space, within which it is interpreted as an indication of a form of symbolism in the 12th century, 500 years earlier than previous evidence had suggested for North America. Moreover, the investigation suggests that such bird/smoking symbolisms do not have to be spontaneously generated or diffused by cultural contact, but may reflect very ancient substrata of belief which persist in varying manifestations, depending on specific environments. The hypothesis is both suggested by and supports the data by interpretation and coherence. It is doubtless not the only hypothesis that could be suggested, but in this respect it is not different in principle from hypotheses in natural science, where theories are likewise underdetermined by data, and data are theory-laden.

Two other features of symbolism are suggested by analogical inference and can be illustrated by examples. The first is the hypothesis that symbolism is not a direct one-to-one relation between symbol and social meaning, but has a more complex transformational relation to the social structure. This may be illustrated by a study of Neolithic mortuary practice in England and south-eastern Sweden (Shanks & Tilley, 1982). The bones in barrows are not distributed as complete skeletons, but in non-random piles of different types : skulls, ribs, vertebrae, limbs, etc. Piles of different types are separated by definite boundaries marked by pots or skulls. The

arrangement suggests an emphasis on the collective rather than the individual. On the other hand there are signs of differentiation between individuals, in that adult and child remains and male and female are sometimes separated, in apparent contradiction with the other type-classification. Cross-classifications of this kind are very familiar in social categorization, and may be taken to indicate some degree of contradiction between different manifestations of social order. In recent Marxist-inspired analyses the "contradictions" have been identified with those between social classes, elites, subject-people etc., but there is no need to restrict the idea to class-contradictions; kinship, trade, clan and other differences may provide equally illuminating hypotheses.

Tensions between cross-cutting differences may be represented in a symbol-system which serves to facilitate communication between groups, to reproduce existing social order and to legitimate it by attempting to mediate contradictions, and also to bring into consciousness latent conflict and hence to initiate social change. The authors of the Neolithic mortuary study follow Bourdieu in concluding that "The natural and social world must appear to be self-evident. As we have seen, ritual activities by their very nature are one of the most effective means of carrying this out. Such activities serve to lay down a clear dividing line between the thinkable (the present social order) and the unthinkable (some other social order)." (Shanks & Tilley 1982 : 151).

The same example may serve to illustrate another problematic feature of the relation of the symbolic to the social. There is the difficulty for western thought of conceiving the connection between apparently arbitrary symbols and rituals and the social order of which they form part. As just pointed out, this is not a question of trying to "decode" symbols piecemeal into their "social meanings", but it does involve postulating some non-arbitrary relationships, however complex, between symbolic beliefs and actions and the (to us) more familiar world of family, production, economic exchange, and institutions of power. In the present example, correlations and ancillary evidence may suggest that piles of sorted and differentiated bones have an intrinsic relation to collective solidarity, but it is difficult for us to *think* the inwardness of such a relation. Similarly, pot-decoration (which is fairly accessible to archaeologists) has frequently been used to show how social boundaries and conflicts (which are not directly accessible) are represented, mediated or concealed. It is rarely the case that our dissociated aesthetic sensibilities can think complex connections between decoration and social structure; in other words, *verstehen* does not help us there as in many other areas of human intentionality. Hypotheses relating symbols to society have therefore to rest largely on correlation, analogy and circumstantial evidence across comparable societies.

As a form of scientific inference, analogy has traditionally been regarded as a frail and distant kinsman of inductive and deductive methods. There are, however, now signs of its rehabilitation within logic, philosophy of science and philosophical anthropology. In the next part of this paper I shall outline two of the developments that are particularly relevant to archaeological method. Firstly there is recognition of traditional thought as the so-called "science of the concrete", and secondly there are new justifications of analogical inference within scientific method in general.

THE SCIENCE OF THE CONCRETE

Levi-Strauss begins his classic work *La Pensée Sauvage* by distinguishing between the logic and science of the *concrete* and that of the *abstract*. He makes it clear that in his view this is not a fundamental distinction between “traditional” and “advanced” societies, but is a discrimination that necessarily pervades all languages and cultures. All thought demands order, initially in the form of classifications : “Any classification is superior to chaos... even a heterogeneous and arbitrary classification preserves the richness and diversity of the collection of facts it makes. The decision that everything must be taken account of facilitates the creation of a ‘memory bank’ ” (Levi-Strauss 1966 : 16).

The science of the concrete classifies by means of immediate sensory qualities, by similarities and differences, by analogies of property and structure, and expresses abstract ideas by symbols drawn from the concrete. For example, early plant taxonomies emphasized obvious characteristics such as flower colour or medicinal qualities which gathered plants into groups, facilitated the recognition of herbal remedies, and provided a language of flowers as symbolic of the human passions. The science of the abstract, on the other hand, classifies by means of contiguities and causes rather than directly observable similarities and differences, that is it is metonymic rather than metaphoric, and it generates abstract theoretical structures from which a technical language and mathematical explanations may be developed. Plants are then classified by deeper characteristics not always directly observable, so that simple comprehensive taxonomies can be derived and related to objective features of plant evolution or inter-species fertility. The purpose of the classification is related to what can be done with nature (instrumental science), rather than what can be seen in nature or constructed out of it for the purposes of ordering social life. The language of flowers and all such metaphorical extravagances are discarded in the science of the abstract.

This, however, is to express the contrast much too sharply and from the point of view of “our” science, which positivist interpretations have regarded as superceding the science of the concrete because it is alleged to be the unique route to objectivity about what “really is” in nature. This propensity to relegate the science of the concrete to traditional thought in comparison with modern science can be challenged on two fronts. First, as Levi-Strauss himself emphasizes, all peoples of whom we have knowledge, from the Paleolithic on, show evidence of using induction, hypothesis and testing on the basis of their classifications in the interests of exploitation of the environment. If logical thinking is constituted by the mind’s ability to model the world, their capacity was often greater than ours. For example, questions about potential marriage-partners elicit immediate response, even though the highly complex kinship structures on which marriage rules are based often defeat the anthropologist’s attempts to make them explicit. There is evidence from similar circumstances in modern non-western cultures that, when asked, appropriate rules can be produced, but only to satisfy the slow mental processes of the anthropologist. Non-literate societies, in fact, need to be able to manipulate as many complex mental models as we do.

Secondly, the concept of a symbol system needs to be re-examined. Traditional semiotics sees it as a kind of *code*, in which signs, whether objects, words or actions, are taken to *stand for* something else which is their *meaning*. Such a theory suggests that symbolic systems can simply be decoded piecemeal into their meanings, in much the same way as the discredited Fido-

fido theory of word-meaning held that words stand atomistically for their referents. Even currently acceptable theories of language that depend on correspondence or satisfaction-meaning are bad models for symbolism, because they still imply a relation of "standing for" states of affairs that would make utterances true, although the units of language are now sentences and not words. These theories are still literalist, that is they introduce the additional elements of "truth", which is problematic in the case of symbolism, because if symbolism is not reducible to "literal" concepts, it is not clear how judgments about its truth can arise.

Pointing out that symbolism is not like this has led some to suggest that it is not a language at all, but this does not follow. A better model for the special features of symbolism is based on a more adequate non-correspondence theory of language. Such a model is implicit in many of the metaphors of recent archaeological writing, notably in Ian Hodder's *Reading the Past*. Hodder treats data as *text* to be read ; all texts presuppose some *rules* of connection which it is the archaeologist's task to uncover ; the interpretation of all texts depends on *context* ; and most importantly the metaphor of *meaning* applies to both texts and objects ; "the meaning of an object is derived from the totality of its similarities and differences, associations and contrasts" (Hodder 1986 : 138). I have argued elsewhere (Hesse 1988a, 1988b), in relation to linguistic metaphor, that linguistic meanings are not univocal and should not be understood in terms of a relation of "standing for", but in terms of *meaning relations* such as synonymy and heteronymy, structural analogy and inversion, inclusion and contradiction. In this perspective words are like symbols (sometimes they *are* symbols), and ultimately the meaning relations in both types of system rest on perceived similarities and differences between objects, properties and contexts. Symbolism may lack syntactic and semantic rules of the kind studied in linguistics, nevertheless use of symbols is rule-governed, and since symbols and words have a similar semantic basis, symbolism may be said to be a primitive language. The crucial respect in which such a theory differs from standard linguistics is that it is *contextual and dynamic* ; in neither symbolism nor natural language does it make sense in general to ask of isolated objects, processes, actions, words, sentences, or even whole texts "*hat* does it mean ?", where "meaning" is taken to be something outside the context of use. Just as a poem's meaning is a function of its internal organization, which gains significance from a multiplicity of inexpressible resonances in the world and human experience, so it is with symbolism.

ANALOGICAL INFERENCE

The meanings of symbols are therefore functions of relationships within a complex network which constitutes language, culture and social structures in general. Symbols communicate in particular, holistic contexts, and are typically learned by analogies and discriminations within particular situations, not by generalizable rules. The same emphasis on the local, the concrete, and the contextual emerges in studies of the place of analogy in modern science. These indicate that science too rests essentially on inference from the concrete, although it goes beyond it in ways that are connected with the overriding goal of scientific "objectivity" ; unlike symbolism, the "objective" goal of science as generally understood is successful prediction and control of the natural environment.

The logic of analogical inference in science is an extension of what W.E. Johnson (1924 : 43) called *eductive* inference in contrast to *inductive* inference to generalizations and laws. The

traditional problem of induction arose from the attempt to justify two types of elementary argument : the derivation of universal generalizations from particular data ("All crows are black"), and the derivation of properties of the next instance from observed instances of the same kind ("The next crows to be observed will be black"). The latter is eductive inference, and is weaker than induction to generalizations, requiring therefore weaker premises for its justification. It does, however, in common with all types of induction, involve recognition of instances of *the same kind*, and it is here that the notion of analogy enters at the root of all scientific inference. No two particulars in nature are alike in all respects ; what traditional induction has assumed is that instances that are "sufficiently alike", or "alike in essential respects", constitute suitable sets of data for both inductive and eductive inference. Eductive inference from particulars to particulars makes the requirement of *analogy* between instances explicit, and can be generalized to give a probabilistic version of the standard form of analogical inference : if A and B are known to be similar in respects which outweigh their differences, then it is more probable than not that A and B will be alike in further respects. Further generalization of this logic gives the form of argument from models in science ; if two physical systems (for example water waves and light) are similar in some properties and behaviour, then they may be assumed, at least for the sake of a useful hypothesis, to be similar in other related respects. Expression of the forms of these inferences begs many questions which remain to be discussed, but they may be usefully summarized for our purposes in a general *clustering postulate* which represents the underlying assumption of all scientific and indeed everyday inference : all other things being equal, assume that the world will continue to be more similar to than different from what has already been observed.

Eductive inference is not in conflict with other standard accounts of scientific method, but it is essentially local, particular and contextual, and makes fewer demands upon the universalizability of law-like relations and theories. It can therefore be transferred directly from the natural to the human sciences, which also require local, particular and contextual inferences. Judgements of sufficient analogy become crucial, as we have seen in archaeological examples. This should not be regarded, however, as a radical departure from scientific method, but as an extension of it which makes explicit what is always implicit in inductive and theoretical inference, however disguised in the elaboration of theoretical structure in the natural sciences. The method also retains an observational basis, unlike forms of constructivism which claim that all theories in the human sciences are merely in the eye of the beholder.

The pervasive character of analogical inference gives sufficient justification for the view recently expressed by archaeologists, that if the choice is to stop at the limits of data or to make speculative hypotheses on the basis of analogies with comparable systems, then it is better to make the hypotheses, even though there may in the end be no way of deciding conclusively which hypotheses are true. Neither is there any such way of deciding in the natural sciences. It should be noted in conclusion that although this prescription sounds like Popper's justification for "risky" hypotheses, it is not the same, because it appeals to an *inductive* type of reasoning generalized to include analogy, and this is a much better reflection of archaeological and anthropological practice than Popper's strictly hypothetic-deductive methodology. The science of the concrete postulates fundamental analogical structure between particulars in terms of which a logic of induction is possible, and this logic is common to the natural and the human sciences. The task that remains is to spell out from particular examples what are the criteria and constraints for good analogical inductions.

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LE TRIOMPHE DU NATURALISME DANS L'ART PALEOLITHIQUE

par

Michel LORBLANCHET

Le point de vue du chercheur sur les images paléolithiques et son approche scientifique sont influencés par des a-priori inconscients imposés par son époque et son environnement intellectuel.

Bien qu'il soit un phénomène très banal, le conditionnement historique et social des recherches semble échapper à la plupart des préhistoriens. Les plus avertis de ces problèmes sont vraisemblablement les ethno-archéologues de l'école anglo-saxonne qui tendent à rejeter le vocable "art paléolithique" parce qu'il impliquerait, selon eux, la projection d'un concept moderne sur des productions appartenant à un tout autre monde que le nôtre.

L'ART PALÉOLITHIQUE EST-IL UN "ART" ?

En 1988, au congrès international de Darwin, dans un article intellectuellement provoquant, O. Odak critiquait vigoureusement l'étude archéologique de l'art rupestre en réclamant la création d'une discipline nouvelle indépendante. Cette discipline vouée à l'étude des figurations devait, en premier lieu, abandonner le mot "art" dont la définition : "accomplissement de l'adresse humaine ayant pour but de donner un plaisir et non de répondre à une quelconque utilité" ne lui paraissait pas convenir à ce que l'on nomme "l'art rupestre" parce que peintures et gravures n'avaient pas pour fonction première la satisfaction d'un plaisir.

Ces propos rappelaient ceux de M. Conkey concernant "l'art paléolithique" : "En employant le mot "art" pour nommer ces images, nous supposons d'emblée qu'elles ont un caractère esthétique; nous supposons aussi qu'il existe un lien entre leurs fabricants et utilisateurs et nous... un tel terme a suscité des comptes rendus interprétatifs monolithiques et a fait basculer trente millénaires de création d'images dans une seule et même catégorie" (Conkey 1987 : 413).

Il est vrai que l'on doit se méfier du discours esthétique moderne et qu'il faut voir dans les figurations des parois autre chose que leur simple beauté et leurs seules qualités formelles. L'étude de l'art rupestre doit s'efforcer de recréer la perception et l'usage des images par les peuples du passé.

Dans les périodes lointaines et les sociétés traditionnelles ces figurations semblent avoir été souvent considérées comme l'oeuvre des esprits et non comme le résultat d'une action purement humaine. Elles étaient tenues pour des manifestations plus ou moins directes des dieux, parfois même pour des miracles d'autogénération. Il en est ainsi, par exemple, pour l'effigie du Christ sur le Saint Suaire des Chrétiens ou pour bien des gravures et peintures rupestres d'Australie interprétées par les Aborigènes comme des *empreintes* laissées sur la roche par les héros du Temps du Rêve pendant la période de création où le monde conservait encore sa plasticité originelle. Cette essence divine des images s'oppose à notre conception esthétique moderne (Belting 1990). L'artiste des temps anciens n'était qu'un anonyme intermédiaire, un traducteur de la volonté des esprits, intercédant entre le groupe et eux, alors que dans l'art moderne la divinité s'est totalement effacée devant l'artiste qui a perdu sa fonction sociale et religieuse au bénéfice d'une triomphante individualité.

La mise en évidence, dans notre vocabulaire, du danger implicite de projection d'un concept moderne, est utile... les réserves et la nécessaire prudence que nous recommandent M. Conkey et O. Odak, ne justifient pourtant pas le rejet complet du mot "art" qui, depuis des siècles, sert à décrire "l'oeuvre de l'homme par opposition aux créations de la nature" (Larousse) et ceci sans considération de la diversité des formes, ni des contenus, ni des intentions des créateurs. L'art grec, l'art gothique, l'art assyrien, l'art abstrait, l'art primitif ou l'art rupestre sont des "arts", bien que leurs significations et leurs fonctions soient évidemment différentes et variées.

Au nom d'un respect trop théorique et trop simpliste du passé, il serait d'ailleurs erroné de penser que les images préhistoriques n'ont jamais eu d'autre sens et ne se sont jamais prêtées à d'autres usages que ceux qui ont motivé leur création. Elles ont sans doute constitué, au contraire, un réservoir de symboles offert aux générations successives qui les ont investies constamment de sens nouveaux. Les relevés modernes et l'analyse des pigments pariétaux commencent à mettre en évidence la longue utilisation des sanctuaires paléolithiques, la vie des parois ornées marquées par des ajouts et des repeints successifs (Lorblanchet *et alii* 1990. Clottes, Menu et Walter 1990). Toute forme artistique, y compris au Paléolithique, est l'expression d'une vision du monde intégrée au cours historique des cultures. A ce titre, l'art rupestre (ou pariétal) est un témoin qui intéresse en premier lieu l'archéologue. La liberté avec laquelle les groupes traditionnels continuent parfois à accumuler des motifs sur les mêmes parois et réinterprètent les figurations antérieures autorise et justifie en quelque sorte la liberté de notre propre regard. Plus qu'aucun autre, le terme "art" est parfaitement approprié pour désigner ce patrimoine complexe et mouvant que se transmettent les civilisations.

Qui peut nier, enfin, l'impact esthétique, souvent intentionnel, de beaucoup de ces images ou de ces objets ? Quel que soit le message qu'ils contiennent, il serait aussi injurieux pour leurs auteurs paléolithiques de leur refuser le statut d'artiste que de développer, comme on le faisait à la fin du siècle dernier, une théorie affirmant, à l'inverse, que leurs productions étaient purement gratuites. "L'art pour l'art" et le refus de "l'art" sont équivalents dans l'exagération.

Le terme "art" n'est donc nullement réducteur. Il n'implique aucune "interprétation monolithique", aucune classification sémantique "dans une seule et même catégorie" parce qu'il n'implique, en fait, aucune interprétation particulière. Il est simplement l'expression de la liberté et la sensibilité des hommes devant les productions du passé, mais la liberté n'implique pas l'ignorance : en reconnaissant l'historicité des significations et des usages des images rupestres,

nous savons que nous contemplons les oeuvres préhistoriques avec un regard différent de celui de nos lointains prédécesseurs.

Conscients des limites de notre perception, nous continuerons à considérer les images des cavernes comme des "oeuvres d'art". Mais les maîtres de la préhistoire n'ont pas toujours su, eux-mêmes, déjouer les pièges d'une projection de leur goût et de leurs modèles esthétiques sur l'objet de leur étude.

LES "BONS ET LES MAUVAIS"

En 1905, dans son analyse de "La dégénérescence des figures d'animaux en motifs ornementaux à l'époque du Renne" H. Breuil décelait les dérivations schématiques de nombreux motifs zoomorphes. Il montrait comment un dessin de tête de cervidé, de capridé, de cheval ou de bovidé aboutissait à des signes ramifiés de types divers, comment une file de chevaux donnait naissance à une ligne brisée etc... Il soulignait aussi la fréquence du thème de "l'oeil du bison, toujours accompagné de la corne gracieusement incurvée" (p. 110).

Dans cet article remarquable (tiré d'une thèse non publiée) dont le contenu est toujours d'actualité, H. Breuil définissait des tendances fondamentales de l'art paléolithique. Il convient de s'attacher ici à la façon dont il interprétait le phénomène qu'il étudiait. Le texte révèle clairement sa conception de l'art. Le titre montre d'emblée que pour lui la schématisation est une "dégénérescence", que l'art atteint son apogée lorsqu'il copie la nature et qu'il "dégénère" lorsqu'il s'en éloigne. "Les grands artistes, estimait-il, "qui ont gravé et sculpté les chefs-d'oeuvre ou qui les ont peints sur les murailles des cavernes demandaient à une étude directe de la Nature les sujets qu'ils exécutaient avec tant de perfection mais à côté d'eux des copistes plus ou moins expérimentés et informés, copiaient et défiguraient les oeuvres dont ils s'inspiraient, arrivant inconsciemment à modifier profondément, à abolir et parfois inverser, même, le sens d'une figure naturaliste jusqu'à la réduire au misérable rôle d'un motif ornemental" (Breuil 1905 : 120). Il est impensable pour Breuil que le même artiste ait pu tantôt dessiner une figure réaliste et tantôt un motif schématique. Il considérait le schématisme comme une incapacité à atteindre la ressemblance. Selon lui la fonction la plus élevée de l'art était de "copier la nature".

Bien que sa démonstration s'appuyât principalement sur des documents d'art mobilier, il n'hésitait pas à comparer le décor d'une baguette de Laugerie-Basse présentant un motif cornu avec les bisons ramassés du plafond d'Altamira et la prééminence du réalisme dans l'art pariétal lui paraissait tout aussi évidente que dans l'art mobilier. Dans le domaine des figurations proprement pariétales, H. Breuil distinguait deux catégories contemporaines pendant une grande partie de l'Age du Renne : les représentations réalistes groupant les figurations animales et humaines et les représentations schématiques d'objets tels que "tectiformes", "claviformes", "pectiformes", "scalariformes" etc... Ainsi, selon cet auteur, l'ensemble de l'art paléolithique serait dépourvu de motifs non figuratifs. La référence plus ou moins directe au réel serait constante.

Au cours de ses relevés dans les grottes ornées il a le plus souvent choisi les figurations les plus explicites, les plus naturalistes, les "embellissant" parfois selon ses goûts, les complétant quelques fois, ajoutant çà ou là quelques détails, parachevant l'aspect le plus descriptif des

oeuvres qu'il étudiait. Il a fréquemment délaissé toute une catégorie de tracés lui paraissant mal venus ou incompréhensibles, notamment beaucoup de signes simples et ce que les préhistoriens nomment aujourd'hui les "tracés indéterminés". Il existe un "style Breuil..." qui a été dénoncé récemment, en particulier par Ucko (1987) et Bahn et Vertut (1988).

Ayant dominé et monopolisé l'étude de l'art quaternaire pendant un demi-siècle, H. Breuil a exercé une influence fondamentale, à la fois sur la recherche et sur l'opinion publique. Biaisés et incomplets, ses relevés consistaient, en effet, à extraire certains motifs de leur contexte graphique et topographique, à les isoler. En morcelant le dispositif pariétal, ils appelaient déjà, par eux-mêmes, le dénombrement et les statistiques mis en oeuvre par Leroi-Gourhan, puis développés quelque peu par la suite. On comprend la volonté de Max Raphaël de réunir une nouvelle iconographie quaternaire en reprenant les relevés dans l'ensemble des grottes ornées.

D'autre part, c'est à travers la vision personnelle de Breuil que le monde entier a découvert l'origine de l'art. De son oeuvre se dégage la fiction d'un art éternel dont la fonction d'imitation est archéologiquement justifiée. Or le point de vue du Maître de la préhistoire et son insistance trop exclusive sur le réalisme, obnubilant d'autres aspects de l'art paléolithique, peuvent s'expliquer par le contexte dans lequel sa recherche a pris naissance et s'est développée.

Dans la période précédant la première guerre mondiale, période au cours de laquelle le jeune abbé commençait ses travaux, se produit en effet une coïncidence historique troublante entre deux importants phénomènes : d'une part l'émergence de l'art moderne, caractérisée par la multiplication des écoles – impressionnisme, fauvisme, cubisme, puis surtout l'essor de l'art abstrait – et d'autre part la reconnaissance de l'origine paléolithique de l'art et la prise de conscience progressive de sa grandeur à l'Age du Renne. Les historiens de l'art ont montré que le modernisme marque alors une rupture profonde avec la tradition, liée à une révolution du goût. Cette rupture se manifeste de deux façons :

- l'art cesse de véhiculer un message, de remplir une fonction sociale ou religieuse extérieure à lui-même. Il cesse de représenter et d'imiter. Le concept "d'art pur" s'affirme; en l'absence de nécessité absolue, l'art n'est plus considéré, selon l'ancienne formule de Hegel, que comme "quelque chose de superflu" (Hegel 1944, II : 153);
- il devient un jeu formel, "une musique colorée" et s'oriente vers "une intériorité" et le domaine du "spirituel" selon Kandinski. Il refuse l'apparence visuelle. L'artiste moderne cherche plus loin que la simple représentation de ce qu'il voit.

Mais, on le sait, ces bouleversements majeurs n'entraînent pas l'adhésion du public et provoquent de vigoureuses réactions. D'après les propos de Quentin Belle cités par E. Gombrich (1982 : 484), "en 1914, quand on le désignait indifféremment comme cubiste, futuriste ou moderniste, l'artiste post-impressionniste était considéré comme un original ou un charlatan. Les peintres et les sculpteurs que le public connaissait ou admirait étaient farouchement opposés à des innovations radicales. L'argent, l'influence, les commandes, tout était de leur côté".

L'oeuvre de l'abbé Breuil et de ses contemporains semble s'inscrire dans ce courant conservateur qui réagissait contre la trahison de la tradition dans l'art nouveau. Il est même possible que dans le chaos intellectuel du début du siècle, l'enracinement préhistorique de la

fonction d'imitation de l'art, tel que le montrait Breuil, ait pu jouer un rôle dans l'évolution des idées en soutenant indirectement, ou encourageant, le rejet des orientations artistiques nouvelles, perçues comme des aberrations. L'art nouveau semble avoir ignoré l'art paléolithique au moment même où l'on découvrait l'existence de ce dernier... alors qu'il s'est profondément inspiré de l'art primitif qui semblait répondre à son besoin de formes simples et plates, à sa recherche des structures formelles, à sa tendance à la décoration, à son refus du trompe-l'oeil et de la perspective. Tel que le décrivait Breuil, l'art paléolithique apparaissait d'emblée trop savant, trop sophistiqué, en quelque sorte trop proche, parce qu'il reflétait sans doute certaines vues traditionalistes opposées au modernisme.

Pour L.R. Nougier, les figurations paléolithiques sont des "images-réalités", "l'image créée est un morceau du réel. Représenter un animal n'a aucun sens. L'animal convoité que (l'artiste) dessine correspond à une véritable création. Le fait essentiel n'est pas le dessin, c'est l'action de dessiner, de graver, de peindre... l'art préhistorique est essentiellement une action... La contemplation viendra plus tard, par surcroît, avec le raffinement, la perfection du tracé qui aboutissent à l'art véritable" (1966 : 91). Pour cet héritier de Breuil, le réalisme animalier et la magie se confondent pour aboutir progressivement à la ressemblance formelle parfaite marquant l'apogée de l'art quaternaire.

Cependant, selon cet auteur, "les hommes préhistoriques ne sont pas tous de grands artistes! Ainsi sur un total de plus de 1300 figures animalières, 200 d'entre elles se révèlent d'une qualité esthétique remarquable, soit une pourcentage "esthétique" de 15 %. Les pourcentages calculés par espèces donnent des valeurs voisines. Pour les chevaux, pour les bisons, les pourcentages de qualité esthétique sont de 12 et de 15 %. Des espèces exceptionnelles comme le mammouth ou le rhinocéros, atteignent des pourcentages de qualité également exceptionnelle, 18 et 23 %. Le fait est dû aux représentations de grottes de premier plan, réalisées par de "grands Maîtres de l'Art" (1981 : 77).

Nous avons ici un bel exemple de projection d'un certain goût moderne sur les productions paléolithiques, et sans doute, le type même de point de vue que stigmatisaient les propos de M. Conkey et O.Odak. L.R. Nougier peut fort bien exposer son appréciation personnelle des oeuvres paléolithiques... mais peut-on juger l'art paléolithique de manière absolue, en se référant implicitement à une norme esthétique qui se voudrait éternelle et universelle ? En fait, dans ce jugement, se manifeste seulement la tradition académique de la "perfection naturaliste" qui marque toute l'histoire de la recherche sur l'art quaternaire depuis les travaux de Breuil.

A. Laming-Emperaire affirmait, elle aussi, que l'art quaternaire est "essentiellement animalier" et "essentiellement réaliste" (1962 : 27) et elle n'accordait pas une place très importante aux signes qu'elle considérait comme des motifs "schématiques" (1962 : 32), ce qui témoignait de la croyance à l'omniprésence du réel dans la création paléolithique puisque "la schématisation est une simple interprétation du visible" (Lorblanchet 1977).

Le point de vue de Leroi-Gourhan, en rupture partielle avec la "tradition naturaliste", marque, par contre, un tournant dans notre perception de l'art de l'Age du Renne, lorsqu'il déclare en 1965 : "Tout a été dit sur la vérité naturaliste des figures paléolithiques; beaucoup reste peut-être à dire sur le fait que ce sont des symboles et non des portraits et peut-être en définitive sur le fait que la vérité naturaliste est loin d'y être fondamentale" (p. 155). Il insiste beaucoup sur

le caractère symbolique et conventionnel de cet art : "Les Paléolithiques, comme tous les artistes de chaque époque, voyaient avec les yeux de leur société, c'est-à-dire reproduisaient, non pas des animaux, mais des images d'animaux" (p. 153). Plus qu'aucun de ses prédécesseurs, il s'est attaché à l'étude des signes. Il a accordé une place essentielle à ces motifs géométriques, souvent discrets et non naturalistes, ou du moins non "clairement" naturalistes. Nous le verrons plus loin, sur certains points, notamment dans sa conception des "contours inachevés" et de l'évolution de l'art paléolithique, l'approche de Leroi-Gourhan s'inscrit encore dans l'héritage de Breuil. Il a, malgré tout, donné de l'art paléolithique une image bien différente de celle de son prédécesseur.

E. Gombrich rappelle que "l'exigence de reproduire la nature est le fait d'une tradition, non celui d'une nécessité interne. La constance de cette exigence à travers l'histoire de l'art, de Giotto aux impressionnistes, n'implique pas qu'il soit de l'essence ou du devoir de l'art d'imiter le monde réel" (1982 : 473). Contraire à celle que propageait l'abbé Breuil, l'idée que le réalisme ne correspond pas à la "nature" de l'art, mais qu'il fait simplement partie des traditions en perpétuelle modification constituant une trame historique, a fait son chemin au cours du 20^e siècle et s'est imposée naturellement à l'esprit des préhistoriens. A l'affirmation de Leroi-Gourhan selon laquelle dans l'art des cavernes, "la vérité naturaliste est loin d'être fondamentale" correspond celle, plus générale, de E. Gombrich : "Il n'est pas du devoir de l'art d'imiter le monde réel." Cette évolution, liée notamment à l'essor de l'art abstrait, a permis aux successeurs de Breuil de découvrir de nouveaux aspects de l'art paléolithique.

L'HYPERRÉALISME DES FIGURATIONS FÉMININES

Les plus récentes études s'accordaient pour reconnaître le caractère généralement conventionnel et stylisé des figurations féminines paléolithiques. Pour Leroi-Gourhan ces figures et figurines obéissent à des règles strictes. Ce sont parfois "des défis au bon sens anatomique"... ce sont "des oeuvres d'art et non des pièces d'anatomie" (1971 : 480).

Selon H. Delporte, il s'agit également le plus souvent de "représentations stylisées du corps humain", dont la variabilité répond "à l'existence de canons esthétiques, psychologiques ou autres", à des "normes figuratives" différentes selon les "époques, les régions et les ethnies du Paléolithique supérieur" (1979 : 270-73).

Dans une thèse très documentée et une série d'articles, le Dr J.P. Duhard, renouant avec la "tradition réaliste", vient de reprendre l'étude des quelques 130 figurations féminines paléolithiques (1987, 1988, 1989, 1990). Il insiste sur leur réalisme qui serait à la fois "sexuel, kinésique, physiologique, sociologique et pathologique" (1989). Selon lui, beaucoup d'exemplaires présentent une morphologie si précise qu'elle révèle "l'âge du sujet et son histoire physiologique". Les régions "pelvi-abdominale et mammaire", souvent mises en exergue, montrent que 70 % des figurations gravettiennes sont des représentations de femmes enceintes. L'adiposité des corps est aussi très fréquente (stéatopygie postérieure ou étalée, stéatocoxie, stéatotranchantérie). Elle paraît associée à la gravidité. Si la vulve n'est pas toujours figurée, les seins, par contre, le sont très souvent. La plupart des sujets représentés sont des "femmes-pares, ayant eu au moins une grossesse et allaité probablement longtemps », ainsi que l'indique en particulier la "forme ptosée" de leurs seins (1990). En se fondant sur une analyse des postures et "du langage du corps féminin", l'auteur interprète certains motifs comme des représentations

d'accouplement ou comme des scènes de parturition (4 cas, à Grimaldi, Laussel, Tursac, et La Marche). La "gestuelle des membres supérieurs" lui semble "parlante". La position de la main sur le ventre s'explique par le fait que, "pendant la grossesse, la femme porte fréquemment la main sur le ventre", ce geste serait, à lui seul, un indice de gravidité. Le même auteur décèle sur certains visages paléolithiques, l'existence d'une "mimique faciale", de "jeux de physionomie" exprimant la colère, le plaisir ou la douleur... (1989, 1990).

Sur certains points, ses opinions et ses analyses approfondies font indiscutablement autorité, notamment lorsqu'il confirme la diversité des images féminines paléolithiques, invitant à exclure toute hypothèse de "motivation univoque", comme certains l'avaient déjà noté, notamment H. Delporte. Son point de vue de gynécologue lui permet de confirmer définitivement qu'un certain nombre de ces représentations sont liées à la fonction de reproduction. Je crois cependant que, contrairement à Leroi-Gourhan, le Dr. Duhard oublie souvent que ces figurations sont "des oeuvres d'art" et qu'il a une conception exagérée de leur réalisme. Quelques exemples tenteront de le montrer (fig. 1-4).

Selon lui, la femme en bas-relief de Laussel, dite "Vénus de Berlin" est un "exemple assez remarquable d'animation du corps, où l'attitude hanchée est particulièrement bien rendue avec, du côté gauche de l'appui, un pli soulignant le dépôt coxal des graisses et, de l'autre, une hanche élargie et plus haute, entraînant dans son mouvement le pli inguinal, à la fois plus long et plus haut, par rapport au pli opposé" (1989 : 475). En réalité la précision anatomique que le Dr. Duhard distingue dans les contours de cette femme est contredite par l'aspect général plutôt sommaire du bas-relief. La légère différence de tracé des deux hanches n'est pas forcément plus significative que la différence de forme des seins – le sein droit est rond et le gauche allongé et difforme – que la hauteur trop courte du torse ou que l'absence totale de modelé et l'amincissement progressif du bras droit, qui ne correspondent à rien de réel, le coude étant d'ailleurs mal placé. Le manque de précision est souligné encore par le fait que l'objet courbe que le personnage tient à la main n'est pas identifiable (fig. 3). Ainsi, dans l'ensemble des caractères morphologiques présentés par le sujet, le Dr. Duhard choisit ceux qui correspondent à son idée de l'animation et du réalisme figuratif et il néglige les autres.

Sa lecture des femmes du plafond du Pech-Merle paraît également discutable. "La première et la troisième" – écrit-il – "sont comparables : fort segment pelvi-crural, abdomen saillant dans sa partie basse et sillon hypogastrique accusé, traduisant un certain degré de ptose. Chez la seconde le massif pelvi-crural est moins volumineux, l'abdomen saillant dans sa partie moyenne, ombilicale, sans indication du sillon hypogastrique; mais il faut tenir compte du déplacement des organes intra-abdominaux – en raison de la flexion plus marquée du tronc – qui pourrait effacer un éventuel pli hypogastrique... la forme des seins, très étirés, sans être d'un volume excessif évoque naturellement un passé de nourrice" (1989 : 257). Ce texte appelle trois commentaires :

a. Le contexte topographique

Les femmes sont placées sur un plafond; elle peuvent donc être vues sous des angles différents. Les déformations dues à la parallaxe modifient légèrement la vision des courbes ventrales en étirant les silhouettes. Lorsque l'on regarde ces femmes du fond du couloir, sous les blocs, la première apparaît légèrement penchée en avant, la seconde presque assise et la troisième renversée en arrière; par contre, observées du sommet du bloc où s'est tenu l'artiste, elles se

présentent de dos. La posture "penchée en avant" est donc possible pour l'une d'entre elles, selon un angle de vue bien particulier, mais ce n'est pas une règle absolue. D'autres postures sont également perceptibles.

D'autre part, position et posture des figurations ont été commandées par celle de l'artiste, ainsi que j'ai pu m'en rendre compte en effectuant les relevés et en me plaçant à l'endroit où il se trouvait. Les motifs figuratifs, mammouths et femmes, sont situés exactement à l'aplomb du rebord du bloc où se dressait le dessinateur. C'est la topographie des lieux qui a dicté la construction du panneau et l'orientation même des figures (Lorblanchet 1988) (fig. 1-2).

b. Postures et formes des seins

La forme des seins n'est sans doute pas significative; on ne peut pas prendre prétexte du fait qu'ils semblent "pendre" sous le thorax pour affirmer que ces femmes sont "penchées en avant", car il est clair que le graphisme de leur poitrine, comme de l'ensemble de leur contour répond à un "réalisme intellectuel" – pour employer une formule de Luquet – et non à un "réalisme visuel". Lorsqu'ils sont figurés par paire, l'intention est de montrer les deux seins à la fois en les séparant, soit en les plaçant l'un au-dessus de l'autre (femme b), soit en leur donnant des orientations divergentes (femme a), pour qu'ils ne se recouvrent que partiellement et demeurent tous deux visibles, ce qui dans les deux cas est irréaliste. Les pattes des animaux, notamment des mammouths du même panneau, ont subi le même traitement. Tout dans les silhouettes indique que ces motifs féminins sont stylisés et symboliques : l'absence de têtes et de pieds sur deux d'entre eux, les bras réduits à des moignons, ainsi que le dessin incomplet et sommaire du pied de la première figure. Il est probable que le développement mammaire n'est qu'une accentuation graphique exaltant symboliquement la féminité. Comme les animaux et les autres motifs du panneau, les femmes du plafond du Pech-Merle flottent librement dans l'espace : elles n'ont d'autre orientation que celle qui fut imposée par le lieu et les conditions de leur exécution. Nous nous éloignons du réalisme anatomique auquel voudrait croire le Dr. Duhard lorsqu'il s'efforce de mettre en évidence un prétendu "pli hypogastrique" impliquant une posture à demi redressée de ces dames.

c. Problèmes de relevés

Les relevés de l'abbé Lemozi utilisés par le Dr. Duhard manquent de précision : mes relevés effectués en 1985 montrent que les ventres féminins possèdent une courbe plus régulière que ne l'avait enregistré Lemozi, comparable à celle de beaucoup d'animaux, notamment des chevaux de la même grotte, et qu'ils ne présentent donc pas de repli évoquant une "ptose", du moins dans son état pleinement caractérisé (fig. 2).

Bien d'autres interprétations du Dr. Duhard sont difficiles à accepter parce qu'elles ne tiennent pas suffisamment compte des contraintes d'exécution des oeuvres qu'il étudie. Sur la figurine du Courbet récemment découverte il diagnostique une "gravidité" en se fondant sur la proéminence du ventre et la gestuelle des membres supérieurs (1989 : 181). Cette petite figurine (25 mm de haut) a été découpée dans un galet plat "aux formes initialement évocatrices". Elle a été façonnée par plans successifs à l'aide d'un outil tranchant dont on peut voir les traces sous forme d'entailles et de "raclages" qui lui donnent un contour anguleux, presque géométrique, "les volumes anatomiques étant simplifiés à l'extrême" (Ladrier 1990). La proéminence du ventre

qu'invoque le Dr. Duhard pour conclure à une grossesse – opinion partagée par E. Ladier, auteur de la découverte – pourrait être simplement due au découpage de la silhouette dans le galet plat, découpage réalisé par une série d'encoches et de biseaux étagés. Ainsi les séparations des seins et du ventre, et du ventre et des cuisses sont obtenues par deux encoches profondes qui délimitent, dans l'intervalle, un ventre minuscule haut de 4 mm (fig. 3). Son contour légèrement angulaire semble lié au procédé de fabrication et au style général de la figurine, plutôt qu'à une intention délibérée de représenter un état de grossesse. E. Ladier interprète de façon convaincante la masse en avant du thorax comme deux seins stylisés, – non des bras comme le pense le Dr. Duhard –, leur forme légèrement pointue s'expliquant encore par la minceur du support. Par sa stylisation géométrique la statuette du Courbet, datée du Magdalénien moyen, annonce les figurines de Petersfels et les profils de Gönnersdorf-Lalinde caractéristiques du Magdalénien supérieur.

Avec la même ignorance des contraintes d'exécution, le Dr. Duhard interprète les formes de la Vénus de Montpazier dans lesquelles il voit “une anomalie du bassin osseux entraînant une dystocie d'accouchement et réalisant une viciation pelvienne” (1989 : 587). J. Clottes et E. Cerou avaient pourtant montré comment la statuette avait été sculptée dans un nodule naturel de limonite, “sans nul doute choisi parce qu'il possédait déjà une ressemblance morphologique avec le sujet représenté” (1970 : 437)... “la forme d'origine du minerai a été en grande partie respectée... le sujet est très cambré, mais cela résulte surtout de la dépression (naturelle) qui a donné son aspect particulier à la statuette, dessinant la cambrure lombaire et esquissant le départ des fesses, de ce fait très basses”... “le ventre apparaît légèrement décentré vers la gauche, en raison d'un méplat naturel qui occupe tout le côté droit du ventre et se poursuit sur la cuisse droite” *op. cit.*... Il semblerait donc que cette “viciation pelvienne” soit plutôt d'origine géologique.

Les deux cas d'accouplements humains que le Dr Duhard considère comme certains sont à mon avis simplement possibles. La plaquette de la Marche n° 39 porte un enchevêtrement de traits dans lequel le Dr. Pales discerne avec toute la prudence qu'exige un tel motif : “deux personnages affrontés jusqu'à la fusion des corps... sans préjuger de la nature de cette fusion” (1976). Pourtant le Dr. Duhard interprète ce dessin ambigu comme “un coït frontal, vu de profil et note que, dans cette position “pour que les organes génitaux externes soient visibles il faut un artifice de dessin” (1989 : 500). Dans le chevelu de traits, il serait facile, en effet, d'en choisir quelques uns pouvant évoquer un étrange phallus, mais il s'agirait d'un choix. L'interprétation de ce rébus comme coït est inévitablement empreinte de subjectivité (fig. 4).

Quant au couple de la plaquette d'Enlène décrit comme un accouplement à la fois par R. Bégouën, J. Clottes, J.P. Giraud, F. Rouzaud (1982-84) et par le Dr. Duhard, nous nous contenterons de faire observer qu'il est difficile de savoir s'il s'agit d'un homme et d'une femme étant donné “l'absence de tout caractère sexuel déterminant”, selon l'aveu même des fouilleurs d'Enlène (1982 : 108) (fig. 4). La présence des traits orientés vers l'avant, sur la tête de l'un des personnages, ne suffit guère pour certifier qu'il s'agit de cheveux et la présence de cheveux ne suffit guère pour certifier qu'il s'agit bien d'une femme. L'absence de figuration des sexes, ne peut être invoquée au nom d'un réalisme visuel impliquant leur dissimulation dans un coït de profil, puisque ces créatures stylisées aux têtes conventionnelles en museau n'ont rien de “photographique”. L'absence des sexes n'est pas plus significative que celle des bras chez l'un d'eux. Le gros ventre de ce dernier individu aurait même pu étayer son identification comme

femme puisque J.P. Duhard classe les “gros ventres” parmi “les critères de présomption du sexe féminin” (1989 : 38) et que le Dr. Pales avait identifié le corps ventru comme étant celui d'une femme (1976 : 118). Considérer le gros ventre comme moins “féminin” que les “cheveux” est une option personnelle. L'interprétation comme coït est possible mais le style elliptique de ces figures suscite inévitablement l'imagination; d'autres lectures seraient sans doute possibles. Le Dr. Duhard note que Bégouën et Clottes “voient sortir une langue entre deux grosses lèvres” de l'un des protagonistes et, trouvant dans ce détail une confirmation du coït, il se pose aussitôt la question : “Est-ce une mimique d'extase ou de souffrance ?” (1990 : 250). Cette interrogation rappelle que la lecture de beaucoup de figurations paléolithiques, plus schématiques et conventionnelles que clairement réalistes, comporte une projection de nos propres sentiments.

En dépit de leur caractère technique, je ne suis donc pas convaincu par toutes les interprétations du Dr Duhard. Certaines d'entre elles ne sont pas sans évoquer l'érotisme un peu forcé de D. Guthrie allant jusqu'à comparer les femmes de Gönnersdorf aux modèles dénudés de *Play-Boy*... C'est à des comparaisons-projections de ce dernier type que s'appliquait sans doute la juste remarque de P. Bahn et J. Vertut dénonçant “le point de vue “macho” de certains chercheurs sur l'art paléolithique et leur préoccupation pour la chasse, la bagarre et les filles” ! (1989 : 165).

Le parti pris réaliste peut donc interpréter à sa manière d'innombrables motifs, y compris les plus schématiques et les plus stylisés. Certains tracés paléolithiques résistent cependant à toute interprétation de ce genre.

LE “PARASITISME GRAPHIQUE”

Comme l'art mobilier, l'art des cavernes présente à côté de panneaux de motifs figuratifs explicites et réalistes, un vaste ensemble de tracés, extrêmement diversifiés, enchevêtrés, superposés, de lecture difficile qui ont été rarement correctement étudiés ou relevés. L'intérêt que les chercheurs portent à ces tracés a considérablement évolué au cours des dernières décennies, cette évolution traduisant un changement profond dans leur perception de l'art quaternaire.

Breuil les a fréquemment négligés, par exemple à Altamira, Gargas etc...— en les nommant “traits parasites” ou les caractérisant d'une formule lapidaire : “fouillis de traits”, “entrelacs”, “arabesques”. Dans son effort pour mettre constamment en évidence les “belles” images réalistes, il méprise ces dessins. Ses relevés consistent à extraire de l'ensemble orné les motifs les plus figuratifs et les mieux structurés. Un tel choix démontre la subjectivité et la partialité de son point de vue.

Il accorde en outre à ces tracés une valeur chronologique, en les considérant comme les premiers balbutiements de l'art paléolithique. Il les interprète comme les manifestations d'une absence de maîtrise graphique, d'une incapacité originelle à représenter correctement le réel. Nous retrouvons là le parti pris de la “perfection réaliste” qui caractérise l'ensemble de son oeuvre et a exercé une influence durable sur la recherche qui tente aujourd'hui de s'en dégager. Les descriptions successives du chevelu de gravures enchevêtrées couvrant les parois de l'Abside de Lascaux illustrent clairement l'évolution du regard porté sur l'art pariétal.

En 1952, H. Breuil voit dans cette coupole “une véritable toile d'araignée de grafittes de toutes dimensions...; appartenant à des taureaux, chevaux et cerfs...” Il ajoute : “Leur relevé demanderait certainement plusieurs années et il se peut que durant cette opération, l'on découvre des figures d'autres animaux” (p. 131). Ainsi, dans ces réseaux de traits, il ne distingue que les motifs animaliers naturalistes; il réduit ces nappes de traits à leurs éléments figuratifs, en ignorant ce qui les accompagne. Il semble mettre en doute l'utilité même de leur relevé (“il se peut...”) qui ne peut avoir d'autre but que la recherche et l'extraction de motifs identifiables.

En 1965, puis en 1971, A. Leroi-Gourhan, ne connaissant pas encore les détails des relevés effectués par A. Glory, interprète les tracés de l'Abside comme des “contours inachevés”. Cette notion marque déjà une évolution importante par rapport au mépris de Breuil pour ce qu'il nommait les “traits parasites”. Les “contours inachevés” ont maintenant une place dans le sanctuaire, ils correspondent à une sorte de rebut inorganisé, intégré à une place théoriquement constante dans l'architecture générale du dispositif.

Il souligne “le contraste entre les majestueuses peintures d'animaux et de signes” des autres parties de Lascaux et “l'Abside où se pressent les unes sur les autres des centaines de petites figures gravées, mêlées de signes barbelés, de comètes, d'ovales, de séries de traits parallèles, dans un désordre en apparence complet”. Le point de vue est bien différent de celui de Breuil puisque la diversité des tracés est mise en évidence... mais le “désordre” subsiste et il est attribué à une accumulation anarchique des motifs au cours du temps : “Les grattages ont dû débiter très tôt et se prolonger dans cette zone, pendant toute la vie du sanctuaire” (1971 : 127).

En 1979, D. Vialou étudiait et publiait les relevés de l'Abside effectués par A. Glory. En se fondant sur ces relevés, il établissait les premiers et seuls décomptes précis de cet ensemble. Il concluait : “La distribution spatiale de toutes ces figures est relativement claire, homogène, également répartie.” (p. 296). L'évolution est très nette par rapport à l'impression de confusion ressentie par Leroi-Gourhan, antérieurement à la publication des relevés. D. Vialou poursuivait : “Il serait tentant, à la lecture des inventaires de l'Abside, d'écarter les 35 % de bribes, lignes, fragments... *inutiles*, et de ne considérer que les 701 figures figuratives ou même les 403 clairement définies : ce serait se méprendre comme on le ferait pour l'étude d'un sol archéologique en écartant les produits de débitage, les vestiges non manufacturés... des outils *typologisés*; ce serait méconnaître l'association graphique la plus absolue d'un dessin animal, d'un signe avec son environnement *topo-graphique*. Les parois de l'Abside témoignent d'une complexité décorative chargée de sens : chaque élément est graphiquement constitutif” (1979 : 296).

En 1984, A. Leroi-Gourhan se demande “quelle peut avoir été la signification de cette débauche de figures emmêlées, comme si, pendant un temps très long, des générations de fidèles avaient placé une trace sensible de leur fréquentation. Cette situation correspondrait à la structure de nombreux sanctuaires de tous les temps : une iconographie, oeuvre d'artistes en possession des techniques permettant l'exécution du décor mythique et une iconographie votive dont les exécutants sont les fidèles eux-mêmes; mais ce n'est pas le cas ici : l'exécution des quelques mille soixante-treize unités graphiques (décompte D. Vialou) est l'oeuvre des mêmes hommes que l'exécution du décor général. La qualité des figures, la constance du style inclinent vers un autre modèle... doit-on admettre que de nombreux siècles ont vu occasionnellement les hommes pénétrer dans la caverne et procéder à une rénovation de certaines parties; ce schéma rend compte

des nombreux repeints ou regravés; rectifications des ramures, des yeux, des sabots : ces corrections vont en faveur de la fréquentation périodique" (1984 : 195).

Cette rétrospective, concernant un des ensembles les plus caractéristiques de l'art des cavernes, montre que les tracés baptisés aujourd'hui "tracés indéterminés", se sont imposés avec une force grandissante dans les études d'art pariétal, atténuant quelque peu la prééminence du "réalisme". La réalisation de relevés détaillés a favorisé cette évolution. Ce type de tracés existe en fait, sous des formes diverses, dans presque tous les sites ornés et dans tous les secteurs des cavités.

Il est cependant intéressant de noter que subsiste encore parfois un point de vue dépréciatif, perceptible dans les écrits les plus récents utilisant des vocables péjoratifs tels que "bribes inutiles", "débauche de traits"... et dans l'hypothèse, un instant envisagée par Leroi-Gourhan selon laquelle il y aurait deux ensembles différents à Lascaux, les grands motifs réalistes, oeuvres de Maîtres, et les "débauches de traits gravés" ou "contours inachevés" de l'Abside, oeuvres de "fidèles". Il n'est pas interdit de voir dans cette hypothèse un des derniers avatars du vieux parti-pris du "naturalisme triomphant", de la vieille théorie des "copistes" et des "maîtres" proposée dès 1905 par H. Breuil.

Avec la volonté d'intégrer pleinement les "tracés indéterminés" dans la création paléolithique, j'ai tenté, de mon côté, de mettre en évidence "la continuité graphique de l'art paléolithique". Il m'a semblé que l'on pouvait, en général, percevoir dans ces ensembles de traits "une sorte de placenta cosmique, un magma originel où tous les êtres vivants et imaginaires se confondent en des jeux formels... mais l'imbrication de tous ces graphismes, structurés ou non, est telle que l'on y perçoit une intention... les éléments figuratifs se dégagent du magma informe... (1986 : 9) "avec leur potentiel d'images latentes, ces tracés pourraient avoir une fonction particulière dans la grotte, ils pourraient être liés à une mythologie de la création" (1988).

La systématique du réalisme chère à beaucoup de chercheurs peut orienter également leur conception de l'évolution de l'art paléolithique.

LES "PROGRÈS ARTISTIQUES" DE L'HOMO SAPIENS

En 1935, H. Breuil dessine le schéma d'une évolution de l'art des cavernes en deux cycles successifs dont les stades se répètent à peu près identiques, à des millénaires d'intervalle (Breuil 1935).

Les deux cycles – "Aurignaco-Périgordien" et "Solutréo-Magdalénien" – montrent, en effet, le même processus d'élaboration progressive des tracés, d'abord linéaires et partiels puis plus épais, "baveux", se complétant par des remplissages en teinte unique, d'abord plate puis modelée, conduisant enfin à la bichromie et même, dans le second cycle, à la polychromie qui marquerait l'apogée de l'art quaternaire. Le système de Breuil se caractérise donc par un processus de complexification des formes qui se serait répété deux fois au cours du Paléolithique supérieur sur une durée estimée alors à 40.000 ans et que se serait prolongé par un déclin marqué par la miniaturisation des figures et leur réduction progressive en motifs géométriques.

Un autre aspect de la chronologie de cet auteur concerne l'évolution de la perspective. Ce qu'il nomme la "perspective tordue" ou "semi-tordue", c'est-à-dire le rabattement des encornures et des sabots, vus de face sur un corps de profil, propre à la phase ancienne, se serait progressivement corrigé pour parvenir à la perspective normale en fin d'évolution.

Trente ans plus tard, alors que l'invention des méthodes de datation par le radiocarbone avait réduit l'estimation de la durée de l'art des cavernes à 15 ou 20 millénaires, et à 25 ou 30 millénaires celle de l'art mobilier, A. Leroi-Gourhan proposait un schéma évolutif en quatre styles (I à IV) s'enchaînant harmonieusement, bien qu'il puisse y avoir quelques légers chevauchements dans l'espace et le temps. L'évolution serait caractérisée dans son ensemble par la persistance d'une lignée de formes géométriques : les signes, variant dans leur morphologie d'une période à l'autre et d'une région à l'autre, et par le naturalisme grandissant des motifs figuratifs (Leroi-Gourhan 1965). A travers ces quatre styles existe une continuité, une longue progression sur des millénaires conduisant à la fin de l'ère glaciaire à une chute d'autant plus brutale qu'elle est unique puisque l'évolution ne comporte aucun temps mort, aucun retour en arrière.

A l'exception du caractère double ou unique du parcours stylistique, les systèmes de Leroi-Gourhan et de Breuil sont semblables sur l'essentiel. Ils conçoivent tous deux l'évolution de l'art de l'Age du Renne, et de l'art des cavernes en particulier, comme une montée vers le naturalisme figuratif. Dans l'optique de Leroi-Gourhan, comme dans celle de Breuil, les graphismes "synthétiques" des débuts, utilisant une perspective biangulaire (ou "perspective tordue" de Breuil), conduisent progressivement aux graphismes "analytiques" magdaléniens accédant à une "perspective uniangulaire".

Ces deux systèmes évolutifs linéaires, reposent sur l'idée implicite d'un progrès paléolithique d'ordre psychologique et biologique, marqué par le passage du réalisme intellectuel au réalisme visuel. Ils illustrent, en réalité, une conception traditionnelle de l'Histoire de l'art vue comme le développement ordonné des styles selon la métaphore biologique de la croissance, de la maturité et du vieillissement. Ce modèle en vigueur depuis le 16ème siècle, de Vasari à Hegel, perçoit une "voie toute tracée dans l'histoire qui mène l'art vers un classicisme universel" (Belting 1990). C'est précisément ce progrès permettant à la norme de s'accomplir, que met en évidence Leroi-Gourhan quand il souligne avec insistance "le caractère *normal*, progressif, de l'histoire de l'art le plus primitif qu'on connaisse, avec un début, un apogée long et nuancé, une fin..." (1971 : 74), ou encore quand il compare l'évolution de l'art paléolithique à "une trajectoire où l'on discerne une période d'enfance très longue, un apogée qui dure à peu près cinq mille ans et une chute qui s'accélère brusquement" et qu'il conclut : "C'est donc un "*art normal* ", qui a eu ses gaucheries de débutant, sa maturité, ses tours de mains, un académisme même..." (1971 : 159). La "norme" vers laquelle tendrait l'évolution de l'art quaternaire serait, ici encore, la perfection naturaliste, celle des peintures polychromes d'Altamira et du vérisme des gravures de Teyjat.

Comme, au début du siècle, la théorie du "tout naturaliste" de l'art de l'Age du Renne, le point de vue de Breuil, de Leroi-Gourhan et d'autres préhistoriens sur l'évolution stylistique s'explique par une projection des idées de leur temps sur l'objet de leur étude. Ce modèle traditionnel du développement à sens unique des formes artistiques à travers l'histoire, a été contesté à plusieurs reprises. Certains auteurs ont mis l'accent sur l'ancrage historique et culturel

de l'art offrant une "vision historique du monde" (Hegel 1835); à l'inverse, d'autres ont souligné la "vie des formes" évoluant selon leur propre loi, l'autonomie de l'art, sa nature intemporelle permettant la contemporanéité de styles différents se développant indépendamment les uns des autres – comme le concevaient Wölfflin (1915), L. Hourticq (1927) ou H. Focillon (1939) – des formes précoces pouvant être contemporaines de formes tardives.

La logique d'un développement linéaire dans l'histoire de l'art, aussi bien aux cours des périodes historiques que paléolithiques figure parmi les concepts qui sont maintenant fondamentalement remis en question. D'ailleurs l'idée de "trajectoire" stylistique au cours des 20 à 40 millénaires du Paléolithique supérieur selon Breuil et Leroi-Gourhan est sans doute une forme "d'illusion d'optique créée par l'éloignement qui nous fait croire que les siècles précédents ont connu une succession de styles plus ordonnée" (Gombrich 1982 : 482). "Lorsque l'on ne dispose pas d'un ensemble de références chronologiques suffisamment large, de jalons permettant d'ordonner la séquence des styles, l'on en vient involontairement à organiser les données selon un schéma théorique et selon un ordre purement "logique" (ou du moins conforme à *notre* logique). Sans doute le linéarisme du schéma proposé par Leroi-Gourhan est-il lié au fait que seulement 25 sites sur les 275 grottes ornées paléolithiques actuellement connues en Europe et Russie ont été datés objectivement en chronologie absolue" (Lorblanchet 1989 : 149). Ainsi théorie et idées préconçues compensent l'indigence des faits.

Les critiques actuelles

Depuis plusieurs années, quelques auteurs ont exprimé leurs désaccords avec certains aspects du système chronologique de Leroi-Gourhan. Ces critiques ponctuelles qui ne s'attaquaient pas à l'ensemble de l'édifice ont été dernièrement relayées par des remises en question beaucoup plus fondamentales. C'est ainsi qu'un rejet partiel des chronologies de H. Breuil et A. Leroi-Gourhan ont amené J. Combier (1984) et M. Lorblanchet (1972-1988b) à ne distinguer que deux "phases stylistiques" ou "deux groupes de grottes ornées" dans la vallée du Rhône et le Quercy, l'un centré sur le Solutréen et l'autre sur le Magdalénien moyen-supérieur.

B. et G. Delluc ont mis en doute la distinction des styles I et II de A. Leroi-Gourhan en soulignant l'imbrication des caractères de chacun de ces deux styles qui doivent être fondus dans une "phase primitive" couvrant à la fois l'Aurignacien et le Périgordien. Cet amalgame est d'autant plus souhaitable que, conformément aux données stratigraphiques, les gravures pariétales de Pair non Pair peuvent être datées aussi bien de l'Aurignacien que du Périgordien alors que l'existence d'un art pariétal bien caractérisé dès l'Aurignacien paraît s'imposer de plus en plus (Delluc 1984). A. Roussot et B. et G. Delluc (1986 et 1989) considérant que le système de Leroi-Gourhan n'embrasse pas la totalité de l'évolution artistique du Pléistocène, notamment les découvertes récentes de la fin du Magdalénien, proposent de rajouter un "style V", coiffant d'un chapeau neuf un édifice bien tremblant.

J. Clottes a montré que la distinction des stades "ancien" et "récent" du style IV ne reposait pas sur des arguments solides. Il a souligné "la persistance au Magdalénien final d'un art pariétal que nous ne sommes pas en mesure de reconnaître et que nous confondons peut-être avec ce que nous attribuons communément au Magdalénien moyen" (Clottes 1990 : 546). D. Vialou met lui aussi en question la séparation entre les styles III et IV. Selon lui, la seule distinction justifiable entre III et IV reposerait sur l'apparition dans l'art mobilier d'une abondance d'instruments et

outils décorés (1989)... Cela est-il suffisant ?" Pour cet auteur également la séparation entre le style IV ancien et le IV récent paraît insaisissable (1989).

Aujourd'hui les critiques semblent se renforcer et s'étendre à l'ensemble du système de Leroi-Gourhan dont la rigidité est dénoncée. P.J. Ucko stigmatisant ce qu'il nomme "les débuts illusoires dans l'étude de la tradition artistique", a pertinemment montré que les conventions stylistiques n'ont pas toujours une signification chronologique, qu'elles peuvent n'être en fait qu'un langage social, constituant "un système de transmission de messages différentiels". La figure dite silhouettée a pu être en réalité un style en soi, lisible d'une façon donnée par "ceux qui savaient, et connaissaient la convention en question" (p. 69). Cet auteur qui insiste sur "le caractère indatable de la majorité du matériel iconographique" (p. 59) considère que "toute explication unitaire" dans les domaines chronologiques ou sémantiques... est "inadéquante pour couvrir la pléthore de conventions stylistiques et la variété d'images est de techniques qui couvrent les quelques 25.000 ans que nous appelons Paléolithique supérieur..." (1987 : p. 70).

En 1986, rappelant la présence de véritables portraits humains, très sophistiqués, dès l'origine de l'art de l'Age du Renne (Vénus XV de Dolni Vestonice et tête de Brassempouy) puis sporadiquement en des lieux et des temps divers du domaine paléolithique, je considérai que "la logique de transformation des formes humaines au long du quaternaire était liée à l'évolution de l'idée de l'homme, qu'elle était avant tout conceptuelle et non chronologique. La plupart des images humaines d'aspect grotesque sont "des anti-portraits traduisant un refus volontaire de l'humain" et non des tracés maladroits que l'on pourrait qualifier "d'archaïques" (1986 : 36). Un an plus tard je concluais "il faut admettre que les facteurs sociaux, culturels ou religieux interviennent généralement dans l'évolution des styles et que la chronologie artistique de l'Age du Renne, pas davantage que celle de ses industries, ne peut être seulement fondée sur un développement continu de l'esprit humain impliquant une forme de "progrès". Le naturalisme "grandissant" tout au long des millénaires, tel que le conçoit A. Leroi-Gourhan (ou H. Breuil), est une notion surprenante..." (Lorblanchet 1988a : 291).

P. Bahn et J. Vertut remarquent que "beaucoup de choses demeurent incertaines dans le domaine de la chronologie". Rejetant vigoureusement les conceptions linéaires de Leroi-Gourhan et Breuil, ils estiment que "l'art paléolithique n'eût pas un seul commencement et un seul apogée mais il en eût plusieurs... pendant les 20.000 ou 25.000 ans de sa durée il y eût des périodes de stagnation, de progrès et même de régression avec tout un jeu d'influences, d'innovations, d'expérimentations et de découvertes. L'évolution de cet art a dû être buissonnante avec des rameaux parallèles, une masse de pousses divergentes et des floraisons locales soudaines." (Bahn et Vertut 1988 : 65 et 66).

Par contre, un an auparavant D. Vialou avait jugé satisfaisant que "dans chacune des grandes civilisations occidentales créatrices d'art pariétal, un ou plusieurs sites pariétaux aient été datés par contact stratigraphique direct". Il avait conclu un chapitre de son livre sur "l'art des cavernes" en remarquant : "Au total et malgré quelques incertitudes et des inconnues, il apparaît que l'art paléolithique mobilier et pariétal est un des mieux connus dans sa chronologie et des mieux datés de tous les arts préhistoriques" (1987 : 60 et 61). Le débat actuel sur la chronologie de l'art pariétal paléolithique se situe donc entre l'optimisme de D. Vialou, illustrant une traditionnelle conviction française dans ce domaine de la recherche, et le pessimisme de P.J. Ucko, P. Bahn et

J. Vertut caractéristique du scepticisme anglo-américain, qui tend à nier toute chronologie de l'art des chasseurs de rennes.

CONCLUSIONS

Mon propos n'était pas, bien entendu, de nier tout élément réaliste et naturaliste dans l'art paléolithique. Il était simplement de tenter de montrer comment la recherche sur l'art quaternaire s'inscrit dans une époque, comment elle est influencée par les idées en vogue et la conception que les chercheurs se font de l'art en général. L'étude de l'art préhistorique n'est pas un domaine secret, hors du temps; comme toute science, elle participe, consciemment ou non, au mouvement du savoir dont elle bénéficie et qu'elle alimente tout à la fois. Il était utile de dénoncer le "parti-pris du réalisme" fondé sur l'idée d'une duplication constante du réel dans l'art paléolithique. Héritée de Breuil, cette idée correspond, en fait, à un éclairage ponctuel placé par une conception esthétique traditionnelle sur un des aspects parmi d'autres de l'art paléolithique. Malgré l'apport de Leroi-Gourhan, et bien qu'une image nouvelle de l'art paléolithique émerge peu à peu des travaux en cours, ce point de vue réducteur oriente encore trop de recherches actuelles.

Il convenait de discuter aussi une méthode très commune de description et d'analyse des oeuvres pariétales et mobilières illustrant l'a priori réaliste. La méthode consiste à comparer un motif – le plus souvent animalier ou humain – avec ce que nous connaissons aujourd'hui de ce qu'il représente, pour ne mettre finalement en relief que les éléments de son graphisme rappelant étroitement ceux du vivant... en ignorant tous les autres caractères du tracé. Une telle comparaison morphologique est certes indispensable, mais elle devrait être moins partielle, plus objective, et devrait surtout s'éclairer d'une comparaison stylistique de figures du même type afin de mieux séparer ce qui appartient au *dessin* et ce qui revient au *sujet* lui-même, car ces motifs sont des "images" et non des "réalités". A cette méthode comparative traditionnelle, empreinte d'une certaine naïveté, manque le recul esthétique, c'est-à-dire la prise en compte de tous les éléments ayant contribué à la réalisation de l'oeuvre étudiée, non seulement les manières de l'artiste, les conventions sociales qu'il utilise, mais également les contraintes du matériau, les conditions d'exécution et de perception du motif.

Notre époque est peu favorable aux traditions : il est probable que les ruptures et les interrogations qui accompagnent l'art du vingtième siècle, profitent également à l'étude de l'art ancien, notamment de l'art paléolithique. Dans le climat d'effervescence artistique qui marque notre temps, les questions sur les principes universels de la création, la fonction, la naissance et l'évolution de l'art, prennent un relief particulier. Elles dynamisent l'étude de l'art paléolithique qui peut sans doute, à son tour, éclairer et féconder l'histoire de l'art tout entière. Elles alimentent, par exemple, la réflexion du préhistorien sur l'origine de l'art (situer et décrire cette origine dépend de la définition précise qu'il donne au phénomène artistique)... Elles lui permettent de découvrir des aspects inédits de l'art des cavernes et des objets, en l'avertissant toutefois que les éclairages nouveaux ne sont jamais définitifs, puisque chaque génération de chercheurs perçoit sous un éclairage différent le sujet qu'elle étudie. Les seuls progrès indiscutables de la recherche actuelle sont réalisés dans le domaine des relevés et de la chronologie, c'est-à-dire dans l'étude des contextes archéologiques et la datation directe des pigments pariétaux. Le regard nouveau que les préhistoriens posent sur l'art paléolithique confirme et souligne, au-delà du travail de Leroi-Gourhan, la diversité des styles et l'importance

du non figuratif, notamment des “tracés indéterminés”. Dans certaines cavités, à côté des animaux, des humains et des signes, ils représentent, en effet, du tiers à la moitié des unités graphiques. Dès 1977, P. J. Ucko remarquait : “L’art paléolithique n’est pas exclusivement un art animalier; il est tout autant un art géométrique ou un art non figuratif” (1977 : 9).

D’autre part, même lorsque ses figurations sont descriptives, l’artiste paléolithique ne “copie” jamais le réel, il l’interprète. Lorsque la tendance naturaliste se manifeste, elle s’exprime selon des conventions variées. L’art paléolithique n’est pas un art libre et personnel. C’est un art social régi par des règles strictes. Au même moment, on ne construit pas un “beau” bison de la même façon dans les Pyrénées et en Périgord... Pas davantage que celui du schématisme et du géométrisme mésolithiques, l’épanouissement du naturalisme magdalénien dans certains secteurs de France et d’Espagne, ne peut être considéré comme le témoignage d’un développement du cerveau humain. La logique d’un déroulement linéaire de l’histoire de l’art paléolithique a fait son temps. Le concept d’un processus évolutif unique est en passe d’être abandonné. Une grande méfiance s’affirme même envers toute chronologie stylistique, tandis que s’amorce un nouveau et nécessaire “retour aux documents”. Nous savons maintenant que de nombreux facteurs ont influencé l’évolution des styles mais l’influence exacte du temps, sans doute variable selon les régions, nous échappe encore. Elle ne peut être totalement négligée, mais elle fut sans doute moins importante, moins directe et moins exclusive que ne le concevait Leroi-Gourhan dans un système qui apparaît aujourd’hui trop simple.

Nous devons désormais mettre en valeur l’ancrage historique et culturel de l’art paléolithique. A l’hypothèse de l’indépendance de l’art se développant hors de l’emprise directe des cultures, il faut substituer celle d’un art profondément enraciné, au contraire, dans le cours des civilisations de l’Age du Renne, car toute oeuvre d’art est, en premier lieu, un produit historique. Il est probable que l’insuffisance des bases chronologiques objectives expliquait la séparation des évolutions de l’art et des industries dans les théories de Breuil et Leroi-Gourhan. Retrouvant ses racines, l’art paléolithique doit descendre de son Olympe pour investir terroirs, habitats et sanctuaires. Cesser d’être conçu comme un tout homogène évoluant dans une seule direction, se diversifier et s’humaniser. Nous retrouverons ainsi, non seulement un art magdalénien, par exemple, mais un art magdalénien poitevin ou ardéchois, un art de la vallée de l’Anglin, ou de la Beune, un art de tel ou tel site, chacun de ces ensembles évoluant selon sa logique propre en une succession de phases et de moments non programmés à l’avance. D. Vialou a déjà mis en lumière le “particularisme de chaque dispositif pariétal”; selon lui “il n’y a pas de “stéréotype de la grotte ornée” (Vialou 1987). Il apparaît même que plusieurs sanctuaires de types différents peuvent être abrités par la même grotte, les uns secrets et réservés, les autres ouverts à un public, tels que le grand plafond (ouvert) et le diverticule des masques (retiré) dans la caverne d’Altamira (Echegaray 1986).

Grâce aux progrès des relevés, des fouilles et des datations de pigments, la chronologie stylistique traditionnelle sera ainsi bientôt remplacée par une “succession de sociétés et de cultures possédant chacune leur répertoire de formes et de fonctions artistiques” (Belting 1989 : 47). Privilégiant des facteurs sociaux, nous préférons interpréter par exemple la tendance naturaliste dans le Magdalénien moyen des régions occidentales, comme “un changement de fonction de l’art... traditionnellement secret et ésotérique et sans doute réservé à un petit nombre d’initiés, l’art pariétal se serait alors ouvert plus largement à des communautés plus nombreuses” (Lorblanchet 1989).

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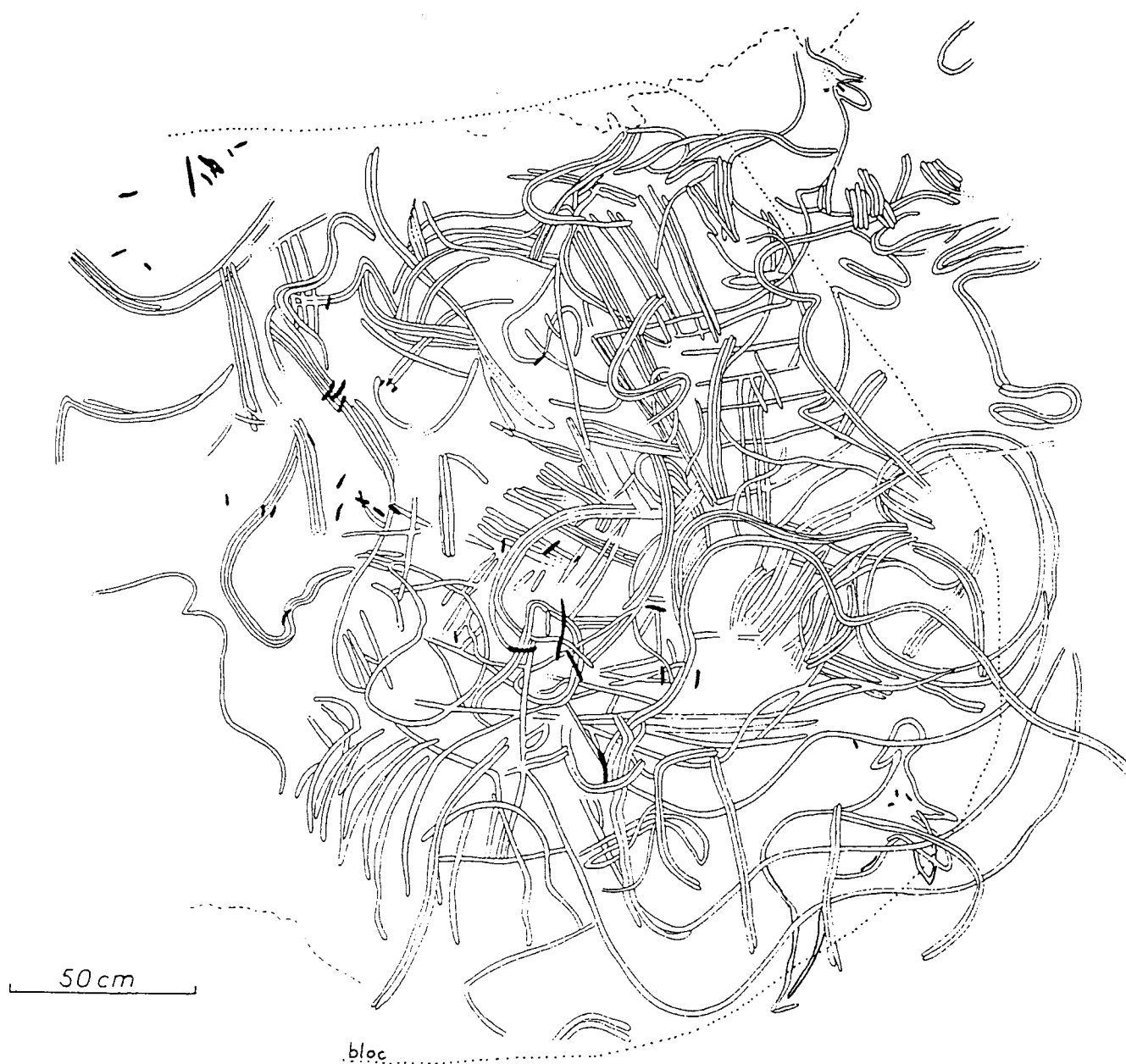


Figure 1 : Relevé par M. Lorblanchet du groupe A du Plafond des Hiéroglyphes de Pech-Merle (Cabrerets-lot), avec projection du contour du bloc sur lequel se tenait l'artiste.

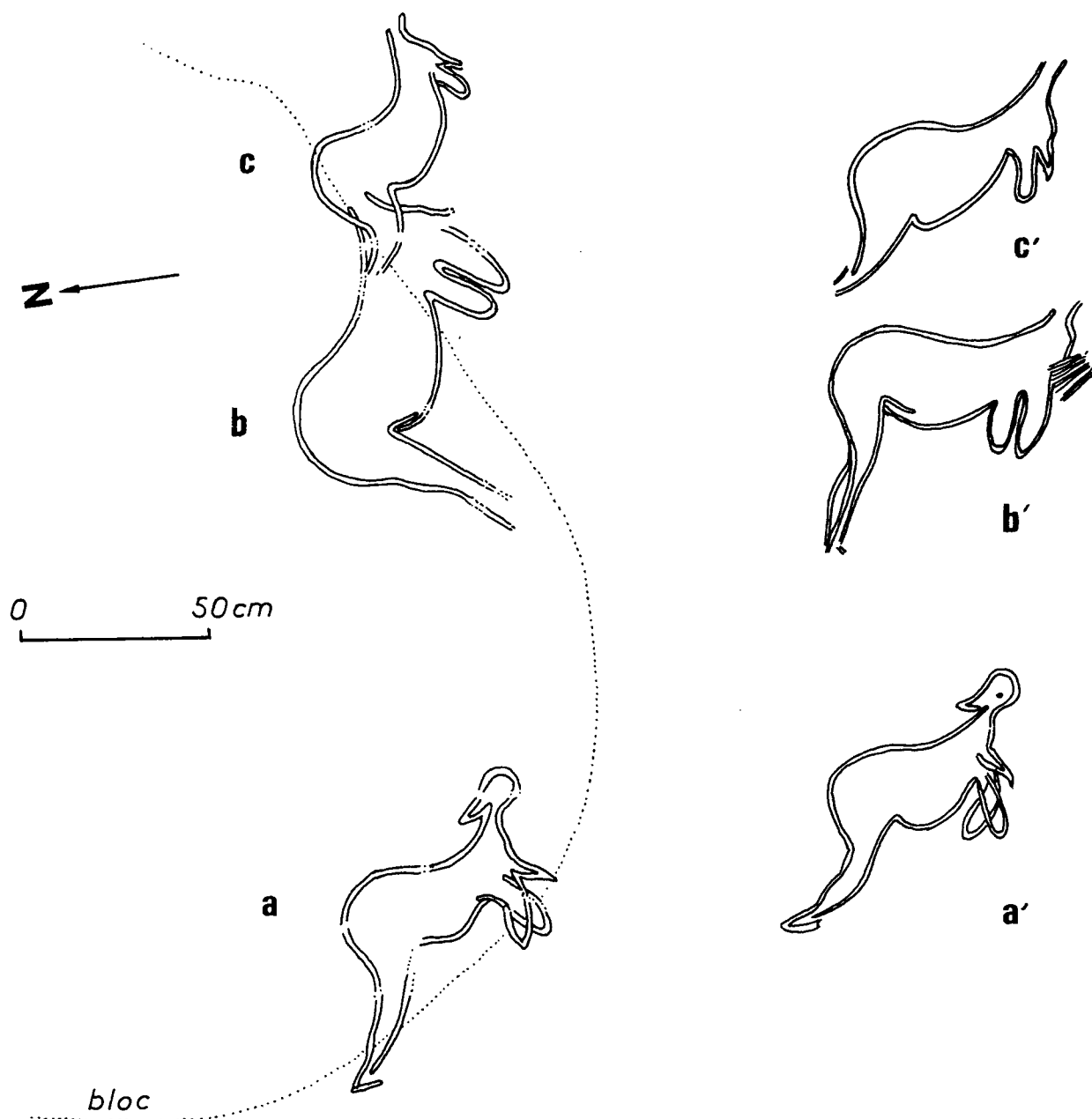


Figure 2 : A droite, détail du relevé des femmes (a, b, c) du groupe A par M. Lorblanchet avec projection du contour du bloc où se tenait l'artiste. La position des femmes est respectée. A droite, détail des relevés de A. Lemozi (a', b', c') repositionnés par J.P. Duhard 1989a : 254). La "ptose" ventrale paraît plus marquée sur les relevés de Lemozi.

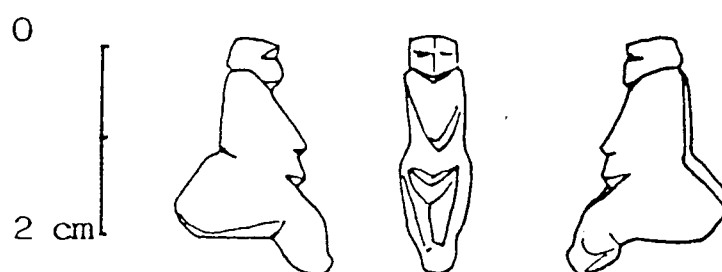
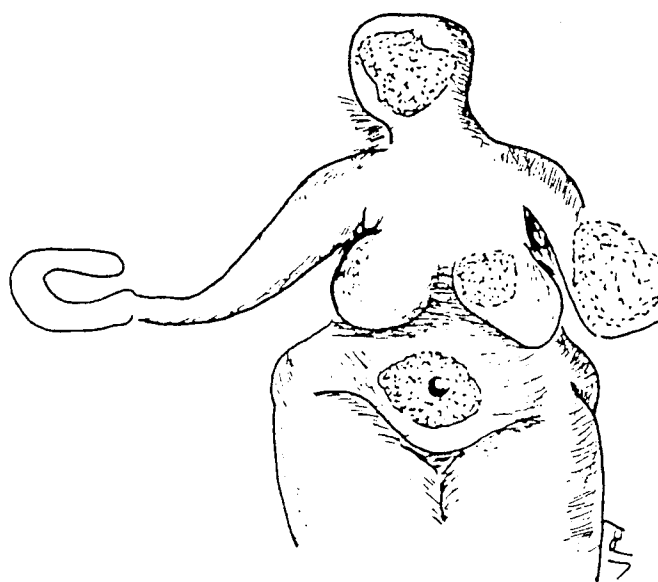


Figure 3 : En haut : "Vénus de Berlin" (Laussel. Relevé Duhard, 1989a : 475). En bas : "Vénus du Courbet" (Penne, Tam; dessin Ladier 1990).

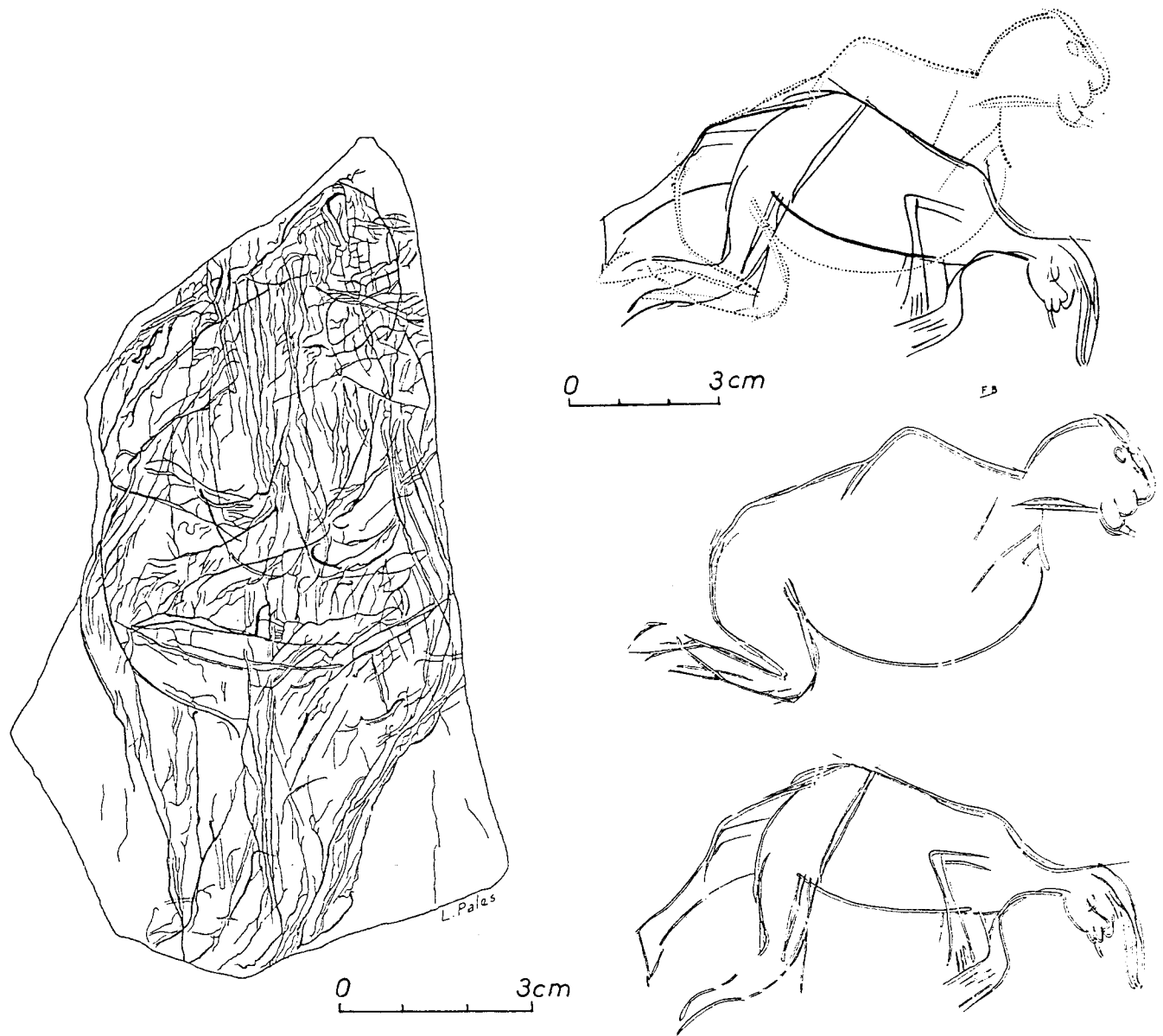


Figure 4 : Accouplements humains paléolithiques ? A gauche, La Marche (Relevé Pales 1976, planche 89, observation n° 39). A droite détail de la plaquette d'Enlène (Bégouën, Clottes, Giraud, Rouzard 1984, relevé F. Briois) avec dissociation des personnages.

SUBJECTIVITY AND THE RECORDING OF PALAEOLITHIC CAVE ART

by

P.-J. UCKO *

ABSTRACT

This chapter aims to review some of the changes in approach which have taken place in the study of palaeolithic parietal art over the past twenty years. In so doing, it highlights the nature of strategies involved in such studies, and uses the Spanish decorated cave of Hornos de la Peña as its primary example. The review reveals a general lack of precision in much work on parietal art, stemming from indecision about the nature of any "art system of communication" which may have been involved. In this context, therefore, it may not be surprising to find much interpretation still concerned with species identification on the one hand and with distributions of motifs in the caves on the other. It concludes by emphasizing the intimate relationship between the investigator's own preconceptions and the research methodology adopted.

PREAMBLE AND HISTORY

It is more than twenty years ago that, together with Andrée Rosenfeld, I first reviewed palaeolithic cave art studies (Ucko and Rosenfeld 1967) and more than ten years ago that I tried again (Ucko 1977 : 8). Since then there have been many important, exciting and new discoveries, as well as advances in certain photographic and other methods of recording. However, when one turns to interpretation, most publications appear to assume that we all know what we are doing and that we have good theoretical grounds for so doing. I doubt that we really do and I shall argue here that even recording techniques and conservation activities *must* depend on a solid framework of method and theory.

I directed research in the cave of Hornos de la Peña (Santander) from 1971 till 1973 (Ucko 1987). At the time the team-work approach adopted was very unusual; I was assisted by an outstanding team : Ms E. Dowman (then of the Institute of Archaeology, London), Dr R. Layton (Department of Anthropology, University of Durham), Dr A. Rosenfeld (Department of Anthropology, University College London) and Professor M. Weaver (Heritage Foundation of Canada). Also unusual were at least some of the stated aims of the project; I quote them in summary from an application for funds :

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Speleological Investigations

1. to establish the entrance(s) of the cave;
2. to establish the relationship between original floor levels and access to the cave;
3. to establish the relationship between the original floor levels and the specific art works;
4. to establish the exact plan of the cave (and exploration of any extensions to the cave discovered during the investigation).

Technological Investigations

1. to investigate the conditions relating to differential preservation within particular areas of the cave;
2. to investigate the conditions relating to differential preservation of different parts of a particular representation;
3. to determine and establish objective criteria for the differentiation of natural rock fissures from artificially engraved lines.

Surveying Investigations

1. accurate recording of all the representations by tracing;
2. accurate recording of all the representations by photography;
3. construction of a model of the cave to enable research in laboratory;
4. to establish the relationship between cave features and art works;
5. to establish the relationship between cave areas and art work (Leroi-Gourhan 1965; Ucko and Rosenfeld 1967);
6. to establish the relationship between floor heights and art localities;
7. to establish the relationship between cave entrances and art works;
8. to define the criterion of visibility of art works.

Primitive Art Investigations

1. to consider the spatial distribution of art works within the cave;
2. to consider the spatial distribution between art works;
3. to consider the role of orientation within the art works;
4. to identify and consider the role of superpositioning within the art works;
5. to consider the role of relative size of representations;
6. to consider the role of style and techniques within the art works;
7. to consider the variable of date within the art works;
8. to carry out experiments of lighting within the cave – the replication of palaeolithic lighting with special reference to shadows and natural features in the cave;
9. to assess the relative visibility of representations within the cave;
10. to define the visual context of each representation;
11. to define what constitutes “related” representations or lines;
12. to consider the iconographic content of the art;
13. to carry out a comparative analysis of the content and style of the Hornos de la Peña representations.

The reason that we chose these items to record was *because we thought we knew what the relevant questions were which had to be answered*. It is relatively obvious that specific points which are considered worth recording at any particular moment are always those which we think are not obviated by the "known", but it is exactly what we consider "known" which so often turns out to be based on the most fragile of information. This approach is in sharp contrast to, for example, an art-historical or literary approach to the study of human endeavour, where basic methodologies and aspirations of the research are apparently unchanging and the assumed "clues" to understanding are taken to be relatively inviolable. Most of the work on palaeolithic (at least cave) art at the time of the Hornos enquiries sat firmly within the comfortable assumptions of art historical analysis rather than being concerned with the formulation of research aims and methods appropriate to the study of palaeolithic art as a system of communication and information sharing.

As we shall see below this situation has greatly changed over the past ten years in the works of a limited number of researchers, but these are exceptional in recognising that methodology cannot be usefully pursued outside of the framework of a research design and research strategy.

It can, of course, be argued, as it certainly was by us during our work at Hornos de la Peña, that one cannot begin to formulate hypotheses and research strategies until one knows what is actually contained within a cave. But our experiences in Hornos de la Peña show how difficult this apparently easy demand can be. Let me therefore present a few examples of specific problems :

1. Del Rio (1906 : 50) reported that he thought he saw the representation of a bison within a maze of superimposed lines in an area of the cave (Fig. 1, his C) where we found nothing remotely resembling a bison. The presence or absence of such a representation significantly alters any distributional interpretation along the lines of Leroi-Gourhan. It is important to realise that Breuil also did not record any bison in the area described by Del Rio. Quite apart from the implications for a Leroi-Gourhan-type analysis, this remark by Del Rio raises the question as to whether or not representations were clearer in 1906 than in 1911, let alone nowadays.

Del Rio gives much more detail (1906 : 51-2) about a representation of a deer, including its measurements and line qualities and the fact that it was carved in low relief, supposedly in the entrance area. The entrance area has been subjected both to natural flaking and to human and animal interference. We were quite unable to match the line qualities given by Del Rio with anything still surviving in the entrance and, as far as our research is concerned, there is no deer in the entrance chamber. One might simply suspect physical destruction of a representation or a mistaken identity by Del Rio either of various amorphous lines (Ucko 1987 : pl. 1) or of an "apparent" horse (Ucko 1987 : Pl. 2, Fig. 3 a-d) published by Breuil, were it not for the fact that both Del Rio and Breuil had *misplaced* the representation of a deer (Ucko 1987 : Pl. 4, Fig. 4) *which is in fact deep down in the cave*, and placed it in the entrance region (at 2 in Fig. 2). Unfortunately, the whole question of location in this cave remains one of the obscurities of previous publications, for a cervid (Del Rio, Breuil, and Sierra 1911 : Fig. 93, 2; Ucko 1987 : Figs 7a-b) was published as being in Chamber D (Fig. 3, Location 2) but is in fact in Chamber E (Fig. 3, Location 42) and this reflects the unreliability of the existing published data base.

These examples show how difficult it is to correct the data base when a cave has been left open to the ravages of one and all. As will be seen later, it is known from Hornos de la Peña that people scratched representations on the walls at different times and local tradition has it that the "amorphous" lines in our area 44 were known to Del Rio but not published by Breuil. After extensive research (including washing of the wall which appeared to have been smeared with a mud wash), we concluded that this area contained lines which were of a different quality from recent initials and that there was no reason against assuming that some, at least, of the lines could be of considerable antiquity, whether made by man or other beast.

2. It was only after the completion of our lengthy period of research and our extensive exploration of both old and newly-discovered galleries, and despite the fact that the cave had been relatively well published by Breuil, that we realised that we might have missed one whole category of palaeolithic art even if it had been present in the cave. Because of our preconceptions in the early 70s about palaeolithic art, we concentrated on walls and roofs (the floors having been totally disturbed by previous visitors) and we were "programmed" to look for representations of a size known to us from previous experiences of palaeolithic art. It was only after this period that Lorblanchet introduced us to the minute and finely-engraved representations of Ste-Eulalie (Lorblanchet *et al* 1973). There is little doubt that despite three seasons work, we could well have missed this sort of representation.

3. One zoomorphic representation stands out at least as clearly as any other in Hornos de la Peña; a representation whose precise species is hard to specify (Ucko 1987 : Fig. 14, Pl. 9). This figure had not been published or mentioned previously in secondary descriptive accounts of the cave contents. Despite intensive investigation, we remain unclear as to its date. The patination of some lines might suggest that it is not extremely recent and some lines run over old stalagmite but, even so, we have not determined any way to assign it (or part of it) to any of the specific periods represented by archaeological debris in the cave. The patination of the various lines making up this representation are not similar, and it is indeed conceivable that lines, such as at least one "leg", were added to the basic shape at different times. It is equally possible that all of the lines, except those of the "head" are of relatively recent origins (and see Delluc and Delluc 1971 for similar problems in other caves).

4. On a particular crumbling stalagmitic column far inside the cave of Hornos de la Peña is a complex of lines which preoccupied us throughout our research. The lines, located on a decaying brown stalagmite, were first thought to represent a possible engraving of a bird, an identification later tentatively switched to a deer with head stretched upwards (Ucko 1987 : Fig. 5). Our interest in this "representation" was heightened in the context of the "missing deer" identified by Del Rio, and as described above. We carried out extensive binocular examination of the quality of the lines which revealed that they were not all cut to the same depth into the stalagmite and also that many of them petered out and did not join each other, as first thought on the basis of a superficial visual impression. This analysis was then followed up by moulding and we have concluded that there is nothing definite to indicate that all except possibly one of the lines are not natural.

These examples serve to reinforce Clottes' recent (1989) warning about the unreliable nature of many palaeolithic cave art inventories and the inevitable harmful consequences to interpretation that such inaccuracy implies. Much of what follows is the result of failure by our team to achieve

some of the proposed research aims set out above at the beginning of this paper, together with an attempt to learn from our experiences.

THE POINT AT ISSUE

The fundamental question is "Why do we choose to record what we do when dealing with palaeolithic cave art?" There are those who claim that we can relax because our techniques are being "depersonalised" (Delluc, Delluc, *et al* 1981 : 16-19); they are not, and they can never so become.

Let me try to put this fundamental point more explicitly and with some of its complex implications :

a) A tracing by Breuil, a photograph by Vertut, a sketch by Lemozi, or a replica by Ligot and Lehanier, are all the result of subjective choice, albeit in varying degrees and involving different technical choices, but all are subjective and partial (distorting) records only (see Rigaud *et al* 1984 for many examples). This is strikingly true even of Lascaux II which is so often acclaimed as a "faithful copy" whereas, in fact, it only seeks to reproduce about a third of the cave's depth and only part of its content (Dauvois and Vézian 1979); it is self-evidently ludicrous to claim that the replication is so good that it is almost possible to claim that "Lascaux has reopened" (Delluc and Delluc 1984 : 84).

Let us now turn to our work at Hornos de la Peña and compare it to Breuil's. We found that his drawing (Ucko 1987 : Fig 29a) of "a (nother) complete primitive ox... is... roughly drawn; it has only one straight horn pointing forwards and one leg per pair, no tail and an arrow stuck in its flank" (Breuil 1952 : 353), omitted the middle section of the line which traverses the supposed animal body (Ucko 1987 : Fig. 29b), thus transforming the upper end into a horn, the lower end into an arrow. In fact it is true that from certain angles in the cave the central part of this line is almost invisible [and does not show up in photographs (Pl. 1)] but from other angles it is quite clear (Pl. 2.). Breuil was clearly influenced in his perception (i.e. "choice") of this representation by his pre-supposition that palaeolithic art was to do with sympathetic hunting magic, as, of course, he and others have been in their identification of "breath" and "tongues" on various animal representations (Breuil 1952 : 339; Del Rio, Breuil and Sierra 1911 : 102).

It is vital to recognise that this "biasing" of the record is evident not only at the level of a specified particular image, in terms of which line is chosen to be included or omitted from a particular trace or sketch (or given especial lighting or shadow in a photograph), but just as much at the more general contextual level of how much of a "surround" is included in a trace or sketch or photograph.

In Hornos de la Peña, during experiments with fat lamps, we found that it was more difficult to distinguish between natural and artificial lines on the cave walls, that deep engraved lines or cracks stood out well, that many features of the cave surrounds combined in the flickering light of the burning fat to give an impression of movement to the lines on the walls, and that shadows from protrusions normally came from the top, as the lamps cast no downward light and lamps were always placed on the floor when activities were carried out.

Despite important discussions over the last ten years by Lorblanchet for Marcenac (Rigaud *et al* 1984 : 34), Sainte-Eulalie (Lorblanchet *et al* 1973), and by Vialou for Aldène (Vialou 1979), about the use of natural rock features, bear scratches, etc., in the “composition” of palaeolithic cave art, there is no general consensus amongst researchers as to what a *comparative* data base of palaeolithic art should specifically consist of – and yet this can be claimed to be one of the essential dilemmas of palaeolithic art studies (Dauvois and Vézian 1979). Is the data base to include only the artificial man-made lines within a cave or also all the possibly “associated” natural features and, if so, what will be the usable definition of “possibly associated” ? At Hornos de la Peña, it is clear that Breuil did not distinguish between the artificial and natural when the “feature” made “anatomical sense” to him, as in the case of eyes and pupils.

Indeed in at least one case we suspect that Breuil’s identification of an ibex (Ucko 1987 : Fig. 28) incorporates an area of probably bat scratches (and see Clot and Cantet 1974 : 82) as its beard (Fig. 11 A-C). If the data base is to include the use of non-man-made items when they are deemed to have formed part of a palaeolithic “composition”, the content of the data base is bound to remain to a large degree subjective and unstandardised. Vialou’s publication of Aldène (Vialou 1979 : 64-69) demonstrates this point excellently with regard to bear scratches, the use of natural lines, and assumed mammoth iconography.

There is a similar lack of precision in works which acclaim advances in photographic techniques, and analyses by, and through, photography. None ask what is the “unit” which they are seeking to analyse or “record” (further discussion below). Similarly, almost all discussions of methods of conservation and how caves should be treated in the context of public tourism all assume that we all know what image, or complex of images, area of a wall, or part of the totality of the cave should be so “conserved” and which parts need not be so treated. An important consequence of this approach is the assumption that we are all agreed on what is important for the public “to be told” and understand about palaeolithic art. The suspicion remains, however, that no one has formulated the question for debate and that conservation (and even recording ?) is usually undertaken as a response to disaster (e.g. Lascaux, Altamira, Niaux, etc.) and choices made simply for pragmatic tourist-based reasons. For example, it is more than likely that if a Niaux II were to be reconstructed (along the lines of Lascaux II) it would certainly be the Salon Noir which was reconstructed simply on grounds of tourist interest and “aesthetic” beauty (and therefore protection) without any overt discussion of what the public *should* be shown of Niaux, of which the Salon Noir is only one small part of a coherent whole (Vialou 1981 : 19). After all it could well be argued that the public should be “educated” to appreciate the existence of what Vialou (1981 : 22) calls “Magdalenian space” and this, it could be claimed, could be much better demonstrated in quite different parts of Niaux. It is heartening, therefore, to find the very recent suggestion (Rouzaud *et al* 1989 : 65) that La Magdeleine cave should be fitted out so that visitors will see the art works at the same physical eye level as did their originators.

There is often a similar lack of precision by those who stress the need for multi-inter-disciplinary team research in caves. Are they talking about the sort of team research as referred to for Pech Merle (Rigaud *et al* 1984 : 35) or la Magdeleine (Rouzaud *et al* 1989), and as carried out at Hornos de la Peña, which in the 1970s consisted of six academics selected for their particular expertise in photography, moulding techniques, excavation experience, research into “primitive art”, etc. together with students to carry out speleological investigations and to assist with recording work ? Or are they in agreement with the despairing words of Pfeiffer, already

criticized by me (Ucko 1984), that "it is time for something more systematic; visits to a set of selected sites by a working group made up of not only an archaeologist and an artist but also of a dancer, psychologist, architect, acoustical engineer, a producer or professor of drama involved in staging and stage effects, and a zoologist with an experience of observing the behaviour of wild animals" ?

As Robert Layton put it in one of the formulations of the Hornos research project (and see Rouzaud *et al* 1989), we need to be very precise about what experiments with different lighting (or sounds) can achieve. On the positive side, the use of different lighting procedures in caves can act as a corrective to simply following the influences of Breuil's published drawings and perceptions. Different lighting conditions may also reveal different lines and emphases from those studied by previous researchers. In our experiments at Hornos de la Peña we tried to capitalize on these questions by dividing students into those who had, and those who had not, previously seen publications of palaeolithic art and each category of student produced sketches of cave wall areas lit respectively by oil or fat lamps, acetylene, and fluorescent tubes. At no point, nor in any way, was the assumption made that we thought that we were looking at the art "through the eyes of palaeolithic man", since what he perceived depended on both physiological development and the results of cultural conditioning, both of which may have been very different from our own; and in this we differ both from the Delluc (1981 : 16) and from Dauvois and Vézian (1979), all of whom appear to believe in the possibility of recreating palaeolithic perspectives (and see Layton 1987 : 211). Candles and acetylene lighting were often used by Breuil, and such lighting conditions can be simulated by us; in addition we have his sketches and tracings. This does not, however, mean that we can ever exactly replicate even his individual perceptions.

Many of the above remarks reflect, I think, the continuing level of unreality in many of the palaeolithic art studies of today. The lack of precision which I have reviewed perhaps could be claimed to coincide with a period of stagnation in palaeolithic art studies were it not for the exceptional studies of the past ten years or so, to which I have already referred above. Despite these exceptional publications, however, it remains true that most practitioners of palaeolithic cave art studies appear to lack clarity in formulating what they think the study of prehistoric art is all about, and therefore there is a lack of precision in assessing what problems (which may be diverse and even heterogeneous but which need nevertheless to be clearly defined) are pertinent and of interest. The lack of public discussion of these basic points of principle in palaeolithic cave art studies is in sharp contrast to the recent burst of sophisticated reviews of other prehistoric rock art studies (Lewis-Williams 1983; Meighan 1981 : 110) and their aims, some of which have taken note of the rather longer-lived and very theoretically oriented investigations of living "primitive" art systems (Merlan 1989). Let me attempt to make some of these dilemmas both explicit in general terms and also pertinent to our previous work in Hornos de la Peña.

Methods of study adopted in the analysis of palaeolithic cave art are guided by the researcher's theoretical orientation. The first priority of any theory must be to determine what are viable questions to ask of palaeolithic art, and which questions, however interesting, cannot be answered. As Layton (1987 : 212-3) has pointed out, the fact that cave art is rich in its varied motifs, and remains where the artist(s) put it suggests that several useful questions should be able to be asked with respect to it; however, not all are equally easily answered.

Let me take, as the first parameter of enquiry, or hypothesis, that the “sets of paintings (or engravings in caves) constitute performances or texts created within (an) artistic system” and that, to a certain extent at least, “the complexity of their structuring is a matter for empirical analysis” (Layton 1987 : 213). This is, in fact a very similar starting point to several other authors (Sauvet, Sauvet and Włodarczyk 1977; Vialou 1982 : 42) and, as has already been seen, it was also our original parameter of research for our investigations at Hornos de la Peña.

We all know that in fact assessing such assumed structures is not easy; (a) differential preservation may have destroyed some of the original corpus; (b) there may have been complementary subsystems which leave no record (e.g. ritual).

b) Classifying depictions as representations of “the same motif” (e.g. “bison”, “claviforms”) is often difficult. It is good to see that Sauvet *et al* (1977 : 548) have made this situation explicit; what they have not stressed is that current practice must of itself result in the arbitrary, and hence subjective, imposition of “categories” of “sign types” onto the data.

The treatment of each cave as a single unit of analysis is also questionable, and less generally recognised; if we take, for example, Leroi-Gourhan’s interpretation of Hornos de la Peña, we find that he suggested that different areas of concentrated engravings stand out both from each other and possibly also from a scatter of less visually spectacular motifs. If such a situation were indeed the case it might reflect complementary cultural themes or it might mean that the motifs were entirely unrelated to each other; in the latter case, any treatment of the cave as a meaningful single unit would surely be unwarranted. The same point has also to be made at a different level of analysis. Long ago Rosenfeld and I argued (1967 : 224) the need for the study of groups of caves as complexes; the Dellucs’ study (Delluc and Delluc 1983) of three decorated caves near Domme raises again, in an acute form, the question whether the distribution of art within the single cave is really the appropriate dimension within which to try to establish regularities and irregularities of art motifs.

At this stage it must be made explicit whether or not we think that we can answer the question, “What was the specific content of the belief system” ? Fascinating though a positive answer to this question would be, it probably is impossible to deduce the belief system because it was the product of a particular, unique cultural trajectory through time. Indeed, in this context comparative surveys of non-palaeolithic rock art do not help for, contrary to the partial résumé of convenient practices produced by the Sauvets (Sauvet, Sauvet and Włodarczyk 1977 : 552), they in fact show a multiplicity of functions which rock art may serve as well as a huge variety of non-sacred “messages” which can be conveyed by art “motifs” (Ucko 1977 : 42; Merlan 1989). Moreover, in the general context of the development of human cultural systems, the specific content seems less important than the structural complexity of the system. I am therefore arguing that, within the particular parameter of art as a communication system, which I have chosen to explore first, the most useful questions to ask in the analysis of palaeolithic art are those concerned with identifying the size of the corpus of motifs which constituted the “vocabulary” of the art, the frequency with which they were used, and any regular patterns in their distribution across a region which may reveal structured rather than random use.

It is easy to see how this approach to the study of palaeolithic cave art leads to significant differences between this article and those conventional discussions which claim the need to be

able to assign animals to subspecies in order to understand the Upper Palaeolithic environments and ecologies. The well known and large literature attempting to assign palaeolithic representations of animals to species and subspecies does so without much success and inevitably leaves several "animal" representations defying such classification (Layton 1987 : 219; Clottes 1989). Recently, Vialou (1982) has shown how more sophisticated attempts to identify animal species even at Niaux have also failed for at least three representations. There are two possibly more important related questions to be asked : first, is this really what a study of palaeolithic art should be concerned with and, second, is a clear differentiation of animals into species and sub-species what the palaeolithic artists were concerned with ? Are we agreed as to our own etic interests, and were the emic aims really to differentiate species and sub-species by certain diagnostic features ? I do not believe that the Sauvets (Sauvet and Sauvet 1979 : 341) are right in simply claiming that palaeolithic artists always kept species-diagnostic features present, even in their simplest works. On the contrary, as Vialou (1979 : 212) and the Dellucs (1974) have shown, many "animal" representations remain species non-diagnostic even when the most objective "tests" (derived from palaeolithic art canons) are applied to them : "Magdalenian art styles... are very diverse. Some are close to zoological reality while others demonstrate stylistic freedom" (Vialou, *ibid.*). In this case, the Sauvets appear to fall within the general trap of seeking to find the sort of evidence with which they can work; the emic is being made to conform to the etic. Vialou's (1979 : 60-63) recognition of a category of "indeterminate" representations at Aldène is refreshing in this context as is the Delluc' (1974 : 37-38) acceptance that E11 at Villars cannot be certainly assigned as either feline or horse. Indeed, are we certain that the investigation of the area between (and possibly overlapping) the emic and etic is really appropriate in this palaeolithic context for the recognition of species-diagnostic animal traits ? (and see Clottes 1989; Lorblanchet 1989; and especially Merlan 1989 : 22-4).

None of these considerations seem to be discussed by those who simply assume that zoological interpretation and classification has some sort of objective significance. In fact we need to remember that some ten years ago a zoologist pointed out, with regard to a group of Australian rock art, that "out of 22 items... my diagnosis was wrong in 15 (and only) superficially right in 7" (Macintosh 1977 : 194). Those studying palaeolithic art with the above approach seem to remain unaware of the point stressed by me (Ucko 1977 : 16) that what is considered to be "zoological observation" is, like the study of palaeolithic art, a matter of subjective choice. The profoundly negative results of an overtly and more or less exclusive zoological approach to palaeolithic art recording and interpretation at a specific level can be noted at Hornos de la Peña with regard to Breuil's subjective comments about the particular *quality* of the representation of an animal hoof (Del Rio, Breuil, Sierra 1911 : 96) and, at a general level, in the way that intentional palaeolithic ambiguity may be being forced into a western scientific straitjacket (and see below).

To continue with Hornos de la Peña (although the same is true of many other caves), Breuil and Saint-Périer (1927 : 146-147) referred to the several finger tracings (Pl. 3) as simple decorative motifs in meander form, and pointed out that they could have been inspired by the form of "serpents"; in other parts of the cave also, Breuil *et al* suggested the interpretation of specific representations (Pl. 4) as possible snakes (Del Rio, Breuil and Sierra 1911 : 96). It would be tempting indeed to try sub-species identification of the possible snakes of Hornos de la Peña but we should ask (a) whether snakes actually existed in the Upper Palaeolithic or not, (b) are we really certain that we can distinguish the intended representation of a snake from other

images which are treated as "signs", and (c) if representations are assumed to be snakes, why not worm representations ?

This same set of concern can be approached from the opposite direction. As Rosenfeld and I have pointed out (Ucko and Rosenfeld 1972), "If a representation cannot be recognised as a known animal, or if the head of a figure cannot be assigned to a particular species, there is no *a priori* reason why it should be thought to represent an anthropomorph with a mask. If the head (or whole body) appears to us to look like a camel or other animal which cannot have existed in Europe according to our present knowledge of fauna in Upper Palaeolithic times there are three possible interpretations which we may adduce : (a) that the resemblance to the camel or etc. is purely coincidental, (b) that our assessment of Pleistocene fauna is incorrect, or (c) that the palaeolithic artists had gained the knowledge of this animal either previously or in other areas" (and see Del Rio, Breuil and Sierra 1911 : 98). When we get to this level of species interpretation it is not entirely clear to me why Breuil's interpretation of certain lines cut in the clay at Hornos de la Peña as a possible "isard" (Ucko 1987, Fig. 8) because it "has only a very small head" and despite "the absence of horns" (Breuil 1952 : 353; Del Rio, Breuil and Sierra 1911 : 93) should be any more correct than a student's view of the same lines with electric light as a seal (Layton *ms*), an interpretation which incorporates a few natural cracks in the clay surface as fat folds behind the head and ignores the small mark which Breuil interpreted as an eye. The same sort of problem, and its consequent influence on the subjective nature of the published record, can be seen in the discussion by Del Rio *et al* (1911 : 96) of the "non-bovid" ears of another representation.

This discussion highlights the continuing naive treatment in most publications of questions of style, whereas a few recent publications have started sophisticated discussion of this concept. Strictly speaking, identifying what individual motifs represent is the step which first leads to the establishment of the "meaning" of the art. The motifs are signifiers and the concepts that they represent are the signified. Most appear to consider this step self-evident, i.e. they consider the representational quality of motifs to be transparently obvious. However, some are much more obvious than others and we would be foolhardy to assume that we have satisfactorily "understood" palaeolithic art style, or to forget that the resemblance between the visual motif and the natural model is never more than partial and is always a matter of convention. Because most of us appear to consider some palaeolithic art representations to be "naturalistic", are we therefore being logical in our apparent assumption that diagnostic zoological features of an animal (species) will be present overall within the art works of palaeolithic peoples ? One thing is indeed clear and that is that if the answer is "no", then a statistical analysis based only on those representations/features which are recognised by us as animals [let alone specific species (subspecies)] may be highly misleading with regard to any statements about the ecology of the period.

To assume that what is "naturalistic" (usually undefined) to our 20th century eyes reflects a concern for anatomical accuracy by artists of another culture (whether contemporaneous or prehistoric) is at least dangerous if not totally naive. The situation is somewhat complex, for the idea of "realism" is derived from the art itself and not from actual knowledge of the prehistoric animals themselves. Thus when people describe a representation as "realistic/naturalistic" they usually mean that the representation is detailed, and detail is, of course, not necessarily

synonymous with realism; nor, of course, should lack of detail be equated with chronological age (Delluc and Delluc 1973 : 205).

Lack of clarity about this whole complex problem in the context of palaeolithic art studies has led to the repetition of all kinds of fanciful and totally unjustified theories and interpretations about humans/anthropomorphs, their role in ritual, and even about the nature of cognitive development in the Ice Age (Vialou 1983 : 96). Some time ago Rosenfeld and I (Ucko and Rosenfeld 1972) tried to put the problems of the correct identification of "anthropomorphs", into two wider contexts ; (1) the anatomical and/or stylistic features which might allow us to accurately recognise the intended anthropomorph, and (2) the implications for the interpretation of the function(s)/meaning(s) of palaeolithic cave art of various possible categories of anthropomorphic representations. It is striking how, until recently (Layton 1987; Lorblanchet 1989 : 8) most people have shied away from the many points of method and principle raised in that review when they have come to discuss and describe their own recent discoveries (whether on La Marche plaquettes, within overall reviews of Aurignacian representations, or in the context of specific cave content). However, the questions posed in 1972 do not simply go away, and there is continued evidence of the subjectivity of many such prior identifications (e.g. Roussot, Frost and Daubisse 1984; Delluc and Delluc 1974 : 30, 52-3; Vialou 1982 : 29), as well as the dubious nature of new ones (Delluc and Delluc *et al* 1981 : 38-39, 71-72).

Even Lorblanchet appears to accept assumptions regarding what is "human" in art, so that he feels that he can, apparently, make decisions about what is "human", what is "animal", and what is non-representational, as well as their respective associations, on the Hieroglyphic ceiling at Pech Merle (Clottes 1983 : Fig. 21, 484-485; but see Clottes 1989 and Lorblanchet 1989). At Hornos de la Peña there are several panels where even Breuil (Del Rio, Breuil and Sierra 1911 : 91, 104) gave up, although the lines on the wall could as well have depicted anthropomorph or bird or animal or "sign" (Ucko 1987 : Fig. 28). Confusion of lines, in the context of Hornos de la Peña anthropomorphs, leads me inevitably to the best known figure of this cave.

Whatever else one may wish to say about the lines in this area of the cave the so-called anthropomorph (Pl. 5) is undoubtedly a very different creature from the one that is normally reproduced in publications (Fig. 5) and described by Breuil (1952) as a "human figure... resembling a monkey... (with)... false tail. This strange person... (whose)... head, with its rather flattened top, is worthy of neanderthal man." Breuil continued his publication "I left out the erect sex, as I was not sure, when I copied it, that it belonged to this figure. A little later I realised that it did belong. Such is the Man of Hornos, the only one in the caves, perhaps the god of this cavern..." (1952 : 355; Del Rio, Breuil and Sierra 1911 : 98).

The last few have shown (Vialou 1979 : 23-24; Delluc and Delluc *et al* 1981 : 37-39; Lorblanchet *et al* 1973 : 30, 270-275) a much more critical awareness of the problems of correctly identifying "chronology" by a study of superpositioning and/or quality of lines. For Airvaux and Pradel (1984 : 213), admittedly speaking about plaquettes, it is normally not possible to establish chronology through a study of superpositioning. Our studies at Hornos concentrated on such analyses (binocular and through mouldings) and would confirm the rarity of being able to say anything definitively. What does seem relatively clear, however, about the "anthropomorph" is that the so-called penis must have been completed after the nearby horse which in turn was finished after the lines were drawn for the body of the "anthropomorph". Even

if the superpositionings represent only a few minutes of difference in time, it is at least interesting (in the same sense that it is interesting to see how the Aldène "circle" was built up (Vialou 1979 : 38-39) to know that the "anthropomorph" was not originally conceived as an ithyphallic individual (if that is what was finally intended) but that the so-called penis was added to the basic body form at a late stage of the engraving process. I am sure that it is not merely coincidence that the moment "anthropomorphs" are under consideration even those researchers who most attempt to be "objective" about palaeolithic cave art begin to adopt subjective terminology. Thus, the Sauvets describe such representations as "ruling" over the engraved animals (Sauvet, Sauvet and Włodarczyk 1977 : 557) and Vialou thinks that "it" "dominates" them (Vialou 1981 : 138) ; none gives any convincing reasons for this interpretation. Continued use of such undefined terms as "anthropomorph", together with subjective statements about their function, by palaeolithic art specialists shows their continuing unconcern about the fundamental characteristics of art as a communication system where the important methodology consists of identifying regularities of motif use. What we term "naturalism" and "schematisation" are more likely to be associated with the nature of the *use* of art in different contexts and with the intentional use of ambiguity of representation rather than with anything else. This statement is based on the findings of several art studies of modern "primitive" art systems (Biebuyck 1977 : 65 ; Layton 1985 ; Layton 1987) and the possible existence of intentionally ambiguous representations in palaeolithic cave art which is beginning to be acknowledged (Layton 1987 ; Ucko 1987; Vialou 1983 : 84; Ucko and Rosenfeld 1972; Delluc and Delluc *et al* 1981 : 49-51).

The continuing (but see Lorblanchet 1989) naive differentiation of animal and human representations into "naturalistic", "realistic", and "other" provides fodder for the next obvious deficiency of many of the approaches to palaeolithic art – namely the continuing assumption that there is indeed a meaningful categorical break between such images and those so conveniently cast aside under the rubric of "signs". If "signs" are not conceptually animals or anthropomorphs, which of them are "naturalistic" and which "schematic"/unfinished/ etc ? What is the definition of a "sign" ? Are *all* "punctuations" "realistic/naturalistic" and, indeed, are they all really one and the same "category" of sign, whether painted or "engraved" (Vialou 1982 : 31-33) ? Finally, what does acceptance of a category of signs imply in terms of our interpretations ? Although Sauvet *et al* (1977 : 555) have discussed some of these points briefly in the context of handprints, general discussion is still lacking.

We need to be explicit about the assumptions that we are carrying into our work of recording and analysis. When we look at animal representations we apparently tolerate a considerable variation in the mode of portrayal; some outline figures seem as readily identifiable as are solid polychromes. We are assuming that the significance of differences in the shape of heads, horns backlines etc., can be appreciated by us because we assume that we know that these are features diagnostic of bison, deer or horse. However, when we look at what are conventionally nowadays classified as "signs" – i.e. where there is little or no "clue", at least to our eyes, of what the representation is supposed to signify – researchers have tended to appeal to supposed typological derivation (e.g. Leroi-Gourhan and Breuil) or cave location (Leroi-Gourhan). For Hornos de la Peña, at least, claims that signs begin or end a figurative unity seem inapplicable and of no help in the recognition either of sign types or so-called cave panels.

The problem with Hornos de la Peña is that we have discovered a large number of apparently non-zoomorphic lines (such as loops (Pl. 6) and circles) and complexes of lines

(Pl. 7 and Ucko 1987 : Pl. 11 in Ucko location 108), but that we do not know how to date them (see below, and also see Delluc and Delluc 1983 : 57). For Breuil there really was no apparent problem for he tended only to see animals, and omitted many areas where there are definite, apparently aged, engraved lines that do *not* look like animals. Sometimes, however, in his account he did note that there were many non-zoomorphic finger tracings which, except in one case, he did not reproduce. Also, where he saw a mass of superimposed lines, Breuil tended to isolate those configurations within them which could be made to resemble animals. In several more recent researches, recording has been extended to include only those non-zoomorphic lines which are supposed to conform to a standardized inventory of repeated "sign" elements. Works such as those of Lorblanchet *et al* 1973 : 310-313, and Vialou (1979 : 45) which give detailed recordings of all apparently non-figurative representations without attempting to fit them into any typology, are still few and have yet to be assimilated into general theoretical discussions. However, Vialou (1982 : 27) has begun to confront the problems.

If we exclude from the contents of Hornos de la Peña all those apparently man-made non-zoomorphic representations which, either because of lack of patination, superficiality of line, "freshness", or obvious character as an initial or name, are not securely or easily datable to the Palaeolithic we are left with items whose distribution in the cave is scattered and their possible "association" with zoo/anthropomorphs various and apparently non-standardised (Fig. 6).

All this, of course, again raises essential questions about aims and methodology of research. What we consider to be "simple" (or the simplest of) motifs are those most difficult to classify and, as I have suggested elsewhere (Ucko 1977 : 9), probably the most likely to have been used (or "invented") most often in different places and at different times, and possibly with varying "significations". As Layton (1987 : 220) asks : (1) Have we the right to consider all rectangular "grids" (Pls. 8, 9) (and see Sieveking 1981 : 327; Lorblanchet *et al* 1973 : 30, 310-313; Roussot-Larroque and Roussot 1973; Sauvet and Sauvet 1983 : Fig. 14) as representative of a single motif-type ? (2) Is it not possible, or even likely, that the "grid" was spontaneously rediscovered at different times during the palaeolithic, a question which Sauvet *et al* (1977 : 548-551) have also been investigating ? (3) Is the palaeolithic "grid" really not a representation of animal/anthropomorph/artefact/plant, but really a separate signifier of a different (more complex) concept ?

We arrive now at the third major set of criticism about the ongoing work of palaeolithic art investigation. Despite the length of time that has elapsed since Leroi-Gourhan's first publications, it is striking that most subsequent publications still seem simply concerned to point out the individual addition to a particular cave record and the reinterpretation of a particular image within a cave, or with criticising his original simplistic interpretation of cave art as an expression of sexual dichotomy, as opposed to really tackling the essential problems of his revolutionary work. Notable exceptions to this observation are the sensitive publications by Lorblanchet *et al* (1973 : 313-317) for Sainte-Eulalie, the Dellucs (1974 : 59-63) for Villars, Vialou (1982) for Niaux, and Rouzaud *et al* (1989) for La Magdeleine. Nevertheless it remains a striking fact that there are no attempts at all to deal with, or discuss, Leroi-Gourhan's more recent and more complex hypotheses (1982); this may be because the details of his analyses are now effectively dismissed by most colleagues who treat them with silence and some of whom are now trying out quite different "schemata" of their own (Vialou 1983 : 38-45).

The real problems at issue for the recording of palaeolithic art are both quite different and much more subtle. Do we accept the assumption that the *number* of representations of a particular image is irrelevant, and that a simple count of presence or absence of image is all that is relevant? Do we accept that *size*, *colour*, or *technique* is irrelevant to either the emic intention or the etic understanding of the palaeolithic art communication system? Do we accept that “*finished*” or “*unfinished*”, “*naturalistic*” or “*schematic*”, placed high or low on a wall or “*hidden behind*” a boulder, a simple presence or absence within an arbitrarily designated “*area*” is sufficient to be the basis of our comprehension of the underlying art system? Leroi-Gourhan’s so-called statistics were attacked by Rosenfeld and myself as soon as his book was first published (1967 : 196-217) and have been commented on and criticised significantly by others also (Parkington 1969). If *any* of the above questions were to be answered in the negative, the assumption of “regularities” in cave distributions might well collapse. Almost no one since Leroi-Gourhan (but see Layton 1987 : 23) appears to be willing to address themselves seriously to this overall question. The major exceptions include Vialou (1983 : 93-94), who does present some discussion of the problem as well as some new “statistical” (= numerical) analysis and who does, for Niaux, make some use of individual “*motifs*” and their colouring (Vialou 1989 : 35), and Rouzaud *et al* (1989) who have recently attempted to infer relationships between individual representations within a cave in a quite new – and minutely detailed – way. Also exceptional are the Sauvets who have attempted (1979 : 341, 346, 349, 352) to tackle these questions, but their decision to treat numbers, size, and technique of representations as at least secondary in importance to what they call the “*theme of the assemblage*” is simply a matter of assertion, not adequately demonstrated by the statistical tests which they adduce on the basis of a somewhat questionable data base.

Nevertheless the Sauvets’ attempt to link their method and findings to linguistic analysis of communication systems is admirable. Furthermore the Sauvets *do* attempt to use the number of individual representations of species to deduce interspecific relations. The problem at issue here is, however, whether it may still be premature to accept the validity of their thematic approach at all.

Let me therefore return again to Hornos de la Peña. By 1911 Breuil had identified 27 discrete configurations, or *motifs*, in the cave (Fig. 2). All but one (meanders at location 23 of Ucko’s survey) of these he identified as animal, part of animal, or anthropomorphic representations (Fig. 7). At the end of our work at Hornos de la Peña we had identified some 140 discrete configurations (Fig. 3). In a few cases we had found additional representations of animals which Breuil would certainly have accepted as palaeolithic, but in most cases the additional configurations were indecipherable and some at least could have dated from a variety of periods. In some instances we question the accuracy of Breuil’s animal identification and details. For example, Breuil *et al* identified “a very rough engraving of the head of a bison reduced to an “*accolade*” line going from the muzzle to the top of the forehead, and to a strongly incurving horn, a rudimentary ear and a small ‘fusiform’ eye” (Ucko 1987 : Fig. 26a; 29 : 93). Our examination of the area (Fig. 3, location 112 and Ucko 1987 : Fig. 26b) showed that the situation was very different from that published, and Breuil had clearly omitted several lines and altered the position of others. The line which Breuil took as part of the back of the bison is in fact one of the strokes making up the letter M, a letter which shows up clearly in photography (Ucko 1987 : Pl. 16) (and compare the Dellucs’ problems at La Martine regarding Defoix’s 1669 inscription (Delluc and Delluc 1983 : 21, Fig. 14)). Superpositioning is relatively clear – most of

the lines of the M predate the line taken by Breuil to be part of the bison. Various details as recorded by Breuil are distorted, particularly the so-called "oval-shaped eye" which is an arbitrary selection from three curving lines. The front part of Breuil's bison face is also an arbitrary selection but this time it incorporates several natural cracks.

The only other detailed analysis of this cave is that by Leroi-Gourhan which features a supposed ibex in the open-air entrance region, presumably signifying, to Leroi-Gourhan, the essentially "male" nature of the area when coupled with the engraved horse; both he presumably saw as "opposed" to the bison which Breuil recorded on a block in the entrance, but which has long disappeared. Unfortunately Breuil's published plan was in error and Breuil's cervid is, in fact, far inside the cave (see above) and no ibex exists in the entrance region ! Confused by Breuil's admittedly simplified published plan of the cave, Leroi-Gourhan claimed (1965 : 248, 318) a discrete physical area as a "second sanctuary"; no such discrete area can be objectively distinguished and in any case it is impossible to match Leroi-Gourhan's description of the supposed contents of this "part" of the cave either with Breuil's account or with our own reanalysis of it. He does not publish his normal type of distribution plan for Hornos and in fact refers to its distribution of art as "disconcerting".

I return to my starting point : why have individuals chosen to record the things they have ? Is it, for example, because Leroi-Gourhan never did the necessary work to correct and identify each individual addition to the published record that he therefore *had* to adopt the view that *one* representation of an animal species was the equivalent of *many* such representations ? It is not clear why we should continue to believe that it is sufficient to count the *frequency* of particular animal *themes* on the assumption that they can be determined by simply noting *absence* or *presence* of *motifs*. The recording of the distribution of representations of animals at Hornos de la Peña by "motif" versus "theme" can be shown to change our comprehension of the data. This whole question is of course of significance to more than representations of "animals/anthropomorphs", for it is clear that the same set of assumptions underlie many people's recording and analysis of "signs". As we all know, the analysis of the distribution of themes or motifs within caves depends also on the way that cave "areas" are defined. Space does not allow me to reiterate points made elsewhere (Ucko and Rosenfeld 1967) about the subjective nature of our categorisation and delimitation of "panels", etc., concepts which may well not have been part of the palaeolithic mental system (Sauvet and Sauvet 1979 : 340).

I want to move, as the last general area of my review, to another question concerning the caves used by palaeolithic artists, but to a point of much wider significance than the definition of cave "panels", etc. Almost everyone appears to assume that we know what was the palaeolithic emic view of *a* or *the* cave. Many who deal with conservation appear to assume that we know what the significance of "cave" was within palaeolithic ideology, and virtually everyone who tackles the question of cave contexts at all, assumes that "difficulty of access/approach/working area" would have been similarly regarded in the Ice Age. In fact this approach contrasts markedly with our normal treatment of palaeolithic techniques and technologies, for few have posed the question, "Why did all palaeolithic artists not confine themselves to painting or finger-tracing, as both of these techniques were presumably less arduous than engraving or low-relief ?" Few, if any of us, have taken the step of arguing simply on the basis of "difficulty" that engravings and low-relief works can be automatically excluded from a possible category of "doodle", so that they must reflect a desire for permanence of record...

In fact, we know in all cultures that all social boundaries are not always coterminous with actual physical conditions or situations. "Difficulty" of context is, by and large, a socially and culturally defined phenomenon. What right have we to assume that an etic classification of "cave area" is a useful reflection of emic attitude ?

In order to stress the subjectivity of many of the current approaches to "the cave", I now describe the history of Hornos de la Peña, adopting a rather different style from the norm : as far as we know the first "human" explorers of the cave that we now call Hornos de la Peña were those living some 30,000 years ago. Why they first went into the interior of this (and other) caves is not known.

As far as we can tell from the archaeological record, prehistoric humans continued to go into this cave for some 20,000 years and there is some suggestion that people entered at least the entrance regions of the cave in the Neolithic and Bronze/Iron age periods (c. 3000–2000 B.C.). The next thing that we know, from literature, about Hornos de la Peña is that H. Alcade Del Rio squeezed through a narrow aperture to get into the hidden parts of the cave in 1903 AD. His view of the cave's contents, judging from his publications, was partial (Del Rio 1906) and far removed from our own. Subsequently, during 1906 and 1910 AD, H. del Rio and L. Sierra were joined by H. Breuil, H. Obermaier and J. Bouyssonie, and excavations were carried out in the "entrance" area. Local rumour states that a joke was played on Del Rio; a friend of his used a nail to draw a representation of an animal somewhere inside the cave ! During this time also the art was studied and the final image of Hornos de la Peña established according to Breuil's vision of it (Del Rio, Breuil and Sierra 1911 : Fig. 83-4).

From this period on, the cave was known to those interested in palaeolithic archaeology and access was open to all. Local tradition says that at least goats (and almost certainly cows also) were occasionally herded in the mouth of the cave and that considerable defacement of the rock surfaces (whether inside the cave or only in the entrance chamber) occurred. Those who entered the cave certainly included some specialists, for a plan of the cave made in 1919, but there is also strong local rumour that many stalagmites were removed from Hornos de la Peña in order to decorate the thermal baths nearby. Other rumours mention the attraction of fountains in the cave and suggest that people went into the caves to get the excellent water. In 1924 AD a door was erected, perhaps on the direct orders of King Alfonso XIII and the cave was declared an architectural/artistic monument on the 25th April in that year.

Local information is unequivocal that the cave of Hornos de la Peña was used as a refuge during the Civil War from aeroplanes apparently directing attacks at an airfield at Los Corrales, and reports state that some people stayed "in" the cave for several days, whereas most only went there for a day or a few hours at a time. Local repute also reports that the people sheltering in the entrance area of the cave entered the actual cave out of boredom and to explore. There is no information regarding the fate of the door erected in 1924. It is possible to surmise, that as the area was one which had to react to major thrusts of Franco's advance, and as most of the local people were united in opposing Franco, that the door may well have been broken open or that it may have been opened by a local sympathiser.

To the Spanish Government in 1953 the cave was important enough for the Antiquities Department to take control of it and to carry out works for its upkeep. Since then official guided tours are mounted on request and there have been sporadic visits by photographers, speleologists and archaeologists, culminating in our team research between 1971 and 1973.

In the current vogue of attempting to define “areas” of caves as “entrance” regions, “central” regions, and so on, there is little reason to be sanguine that we can really assume a constancy of *attitude* to such “areas”. Even within a “cultural” group of researchers such as our own, attitudes to the cave varied from “the urge to explore and find new niches, chambers and corridors” to utter terror with consequent urination ! It would surely be too simple to suggest that all members of the French culture in the 20th century share Leroi-Gourhan’s view of the essential femininity of caverns. Perhaps more seriously, it can be seen simply from the evidence presented above that the attitudes of Del Rio, Breuil, the shepherds and our own research team *cannot* have been the same, as their respective knowledge of the different areas, extent and plan of the cave, varied (Fig. 8). All peoples’ attitudes to things are at least in part conditioned by their usage of them. The history of the use of this cave suggests that it was both varied and compartmentalized differently at different times.

I wish to underline the importance of two points for they remain those which seem not to have been adequately recognised. The literature of the past few years shows a remarkable sophistication of published cave plans indicating such elements as floor levels and cavern roofs, potential cave entrances, and possible relationships between cavern systems (Delluc and Delluc 1974 : 6, 16; Delluc and Delluc 1983) meeting many of the requests urged in Ucko and Rosenfeld many years ago (1969 : 108-14, 196-7). Despite this sophistication of recent publications and despite a whole book devoted to Palaeospeleology (Rouzaud 1978), few authors except Clot and Cantet (1974 : 96), appear to have confronted the first critical question which concerns the validity of an at last tripartite classification of decorated palaeolithic caves (e.g. Rouzaud (1978 : 123) does not deal with this question at all and although he refers to our book he does not mention this point and he does not include it in his bibliography, while the Dellucs (1983 : 70) stick to Laming-Empeire’s oversimplified classification based entirely on which regions of the cave received daylight, even when their own analysis of Comarque (Delluc and Delluc 1981 : 78-86) shows it to be quite unsatisfactory). The second point at issue is a somewhat different one – sophistication of description and detail of accurate planning is inevitably accompanied in the above recent literature by an assumption that palaeolithic *attitudes* to cave areas (niches, low passages, etc.) would have been the same as those of present day (at least) speleologists. Even terms such as “niche”, and descriptions such as “difficult of access” (Delluc and Delluc 1974 : 6; Rouzaud 1978 : 124, 135-136; Vialou 1979 : 29, 36) unwarrantably presuppose a communality of perceptions and reactions by those of the past with those of today.

CONCLUDING REMARKS

Part of the fascination to us all of cave and rock art studies is that we have before us the physical positioning of the art. However the chequered history of use of Hornos de la Peña must serve as a warning for, in this case, the “canvas” has been “slashed” and “violated” almost beyond recognition. Two illustrations demonstrate the necessity for the recording of every detail of “use” of a cave *throughout* its (especially modern) history.

As mentioned earlier, one of the aims of our team research at Hornos de la Peña was to investigate the visibility of representations and to this end we closely examined stalagmite and stalactite breakage both in the regions where there was evidence of art activity and in areas where there was none. In general our findings were inconclusive for (1) where there were breakages

they did not appear to affect visibility and (2) many of the breakages could have been modern. There is, in any case, some question as to whether any findings could have been conclusive for the construction of stairs and the digging of "floors" could have been accompanied by stalagmite/stalactite breakage and the local history of this particular cave attests to its plundering for stalagmitic concretions to "decorate" nearby tourist and health resorts. Much the same unsatisfactory situation applies to the Dellucs' study of Villars (1974 : 10-11) [and see their comments on La Martine (1983 : 30, note 9)].

In several localities in the cave of Hornos de la Peña are natural "holes" in the ceilings which are filled with mud. Several of these mud-filled cavities are many metres higher than the tallest human standing upright on either the modern or the ancient floor level. A few of these cavities have had holes "jabbed" into the mud either with finger(s) or more probably with some implement(s) (Pls. 10, 11). Other caves are reported to have similar clay-filled natural holes, some also with artificial punctures and it is argued (Clot and Cantet 1974 : 90-91) that they are likely to be of palaeolithic date. It is as inconceivable to us today to understand the nature of such action be it that such activity took place 30,000, 3000, 300 or 3 years ago (and we are unable to prove at which date this activity did in fact take place).

Investigation of the "use" of a cave, not only through the study of footprints, but also by studying breakage of concretions, re-use/retouch of artistic representations, etc. is a means of discovering such social usage and contexts – and is a thoroughly legitimate parameter of enquiry. Unfortunately, in the case of Hornos de la Peña, continual neglect of the evidence contained in the cave has led to the contextual evidence having become virtually indecipherable (Hornos de la Peña is, of course not exceptional in these regards (see e.g. Villars : Delluc and Delluc *et al* 1974 : 46-47, note 9; Montespan : Bégouën and Clottes 1988). It is clear that the history of interest in Hornos de la Peña is a complex one [but not more so than some other caves like La Martine (Delluc and Delluc 1983 : 9, notes 2, 3)] and this was, indeed, one of the reasons for the choice of this cave for study – how successful would we be in the recognition and determination of the age of engraved lines ? At Hornos de la Peña there are the occasional indicators such as the newly discovered "bearded (rein) deer with open mouth" (Fig. 9, Ucko 1987 : Pl.8) partially under (datable ?) stalagmitic flow and the (non-existent ?) "bison head" identified by Breuil but really consisting of an amalgamation of pre-1911 AD initials and natural cracks (Ucko 1987 : Fig. 26). However it must remain true to say that where a cave has remained open, and where previous unrecorded conservation and preservation activities have been carried out, so the problems of being able to correctly date particular engraved lines become acute (see e.g. the whitewashed area of Hornos de la Peña; Vialou (Vialou 1979 : 71) for Aldène; and the Dellucs (1983 : 21) for La Martine and (1981 : 76-77) for Comarque.

These particular problems of Hornos de la Peña serve to highlight the advantages and deficiencies of specific techniques of recording but they also focus attention on many of our preconceptions about palaeolithic art in general. In some cases the points raised are relatively obvious ones. Thus the fact that the newly discovered reindeer (Fig. 9), mentioned above, was under stalactitic flow may incline one to assign a palaeolithic date to "signs" under stalactite but the fact that there are such non-natural marks under such concretions does not, of course, determine whether they were made by man or by bear/bat/ etc.

Less obvious, perhaps, and certainly a result that we did *not* expect was the recognition that despite our own experience we continued to be uncertain in some cases as to what were natural and what were artificial lines. We had devised a technique of recording by tracing which clearly differentiated between lines which we took to be artificial and those that we took to be natural, and we carried out some random cross-checking of our tracings by having the same cave representation recorded on more than one occasion. Throughout the two seasons of intensive tracing work there were always some lines about which it was difficult to be precise [and see the problems at La Magdeleine (Rouzaud *et al* 1989) and Bois du Cantet (Clot and Cantet 1974 : 75)], but it was only in comparing the tracings of several seasons that it became clear that confident ascription to both "artificial" and "natural" had occurred about the *same* cave lines (Ucko 1987 : Fig. 28 d, e, and see Rouzaud *et al* 1989). Subsequent microscopic analysis of the lines in question did not solve the problem.

It would be tempting, and also reassuring, to be able to claim that such uncertainties applied only to rare examples of faint lines in inaccessible areas of the cave often covered by subsequent stalagmite flow. Unfortunately this is not so, for uncertainty still surrounds one of the most accessible of representations in the daylight entrance of Hornos de la Peña. Below the evident left hoof of an animal (Ucko 1987 : Pl. 1) are two lines about which we are still undecided as to whether they are *artificial* (and weathered) or weathered *natural* cracks. Nor are problems confined always to engravings, for we find it impossible without destructive sampling, to always distinguish between faded black paint and natural stain (Clottes 1988 : 296 for similar problems at the cave of Mayrière Supérieure; Delluc and Delluc (1983 : 34) for even worse problems at La Martine). In Hornos de la Peña, where paintings were evidently few (Pl. 12), there are numerous black marks whose nature still remains obscure but most of them are located in Gallery B which also contains a relatively clear painted depiction of a horse (Pl. 13). Clottes (personal communication, 1984) maintains that experts are still unable, in the case of Roquecourbère, to distinguish with any real degree of certainty between the "modern" (forged) line and the really old. The same is true of the engraving in Hornos de la Peña described above (Ucko 1987 : Fig. 14) which, at least today, is amongst the most evidently visible of all the representations. Is it really possible that neither Del Rio nor Breuil ever saw it and, if they did, why did they not bother to even mention it in their publications ? There is undoubtedly the need for a sympathetic study of those representations which the older authorities, such as Breuil, failed to recognise or accept as palaeolithic (see e.g. Delluc and Delluc 1971 : 245).

In the specific case of Hornos de la Peña there is an added major problem, for we suspect that misguided "conservation" activities in the past have resulted in the (unrecorded ?) smearing of walls containing disfiguring "modern" inscriptions with a mud and "plaster" wash, thus making the identification of line qualities even more difficult than before. Of our c. 140 discrete configurations, mentioned above, we would not be surprised if up to 90 were caused by human activity *since* 1906 and many of these "scratches" were almost certainly due to careless exploration and ill-conceived conservation measures.

The final point which I would have liked to attempt to disentangle was, therefore, how far the choice of the unit of analysis at Hornos de la Peña from 1971 to 1973 as the individual *motif* as *forced* upon us as the parameter of investigation (because of the treatment that the cave had suffered over the thousands of years) and how far it was chosen because we started with a research design that set out to test that prehistoric cave art was a system of communication with

one or more structures of its own which allowed messages to be “read” and “conveyed” to those of the same cultural and perceptual background. Retrospectively this is almost impossible to do, for our aims went much wider than this single parameter of enquiry. What is clear, however, is that the analysis of palaeolithic cave art as a communicating system of ordered *themes* does not require the same sort of detailed recording as does the analysis of such a system which is assumed to be based on ordered *motifs* (which is the parameter which we chose to adopt at Hornos de la Peña). It is equally true that the level of detailed recording required by those who believe that recognition of different styles of *individuals* is possible in cave art is infinitely greater than that demanded by the *above* parameters of enquiry (whatever the chosen unit(s) of analysis). It is certainly the very accurate recording of *all* individual *artificial* markings on cave walls/floors/roofs which can give the best evidence of actual use of the art (especially evidence of “retouching” whether in palaeolithic cave art in general (Rouzaud *et al* 1989; Marshack 1984; Lorblanchet *et al* 1973), in Hornos de la Peña itself (Del Rio, Breuil and Sierra 1911 : Pls. 7, 8, 56), or in other rock art complexes (Ucko 1977). There is also no doubt that it is only through the continual accurate checking of individual representations within so-called “categories” of artistic expression that the occasional “revelation” may occur with regard to assumed prehistoric technological activity [whether this be about palaeolithic negative hand prints or about Australian freehand painting versus composite stencil work on rocks (Walsh 1983)].

I have tried to show in the preceding pages that strategies for research, and the assumed relevant accompanying skills and levels of recording, are most often based on the assumptions of the investigators regarding the significance of the art works which are being investigated. I have also suggested, through the example of Hornos de la Peña, that, at the same time, particular research strategies are sometimes forced on the researcher by the nature of the data itself as well as by considerations of time and expense involved. In the case of Hornos de la Peña our original set of parameters for investigation involved enormous expense and time. Practical aspects of money and time also pose a major dilemma for palaeolithic art studies in general and undoubtedly continue to encourage investigations which do not focus on the very accurate recording of all individual lines or the attempted determination of what is natural and what is artificial/human or artificial/animal. There is a certain poignant irony to my conclusion that *most* of the ongoing work on palaeolithic cave art is not carried out in depth, that such partial recording work is best suited to research strategies which assume that palaeolithic art was a communication system based on *thematic* content, but/and that most of those who are actually carrying out such recording work do not in fact necessarily believe at all that palaeolithic cave art was a system designed for public communication and consumption.

Whatever else may have been elucidated, I think that one message is beyond dispute – it is certainly timely that we take control of our destinies as experts about palaeolithic cave art. To do so our main job in the future may have to be to persuade and convince administrators, Ministers of Culture, UNESCO, and other agencies, of the length of time needed to *adequately* record palaeolithic parietal works of art. We do not deserve to succeed in this endeavour until we can *agree* on the *variety* of our aims and until we can specify the necessary levels of details required by our recordings relative to each of those research strategies which we can confidently associate with the particular aims of specified palaeolithic cave art enquiries.

ACKNOWLEDGMENTS

This article originated in a draft manuscript written in two parts, the first by myself (Department of Archaeology, Southampton) and the second at my request by Dr Robert Layton (Department of Anthropology, University of Durham); it was, at that time, intended only to be a half-hour verbal communication for a colloquium in Périgueux, France, in 1984. However, by the time of the colloquium itself I had expanded my part of the manuscript into a full-length article. Some 50 copies of the article, translated rapidly into French, were circulated at the session on "Techniques et Méthodes". I had also prepared a short version of the paper, in English and French, which was presented to the colloquium. Subsequent to the colloquium, Robert Layton has published his views elsewhere (1985, 1987) and I received various comments and letters about my presentation at the Périgueux meeting. As a result I significantly altered various parts of the previously circulated short article and added various bibliographical references. This was then published in Spanish in *Cien años despues de Sautuola*, edited by M. González Morales, 1989, Santander, Diputación Regional de Cantabria. This present article is a slightly updated and revised version of the Spanish publication, prepared especially at the request of Jean Clottes for inclusion in English in this volume. I welcomed the above request since the distribution of the Spanish publication seems to have been limited in the extreme, and the recognition of the effects of subjectivity in the recording and interpreting of data remains of the utmost importance to future research undertakings.

I would like to thank especially the following for their comments since the Périgueux colloquium : Prof. A. Beltrán, Dr. J. Clottes, Ms. J. Hubert, Dr. M. Lorblanchet, Dr. G. Sauvet, Prof. F. Soleilhavoup, Prof. M. Weaver. I also wish to acknowledge those who assisted me during the preparation of the original version of this chapter, especially Prof. De Bellos, Dr. M. Lorblanchet and Dr. S.J. Shennan. In particular I acknowledge my great debt to Dr. R. Layton and Ms. J. Hubert, many of whose comments and views have been incorporated in the main body of this text, and I must also mention Mr. M. Curtis, without whose assistance all the notes of the work at Hornos de la Peña would have remained in chaos.

All figures were drawn for publication by Christopher Webster of the Department of Archaeology of the University of Southampton. The Plates are by Andrée Rosenfeld.

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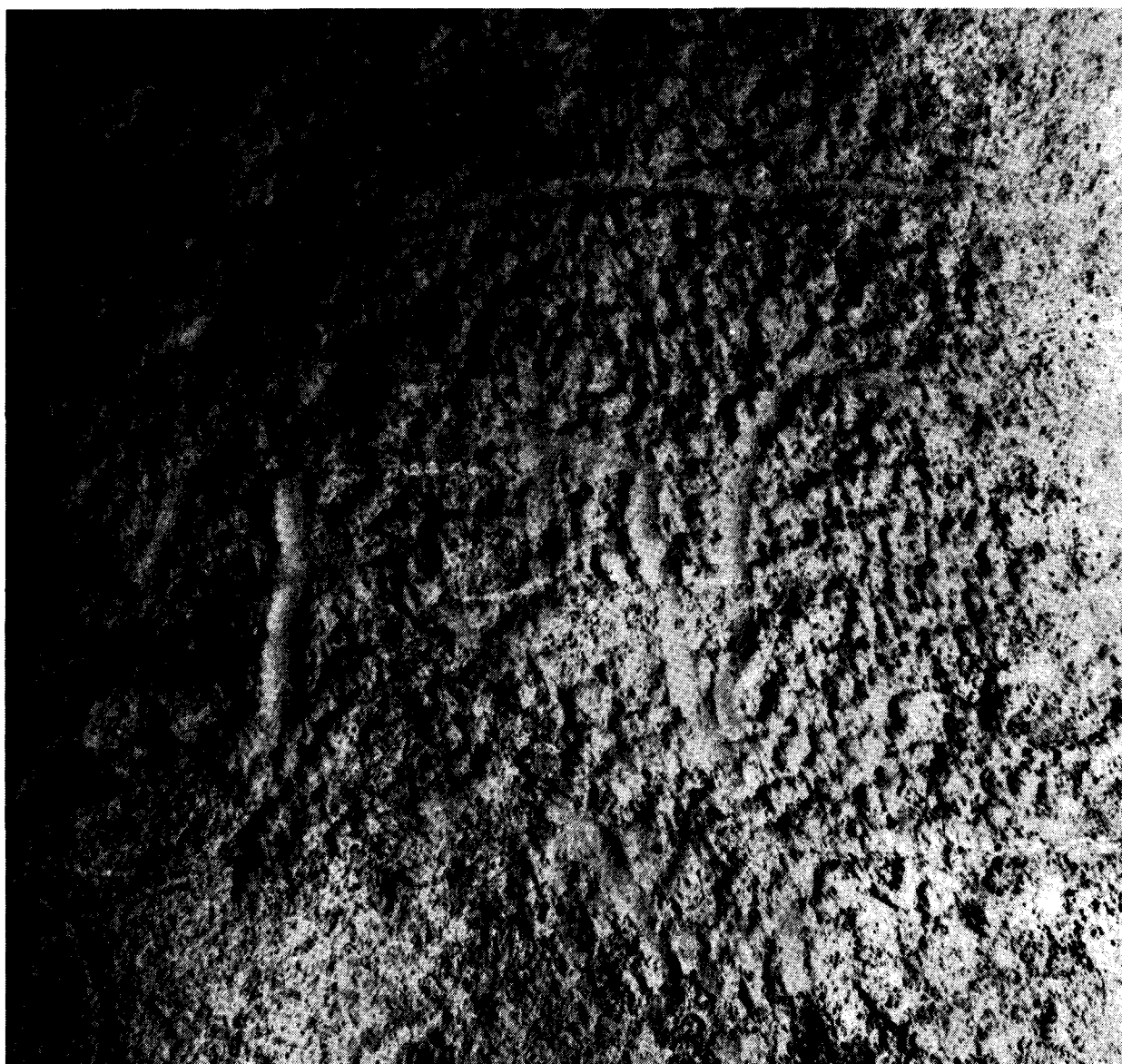
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Pl. 1

Finger tracing of "ox", with photograph apparently showing "arrow" stuck in its flank (Ucko location 128).



Pl. 2

Finger tracing of "ox", with photograph apparently showing line crossing through flank and continuing outside the line of the body.



Pl. 3
Finger-traced meanders (Ucko location 11).



Pl. 4
Engraved "snake" (Ucko location 102).



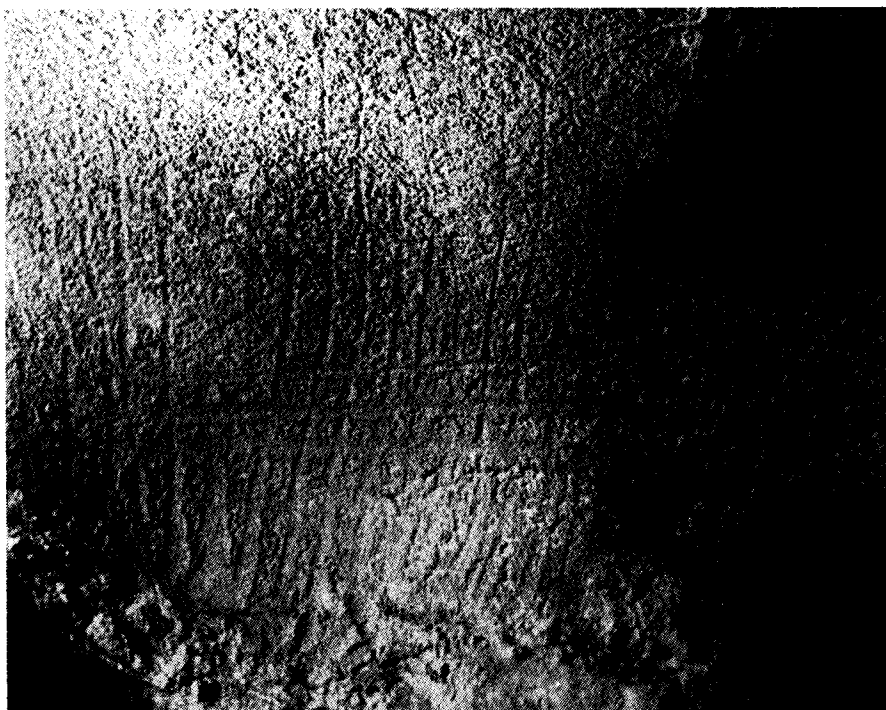
Pl. 5
Engraved “anthropomorph” and some of the lines in the area (Ucko location 96).



Pl. 6
Engraved “loop” (Ucko location 19).



Pl. 7 – Engraved complex of apparently non-representational lines (Ucko location 93).



Pl. 8
Finger-traced and engraved "grid" (Ucko location 60).



Pl. 9
Finger-traced "grid" surrounding natural cavity (Ucko location 101).



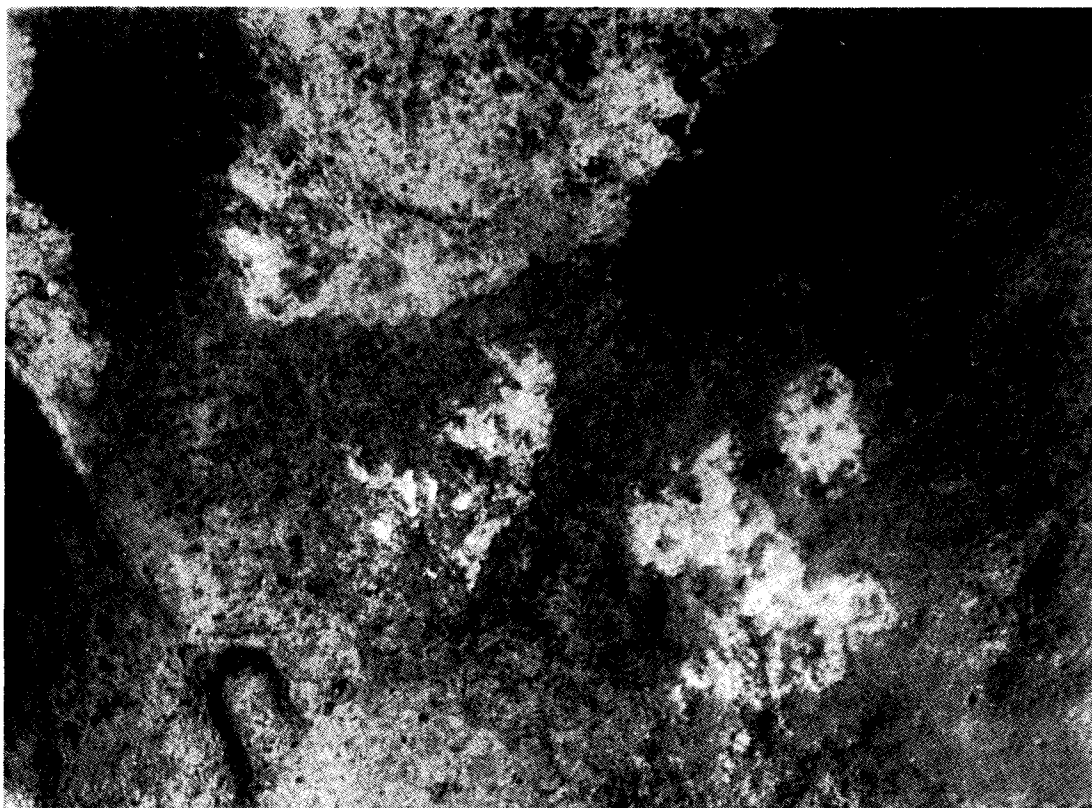
Pl. 10 & 11

Natural cavities filled with mud into which holes have been apparently "jabbed"
(Ucko location 57 and 107).



Pl. 12

Possible painting of dorsal line of an animal and small "tectiform" (Ucko location 26).



Pl. 13

Black painted horse (Ucko location 137).

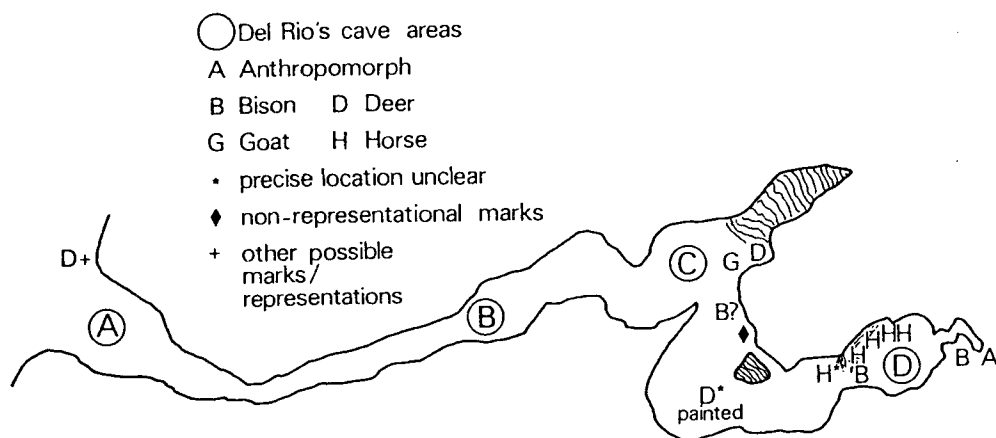


Fig. 1
Del Rio's distribution of animal and anthropomorph representations (1906, Pl. I).

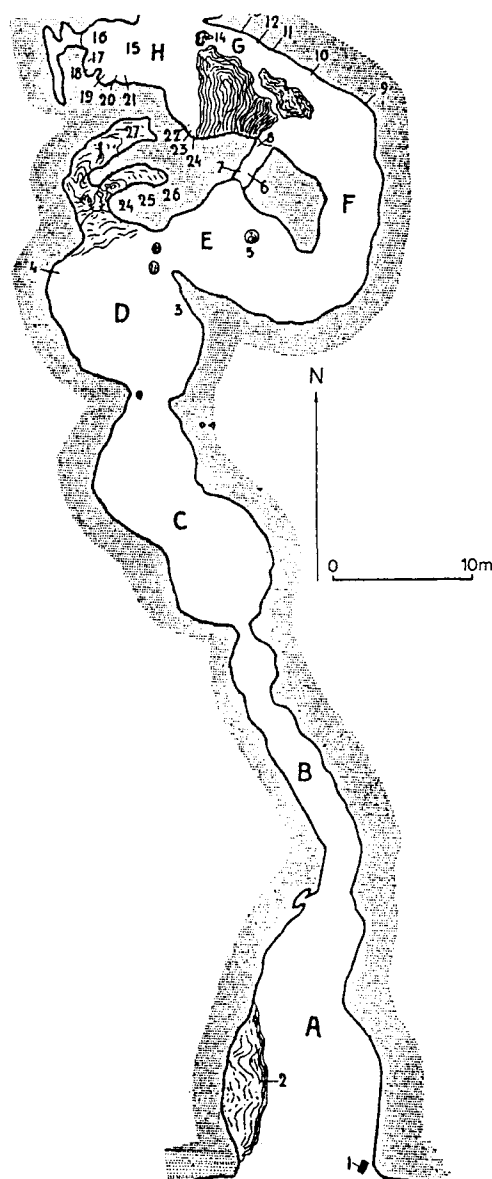


Fig. 2
Breuil's location of representations (Del Rio, Breuil and Sierra 1911, Fig. 83).

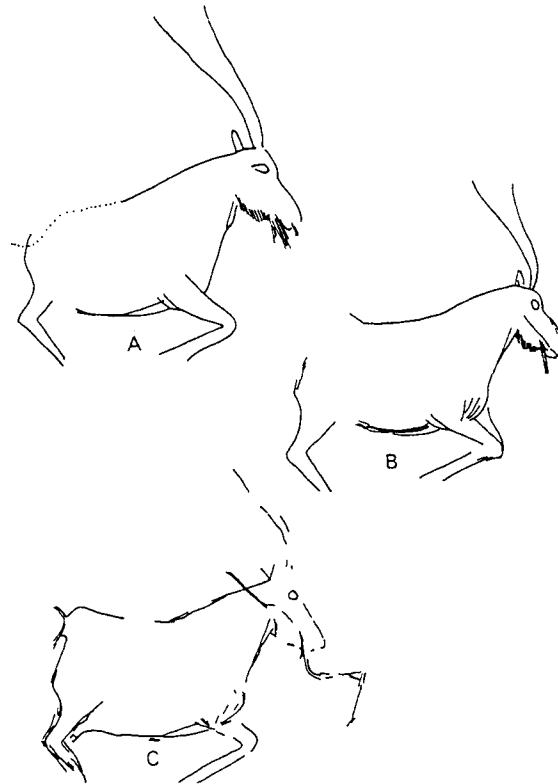
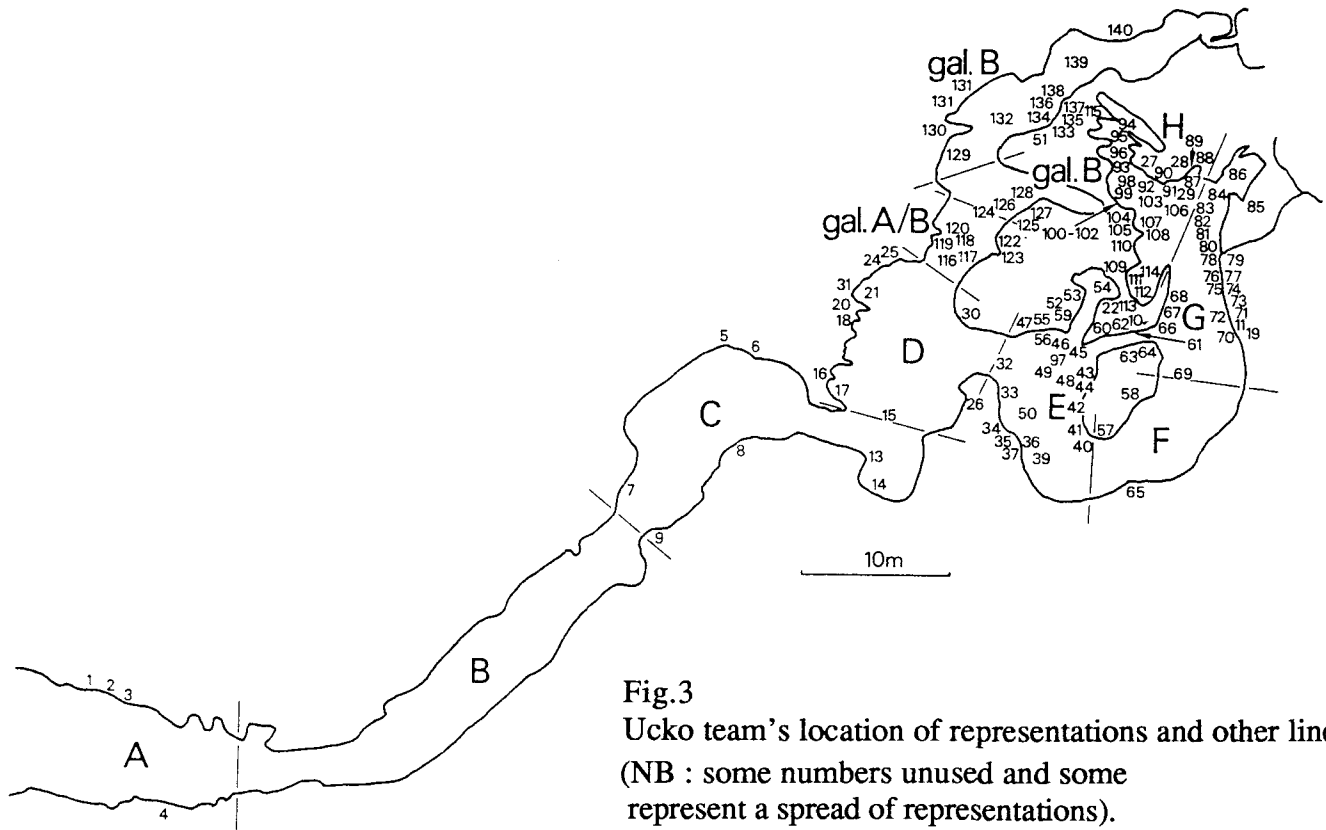


Fig. 4

A & B : two interpretations by Breuil (Del Rio, Breuil and Sierra 1911) of an ibex with beard.
C : Ucko team's tracing omitting beard because of identification of the lines as bat scratches.

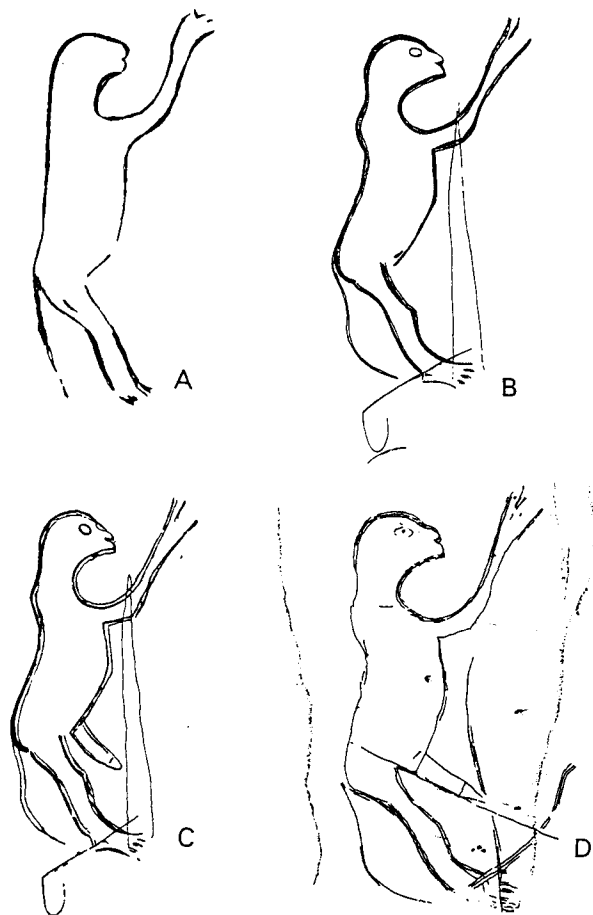


Fig. 5
Engraved “anthropomorph”. A : Del Rio 1906. B : Del Rio, Breuil and Sierra 1911. C : Breuil 1952 and D : Ucko team.

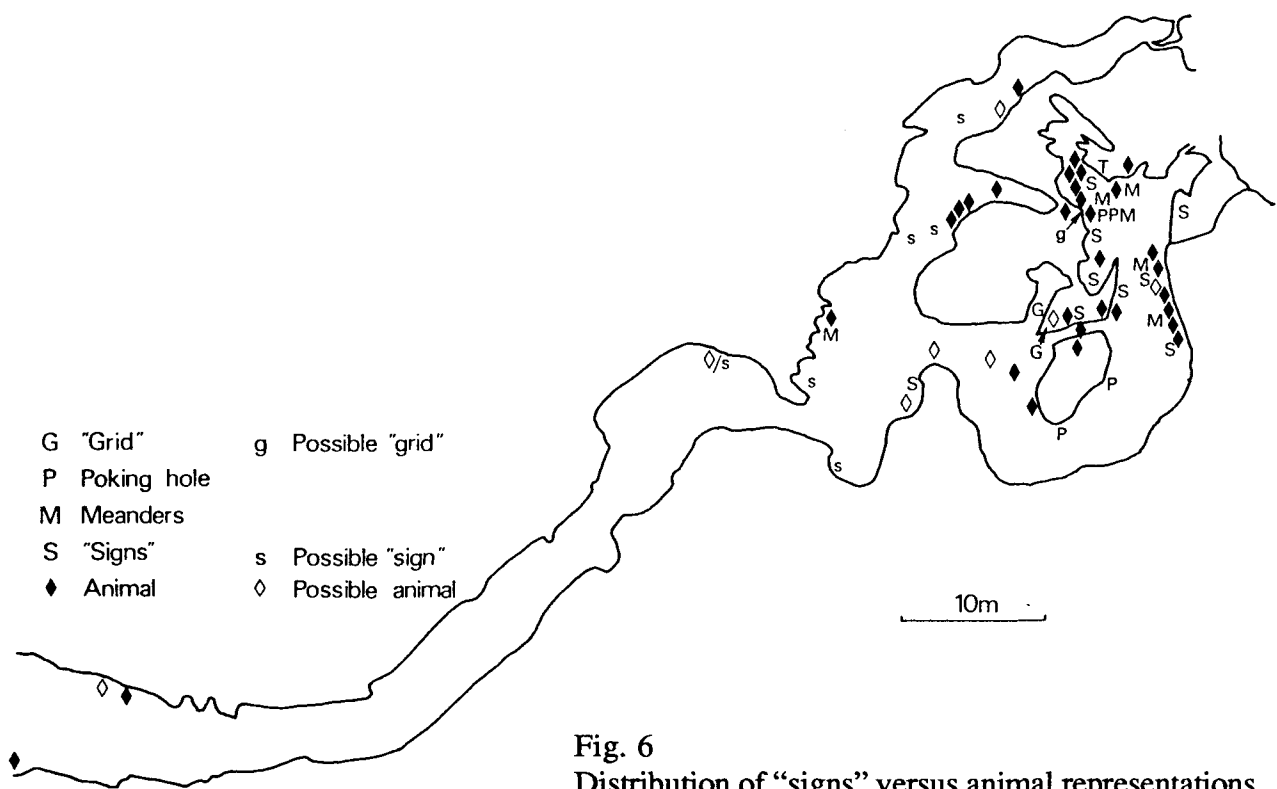


Fig. 6
Distribution of “signs” versus animal representations.

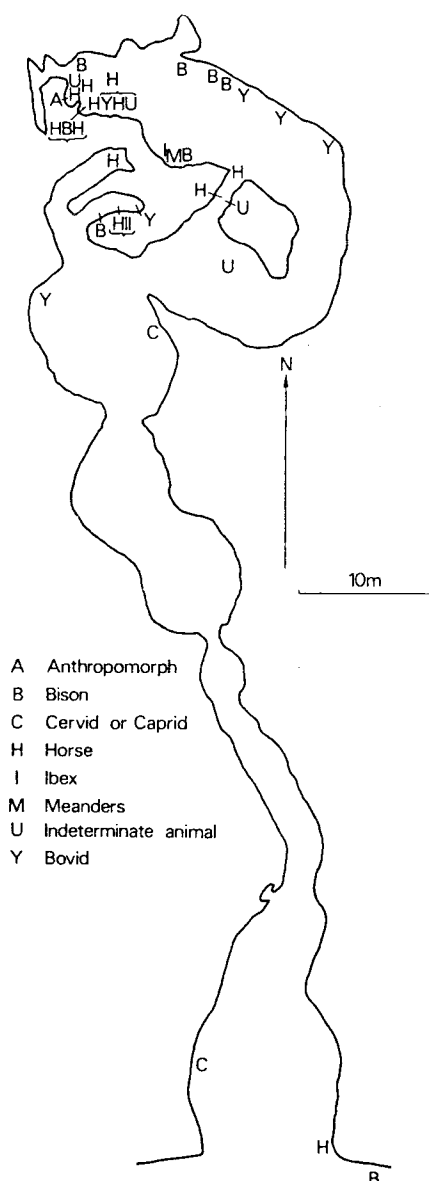


Fig. 7
Distribution of Breuil's identifications.



Fig. 8

A : 1906 Plan (Del Rio 1906). B : 1911 plan (Del Rio, Breuil and Sierra 1911). C : 1919 plan (unpublished). D : Ucko team plan (1971-1973).



Fig. 9 : Engraved (rein)deer with open mouth (Ucko location 67).

THE ANALYTICAL PROBLEM OF SUBJECTIVITY IN THE MAKER AND USER

by

Alexander MARSHACK *

The problem of "objectivity" in the analysis and documentation of symbolic archaeological material is immeasurably compounded in the study of the first widespread and complex body of imagery and "art" to appear in the human record. This difficulty is due not merely to the temporal distance and lack of ethnography for the Upper Paleolithic but, in large part, to the extraordinary variability of these early traditions and to the clear evidence for different classes and types of imagery and image production and use. Many of these symboling modes appear in the human record for the first time.

The desire to address the difficulties that are due to this variability can be seen in the century long effort to break-up the imagery into such seemingly "objective" and easily discussed visual categories as animals/humans; mobiliary/parietal; engraved and carved/painted; realistic and abstract/schematic; male (phallic)/female (vulvas and figurines), as well as such categories as personal decoration, manufactured symbolic artifacts versus non-manufactured surfaces such as engraved stone plaquettes and unworked fragments of ivory, bone and antler, grave goods versus habitation site materials, etc. Each of these categories has, over the years, been additionally broken down into still smaller seemingly "objective" sub-categories. The abstract signs, for instance, have been "objectively" classified by their shape or appearance as tectiforms, scutiforms, claviforms, peniforms, or more simply as ovals, bundles of lines, sets of dots or arcs, thin signs, wide signs, etc. Though all of the above may seem to be "objective", they are, in fact, highly subjective contemporary categories devised by the analyst and often suggested by one or another contemporary theory. Upper Paleolithic imagery certainly did not function on the basis of such "objective" categories.

Without exception these efforts at objectivity have been what I would term end-product description or "product analysis". If symbol systems, however, are intended to *function* in a culture and to carry meaning and information by their *use*, and if the remnant products somehow represent the *participation* of the maker and the user in different symbolic and referential behavioral modes, then these attempts at "objectivity" are, in the name of "science", intended to remove these necessary semiotic and subjective parameters from the analysis and from theoretical discussion. This intention is often admitted and defended on the grounds that other forms of analysis would be "subjective", though the subjectivity of so many of the supposed objective analytical methods and attempts at categorization should be apparent.

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Even the relatively simple matter of recording or documenting the imagery of the Upper Paleolithic has gone through styles of so-called "objectivity", beginning with simple 19th century renditions based on what the eye could see and progressing to increasingly sophisticated means of documentation by use of the magnifying glass, microscope, tracing, photography, ultraviolet and infrared photography, chemical analysis of pigments and paints, and even electron microscopy of engraved strokes, etc. I have myself been involved in developing some of these empirical techniques. In a somewhat broader effort at "objectivity", Leroi-Gourhan attempted in mid-century to determine by statistical means the relationship and association among different classes of imagery in the Franco-Cantabrian caves. He then tried to interpret his statistical results by creating a model of the "ideal" topographic structure for the placement of images in a sanctuary cave. The result was a conceptual model or plan that was as subjective as any yet attempted.

If, in contrast to these efforts at "objective" product analysis and categorization, we assume that human symbol systems are the subjective creation of individuals and groups participating in diverse forms and modes of cultural behavior and expression, then the problem of subjectivity *in the maker*, i.e. in the *production* and the *use* of different types and classes of imagery can be addressed in a different manner. One can approach the analysis not so much in terms of the recognizable images or shapes, or of possible "meaning" which one can never adequately ascertain, but in terms of the individual and cultural *processes* involved both in the creation of symbolic products and in modes of symbolic use. Different classes of imagery not only have different modes of use, but also different types or levels of cultural and individual relevance and reference. Cultures can perhaps be as aptly described by these differences in the functional aspects of their symbol systems as by seemingly objective descriptions of the symbolic products. In any case, one must attempt to address the difficult theoretical problem of the "subjectivity" involved in the different types and modes of symbolic behavior fostered by a culture. I shall attempt to discuss this difficult problem at a number of levels. To the best of my knowledge this approach has not been undertaken before.

TYPES OF SUBJECTIVITY

I have elsewhere suggested that human symbol systems are always "time-factored", that is, they are either made, used or participated in at the proper time and place and for a proper or relevant cultural or individual reason. The concept of "time-factored" symbol systems is not simple. It involves an inquiry into the types of motivation and intent involved at different points in the process of production and use. These include *cultural* time (when an image is made or used), *subjective* time (when in the sequence of a person's life the image or symbolic behavior is relevant and used), and *referential* time (the "time-factored" concepts that may be encoded in the image or the ritual). The complexity of the problem can be seen in one of the simplest and earliest of Upper Paleolithic classes of image, the items of personal decoration.

At the site of La Madeleine in France a five to seven year old child was buried with shell and animal tooth beads forming a necklace, bracelets, armbands, anklets, knee bands, and a cap that was covered with shell beads (fig. 1) (Capitan and Peyrony, 1928 : 122). It is difficult to imagine an Upper Paleolithic child heavily decorated when at play, just as it is difficult to imagine that the decoration had anything to do with the level of that child's sense of "self awareness" as

has been suggested of "personal decoration" by White (1989a, b). The symbolic "awareness" involved was clearly that of the adults and the culture performing the burial. It was a cultural awareness that functioned at many levels.

Personal decoration is often an indication of a *loss* of self and a sign, instead, of membership in one or another cultural class or group. The concept of "self-awareness", not as a recognition of one's own ego in a modern sense, but in terms of membership and of the *loss* of self in a cultural category at a particular time in the life of an individual and at relevant or periodic moments in a culture is so well known as to hardly require elucidation. The La Madeleine child was being incorporated into a number of cultural groups, one of which, of course, was that of the dead.

The sense of "self" that is derived from membership in a culture or a cultural group did not originate and was not established by wearing beads or any other artifactually permanent form of personal adornment, but was probably initially established by other visually mediated behavioral equations that were at play in a culture, beginning with the changing roles that an individual played as infant, child, adolescent, initiate, male, female, mother, hunter, gatherer, etc. Rites of passage are probably more important as an indication of both the attainment and the loss of self by transition to membership in a class at a particular point in one's life than a wearing of beads or other forms of personal adornment. Beads represent a historical development in the elaboration of such traditions. They do not create or describe the beginnings of such traditions. Of greater importance, there are other more perishable and earlier forms of personal decoration that mark the "self" as the member of a class. The ivory, bone, antler, shell and stone beads found in the European Upper Paleolithic merely represent one of the many forms created historically by certain cultures for marking and differentiating individuals in their varying roles (Marshack 1991). These modes of marking "self" represent an aspect of "subjectivity" that is subtle and as difficult to ascertain by product analysis as is the subjectivity of the user or participant in any form of ritual or ceremony. Ethnography tends to provide descriptions of symbolic behavior but seldom explores the complexity of the subjective contents in such behavior, either within the individual or in terms of the variation among members of the group.

The most complex Upper Paleolithic burials, those at Sungir in Russia, suggest a complex form of personal decoration. The burial of one individual consisted of hundreds of hand carved ivory beads (fig. 2a, b). We must ask whether these were a form of decoration assigned by the group or the culture to burials, or the adornment belonging to the individual himself and perhaps utilized in life because of some special cultural role (and therefore included in the burial); or was it part of the role performed by that individual in death (and therefore created for that particular ritual occasion and burial). In one case we would have the subjectivity attaching to the individual's use of personal adornment at some point or stage in life, in the others we would have the subjectivity, not of the individual, but of the group itself. I here recall – as an aside – the elaborate rituals in Mesoamerica involving the extremely complex symbolic preparation and adornment of an individual for sacrificial death. The victim was a prisoner of the culture and had been made the member of a cultural class that was, as a matter of fact, not his. There was, of course, "self-awareness" in the personal adornment given the victim, as well as a "loss of self", but of what type? The huge investment in labor evident in the Sungir "personal adornment" may have been indicative of the ritual, cultural moment instead of a personal marking. This aside is not offered as an explanation of the Sungir burial, but as an indication of the problems one faces

in attempts at interpretations of "self-awareness" at a distance from the mere presence of the artifacts. It also warns against the twentieth century presumption that elaborate grave goods necessarily indicate the presence of rank and status. Since I have argued that symbol systems are context bound and "time-factored" and occur or are made to be used at the proper time and place and for the proper reasons, I assume that the La Madeleine child, for instance was decorated for its "role" on that occasion. The beads were intended for the "self-awareness" of the family and the group rather than as an aspect of the child's own "self-awareness". The child was, in fact, being made, at the time of its burial, the marked member of a number of different cultural categories : a member of those dead, the member of a specialized ritual, the now specialized dead member of a family and "tribe", the presumed spiritual actor in a cultural moment, etc.

In a recent paper I have indicated that the Gravettian "Venus" figurines, c. 26,000 BP, provide evidence that women commonly wore "personal decorations" made of perishable materials, including elaborate coiffures as well as belts, bracelets, anklets, breast bands and collars made of twined cords or thongs apparently cut from animal hides (Marshack 1991a). These perishable materials do *not* show up as remnant products in the archaeological record but they were apparently ubiquitous and important and they apparently marked the mature, potentially fertile female as the member of a category in the culture and society. Significantly, depictions of carved beads are rare among these figurines. Forms of marking and individuation by perishable materials, and by performance of complex symbolic behaviors, probably existed long before the manufacture of beads and their appearance in the Aurignacian. There is a fundamental presumptive, subjective error on the part of archaeologists who consider the mere presence of a product in the archaeological record as the beginning of a cultural behavior. Besides, the evidence for variation among the subjective factors found in "personal decoration" is quite complex.

From the Gravettian site of Dolni Vestonice, Moravia, in Central Europe, there comes a carved set of ivory beads in the form of the female breasts (Absolon 1949; Marshack 1972a, 1991a). At the Gravettian site of Grimaldi in Mediterranean Italy a crudely carved double-sided pendant of black steatite depicts a female with two bodies and faces; she is pregnant on one side but non-pregnant on the other (Marshack 1986). At the Magdalenian site of Petersfels in Germany a set of black coal beads were found, carved in the "buttocks" style of representing the female torso. These different types of imagery are usually classified in the archaeological literature as "female" or as "personal decoration". Assuming, however, the inherent subjectivity of place, moment and context in the manufacture and the use of symbol systems, I have suggested that these beads and pendants were probably made to be worn by certain persons at particular times. They may, of course, have been worn by males, again at particular times, but I have assumed, theoretically, that because of the inherent uncertainties, dangers, fears and desires involved in the processes and periodicities of the mature female, the beads and pendants were probably worn by women at times when they wished to participate in symbolic and mythologized ritual equations related to pregnancy, birth, nurturance, etc. The beads and pendants would have represented a different, specialized class of "personal decoration" – and of *time-factored* "self awareness" – than was involved in wearing the twined cords, belts and bands made of perishable materials that merely marked a female as mature and therefore as potentially fertile (Marshack 1991a).

The "Venus" figurines, on the other hand, may have encompassed a more general concept of fertility and potential fertility, i.e. of motherhood, an ancestor, a female spirit, etc. and have served as a generic symbol intended for more variable long-term use. The specialized feminine images noted above, including the vulvar images that are found in all periods of the Upper Paleolithic, apparently addressed more specific and perhaps more subjective aspects of concern and relevance. I have indicated that there were also other types of female imagery in the Upper Paleolithic, including small crudely and quickly made images that may have been intended for short-term or one time ritual use (Marshack 1972a; 1976; 1986a, b; 1990; 1991a). There were, therefore, different levels or forms of "subjective" cultural reference in the production and use of these images.

As an aside, it is perhaps significant that while the generic naked "Venus" figurines (i.e. from Lespugue, Willendorf, Dolni Vestonice, Kostienki, Avdeev, etc.) often seem to wear signs of their mature womanhood, males in the Upper Paleolithic often seem to wear indications of participation in a ritual or ceremonial performance (the masked "sorcerers" at Trois Frères, the masked dancers in compositions from Mas d'Azil or the Abri Mège) (Marshack 1972a). The Aurignacian ivory carving of a lion-headed therianthrope from Hohlenstein-Stadel, Germany (Marshack 1988, 1989, 1990) suggests someone wearing a lion skin and mask (fig. 3). We have the possibility, then, of personal decoration worn at a particular cultural, ritual moment and, therefore, of an assumed loss of "self" by the actor performing as a lion. The image may equally represent a lion "spirit" or mythological figure in human, therianthrope form, in which case it is the "spirit" that is assuming human form rather than the human who is assuming animal form. But given this possibility, it is therefore also possible that the image of the lion-headed human could be symbolically collapsed and serve in both modes or forms within the same culture.

While it is too early to do more than suggest the possibility of "male" versus "female" forms of personal decoration, used at different times for different purposes, the presence in the Pyrenean Magdalenian of beads "en contour découpé" representing animal heads (horse, ibex, bison) suggests that there may have been specialized types of male decoration: the accoutrements of a shaman, for instance, that may, like the lion-headed anthropomorph, have marked his role and status, or that may have represented his "spirit" animals. Such beads may have been worn only at particular times and occasions. Questions of this type, which are suggestive but cannot as yet be answered, go beyond traditional categories and classifications of image and address the inherent variability found in the human use of image and symbol. Would women, for instance, have worn or utilized other forms of image and symbol when participating in a ritual? We have the example of the "Venus" of Laussel with a marked bison horn in her upraised arm. Was she being depicted as a female participant in a ritual or was she a more generic symbol carrying a sign or attribute. Does the recently discovered Aurignacian stone carving of a "dancing" female from Galgenburg, Austria (Neugebauer-Maresch 1988), represent a ritual performance? If so, why do we have a depiction of the ritual, instead of it having been adequately actualized in the mere performance? We also have the Aurignacian depiction of a human, perhaps a male, in a ritual or a dance in a crude carving on ivory from the German site of Giesenklösterle (Hahn 1986). Were these depictions of rituals that were performed for special purposes at particular points in time and space? If so, do we classify them as human images, as "male" or "female" images, or as depictions of ritual performance? And was a ritual performance more important than its depiction or the attribution of sex to the performer?

The problem of interpreting the subjectivity that may have been involved in the creation and use of different classes of imagery is found also in the Upper Paleolithic Franco-Cantabrian sanctuary caves.

THE FRANCO-CANTABRIAN CAVES

Anyone who has studied the Franco-Cantabrian decorated caves is struck by the variability of the imagery in any one cave and among different caves. It was in an attempt to provide coherence to this variability that Laming-Emperaire and then Leroi-Gourhan provided a structuralist hypothesis of oppositions to explain the association of different classes of imagery in the caves. These mid-century models assumed the existence of a relatively stable conceptual template, a structuralist philosophy within which images were placed on the walls as though on a chess-board of oppositional rules and topographical positions. Any "subjectivity" in the maker and any subjectivity in the "time" of the creation or use of an image, was eliminated, except as it was expressed in the skill of the creator working in a cultural style.

I began my studies of the cave imagery with different theoretical assumptions. These included the concept of "time-factored" and "time-factoring" symboling traditions. I was therefore at once struck by the subjective variability of the imagery in each cave, not merely differences in style and iconography but apparent differences in the "times" of manufacture and use of different types of imagery. There are areas and panels in all the major caves where the accumulation of images suggests ritual behaviors by individuals who were not interested in public performance or communication. The placement of the images suggests that the makers often sought isolated and difficult to reach corners or walls to indulge in *private*, often periodic acts of symbolic marking. The "communication" was, therefore, not between the maker and a public, but between the maker, the act of creation and the cultural image.

In the Spanish Cantabrian cave of La Pasiega (Puente Viesgo) there is an extremely narrow passage or gallery, hardly wide enough for one person to enter and bend an arm, that has an accumulation of geometric forms (tectiforms) clearly made at different times with different ochres and probably by different hands (fig. 4a, b). In the adjacent cave of Castillo there is a small chamber into which one must climb and kneel; the ceiling is covered with tectiforms different from those in the neighboring cave of La Pasiega. These geometric forms were periodically altered and overmarked, and sets of dots made by different paints and fingers were accumulated around the signs as though in periodic ritual use of both the chamber and the signs. In Castillo, also, there are tectiforms that have been renewed by overengraving, overpainting and even encirclement. In the southern Spanish cave of La Pileta, near Málaga, a tiny cubicle into which one must climb and squat has one wall covered with a complex accumulation of different classes of imagery; black horses and bovids that were overmarked by fingers dipped in red or black paint; there are also images of the ibex and serpentine "macaronis" made by different paints (fig. 5, 6). A macaroni section by one paint is often appended to another made by a different paint (fig. 7a, b). These data make it apparent that different individuals climbed into this cubicle to make images, marks and signs of different types at different times, with different skills and perhaps for different reasons. The public, open walls of the cave outside of this cubicle were covered with images of a different type. The variability of the imagery within the cubicle suggests that it was a "sanctuary" which was entered at certain times, by particular persons, for

different symbolic or ritual reasons. Not visible in any of the published photographs or renditions of the composition is the fact that the wall had been repeatedly touched or stroked by fingers not dipped in paint (fig. 8). Objective analysis could, of course, determine the early style of the animals and the fact that different paints or modes of marking were used. But the far more important inference, that different persons had climbed into the cubicle at different times for different "subjective" symbolic, ritual or cultural reasons, could not be derived from a two dimensional rendition or from an objective analysis of the styles of rendition, types of imagery, or even the differences in the paints.

On a limestone wall outside the tiny sanctuary, near a clear pool of water, is the large outline of a fish in black paint (1 m 50). It was renewed by painting a second outline within the fish, and by painting a third schematic fish oval and a small seal inside the fish (fig. 9). Neither the unrealistically large fish (an oceanic fish of the flounder family according to Breuil) or the seal could have been seen in the vicinity of the cave. But each may have been a water-related symbol ritually and mythologically associated with the pool of water. It is therefore possible that the inhabitants moved seasonally to the Mediterranean coast approximately a two days' walk to the south. The macaronis in La Pileta led this researcher to an investigation of ritual macaroni marking in the riverine homesites of Upper Paleolithic Europe in the major Franco-Cantabrian caves. As a result I have suggested that the macaroni image was a water-related motif of symbolic importance in these riverine cultures (Marshack 1975, 1976, 1977, 1979). I have found what I consider to be the water-related band and serpentine (an image almost totally neglected in discussions of Upper Paleolithic symbol except for its categorization as "bundles of lines") in many of the major Upper Paleolithic caves : La Mouthe, Lascaux, Gargas, Trois Frères, Tuc d'Audoubert, Rouffignac, Marsoulas, Castillo, Altamira, Hornos de la Peña, etc. (fig. 10, 11, 12). That insight, half formed at La Pileta early in the research, was purely "subjective", but the decade long inquiry it instigated and the huge documentation the inquiry subsequently provided, attempted to establish certain parameters for analytical and comparative objectivity concerning this class of imagery. It involved an attempt to determine regional variability in the use of this water-related motif, in much the way that the research attempted to determine regional variability in the use of the animal and female image (Marshack 1991a). If I am right in my suggestion for a water-related iconography we are faced with a question concerning the possible meaning that the ritual marking of a water-related macaroni motif could have had in these riverine cultures. The images are clearly not "art" but they are ubiquitous across Europe. Questions concerning subjectivity in the maker and the analyst are, therefore, inherent to the inquiry.

In the Pyrenean cave of Tuc d'Audoubert (Ariège), at the end of a narrow passage-way or gallery there is a tiny cubicle into which one must climb and into which only the head, shoulders and hands can enter. On the ceiling and walls of the tiny cupola above one's head is a crudely engraved small horse, not made with the skill in animal rendition apparent in so much of the other work of the "Cavernes du Volp". Around this crude horse is an accumulation of perhaps eighty "P" signs or "claviforms" discovered by R. Bégouën. There is evidence that these signs were made by a number of different tools and hands, suggesting again a secret place to which individuals came periodically to perform the ritual of marking a particular sign in association with a particular animal. For Leroi-Gourhan this association of an animal and sign might have been considered to be the structuralist opposition of a "female" sign (the claviform) with a "male" animal (the horse); in the time-factored model being suggested here one is instead concerned,

perhaps more objectively, with evidence for the repeated use of a particular sign with a particular animal. No interpretation of meaning is required at this level of analysis. If we assume that the "P" sign is a feminine symbol, its repeated association with the horse could be symbolic in a number of ways, none of which are necessarily those of "opposition"; there could have been supplication for a hoped-for-pregnancy or a safe delivery, with the marking recalling a myth of the horse as an animal somehow related to human pregnancy, etc. The possibilities are large.

The type of private ritual suggested by the tiny crude horse and its surrounding "P" signs is different from the complex "public" composition and melange of skillfully incised animals around the dancing "sorcerer" in the adjoining cave of Trois Frères. My short study of the animal images has suggested that these engravings occasionally depicted species with different seasonal pelages and the "sorcerer" overlooking the chamber may therefore have been performing a ritual of a different type than was conducted within the cupola of the nearby cave of Tuc d'Audoubert. At one side of the great chamber of animals in Trois Frères overlooked by the "sorcerer", there is another tiny, hidden, difficult to reach horse incised at the top of a rock on a wall in a high corner (1958 : 76). The horse was again overmarked with "P" signs, suggesting a private sequence of ritual marking intentionally separated from the context and contents of the great chamber. The ritual act was, in fact, hidden from the "sorcerer" and his public performance.

The interpretive problems caused by such indications of "subjectivity" are often compounded by analysis. Not only were "P" signs periodically marked in association with the small horses in the Cavernes du Volp, but particular "P" signs could at times be reused or renewed. One has a sense that the original maker may have renewed his or her own original image. Fig. 13 is a close-up photograph of a "P" sign near the cave's riverine entrance, associated with another "P" sign and a multiply "killed" or overmarked bison. It is interesting that this process of renewing or reusing an abstract sign or symbol is found as early as the Aurignacian. Incised vulvas in Dordogne homesites were often overmarked and sometimes had their outlines repeated or doubled (Delluc and Delluc 1978 : 266). These modes of image use, renewal and reuse, probably at ritually relevant times, occur from the beginning to the end of the Franco-Cantabrian Upper Paleolithic. I have found handprints in some caves reused or renewed by overmarking. Discussions of style, shape, category of image tend to ignore these processes of use and, therefore, of "subjectivity".

The well-known sculpted clay bison at the far end of the cave of Tuc d'Audoubert also suggests a limited "time-factored" creation, a set of images made at a particular time for a particular ritual. Apparently it was made and used and never again visited. That the images represent a bull and cow bison, with a small bison "calf" nearby, may indicate a "time-factored" seasonal reference in the carving and the ritual that accompanied its creation and use. These suggestions of subjective *and* time-factored differences in the production and use of different types of imagery in the caves are crucial. While they involve "subjective" readings and assumptions by the analyst, they indicate that the study of these materials should involve some discussion of the *problem*, at least, of the many forms of subjectivity in the production, the time, the place, and the use of different classes of imagery and even of the caves themselves. The present discussion also suggests a need for an evaluation of the types of objectivity and/or subjectivity that enter into an analysis, depending on the theoretical models that direct either an analysis or an interpretation.

If, as I have suggested, all symbol systems are, one way or another, "time-factored", then one must add to studies of style and depictive form, studies of the types of use and contexts in which certain classes of image are found. This is not a suggestion for *interpretation* of the images, but a plea for an elucidation or clarification of the range of behaviors and strategies that may have been involved in image production and use. Inferences concerning possible meaning might then be attempted, based not merely on the shape or form of an image, but also on these processual aspects of differentiated context and use.

THE STONE PLAQUETTES

A symbolic material and tradition that stands midway between items of personal decoration, the female image, and the decorated caves, are the non-utilitarian engraved stone plaquettes and fragments of ivory, antler, bone and stone found in certain habitation sites. These are often heavily overmarked with many classes of imagery. As available surfaces the stones are comparable to the open walls of a cave, but because they were marked within the confines of the habitation, rather than in a "sanctuary" cave, they often contain different ranges of imagery and use. They represent an important subset of the Upper Paleolithic corpus, often documenting unique and idiosyncratic *subjective* contents and symboling modes.

At the French Magdalenian site of La Marche (Vienne) one finds engraved "portraits" of humans (infants, children, young men or women, mature females, a "praying" woman, an old man, a dancing male, etc.) (Pales and St. Péreuse 1976). These human images are among the rarest in the Upper Paleolithic. With these human images one finds the more traditional animal images; horse, bear, lion, etc. Significantly, the La Marche stones and the images themselves were often heavily overmarked as though in a ritual use of both the images and the stones. One delicate profile is deeply overmarked both on the face and on the stone around the face suggesting that the image was made for this ritual use (fig. 14a, b). By contrast, at the Magdalenian site of Gönnersdorf in Germany hundreds of schematic female images made in the Magdalenian buttocks style are incised on slate plaquettes; but there are no human "portraits". Again, many of these images were ritually overmarked or reengraved, as they were overmarked on limestone slabs at the French site of Lalinde (Marshack 1976, 1991a). One stone from Gönnersdorf has the engraving of an overmarked female giving birth to an infant or fetus attached to it by an umbilical cord (Marshack 1975); the image is associated with two horse heads. An image that may be comparable occurs on a limestone fragment from the French Upper Perigordian site of Laussel. A head and shoulders seem to emanate from a female, suggesting a birth. Images of birthing are, of course, known in the ethnographic and later archaeological record, the most famous being that of the seated "goddess" or mother figure flanked by lions from the early neolithic site of Çatal Hüyük in Anatolia (Mellaart 1967). The assumption has always been that the image was typically neolithic and was related metaphorically to the renewal of animal, plant and human life. How are we to "read" comparable imagery in the hunting-gathering stage of the European Upper Paleolithic? At Laussel and Gönnersdorf one also finds engraved images of the vulva and the phallus. Both images occur ethnographically and archaeologically throughout the world. Images of "birth" are rare in the Upper Paleolithic, but vulvas and phalluses are relatively common. We therefore have a problem, one that again raises issues that are in large measure subjective. Did images of birth represent a particular ritual moment at these two Upper Paleolithic sites, involving the depiction of an actual birth, while the

vulvas and phalluses were images that referred to related but somewhat different concepts ? Concepts related to a "nonbiological" awareness of fertilization ? At the Magdalenian site of Enlène (Ariège), part of the group of Cavernes du Volp, Bégouën *et al* (1984-85 : 66-69) have documented the engraving of a human copulation on a stone plaquette; the crude scene is in close association with a magnificent Magdalenian engraving of a bison. Did the human scene of "copulation" and the bison represent part of a conceptual whole, even if the two images, one realistic and the other schematic, crude and quickly made, were engraved at different times and for different reasons ? "Feminine" and sexual images in relation to animals are not uncommon in the Upper Paleolithic. An engraving on a fragment of bone from the Magdalenian site of Laugerie Basse (Dordogne) depicts a pregnant naked female lying beneath a standing reindeer bull with the phallus clearly rendered. From La Madeleine in the Dordogne there comes a fragment of bone with the engraving of two cojoined phalluses, one inside a vulva and the other in contact with a "sacrificed" or decontextualized bear head (fig. 15 a, b) (Marshack 1972 : 333; 1986b: 114). There is an emanation from the bear head to the large phallus. Such images raise questions which have not often been considered in descriptions or interpretations of the Upper Paleolithic corpus. How are they to be classified or considered ? Are they "art", depiction, metaphor, mythologized equations ? Was the La Madeleine composition made as part of a ritual moment ? Why and when, in fact, was the image made ? By whom ? How does one eliminate the subjective from our attempt at description ? It is perhaps significant as well that compositions of this type do not occur on the walls of the decorated caves. We seem to have, therefore, a class of symbolic depiction and composition limited to the habitation and apparently made at times that were symbolically and ritually different from those involved in the production and use of the cave images.

It may be of interest that in other regions, i.e. in Central, East and Mediterranean Europe, the variability of symbolic materials and traditions is also great, but the iconography is nevertheless often dramatically different from that in Western Europe (Marshack 1970, 1977, 1979, 1991a; Vandiver *et al* 1989). The regional differences and similarities in symboling traditions and iconographies present us with problems not only of documentation and categorization but with possible differences in the "subjectivity" of the artisans and the contexts, and with differences in the production, time, and uses of the imagery. This compounds the difficulty for the analyst attempting to be "objective" in terms of the descriptive categories he or she has created. I touched on one such regional difference in my description of the female breast images from Dolni Vestonice in Central Europe (See also Marshack 1979, 1991a). There are more difficult problems.

I have suggested the presence of "notation" in the Upper Paleolithic (Marshack 1972a, b, 1991b), indicating that no two instances of notation are alike. These accumulations apparently represented the problem-solving, conceptual efforts of a specialized elite, often apparently representing the *ad hoc* strategies of individuals who were keeping a record of the passage of time and the seasons. These individuals were presumably "specialists" involved in scheduling the economic and ritual sequences of the culture. It is interesting that all the notations document different personal modes and strategies of marking and accumulation, i.e. problem-solving variations within what seems to have been a widely known tradition. Only two examples of notation represent near "mirror" images and these come from a single Magdalenian level at the site of Le Placard (Marshack 1972a : 146-165) and were probably made by the same hand. That there was a widespread and long-enduring tradition of notation but no uniform "style" notation is

significant for our discussion of "subjective" variability. It goes to the heart of the nature of symbolic "style" and symbolic "variability" and the problem of subjectivity, on the one hand, and stylistic constraints on the maker, on the other hand. And this goes to the heart of the difficulties in any effort at "objective" analysis.

One may perhaps assume that as societies or cultures become more complex and politicized, moving from groups of nuclear families with kinship networks, to chiefdoms, villages, and to temples with priestly hierarchies, then to city-states, etc., that there would tend to be an increase in forms of ritual or political centralized authority, resulting in officially sanctioned standardized styles of symbolic production and use. We can perhaps further assume that the idiosyncratic, individual "subjective" variability found in the Upper Paleolithic symbolic materials may therefore represent an early hunting-gatherer stage in symbolic culture and image production and use, despite widely dispersed general concepts, skills and technologies. In the Upper Paleolithic we not only have regional, site, and temporal styles, but we also have the idiosyncratic "subjective" work of highly skilled specialists and, at the same time, often in the same site and level, the evidence of image production and symbolic ritual behavior by less skilled individuals. There is voluminous ethnographic evidence that such subjective differences in artistic and symbolic production and skill, and such variability in the times of image and symbol production and use, are common in what are termed "shamanic" cultures. Whether the Upper Paleolithic variability suggests such an early "shamanic" stage of symbolic specialization, coexisting with other more private "subjective" forms of symbolic, cultural expression, will be discussed in other papers.

The problem of "objectivity" in symbolic analysis, then, like the problem of subjectivity and variability in symbol production and use, needs to be addressed more profoundly and more carefully, both theoretically and methodologically.

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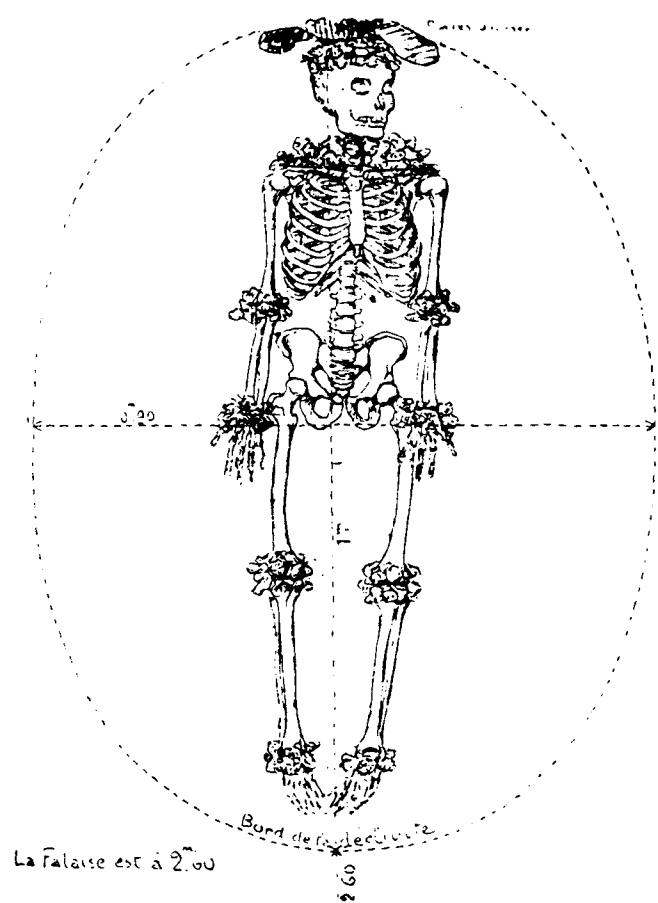


Fig. 1. The original drawing of the child buried at La Madeleine with heavily decorated garments and personal adornments. (After Capitan and Peyrony 1928).



Fig. 2a. Sungir, the USSR. Rendition of the elaborate burial of an adult male wearing beaded garments, bracelets of mammoth ivory and a beaded cap. More than 1,500 beads were carved. Compare to the decorations on the La Madeleine child.



Fig. 2b. Sungir, the USSR. The head of one of the buried adults showing the beaded cap. Compare this cap to the beaded cap of the La Madeleine child (Photo Bader).

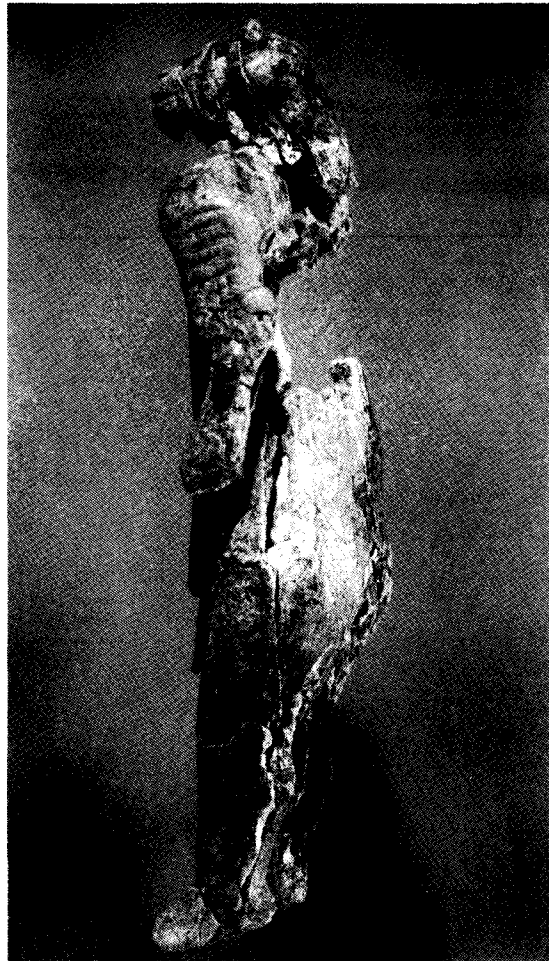


FIGURE 3

Hohlenstein-Stadel, Germany.

Mammoth ivory carving of a lion-headed therianthrope. Aurignacian.



Fig. 4a. La Pasiega, Spain. Some of the signs accumulated in the narrow gallery. The four signs are made with different ochres and are attached to each other either by juxtaposition or lines of connection.



Fig. 4b. La Pasiega, Spain. Detail of some of the signs indicating the differences in the paints used, the juxtaposition of signs, the lines of connection, etc. In Lascaux the addition to earlier signs and the juxtaposition of elements took a different regional form.

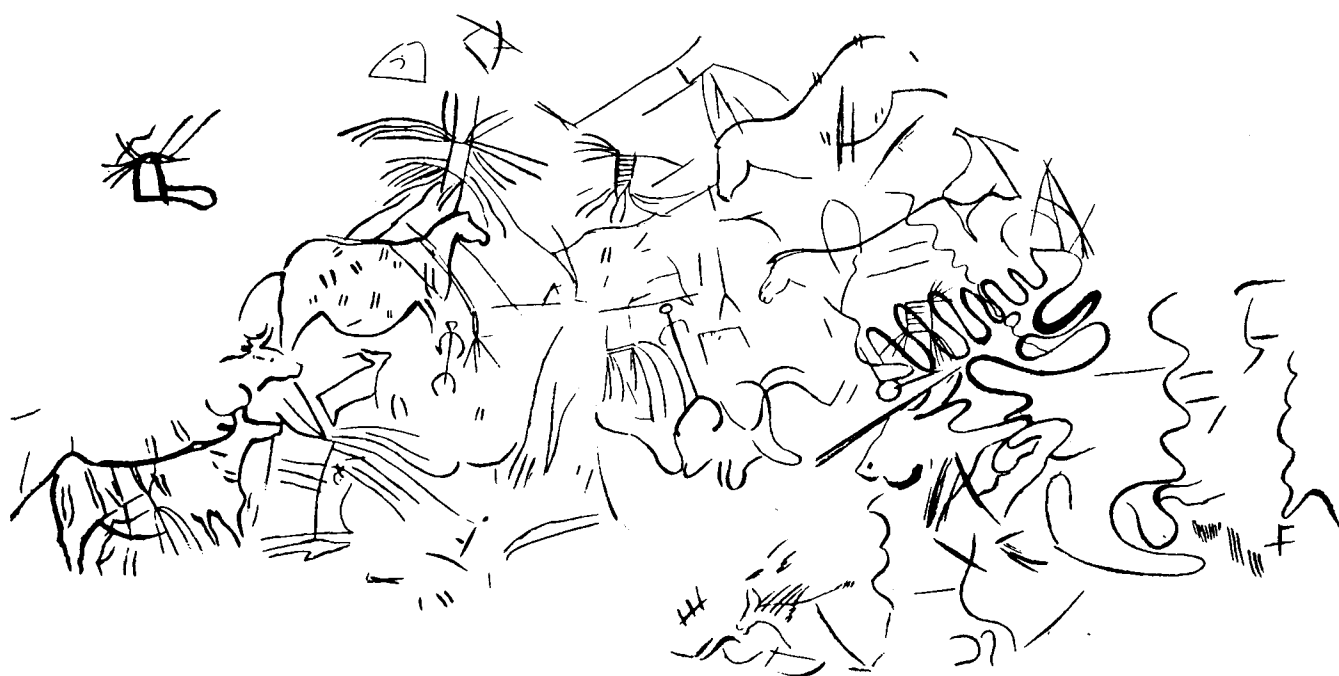


Fig. 5. La Pileta (Málaga), Spain. Schematic rendition of the painted images on the wall of the tiny sanctuary, indicating the overmarked animals, the signs at the top and the accumulation of serpentine “macaronis” at the right. (After Breuil, Obermaier and Verner 1915).



Fig. 6. La Pileta, Spain. Small horse in black paint with double finger marks in red and black paint made by different persons with fingers of different size and spacing. The repeated ritual use of an animal image is common in both home sites and caves in the Upper Paleolithic of Western Europe.

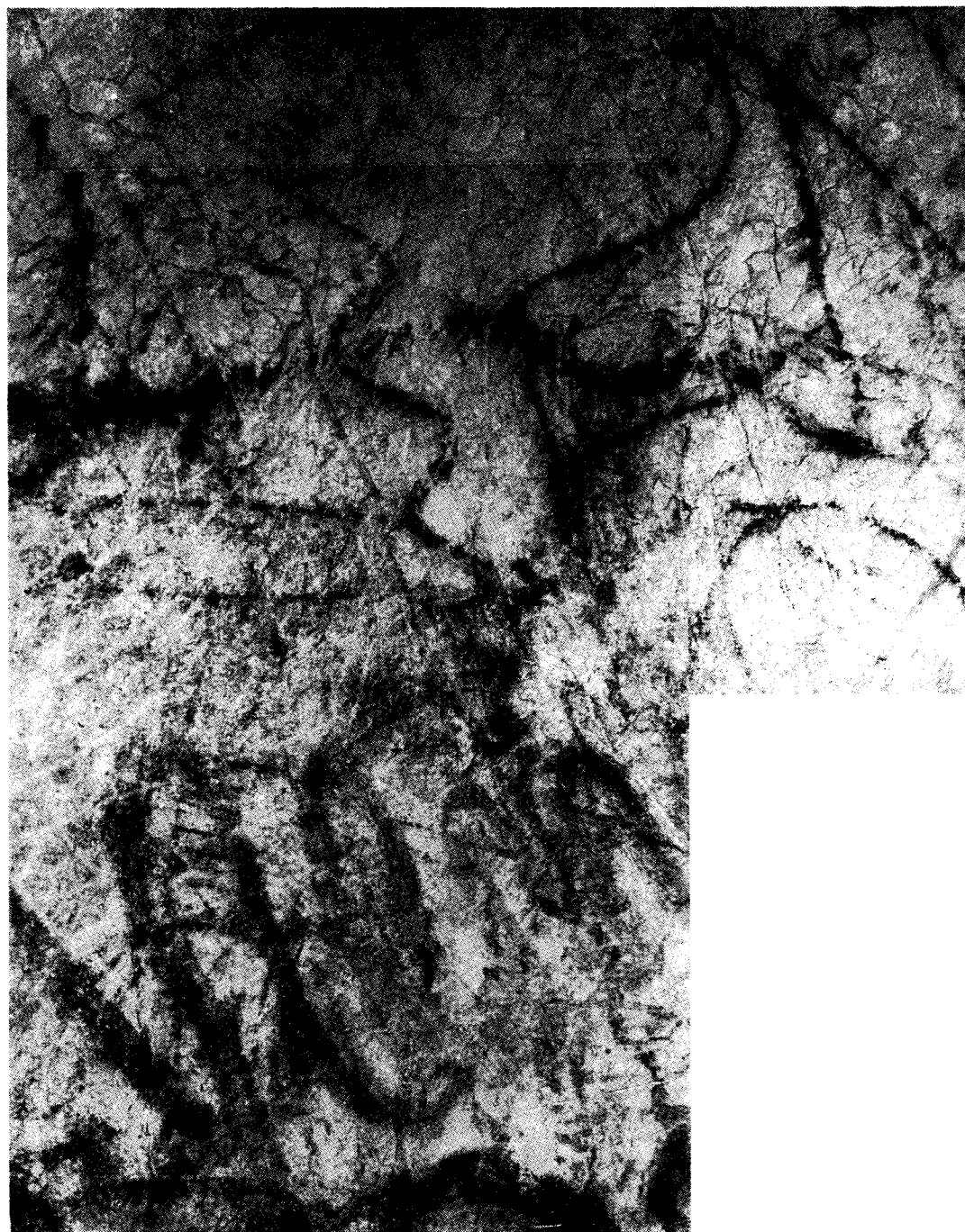


Fig. 7a. La Pileta. Detail of some of the painted macaroni sections made by black crayons and paints of different hardness and intensity, indicating a periodic ritual marking and accumulation of the serpentine motif.

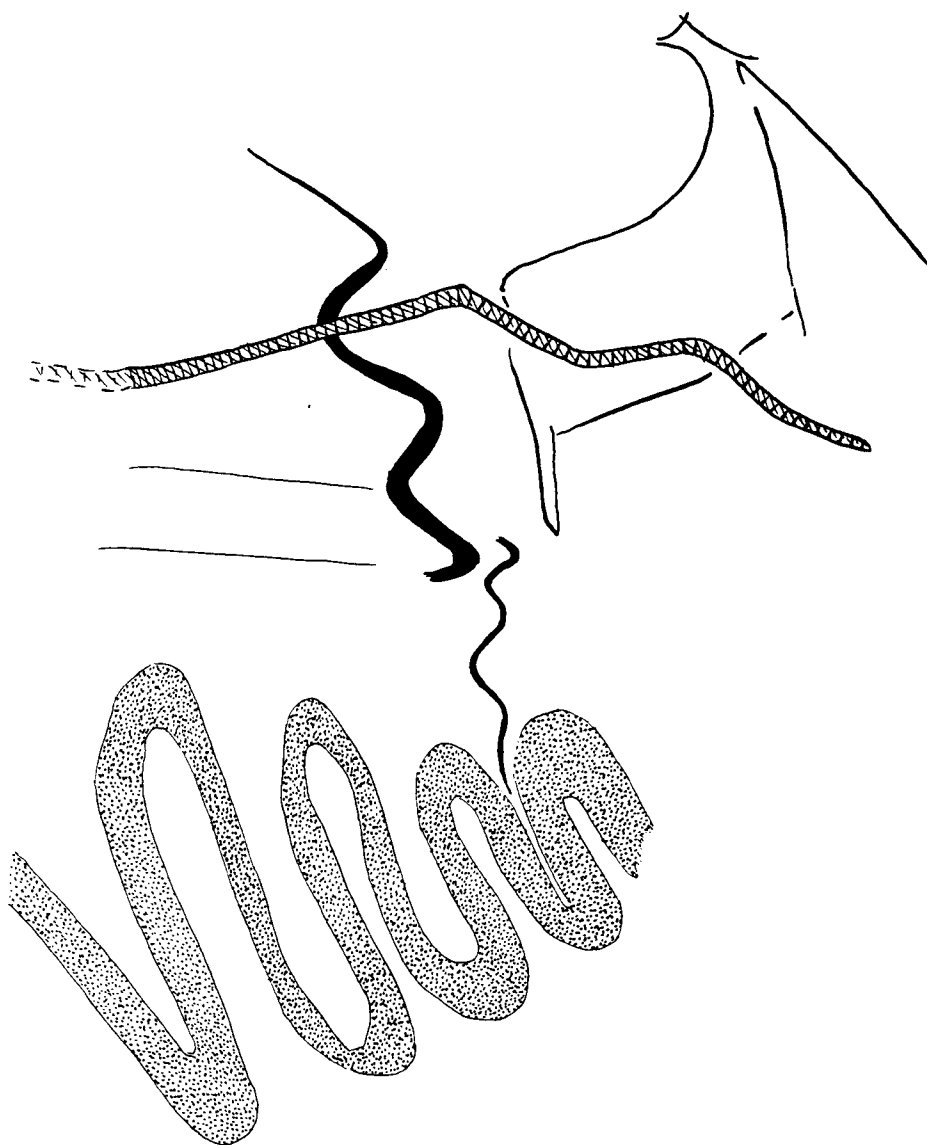


Fig. 7b. Schematic rendition of the photographic detail, Fig. 7a, indicating four serpentine macaronis sections made by different black paints and crayons.



Fig. 8. La Pileta. Close-up detail of the wall of the painted sanctuary in side light, indicating a marking of the wall with fingers and sticks.

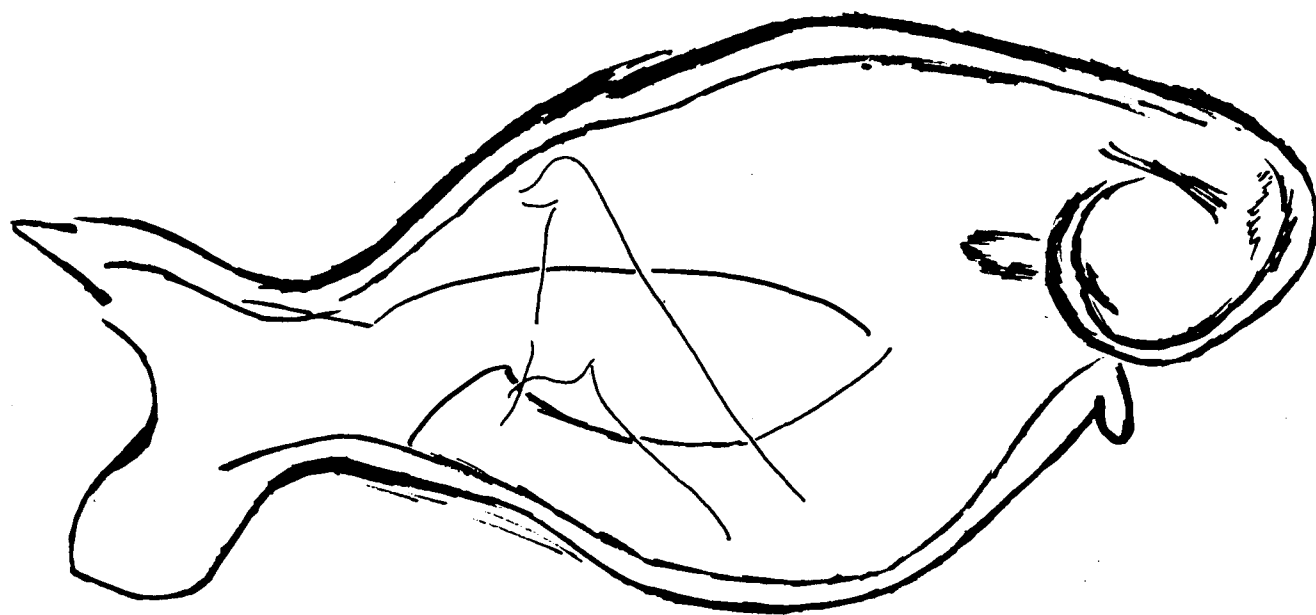


Fig. 9. Schematic rendition of a large fish in black paint that has been renewed with the outline of a second, inner outline and reused by the painting of a small seal and a third schematic fish oval.

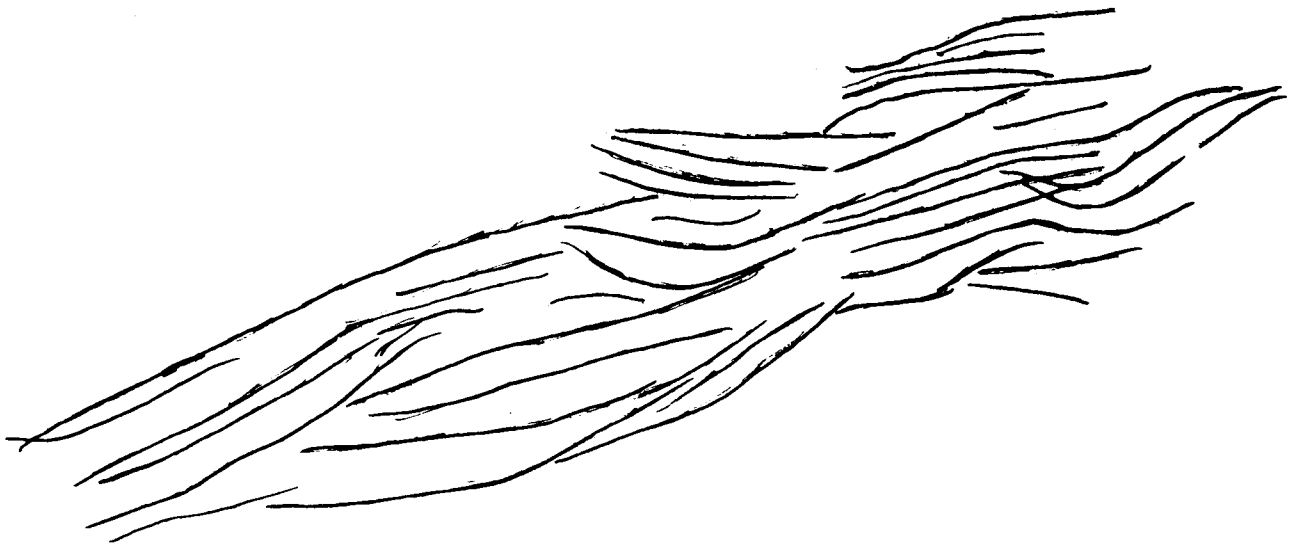
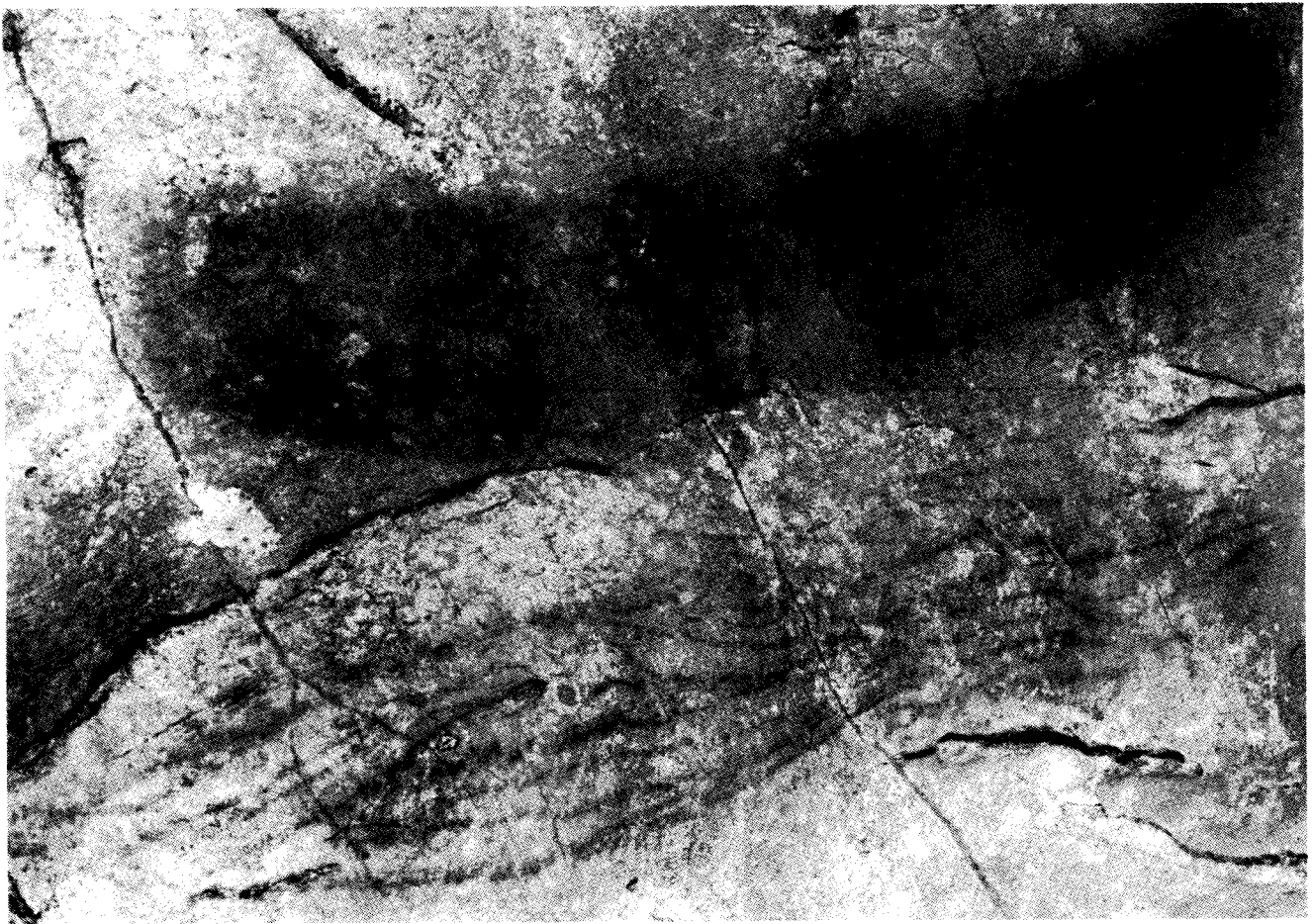


Fig. 10. Castillo (Santander), Spain. A painted red "macaroni" stream associated with a red tectiform. Variations of the "macaroni" band and stream tradition occur in many of the major caves.



Fig. 11. Altamira (Santander), Spain. Schematic rendition of the engraved "comets" and bands among painted signs. The "comet" as a variant of the macaroni/band tradition occurs as far as the Romanellian in Italy and the Mesolithic of Scandinavia. Evolved variants are found in Lascaux. (After Breuil and Obermaier 1935).



Fig. 12. Lascaux (Dordogne), France. Schematic rendition of some of the engraved "comet" and band motifs in the chamber of engravings at Lascaux. It is clear that band sections have been added to an original "comet" form. Compare to a variant of the tradition in Altamira, Fig. 11 (After A. Glory in Leroi-Gourhan and Allain 1979).



Fig. 13. Tuc d'Audoubert. An incised "P" sign near the cave entrance that has been renewed by the addition of a second arc. Another "P" sign is at the right. The overmarked or "killed" bison is below.



Fig. 14a – Tuc d'Audoubert. Close-up detail of a "P" sign that has been renewed or reused with an added arc made by a different point. (Photo J. Vertut)



Fig. 14 a. b. La Marche (Vienne), France. Delicate head of a young adult incised on a limestone slate. Both the face and the stone are overmarked as though in ritual. (After Pales and de St. Pereuse 1976).

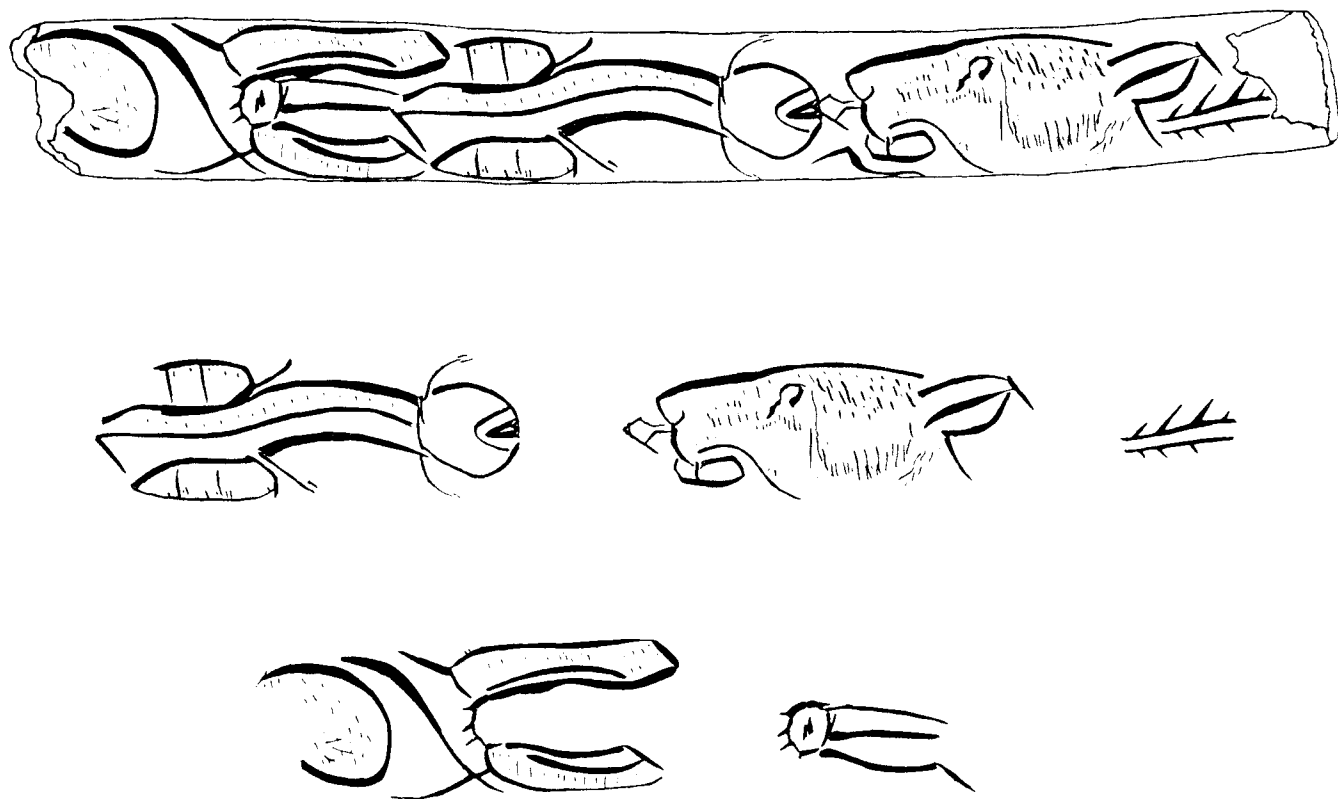


Fig. 15 a. b. La Madeleine (Dordogne), France. Fragment of antler incised with a composition containing two phalluses, one inserted into a vulva and the other in contact with a bear head. The “exploded” rendition indicates the separate elements or images making up the composition.

DOCUMENTATION ET FILTRES CULTURELS

A propos de la faune néolithique et protohistorique de l'Afrique du Nord

par

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Pour étudier la faune du Maghreb à la fin des temps préhistoriques, nous disposons de trois sources principales : celle provenant, bien entendu, des fouilles, celle tirée de l'art rupestre, et, pour les temps plus récents, celle donnée par des textes historiques. Mon intention est de dénoncer les faiblesses ou les anomalies que présente chacune de ces sources. Pour maintenir à cette étude des dimensions convenables, je me contenterai de n'examiner que les temps néolithiques et plus récents et de ne retenir, pour les temps protohistoriques, parmi les données littéraires, que celle d'Hérodote qui, dans les chapitres 191 et 192 du livre IV, nous donne un véritable bestiaire de la Libye. Je ne soumettrai à l'examen que les mammifères sauvages, sans m'interdire quelques allusions à d'autres espèces qui serviront à illustrer mon propos. Il sera donc question, en principe, des mammifères sauvages dans une tranche de temps qui s'étend de 5000 av. J.C. environ à 450 av. J.C.

D'emblée nous devons reconnaître que chacune des sources dont nous disposons est sujette à caution. Commençons par examiner les résultats des fouilles dans les gisements néolithiques. A priori, nous devrions avoir pleine confiance en ces résultats puisque les données sont fournies directement, sans intermédiaire, et que les documents osseux trouvés autour des foyers reflètent nécessairement l'alimentation des hommes qui ont occupé le site à la même époque. En fait, cette documentation est loin d'offrir toute garantie. Je n'insisterai pas sur des faits bien connus¹ : toute fouille ne rapporte pas exactement ce que l'homme a tué; celui-ci peut avoir consommé sur place les proies abattues, c'est particulièrement vrai pour les plus grosses, impossibles à transporter et qui sont dépecées à l'endroit même où elles ont été tuées. Les cinéastes du début du siècle ont maintes fois fixé sur la pellicule ces scènes extraordinaires qui suivaient, en Afrique centrale, la mise à mort d'un éléphant. Ce n'est pas la proie qu'on ramenait au village mais c'est l'ensemble de la population et même les groupes voisins qui se rendaient sur les lieux de l'abattage. Chacun repartait avec une part de viande dégoulinante, un privilège portait un tronçon de trompe ou un pied, morceaux particulièrement délicats; la carcasse et la presque totalité du squelette restaient sur place, après prélèvement de l'ivoire qui, matière

¹ On connaît de très nombreux exemples de consommation sur place du tout ou d'une partie des bêtes abattues par les chasseurs. Pour l'Afrique du Nord on retiendra le cas des Touaregs, cf. H. Lhote, *La Chasse chez les Touaregs*. Amiot-Dumont, Paris, 1951, p. 66. et M. Gast, *Alimentation des populations de l'Ahaggar. Etude ethnographique*. A.M.G., Paris, 1968, p. 132.

d'exportation, pouvait ne pas subsister dans l'habitat et servir de témoin archéologique de cette chasse. Faut-il s'étonner de la grande rareté des ossements d'éléphants dans les gisements néolithiques ? Ce proboscidien (*Loxodonta atlantica*) n'est signalé que dans quatre sites néolithiques, dont deux seulement au Maghreb (Pic des singes et Fort de l'eau).

Autre raison qui fait mettre en doute la représentativité des listes fauniques est ce que j'appellerai, après d'autres, la "fonte" taphonomique, phénomène bien connu qui, dans les sols acides, fait disparaître parfois la totalité des ossements. Bien qu'il soit extérieur au Maghreb, mais parce qu'il est très spectaculaire, je prendrai le cas du gisement corse de Terrina que j'ai fouillé de 1975 à 1981. Il s'agit d'une fosse qui renfermait un dépotoir et les déchets culinaires d'une famille chalcolithique peu avant 3000 av. J.-C. Le mobilier, du fond de la fosse jusqu'au sommet du comblement, présentait une incontestable unité culturelle, mais il n'en était pas de même des restes de faune. Les couches supérieures ne possédaient aucun ossement, sinon quelques dents de boeuf isolées, les couches moyennes n'en renfermaient guère plus et brusquement, au niveau d'un amas coquillier qui était à la base du remplissage, les ossements devenaient très abondants. Fallait-il penser que les habitants de Terrina, d'abord consommateurs d'huîtres, de cochons, de boeufs et dans une moindre mesure de moutons, étaient devenus subitement végétariens ? La réponse, négative bien sûr, fut donnée par la mesure de l'acidité du sol et des sédiments archéologiques : le Ph qui était de 5,28 dans la roche encaissante, de 5,45 et 5,85 dans les couches supérieure et moyenne, montait brusquement à 7,25 dans la couche à huîtres qui avait libéré suffisamment de carbonates pour permettre la conservation satisfaisante des os, même ceux de rongeurs de taille moyenne comme le *Lagomys sardus* qui, ailleurs en Corse, ne fut retrouvé que dans les sites des régions calcaires de Bonifacio ou de Saint-Florent.

Indépendamment de la question de conservation des ossements, il est un autre handicap qui frappe la recherche au Maghreb et encore plus au Sahara, c'est le petit nombre, pour ne pas dire l'absence, de zoologistes, ou mieux d'archéozoologistes, qui s'intéressent à ces problèmes. Les listes fauniques des gisements néolithiques sont donc très rares; c'est avec un bel optimisme que, pour établir quelques statistiques, j'ai retenu 35 gisements néolithiques dont la faune a fait l'objet d'une analyse plus ou moins sommaire; en réalité moins d'une demi-douzaine de ces faunes ont été véritablement étudiées et accompagnées de dénombrements d'individus et de mesures ostéologiques, comme l'exige la recherche moderne. Il s'agit, le plus souvent, de la simple liste des espèces reconnues par le fouilleur lui-même.

Nous devons aussi tenir compte des erreurs d'interprétation et nous montrer d'autant plus prudents que la proposition paraît intéressante. Ces erreurs sont parfois cocasses; en voici un exemple personnel : j'avais soumis à de nombreux spécialistes de la flore des savanes africaines, des fragments rutilants, vernissés, courbes et striés que j'avais recueillis dans le gisement d'Amekni (Hoggar) ; aucun n'avait réussi à déterminer à quelle espèce végétale appartenaient ces gousses ou ces fruits, et pour cause, il s'agissait de fragments d'incisives d'un gros rongeur, l'aulacode ou *Thryonomys*, qui furent identifiées par Th. Monod².

Ces différents inconvénients peuvent être plus ou moins corrigés, il faut s'en souvenir au moment de l'exploitation des données. Mais il est un autre facteur sur lequel nous n'avons

² G. Camps, *Amekni. Néolithique ancien du Hoggar*. Paris, Mémoire du CRAPE, n° V 1969, 230 p. (175). Voir aussi Th. Monod. "A propos d'un alaucode (*Thryonomys*) du gisement d'Amekni (Ahaggar)". *Bulletin de l'I.F.A.N.*, t. 23, sér. A, n° 2, 1970 : 531-550.

aucune action; il nous échappe complètement puisqu'il s'agit du comportement même de l'homme préhistorique. Il faut savoir que la faune, même parfaitement étudiée, d'un gisement préhistorique n'est pas le reflet exact de la faune sauvage de l'époque mais seulement celui d'un tableau de chasse, et encore doit-on tenir compte de l'abattage suivi d'un dépeçage sur place, sans transport, des plus gros mammifères que nous avons déjà signalé *supra*. L'homme préhistorique, au moins dès le Paléolithique supérieur, pratiquait une chasse sélective, parfois presque exclusive comme celle du renne par les Magdaléniens français ³, du mouflon par leurs contemporains ibéromaurusiens sur le littoral de Petite Kabylie ⁴, ou de l'antilope bubale par les Capsiens de l'Algérie orientale et de Tunisie ⁵. De fait, des animaux que nous savons présents au Maghreb puisqu'ils figurent dans l'art rupestre, ne sont jamais retrouvés dans les gisements; ce serait le cas de l'autruche si l'homme n'avait utilisé la coquille de ses oeufs. Le rhinocéros n'est présent que dans deux gisements maghrébins : Dar es Soltane et le Damous el Ahmar, et dans deux sites du littoral atlantique du Sahara ; or, cette espèce est représentée dans 8 stations d'art rupestre du Sud algérois, dans 11 du Sud oranais et dans 58 du Sud marocain. Un tel dénombrement est impossible pour les régions sahariennes mais il faut savoir que le rhinocéros est représenté 119 fois dans le seul Oued Djerat (Tassili n'Ajjer), ce qui le met en tête de toutes les espèces sauvages figurées dans cette vallée ⁶. Une scène célèbre de l'Oued Mathendouch révèle cependant que cet animal, si rarement trouvé dans les gisements, était réellement chassé. Rappelons le cas de l'éléphant encore plus fréquemment représenté, et plus régulièrement que le rhinocéros (son indice de représentativité, calculé sur le nombre de stations, est de 26,2) ⁷. Il n'est présent, nous l'avons vu, que dans 2 gisements. Le sanglier, présent dans 6 gisements épipaléolithiques seulement, l'est dans 19 gisements néolithiques (indice de fréquence 54,3), ce taux pourrait faire penser à un début de domestication, mais cela paraît peu probable ⁸.

³ A. Leroi-Gourhan avait déjà souligné la contradiction entre l'abondance des restes de renne dans les gisements magdaléniens et la rareté relative de la représentation de cet animal dans l'art pariétal. A Lascaux, les débris osseux provenant de rennes représentent 88,7 % des fragments cf. A. Bouchud, *La faune de Lascaux*, dans *Lascaux inconnu*, XIIe supplément à Gallia, Paris, C.N.R.S., 1979.

⁴ C. Arambourg, M. Boule, H. Vallois, R. Verneau, *Les grottes des Beni-Seghoual (Algérie)*, (Archives de l'I.P.H.), n° 13, Paris, 1934. Les auteurs avaient déjà noté l'abondance extrême des ossements de mouflon dans les abris de Tamar Hat et d'Afalou Bou Rhumel. Plus récemment, E. Saxon, "Results of recent investigations à Tamar Hat", *Libyca*, XXII, 1976 : 49-91, avait tenté d'expliquer cette abondance par une possible domestication du mouflon, *contra* G. Camps et J. Morel, "Alimentation des Paléoberbères" dans *Encyclopédie berbère*, fasc. IV, 1986 : 473-483.

⁵ Sur la fréquence très grande des restes d'antilope bubale dans les gisements capsien voir G. Camps et J. Morel, *I.I.* ; J. Morel, "La faune de l'escargotière de Dra Mta el Ma el Abiod (sud algérien)" dans *l'Anthropologie*, t. 78, 1974 : 299-320 et H. Camps-Fabrer, *Un gisement capsien de faciès sétifien. Medjez II, El Eulma (Algérie)*, Etudes d'Antiquités africaines, 1975, Paris, CNRS. (J. Bouchud, *La Faune de Medjez II* : 377-391). Dans les niveaux anciens de ce gisement les restes d'antilope bubale représentent 68 % des ossements conservés.

⁶ Ces chiffres sont donnés par H. Lhote, *Les gravures rupestres de l'Oued Djerat (Tassili n'Ajjer)*. Mémoire du CRAPE, t. XXV, 1 et 2. Paris, 1976.

⁷ Nous appelons indice de représentation (IR), le rapport entre le nombre de stations rupestres retenues soit 145 et le nombre de celles où l'espèce est représentée. L'indice de fréquence (IF) est le rapport entre le nombre de gisements dont la faune a été étudiée soit 35, et le nombre de ceux dans lesquels l'espèce a été reconnue.

⁸ En Provence, la domestication du porc ne paraît assurée qu'à la fin du Néolithique ancien ; c'est du moins l'opinion de D. Helmer, "Les suidés du Cardial, sangliers ou cochons ?" dans *Premières communautés paysannes en Méditerranée occidentale*, Paris, CNRS, 1987 : 215-220 ; en Corse, en revanche, bien des arguments sont en faveur d'une domestication très ancienne de cet animal qui aurait été introduit par l'Homme cf. J.D. Vigne, *Les mammifères terrestres du post-Glaciaire de Corse. Etude archéologique*. XXVIe suppl. à *Gallia Préhistoire*, Paris, C.N.R.S., 1988.

L'examen des données issues des fouilles nous laisse donc assez sceptique. Heureusement l'art rupestre, qui est une source de documentation exceptionnelle par le nombre des sites, des représentations et la qualité de beaucoup d'entre elles, peut compléter ou corriger les données proprement archéologiques. Dans le seul Atlas saharien algérien, en ajoutant les régions périphériques de Tiaret et de Constantine et les deux stations connues de Tunisie, on dénombre 145 stations qui ont fait l'objet de corpus et de relevés à peu près complets⁹; dans le Sud marocain on connaît 243 stations rupestres¹⁰ mais seule une faible partie d'entre elles ont été publiées; il n'est donc pas possible de tenter un dénombrement ou une statistique. Quant au Sahara, c'est par centaines qu'il faudrait compter les stations de gravures et de peintures entre l'Atlantique et les contreforts du Tibesti. Ces chiffres doivent être mis en parallèle avec les 40 listes de faune des gisements néolithiques (Sahara compris) que nous avons pu réunir. Nous disposons donc, grâce à l'art rupestre, d'une masse énorme de documents, mais notre enthousiasme est rapidement tempéré par l'examen de ces représentations; si beaucoup de figures gravées et surtout peintes sont de véritables chefs-d'œuvre de précision anatomique qui permet d'identifier l'espèce sans la moindre hésitation, trop nombreuses sont les représentations stylisées (style de Tazina) ou schématisées à l'extrême au point de rendre impossible la moindre identification du quadrupède. Ainsi la grande masse des antilopes gravées sur les rochers de l'Atlas constitue un ensemble indifférencié, alors que les figures peintes du Tassili sont d'une identification presque toujours certaine. On comprend certes qu'une gravure peu précise ne permette pas de différencier un cob d'un hippotrague, un addax d'un oryx, mais prenons le cas de l'antilope bubale (*Boselaphus probubalis*); ce ruminant, très abondant au Maghreb jusqu'à la fin du XVIII^e siècle, ne peut être confondu avec aucun autre, bien qu'il ait été appelé *begar el ouach*, "vache sauvage"¹¹. Quatre traits distinctifs permettent de l'identifier : son garrot très saillant, sa croupe inclinée, ses cornes petites et anguleuses, sa face très longue et étroite. L'ensemble est particulièrement disgracieux. Cette antilope fut pendant toute la durée de l'Holocène très abondante; elle est présente dans 29 gisements épipaléolithiques sur 35 étudiés, elle l'est encore dans 23 gisements néolithiques sur 35 dont la faune est connue (indice de fréquence 51,4), ce qui la place parmi les animaux les plus chassés, or on ne la reconnaît sûrement que dans 8 stations rupestres (indice de représentativité 5,5). Aussi surprenant est le cas du mouflon à manchettes (*Ammotragus lervia*) ; très abondant dans les gisements (indice de fréquence 48,6), il n'est représenté que dans 6 stations de la région de Constantine et dans 2 de la région de Djelfa (indice de représentativité 5,6). Dans l'état de nos connaissances, il est totalement absent dans les œuvres rupestres néolithiques du Djebel Amour, du Mont des Ksours et de l'Atlas marocain, régions où il subsiste pourtant jusqu'à nos jours. Il importe de noter, en outre, que les 35 mouflons de la région à l'est de Constantine sont tous peints et parfaitement

⁹ Les publications relatives à l'art rupestre de l'Atlas sont bien trop nombreuses pour être toutes citées ici ; depuis les premiers travaux de G.B. M. Flamand, de L. Frobenius et de R. Vaufray, les ouvrages qui apportent le plus grand nombre de documents, bien qu'ils n'échappent pas toujours à la critique, sont ceux de H. Lhote, *Les gravures rupestres du Sud-Oranais*, Mémoire du CRAPE, n° XVI, Paris, 1970. Id. *Les gravures rupestres de l'Atlas saharien ; monts des Ouled-Nail et région de Djelfa*. Pour la région à l'est de Constantine voir G. et L. Lefebvre, *Corpus des gravures et des peintures rupestres de la région de Constantine*. Mémoire du CRAPE, n° VII, Paris, 1967.

¹⁰ Un premier inventaire des stations du Maroc, dû à A. Simoneau, a été publié sans nom d'auteur, en 1977, sous le titre : *Catalogue des Sites rupestres du Sud-marocain*. Il complète géographiquement le *Corpus des Gravures rupestres du Grand Atlas*, Rabat 1959-1961 de J. Malhomme.

¹¹ C'est ainsi qu'en 1766, l'amiral de Bauffremont se vit offrir par le bey de Tunis "une vache sauvage et son veau" qui étaient vraisemblablement une femelle d'antilope bubale et son petit, cf. M. Chirac, *Journal de campagne de l'amiral de Bauffremont, prince de Listenois, dans les pays barbaresques* (1766), Paris, CNRS, 1981.

reconnaissables. Contrairement à ce qui se passe dans l'Atlas saharien, cet animal est très fréquemment représenté dans les peintures du Tassili n'Ajjer; cette différence n'a pas d'explication écologique puisque le mouflon vit encore dans les deux régions, elle ne peut être que d'ordre culturel.

Plus surprenant est le cas du sanglier (*Sus Scrofa*) dont nous avons noté la fréquence dans les gisements néolithiques (indice 54,3) ; or cet animal n'est figuré que 4 fois dans l'art rupestre (indice de représentativité 2,7). Or, il n'est pas sans intérêt de remarquer que notre source littéraire la plus ancienne, Hérodote (IV, 192), affirme, à tort, que le sanglier n'existe pas en Afrique. Ce que répétèrent, sans esprit critique, Pline l'Ancien (VIII, 228) et Elien (XVII, 10). Il semble bien qu'un filtre culturel ait joué contre cet animal. Il est aussi des animaux qui ne sont jamais représentés sans que l'on sache les raisons de cet ostracisme. Certains sont assez rares dans la nature comme le cerf de Barbarie, présent cependant dans 4 gisements; ignoré des artistes néolithiques, il l'est également d'Hérodote qui, comme pour le sanglier, prétend qu'il ne vit pas en Afrique (IV, 192). L'ours (*Ursus arctos*), cité lui comme animal africain par Hérodote et reconnu dans 6 gisements, n'est jamais représenté, de même que l'hyène dont les deux espèces (*Hyaena striata* et *H. crocuta*) sont présentes cependant dans 5 gisements (IF 17). Les singes du genre macaque (magot, *Macaca inuus*), vivant encore dans l'Atlas tellien et le Haut Atlas marocain, ne sont pas plus représentés dans l'art rupestre; on ne sait pourquoi.

Au terme de ce premier examen qu'il faudrait affiner et compléter, il apparaît clairement que l'art rupestre donne une image inexacte de la faune holocène; il ignore totalement certaines espèces à vrai dire peu communes (ours, cerf) ou plus fréquentes (hyène, magot), il en dédaigne d'autres comme le sanglier (IR 2,75), l'antilope bubale (IR 5,5) et le mouflon (IR 5,5) dont les très faibles indices de représentativité s'opposent aux plus fortes fréquences dans les gisements (IF 54,3; 51,4; 48,6). Inversement, les espèces le plus souvent représentées, le buffle antique (*Pelorovis* = *Homoioceras antiquus*) dont l'indice de représentativité est de 29, l'éléphant (*Loxodonta atlantica*) IR 26,2 et le rhinocéros (*Ceratotherium simum*) IR 13,2 (qui monterait à 20 si on prenait en compte les stations marocaines) sont très rares dans les gisements. En bref, les animaux de grand format et les plus pesants sont sur-représentés dans l'art rupestre et les animaux les plus chassés sont sous-représentés. Il existe donc un important filtre d'ordre culturel (et peut-être culturel car ces figurations n'étaient pas sans signification religieuse pour ceux qui les gravaient). Le fait est indiscutable pour celles qui figurent, par exemple, des béliers coiffés d'un bonnet sphérique et parés de colliers qui sont conduits au sacrifice par un homme en attitude d'orant¹².

Ainsi, pas plus que les données archéologiques, l'art rupestre ne fournit une image réelle et proportionnée de la faune holocène. Voyons si la littérature, à peine plus récente, nous apporte une documentation plus valable. Nous avons la chance de trouver dans le texte d'Hérodote, au milieu du Ve siècle av. J.C., deux listes d'animaux sauvages qui vivaient en Libye, c'est-à-dire dans le nord de l'Afrique, textes qui furent admirablement commentés par S. Gsell en 1916 et dont j'utilise la traduction de préférence à celle de Legrand dans les *Belles Lettres* (1956) ou à celle de A. Barguet, parue en 1982 dans la *Bibliothèque de la Pléiade*, qui me paraissent

¹² Sur le bélier à sphéroïde, la dernière synthèse est celle de G. Camps, "Un thème religieux dans l'art rupestre nord-africain ; le bélier à sphéroïde" dans *Studi in onore di Salvatore Puglisi*, Rome, 1985 : 345-387. Voir aussi *Id.*, "Scènes de caractère religieux dans l'art rupestre de l'Afrique du Nord et du Sahara". *Mélanges P. Levêque*, Besançon, 1988 : 65-82.

nettement moins attentives aux questions africaines. Malgré les progrès réalisés dans la connaissance des cultures protohistoriques des Libyens, le commentaire de S. Gsell a fort peu vieilli ¹³. L'une des deux listes données par Hérodote concerne la Libye des Nomades, correspondant à l'Etat lybien actuel et aux confins tunisiens, l'autre, la Libye des cultivateurs, c'est-à-dire les régions telliennes et plus spécialement le Sahel tunisien et son arrière-pays montagneux. Hérodote connaît bien mieux la première que la seconde sur laquelle il ne nous donne que de vagues renseignements. De la faune mammalienne des régions non sahariennes, il cite le lion, l'éléphant, l'ours et l'âne porteur de cornes qui est très vraisemblablement une antilope chevaline du genre hippotrague. L'hippotrague, connu à l'état fossile au Maghreb, vit encore dans la partie méridionale du Sahara. Hérodote mentionne aussi, mais dans un autre chapitre les singes, très nombreux, qui sont chassés et consommés par les Gyzantes que l'on situe volontiers dans le Zaghouan, ils étaient d'ailleurs nommés Zygantes par Hécatee de Milet. Il ajoute des animaux plus ou moins mythiques, serpents gigantesques et monstres à tête de chien ou sans tête du tout, auxquels il ne croit pas, mais qu'il mentionne parce qu'ils ont été nommés par ses informateurs. Comme on le voit cette liste est bien courte et désordonnée; Hérodote ne tente même pas de citer côte à côte les animaux qui se ressemblent ou vivent dans le même biotope. De plus, elle pêche gravement par défaut; on note des absences surprenantes, tant ces animaux sont caractéristiques du paysage faunique maghrébin : mouflon, chacal, âne sauvage. Hérodote ne cite pas non plus le cerf ni le sanglier, or si le premier a toujours été rare, ce n'est pas le cas pour le second; bien mieux, il précise, comme nous l'avons dit, qu'ils font totalement défaut dans toute la Libye, celle des Nomades comme celle des Sédentaires. La minceur de cette liste, les erreurs, le rajout d'animaux fabuleux reflètent l'ignorance générale d'Hérodote sur les Libyens cultivateurs dont il peut à peine citer trois tribus : Maxyes, Gyzantes et Zauèques.

Passant à la description de la faune du pays nomade, Hérodote se montre plus disert et plus précis. Voici les animaux qu'il cite : pygargues, zorcades, bubales, ânes sans cornes, oryes, bessaria, hyènes, porcs-épics, béliers sauvages, dictyes, chacals, panthères, boryes, crocodiles terrestres, autruches, petits serpents ayant une corne sur la tête, dipodes, zégéries, hérissons, galaï. Presque toutes ces espèces sont identifiables. Le pygargue n'est pas le rapace qui porte aujourd'hui ce nom mais la gazelle dama (appelée aussi mohor ou encore Biche Robert), très reconnaissable à sa "culotte blanche". Zorcades : il faut lire dorcades, les plus petites et les plus communes des gazelles (et non "chevreuil" qui est la traduction normale de "dorcas", mais qui n'a jamais vécu en Afrique). Par bubale, Hérodote désigne l'antilope si répandue au Maghreb, le *Boselaphus probubalis*. Les ânes qui ne boivent pas et n'ont pas de cornes, sont les ânes, onagres, dont l'absence était surprenante sur la liste précédente. Les ânes marrons actuels du Sahara ne sont pas toujours proches des points d'eau, car ils trouvent dans les coloquintes sauvages du désert, lorsqu'elles arrivent à maturité, la quantité d'eau suffisante à leur équilibre biologique, d'où l'assertion d'Hérodote qui peut paraître étonnante. Oryes est une mauvaise graphie pour oryx (*Oryx leucoryx*) qui n'a disparu du Sahara septentrional que depuis peu de temps. Ce bel animal vit aussi dans le désert arabe. S. Gsell commet une erreur en croyant que l'oryx n'était pas une espèce des pays arides, il n'y a aucune raison de le suivre lorsqu'il propose de remplacer oryx par addax ¹⁴. Hérodote dit que ses cornes servaient à fabriquer des

¹³ Pour un nouveau commentaire à la lumière des connaissances acquises depuis, on se reportera à G. Camps, "Pour une lecture naïve d'Hérodote. Les récits libyens (IV, 169-199)". *Histoire de l'Histoire*, n° 9, 1985 : 38-59.

¹⁴ S. Gsell, *Textes relatifs à l'Histoire de l'Afrique du Nord. Fascicule 1, Hérodote*, Alger-Paris, 1916 : 98. Bien qu'il ne dise pas expressément les raisons de cette correction, il est possible que le choix de Gsell se soit porté

“lyres phéniciennes”. Les bassaria sont de petits renards dont le nom est conservé chez les Coptes ¹⁵; il peut s'agir aussi bien du gracieux fennec que du renard saharien (*Vulpes Ruppelli*) qui est un peu plus petit que le renard commun et dont les oreilles sont moins démesurées que celles du fennec. Hyènes, chacals, porcs-épics ne méritent pas de commentaires, ce sont des animaux vivant encore au Sahara. La présence de la panthère peut surprendre car on l'attendait plutôt dans la Libye montagneuse et forestière que dans les zones arides, mais ils s'agit vraisemblablement du guépard que les anciens distinguaient mal de la panthère; or le guépard, qui ne chasse pas à l'affût, a besoin de vastes espaces dégagés pour poursuivre ses proies. Il vit actuellement au Sahara. Les béliers sauvages sont en réalité non pas des ovins mais des mouflons à manchette (*Ammotragus lervia*), espèce unique d'un genre qui ne peut être l'ancêtre ni des moutons ni des chèvres domestiques. Les dictyes et les boryes (à moins qu'il ne s'agisse pour ces derniers d'un doublet fautif d'oryes) résistent à toute identification.

Crocodile terrestre : cet animal est mal nommé mais fort bien décrit; il mesure, dit Hérodote, trois coudées et ressemble beaucoup au lézard; il s'agit du varan (*Varanus niloticus*) dont la longueur atteint 1,50 m, ce qui est supérieur aux dimensions données par Hérodote qui, pour une fois, ne peut être taxé d'exagération. Le serpent de petite taille qui porte une corne, n'est pas la vipère à cornes ou céraste, commune au Sahara, mais la vipère heurtante (*Bitis arietans*) qui possède une écaille saillante sur le museau. Hérodote mentionne “trois sortes de rats”, dont le hérisson qui est un insectivore et non un rongeur; les dipodes sont des gerboises, qui ne semblent posséder que deux pattes. Quant aux zégéries, Hérodote dit que leur nom est libyque et signifierait “(rat de) colline”, mais Gsell fait remarquer qu'une scolie de Dioscoride (IV, 123) précise que *zigar* est le nom punique d'une plante appelée en grec “*bounion*” (c'est-à-dire de colline). Je ne vois pas pourquoi il oppose Dioscoride à Hérodote, car l'adjectif “punique” reçut très vite une acception très large et signifia souvent “africain” et donc libyque ou paléoberbère; or on retrouve précisément le radical ZGR sous la forme féminine tazegg^Wart qui désigne, en kabyle, le jujubier sauvage qui est effectivement une plante de colline, mais on peut retrouver aussi dans le nom des zégéries un radical pan-berbère ZGRh qui signifie “rouge”. Nous avons donc deux identifications possibles, les zégéries seraient des “rats rouges” ou des “rats de colline” c'est-à-dire des rochers; dans le premier cas on songerait à l'écureuil terrestre (*Xerus getulicus*), dans le second au daman (*Procavia ruficeps*) ou mieux encore au goundi (*Ctenodactylus goundi*). Ma préférence va vers celui-ci car le goundi est plus répandu que l'écureuil fouisseur et vit en colonies plus nombreuses; il a, de plus, un pelage brun-rouge. Il est donc à la fois de rocher et de couleur rougeâtre. Enfin il a une chair très estimée et a donc plus de chance d'avoir retenu l'attention des informateurs d'Hérodote. Les galai sont des animaux qui prospèrent dans le pays où pousse le sylphium, ils ressemblent, dit Hérodote, à ceux qui vivent à Tartessos, dans le sud de l'Espagne. D'après Strabon (XVII, 3, 4), il s'agit d'un animal ayant l'aspect d'un chat mais avec un museau pointu, description qui convient parfaitement à la genette qui est le vivéridé le plus répandu en Afrique du Nord.

A la différence de la liste précédente, celle des animaux de la Libye des Nomades présente un certain ordre : les cinq premières espèces citées sont des herbivores, viennent ensuite des animaux agressifs, carnivores (renard, chacal, panthère), la place du porc-épic dans cette série

sur l'addax parce que cette antilope a des cornes lyrées, mais les bras d'une lyre, même phénicienne, ne sont pas nécessairement lyrés.

¹⁵ Hesychius reconnaît une origine africaine à ce nom. Voir S. Gsell, *Histoire ancienne de l'Afrique du Nord*, t. 1, 1913 : 312.

pourrait s'expliquer par ses piquants, et celle du mouflon par ses énormes cornes; Hérodote cite ensuite des animaux spécifiques : crocodile terrestre (varan), autruche, petit serpent à une corne (vipère heurtante), puis trois espèces de "rats" et enfin la genette.

Parlant des Garamantes, qui sont situés dans la même partie de la Libye et dont nous connaissons la localisation au Fezzan et aux confins du Tassili n'Ajjer, Hérodote écrit (traduction de S. Gsell) : "On trouve chez eux des boeufs qui paissent à reculons (*opisthonomoi*). Ils paissent ainsi parce qu'ils ont les cornes inclinées vers l'avant, ce qui les force à aller à reculons ... ils ne peuvent marcher en avant car les cornes s'enfonceraient dans la terre (IV, 183)". Comment prendre au sérieux une telle affirmation ? Quelle que soit l'inclinaison des cornes vers l'avant et leur longueur, leur disposition ne saurait être telle que l'animal fût contraint de brouter à reculons, d'autant plus que, privé d'incisive au maxillaire supérieur, il lui aurait été impossible de trancher l'herbe dans un mouvement d'avant en arrière. Ce récit paraît d'autant plus suspect qu'il rappelle le rapt des génisses d'Apollon par Hermès qui les contraignit à marcher à reculons pour tromper les éventuels poursuivants. Mais s'agit-il bien d'un conte issu de l'imagination fertile d'Hérodote ou de celle de ses informateurs ?

Une meilleure connaissance de l'art rupestre saharien nous permet aujourd'hui de suggérer une explication. Les Sahariens, particulièrement ceux qui fréquentaient les vallées du Tassili n'Ajjer, ont figuré d'importants troupeaux de boeufs domestiques de la variété *Bos africanus*, identique à celle de l'Egypte pharaonique. Or, parmi ces représentations si fidèles, on remarque certaines anomalies : des bêtes qui, comme sur les fresques égyptiennes, sont dépourvues de cornes et d'autres qui ont les cornes "flottantes" ou "ballantes". Il semble que ces animaux aient souffert soit de caries alimentaires, soit d'une anomalie d'origine génétique; il n'est pas impossible d'ailleurs que le "flottement" de la corne ait précédé sa chute, puisque dans le même troupeau on peut reconnaître des animaux à l'encornage normal, d'autres aux cornes ballantes, d'autres enfin dépourvus de cornes¹⁶. Quand on examine ces figures, on comprend que les bovins à cornes flottantes devaient être gênés pour brouter puisque celles-ci sont rabattues de part et d'autre du mufle. Penser qu'une telle disposition oblige l'animal à paître à reculons n'est pas une déduction si sottise. J'imagine volontiers quelque caravanier garamante décrivant dans un cabaret de Cyrène ou de Lepcis ces figurations de boeufs "dont les cornes inclinées vers l'avant les forcent à aller à reculons quand ils paissent". Le bruit en étant arrivé à Hérodote qui ne put, dans son "allégresse malicieuse"¹⁷, comme le dit joliment Mme de Romilly, résister à la tentation de nous transmettre l'historiette. Somme toute, Hérodote avait une âme de journaliste.

On retiendra donc, dans ces listes incomplètes de la faune africaine données par Hérodote, des notations justes bien que surprenantes (les ânes qui ne boivent pas, les boeufs opisthonomes), le souci de décrire par des images simples l'aspect des animaux (âne pourvu de cornes pour désigner l'animal que nous appelons hippotrague ou antilope chevaline, ou encore

¹⁶ Je ne pense pas que les cornes ballantes soient le résultat de déformations artificielles telles qu'elles étaient couramment pratiquées par les groupes pasteurs du Sahara oriental (on en connaît une centaine de cas sur les gravures du Tibesti) et du Sahara central (où elles sont plus rares). On sait que la déformation volontaire des cornes est une pratique courante chez les éleveurs de bovins du Haut Nil, chez les Nuer, les Dinka en particulier. Sur les cornes déformées des gravures sahariennes voir P. Huard, "Les cornes déformées sur les gravures rupestres du Sahara sud-oriental", dans *Travaux de l'Institut de Recherches sahariennes*, t. 18, 1959 : 109-131.

¹⁷ J. de Romilly, *Hérodote et Thucydide*, introduction aux œuvres complètes d'Hérodote et de Thucydide éditées dans la Bibliothèque de la Pléiade, Paris, 1964.

“Cul-blanc” la gazelle dama, les “deux-pattes” qui sont les gerboises), un certain intérêt philologique dans l'explication du nom des zégéries; il faut retenir la notation que je qualifierais de documentaire sur les genettes qui sont semblables à celles d'Espagne, ou sur les cornes des oryx qui servent à faire un certain type de lyre. Mais ces bonnes intentions ne sauraient suppléer la très mauvaise connaissance qu'Hérodote pouvait avoir de la Libye des laboureurs, en dehors du Sahel tunisien.

C'est donc à une conclusion pessimiste que nous arrivons au terme de cette présentation de la faune holocène; nous ne pouvons la saisir dans sa composition réelle; les données archéologiques sont discutables et révèlent plus le comportement de l'homme vis-à-vis de la faune qu'elles ne donnent une image réelle de celle-ci. L'art rupestre ne reproduit qu'une nature déformée par un filtre culturel qui n'a pas voulu représenter certaines espèces et a magnifié, jusqu'à l'obsession, certaines autres. Hérodote nous apporte des précisions parfois intéressantes mais sur une faune mammalienne bien pauvre et en grande partie ignorée. Remarquons, au passage qu'Hérodote ignore totalement l'existence de la girafe si abondante au Sahara, de même que l'hippopotame; quant au rhinocéros, connu au Maghreb, il avait déjà disparu de la partie nord du désert. Malgré l'imperfection de ces données nous devons cependant nous féliciter de ce que le Maghreb et le Sahara bénéficient de sources aussi diverses qui, malgré leur faiblesse, peuvent être corrélées, critiquées et complétées les unes par les autres. La préhistoire nord-africaine est, dans ce domaine, très avantagée par rapport à la préhistoire européenne qui, pour la même tranche de temps, ne dispose ni des données d'un art rupestre animalier, ni de texte aussi ancien que celui d'Hérodote ... Malheureusement, comme Lacédémone, la Préhistoire nord-africaine souffre d'oliganthropie.

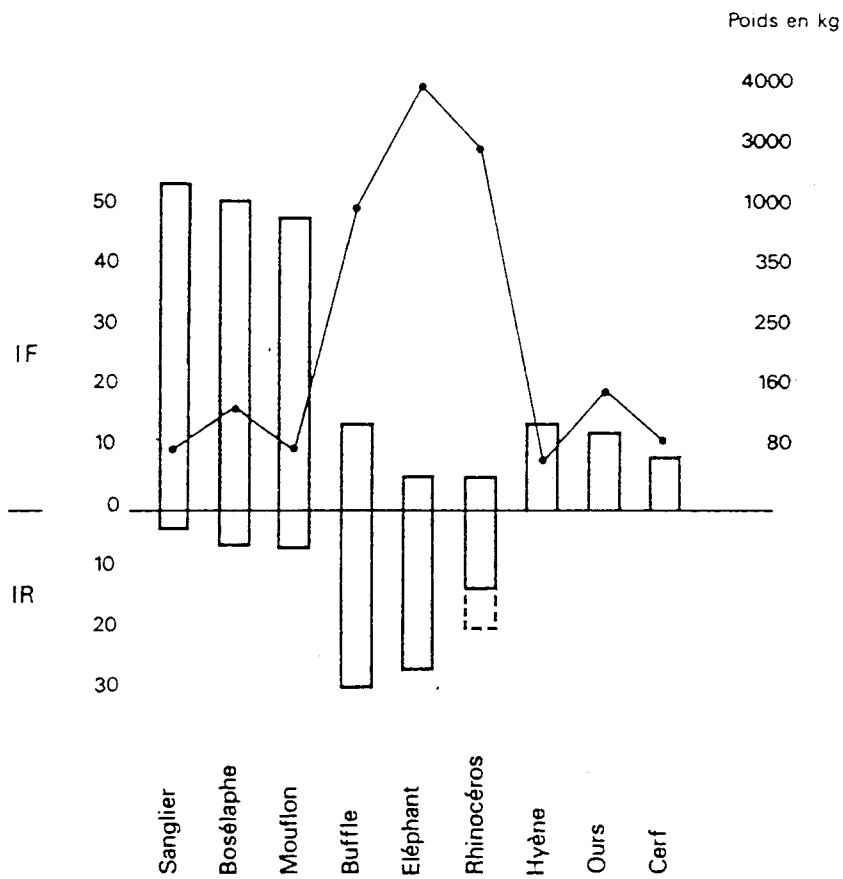


Fig. 1.— Acidité du sol et conservation des os. Le parallélisme de deux courbes montre bien que la présence ou l'absence d'os dans les couches est en étroite relation avec l'acidité des sédiments, contrariée en "d" par l'amas coquillier.

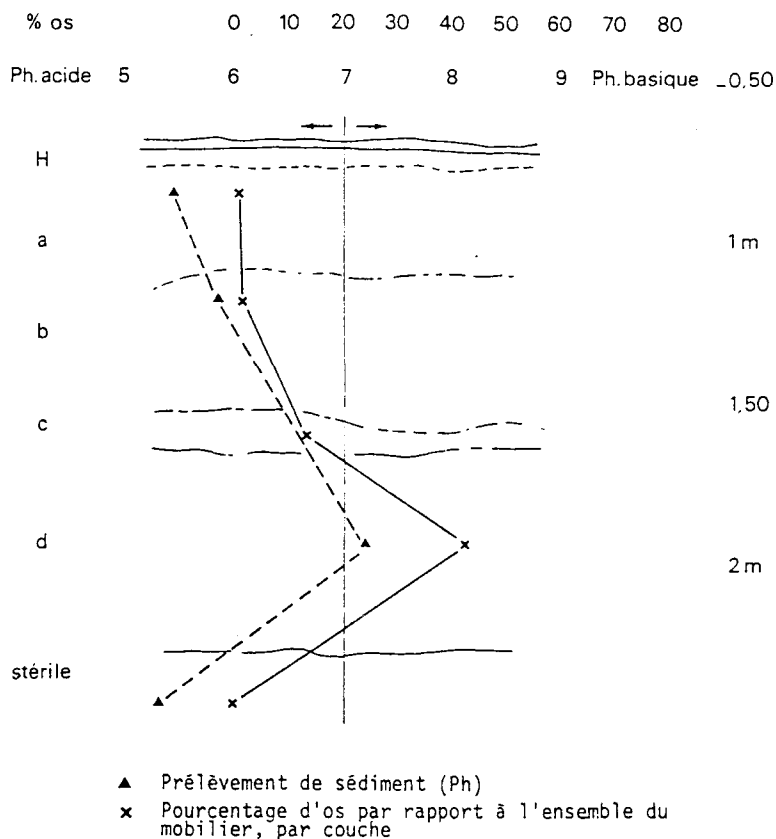


Fig. 2.— Diagramme comparatif entre l'indice de fréquence des espèces dans les gisements (I.F.) et l'indice de représentation dans l'art rupestre (I.R.), ces indices sont associés à la courbe des poids des mêmes espèces. Plus une espèce est chassée, moins elle est représentée (sanglier, bubale, mouflon), plus un animal est lourd plus il a de chance d'être représenté (buffle antique, éléphant, rhinocéros), enfin certaines espèces sont chassées, mais jamais représentées (hyène, ours, cerf).

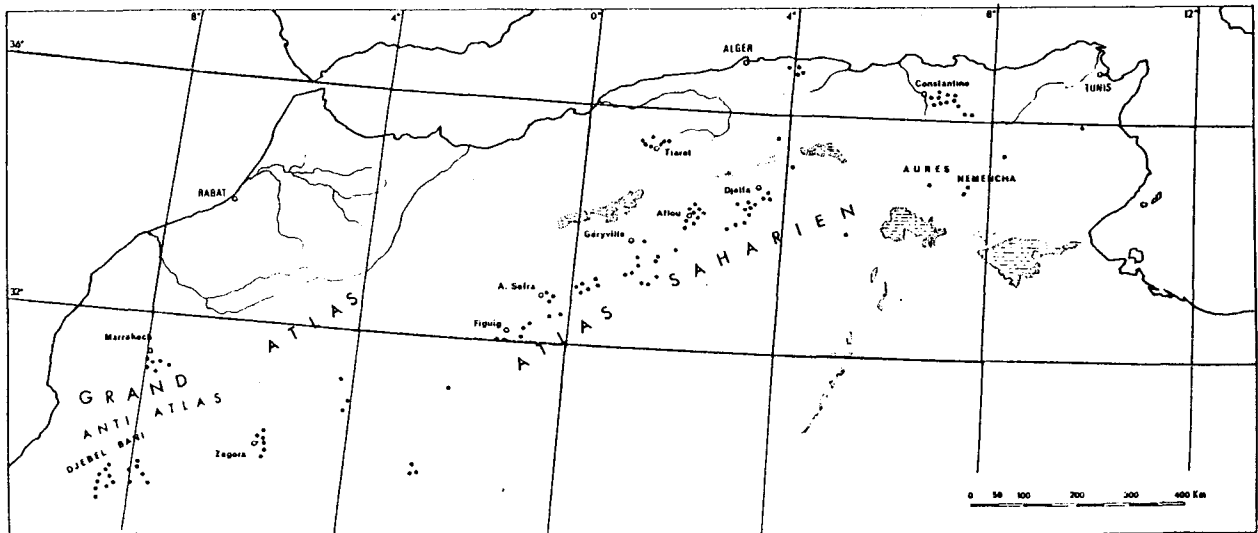


Fig. 3.— Répartition des principales stations de gravures rupestres de l'Afrique du Nord.

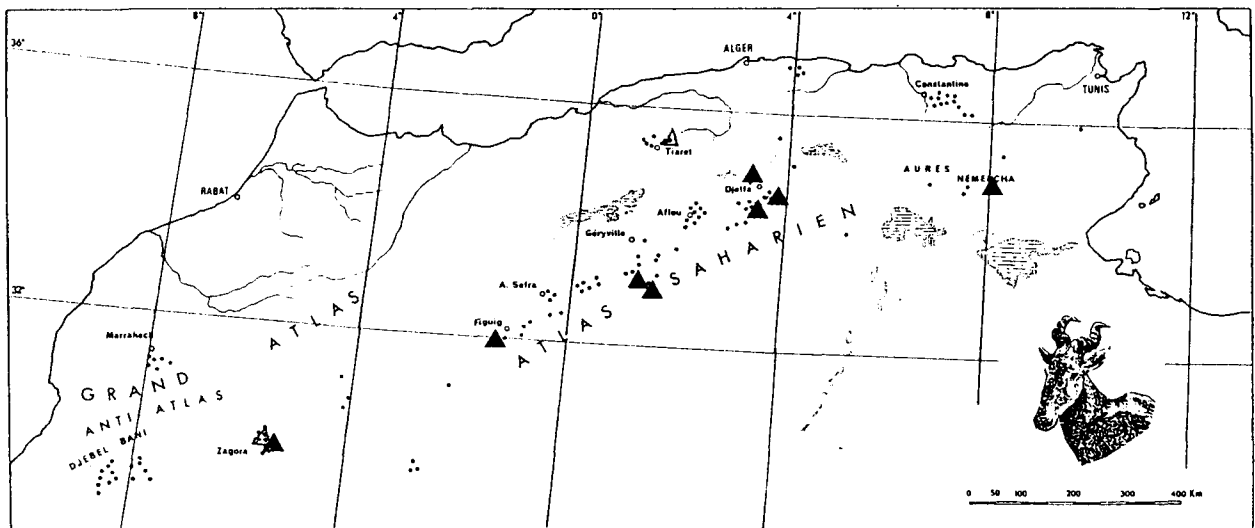


Fig. 4.— Représentation (▲) de l'antilope bubale (*Boselaphus probubalis*) dans l'art rupestre de l'Atlas.

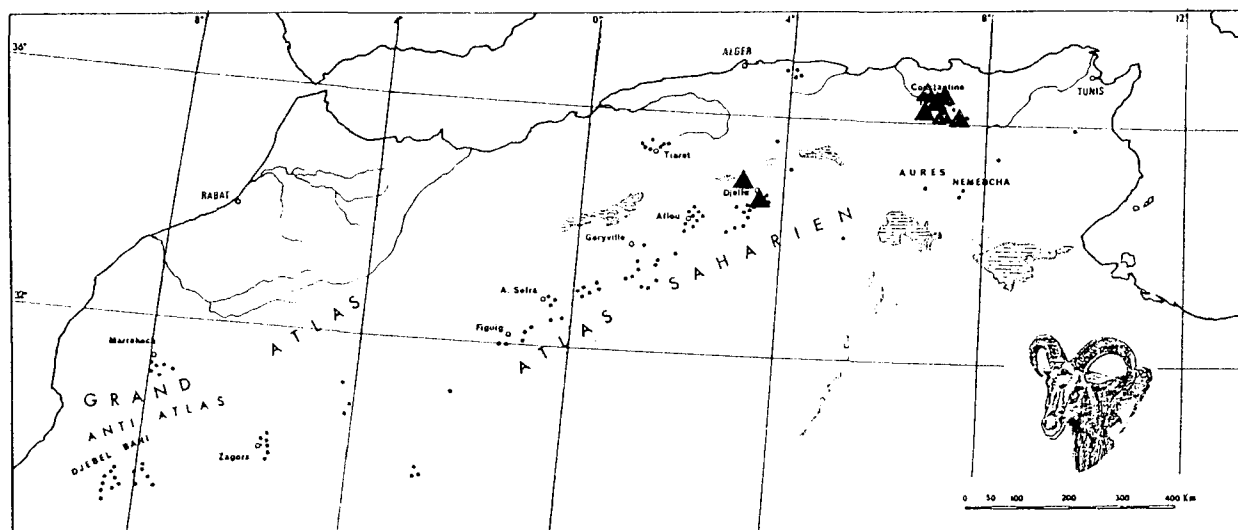


Fig. 5.— Représentation (▲) du mouflon à manchettes (*Ammotragus lervia*) dans l'art rupestre de l'Atlas.

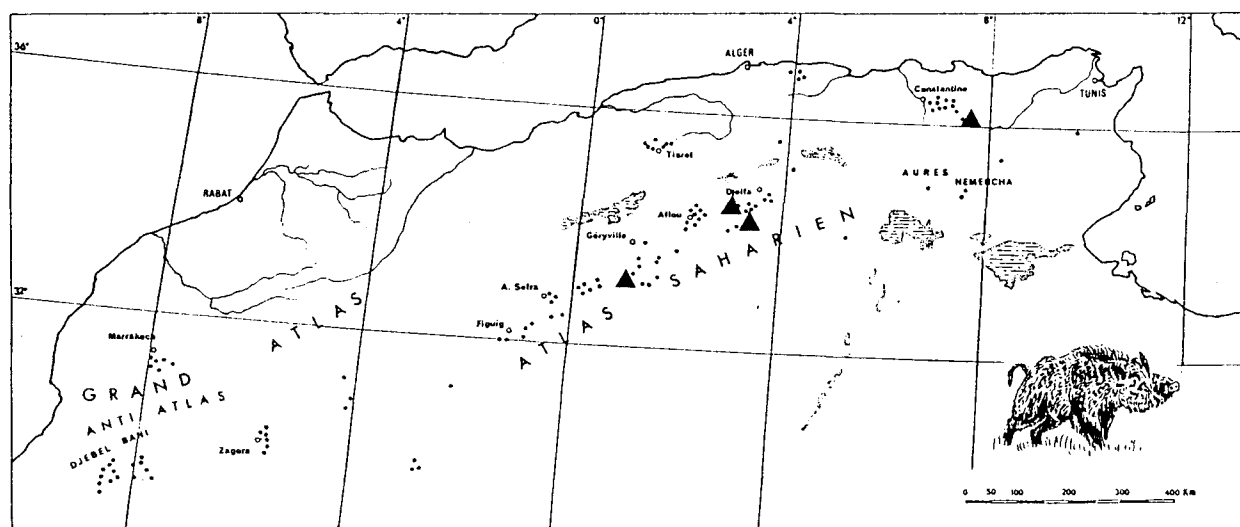


Fig. 6.— Représentation (▲) du sanglier (*Sus scrofa*) dans l'art rupestre de l'Atlas.

PHÉNOMÈNES DE MODE DANS L'ARCHÉOLOGIE FRANÇAISE

par

Jean CLOTTES *

RESUMÉ

La mode se définit comme un suivisme a-scientifique, qui aboutit à des adhésions (ou à des rejets) irraisonnés de méthodes ou de concepts. En France, les phénomènes de mode sont favorisés par la conjonction d'un champ de recherche illimité ("archéologie totale"), d'un manque d'encadrement théorique explicite et d'une forte tradition empirique, comme le montrent divers exemples développés dans cet article. Les effets de la mode se manifestent au niveau du vocabulaire, de la recherche, et surtout de l'interprétation. Il est possible de distinguer des modes à des degrés divers : modes généralisées, spécialisées, ponctuelles. Les modes intercalent un prisme déformant entre l'archéologue et son objet d'étude : en prendre conscience est un préalable indispensable à une objectivité accrue.

ABSTRACT

Fashions can be defined as an unscientific tagging along, which entails an unreasoned rejection or adoption of methods and/or concepts. In France, the development of fashions is helped both by an unlimited field of research ("total archaeology"), a lack of explicit theory and a still strong empirical tradition, as can be shown from a number of examples discussed in this paper. The effects of fashions appear in the vocabulary, as well as on different levels of research and interpretation. It is possible to characterize several degrees in fashions, as they may be generalized, specialized, restricted. Fashions set up a distorting prism between archaeologists and their work : to be aware of their pitfalls is a prerequisite to a better objectivity.

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“Il ne peut être question d'attitudes objectives devant les données archéologiques obligatoirement perçues à travers le prisme déformant du regard de l'archéologue. Ces idées sont à la mode, mais présentent l'avantage sur d'autres thèmes de l'air du temps de comporter une grande part de vérité.”

(Galinié 1980 : 69)

Il est vrai qu'au cours des deux dernières décennies, les articles, livres, colloques sur l'objectivité en matière d'archéologie ont fleuri, moins en France qu'ailleurs sans doute, malgré quelques belles réussites de la réflexion épistémologique (Schnapp éd. 1080; Delporte 1984; Gallay 1986). De ce point de vue, nous pourrions être taxés d'obéir à une mode. C'est peut-être vrai, mais il y a mode et mode.

L'acceptation d'une théorie, comme l'application d'une technique ou d'une réflexion nouvelles après vérification de leur adéquation avec les faits connus, de leur cohérence et de leur efficacité dans le cadre de recherches clairement définies, sont parfaitement légitimes. En ce sens, qu'une théorie ou une méthode qui ont fait leur preuves soient “mode” n'a rien qui puisse étonner ou choquer. Chaque époque porte l'accent sur tel ou tel aspect de la science, en fonction des lacunes constatées, de découvertes fortuites, des avancées dans les techniques et les théories. Les progrès ne s'accumulent pas de façon linéaire, mais par à-coups, des zones sont plus ou moins brillamment éclairées tandis que d'autres restent provisoirement dans l'ombre. Ce n'est donc pas cette acception du terme “mode” qui sera retenue ici.

En revanche, il est des adhésions ou des rejets irraisonnés qui ressortissent bien d'un effet de mode, car ils trahissent des comportements ou des démarches sans support scientifique véritable. Ce sera le cas lorsque des méthodes ou concepts seront rejetés sans discussion, pour la seule raison qu'“ils ont fait leurs temps”, qu'ils sont à contre-courant (voire trop en avance...), ou quand on appliquera des techniques inadaptées à leurs objet parce que “modernes”, ou encore lorsqu'on se ralliera à des théories mal étayées simplement parce qu'elles sont au goût du jour. La mode se définit ainsi comme un suivisme a-scientifique.

Comme pour tous les phénomènes de mode et dans tous les domaines, les causes de ces comportements ont des racines diverses. Souvent jouera la simple séduction du neuf, probablement basée sur une croyance diffuse et implicite en un progrès constant, ce qui est récent devant par nécessité intrinsèque marquer une amélioration par rapport à ce qui précède. Une attitude voisine, bien que plus motivée, sera la réaction contre certains abus du passé, avec retour du balancier qui ira d'autant plus loin que la mode antérieure était plus répandue (cf. ci-après la réaction contre le diffusionnisme). La sécurité apportée par le conformisme ne doit pas être négligée : il est réconfortant de se sentir “dans le vent” (de l'histoire, bien sûr) ou “à la pointe du progrès”, ou tout simplement de faire comme les autres. Rien n'est plus dévalorisant, à l'inverse, que de s'exposer au reproche, fût-il non argumenté, d'être “dépassé”. L'intérêt, au sens le plus matériel et le plus prosaïque du terme concourt au renforcement des modes, lorsqu'elles commencent déjà à être établies : par exemple, la tentation sera grande (et combien y succombent !) d'étoffer un rapport ou un ouvrage d'une quantité d'analyses ou d'études dites “multidisciplinaires”, même si elles n'apportent pas grand chose au sujet traité, si l'on croit, à tort

ou à raison, que ce faisant on accroîtra sa réputation de chercheur sérieux ou ses possibilités d'obtenir des crédits.

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“Notre connaissance du passé vaut pour autant que sont fiables nos méthodes inférentielles concernant nos observations à partir des données archéologiques” (Binford 1980 : 31). Ces méthodes inférentielles comprennent tout autant les théories explicatives et les modèles qui en découlent que les techniques appliquées à la récolte des données brutes et à leur interprétation. “Les vestiges sont statiques” (*op. cit.*), alors qu'ils sont le produit de processus dynamiques complexes que les techniques d'inférence devraient dans l'idéal nous permettre de découvrir.

C'est à ce stade que les phénomènes de modes sont susceptibles de jouer un rôle en orientant, voire en dévoyant les méthodes inférentielles.

Ces méthodes, dans notre pays, n'ont que très exceptionnellement (A. Leroi-Gourhan à Pincevent) été mises en oeuvre à la suite d'une réflexion théorique approfondie et d'une philosophie de la recherche pleinement et lucidement élaborée. Elles ont plutôt acquis droit de cité par accréions successives (Schnapp 1980 : 14), par la vertu d'exemples adoptés peu à peu, souvent non sans réticences (par exemple, influence de B. Soudsky, à la fin des années 60, sur les grands décapages et projet de la Vallée de l'Aisne). De sorte que se sont graduellement accrues d'une part la somme des informations jugées à présent indispensables lors de toute fouille (Whallon 1985-1986 : 23), d'autre part le nombre et la diversité des techniques.

La défiance des archéologues français pour les débats conceptuels et les discussions sur les théories de la recherche, à quelques exceptions près (Gardin 1979), contraste fortement avec l'attitude de nos collègues anglo-saxons, même si une “forte tendance a-théorique, fondée sur l'idée le plus souvent implicite que notre compréhension du passé est fonction du degré auquel nous accumulons et organisons des “faits” ” (Whallon 1985-1986 : 23) existe toujours outre-atlantique. En France, il a fallu attendre la création de la revue *Les Nouvelles de l'Archéologie*, tout à la fin des années 70, pour que des articles destinés aux archéologues “de base” traitent explicitement et de façon régulière de problèmes théoriques (dossiers sur l'ethnoarchéologie, l'archéologie théorique, la *New Archaeology* et l'archéologie aux Etats-Unis, etc.). Dans le présent article, il sera largement fait appel à eux. Mais il serait probablement impensable d'envisager l'organisation de colloques sur l'archéologie théorique comme ceux qui se tiennent en Angleterre depuis des années. Ce constat, à l'occasion en forme de *mea culpa*,¹ a été fait très souvent, y compris au cours de ces dernières années (Otte 1983-1984 : 51; Gallay 1986 : 19), parfois ironiquement : “Si on peut vous demander des comptes sur beaucoup de choses, voire vous priver de travail, ce n'est certainement jamais par insuffisance théorique” (Cleuziou et Demoule 1980 : 14-15). J. Garanger, remarquant que les chercheurs se défient des modèles théoriques, déplore que “tout ceci entrave l'émergence, en France, d'une pensée nouvelle sur la préhistoire et sur les processus généraux d'évolution culturelle” (1986 : 23). Il attribue ces

¹ Le présent article peut être considéré comme tel, son auteur étant pleinement conscient d'être souvent tombé dans certains des travers qu'il expose.

lacunes, sans doute avec raison, à une formation universitaire inadéquate. Les techniques et les approches peuvent changer mais pas nécessairement la perspective théorique, comme le constata J. Sackett (1981) à propos de la généralisation des récoltes aussi exhaustives que possible, des études paléoenvironnementales et paléoethnologiques, des méthodes quantitatives, etc.

L'un des plus éminents préhistoriens français contemporains, parfaitement conscient par ailleurs de la précarité de l'objectivité (Delporte 1984 : 14, 64, 139) et du caractère tout relatif de l'exhaustivité (p. 89), exprime clairement l'opinion la plus communément répandue : "La perspective, ou si l'on veut, l'objectif immédiat, ne sera donc pas de rêver de grandes théories ou de vastes systèmes explicatifs, mais de rechercher les moyens les plus adéquats en vue de *recueillir*, avec exhaustivité et précision, *l'information*, qu'elle soit *intrinsèque* ou *extrinsèque*, la plus abondante et la plus objective" (Delporte 1984 : 135).

A cette définition d'une archéologie à la fois globale et empirique, la plupart des archéologues français souscriront. Les buts explicites de l'archéologie actuelles sont des plus vastes, puisqu'ils visent à la connaissance de l'homme antique dans *toutes* ses activités (Berthoud 1980 : 135; Garanger 1986 : 19; Goudineau et Guilaine 1989 : 21). Pour répondre à ces objectifs se sont développées les études spatiales (détermination des activités journalières, voire des relations sociales), environnementales (y compris les recherches portant sur l'interaction de l'homme et du milieu), celles sur l'économie ou la fonction des outils. Les recherches chronologiques bénéficient des techniques améliorées du ^{14}C (accélérateurs) et des autres méthodes physiques (thermoluminescence, ESP, paléomagnétisme, etc.), ainsi que des disciplines comme la dendrochronologie, la palynologie, la paléontologie etc. (cf. *Le Temps de la Préhistoire* 1989 : vol. 1).

Cette ambition, depuis quelques années affichée, d'une archéologie globalisante est, d'une certaine façon, un acquis oblique de la *New Archaeology*, selon laquelle "la recherche archéologique doit tendre à une *vision globale*" (Galley 1986 : 74), même si elle ne s'inscrit pas dans une perspective conceptuelle bien définie.

La conjonction d'un champ de recherche illimité et d'un manque d'encadrement théorique explicitement formulé, ajoutée au legs du passé et à la tradition empirique toujours vivace, concourt à faire de la France, comme l'a dit L.-S. Klejn (1980 : 296), le "centre mondial" de l'archéologie descriptive. L'empirisme "naïf" (Chalmers 1978) qui consiste à croire que l'on peut tout observer et analyser, a depuis longtemps été dénoncé par de multiples auteurs (plus récemment, cf. Gallay 1986 : 16-20, 57-61; Lewis-Williams et Dowson 1989 : 48-50), même si "la démarche empirique (...) restera, quoi qu'on fasse, dominante et fondamentale" (Gallay 1986 : 210) et si on lui doit d'incontestables succès (*op. cit.* : 227).

L'empirisme ambiant, qui équivaut à un consensus mou puisque sans débats préalables, favorise à l'évidence la naissance et le suivi de modes, car le bien-fondé théorique des méthodes et stratégies ne sera pas ou très peu discuté. Quelques exemples illustreront ce propos.

L'amélioration des techniques de fouilles au cours des deux ou trois dernières décennies a été des plus bénéfiques et a fait faire des progrès spectaculaires à l'archéologie. Il n'est évidemment pas question de revenir sur ces acquis ou de diminuer leur portée. Mais croire en un progrès toujours croissant dans ce domaine et penser que la fouille actuelle "est moins bonne que

celle qui sera effectuée dans 20 ou dans 50 ans" (Delporte 1984 : 62-63) relève vraisemblablement de l'illusion. Dans 50 ans, et même bien avant..., les problèmes à résoudre ne seront sans doute plus les mêmes et les méthodes changeront en conséquence, mais pas nécessairement la finesse ou la minutie du travail de terrain, qui finit (ou finira) par atteindre un stade au-delà duquel les informations à attendre sont sans commune mesure avec la somme du travail accompli. Si on tamise à l'eau 10 seaux de sédiments avec une maille suffisamment fine, on récoltera peut-être les os de 10 ou 15 espèces de micro-mammifères; si on en tamise 100, on accroîtra ce nombre de 2 ou 3; et si on en tamise 1.000, peut-être de 1 ou pas du tout; et si on réduit la maille du tamis les résultats ne changeront guère. Fondamentalement, la recherche archéologique repose sur l'évaluation d'un échantillon, forcément limité et tronqué (Delporte 1984 : 19). Les méthodes d'évaluation de la pertinence de l'échantillon sont beaucoup plus susceptibles de changements et d'amélioration que le raffinement toujours plus poussé de l'échantillon lui-même.

L'effet de mode commence lorsque la méthode prend le pas, par suivisme irraisonné, sur la stratégie de recherche et l'objectif à atteindre. Pour prendre un exemple extrême mais plus fréquent qu'on ne pourrait le croire, relever avec la plus grande minutie tous les vestiges d'un remplissage remanié (terrier², comblement de puits ou de fosses, etc.) ne se justifie généralement pas et constitue une perte de temps et d'argent, au détriment de recherches véritables. L'hyper-minutie inutile a été dénoncée (Cleuziou et Demoule 1980 : 91; Ferdière 1980 : 41; Gallay 1986 : 231). L'objection courante à de telles remarques critiques consiste à exprimer la crainte d'un abaissement de la qualité des fouilles et d'un retour à un passé gratouilleur, ce qui est faire fi de la maturité de la recherche moderne où il devrait être possible de procéder à des choix, fussent-ils douloureux, pourvu qu'ils soient bien étayés, argumentés et adaptés au type de recherche poursuivie (cf. sur ce problème *in* Duday et Masset 1987 : 56-57).

Cette adéquation de la méthode et des objectifs, dont dépendent les résultats finaux, devrait fonder le jugement sur le succès ou l'échec d'une recherche, et non le plus ou moins grand degré de minutie de la fouille, ni même la nature de la stratégie employée considérée dans l'absolu. Après les premiers résultats obtenus à Pincevent, les décapages sur de vastes surfaces, seuls susceptibles de mener à bien des études palethnologiques, ont connu une grande vogue et de beaux succès (Etiolles, Verberie, Villerest, Gönnersdorf, etc. etc.). Dans les cas cités (et dans bien d'autres), une stratégie qui avait fait ses preuves et constituait un grand progrès méthodologique a été légitimement adoptée. En revanche, l'effet de mode joue d'une part si l'on essaie indûment d'appliquer cette méthode à des gisements aux statigraphies beaucoup plus complexes comme le sont souvent les habitats en grottes (Gallay 1986 : 247, 254) ou à certains sites urbains (Courbin 1987 : 333), d'autre part si on critique voire sanctionne un fouilleur pour ne l'avoir pas suivie alors que la nature du site ne le permettait pas.

Le consensus mentionné ci-dessus, en effet, se cristallise en une sorte de doctrine officielle, susceptible d'ailleurs d'évolution(s), qui se traduit par les recommandations exprimées dans les Programmes du Conseil Supérieur de la Recherche Archéologique, et surtout par les décisions prises au niveau de la Sous-Direction de l'Archéologie du Ministère de la Culture après avis de ce Conseil. Cet organisme, le C.S.R.A., est formé par les représentants, en grande majorité élus, de tous les corps scientifiques traitant de l'Archéologie en France. Il exerce une très forte

² "Les archéologues doivent rester des ethnologues et des historiens et ne pas se transformer en spécialistes du mode de vie des rongeurs" (Gallay 1986 : 231).

influence, par les arbitrages rendus, les crédits affectés, l'accent porté sur tel aspect plutôt que sur tel autre, sur la conduite et l'évolution de la recherche de terrain. Des efforts non négligeables ont été accomplis, au cours des quatre dernières années (1985-1989), pour définir des priorités et même des stratégies de recherche. Cela fut fait, par la force des choses, de façon pragmatique, c'est-à-dire en recueillant les avis des uns et des autres dans le cadre de programmes, chronologiques ou thématiques, préalablement fixés, et non à partir d'une réflexion théorique. En conséquence, à l'intérieur de chaque programme des priorités ont été établies et des axes de recherche tracés, avec le concours de très nombreux spécialistes, et cela marque une avancée considérable par rapport à un passé récent, mais les programmes n'ont pas été évalués les uns par rapport aux autres. Une hiérarchisation formelle, d'ailleurs, n'est peut-être ni souhaitable, – il ne faut pas figer la recherche –, ni pratiquement réalisable, – chaque spécialiste ne jure que par l'importance de son domaine de travail, et qui, sinon des spécialistes, pourrait procéder aux évaluations ?

Nous en revenons à la notion diffuse d'une archéologie totale, dont les contradictions éclatent lorsqu'elle est confrontée aux urgences des destructions et des sauvetages qui s'ensuivent. A. Gallay, à ce propos, écrit que “les partisans de ces opérations avancent souvent que chaque site est unique, et qu'il faut par conséquent tout sauver”, cette idée étant “une conséquence malheureuse des stratégies de recherches qui se veulent exhaustives”, alors que “la matière archéologique est peut-être plus redondante qu'on ne le pense” (1986 : 245-246). La conséquence en est que “l'on accumule des données qui ne seront jamais analysées, car elles ne répondent à aucun objectif clairement défini” (p. 240).

Plutôt que la notion d'*unicité* des sites archéologiques, ce qui est en cause, à mon avis, serait l'idée voisine mais différente, largement répandue, qu'un site en vaut un autre, alors que la valeur pour la reconnaissance du passé de l'Homme d'un habitat du Mindel-Riss, par exemple, est hors de toute proportion avec celle de sites beaucoup plus récents fouillés à de très nombreux exemplaires.

De vives critiques à ce sujet ont été récemment formulées dans *Les Nouvelles de l'Archéologie* : “– Le choix des opérations n'obéit à *aucune politique scientifique*, mais dépend de l'argent de l'aménageur; – le *désir de tout sauver* conduit souvent à de piètres résultats et quelques fois à la destruction de sites-clefs sur lesquels on a eu le tort de ne pas concentrer l'effort” (N.A. 1988 : 31). Le manque de publications et l'“accumulation répétitive de données non étudiées” (*op.cit.*) sont également dénoncés (cf. aussi N.A. : 1986-1987 : 51, 53). F. Bostyn et L. Vallin (1988 : 10-12) soulèvent exactement les mêmes problèmes et ajoutent que, dans les grands travaux qui absorbent actuellement la grosse majorité du potentiel archéologique français, “la Préhistoire est très souvent absente”, et que “si les fouilles urbaines sont privilégiées, la Préhistoire est laissée de côté” (p. 11)³.

Ces effets pervers sont indiscutables, tout comme l'est à l'inverse l'accroissement considérable des connaissances dû aux sauvetages, surtout en matière d'archéologie récente et urbaine. Il est vrai aussi que la mode commence à tourner et que, de plus en plus, des choix

³ Constatation qui n'est pas uniquement hexagonale, “Any deeply buried sites, in particular those of prehistoric date will not be revealed. Prehistoric sites also, due to the usually tenuous nature of the remains, may also not be immediately apparent” (J. Rady : *Eurotunnel : Stage III Archaeological works*, rapport du Canterbury Archaeological Trust, *Nouvelles de l'Archéologie*, 1988, n° 32, cité p. 11).

drastiques sont faits pour répondre à de vraies questions, au lieu de fouiller tout ce qui se présente. Par exemple, ce fut le cas sous la direction de A. Delpuech, à l'automne 1989 sur le futur tracé de l'autoroute A5 dans la Basse Bourgogne.

L'empirisme d'une archéologie tous azimuts a induit une vogue sans précédent, plus ou moins bien contrôlée, des analyses de tous types; si l'archéologie théorique ne fait pas recette en France, il en va fort différemment de l'archéométrie!... Cette vogue, très bénéfique au développement de l'archéologie, a néanmoins suscité des abus et des effets de mode divers, dont les spécialistes des laboratoires, appelés aujourd'hui "archéomètres", ont été les premiers à souffrir et à se plaindre (Picon 1979 : 7, 9 : Langouët 1979 : 19; Berthoud 1980; 135-137; 1984 : 54, 57-58). Ces abus sont en gros de deux types (analyses injustifiées; interprétations abusives) et résultent de la même cause profonde, à savoir le sentiment que des analyses par les sciences dites exactes apportent une caution scientifique et valorisent un travail de recherche⁴, ce qui bien entendu n'est vrai que dans la mesure où ces analyses sont parfaitement adaptées à un problème précis à résoudre. Or, malgré les mises en garde répétées des archéomètres, les demandes d'analyses inutiles sont légion! Par exemple, J.-Ph. Rigaud, directeur du Centre National de Préhistoire (Périgueux), a reçu dans les trois dernières années plusieurs demandes d'analyses palynologiques destinées à *dater* des fossés néolithiques et de l'Âge du Fer, ainsi que des demandes d'analyses sédimentologiques pour *dater* des niveaux holocènes.

Le phénomène est accentué par le développement des grands sauvetages financés à 100 % par les aménageurs. Ces grosses opérations sont en effet conduites avec des crédits sans commune mesure avec ceux attribués par l'Etat aux fouilles programmées, et le chapitre "analyses" y est un *must*. Or, au moment où s'engagent les négociations financières, c'est-à-dire avant la fouille, il est le plus souvent impossible de savoir quelles analyses seront légitimes ou non, de sorte que les sommes considérables de ce chapitre sont en fait laissées à la seule discrétion du responsable du chantier et à la perspicacité des laboratoires auxquels il s'adresse. La création récente de laboratoires commerciaux de services n'est pas, dans ce domaine, le plus sûr garant que ces sommes seront toujours employées à bon escient. L'un des effets pervers, et non des moindres, de cette situation est que les laboratoires sont engorgés de demandes payantes qui relèguent bien entendu à des dates ultérieures des analyses scientifiquement plus rentables, mais moins bien dotées sur le plan financier.

Cette "mode des études multidisciplinaires" (Gallay 1986 : 278) "n'est qu'une des facettes des problématiques dites exhaustives, et procède du même type d'illusion" (p. 279). Elle se traduit fréquemment au niveau des publications par la parution de gros ouvrages où sont juxtaposées des analyses nombreuses qui gonflent le nombre de pages sans apporter de renseignements pertinents, au prétexte souvent invoqué qu'il faudra de nouvelles études du même type pour être à même d'en tirer (quand ?) des conclusions.

L'autre aspect pernicieux du phénomène est l'usage abusif des analyses, dû à une mauvaise compréhension de leurs limites et de leurs possibilités, malgré là encore les efforts méritoires des archéomètres pour éclairer les archéologues à ce sujet. Par exemple, il est vain d'espérer établir les phases successives d'une nécropole mérovingienne qui a duré un siècle et demi ou deux au maximum au moyen de trois datations ¹⁴C, compte tenu des sigmas de chacune de ces datations

⁴ Cela explique la mode des mathématiques en archéologie dans les années 1970 et celle de l'ordinateur dans les années 80.

qui se recouperont inévitablement. De même, les spécialistes du radiocarbone ne peuvent que s'insurger lorsque, comme c'est très souvent le cas, des dates sont données comme absolues, sans leurs sigmas, encore plus quand, sur les quatre couches de la grotte de La Vache (Ariège) (il s'agit d'ailleurs de niveaux artificiels à l'intérieur d'une même couche sans subdivisions apparentes), la couche IV étant datée par le radiocarbone de 10.900 BC (± 60 , non cité) et la c.II de 10.590 BC (± 105 , *id.*), la couche III est "estimée" à 10.745 et la couche I à 10.435 (Nougier 1984 : 173).

Ces aberrations traduisent peut-être aussi une certaine fascination naïve pour les sciences exactes, surtout lorsqu'elles mettent en oeuvre des techniques que nous ne sommes pas armés pour contrôler ou dont les modalités nous échappent en grande partie sinon totalement. Deux exemples, pris dans des domaines très différents, illustreront cette mode du scientisme.

Lorsque A. Marshack a publié sa théorie des notations (1972) il s'est efforcé d'être aussi objectif que possible, mais il n'a pas *prouvé* qu'il s'agissait bien de notations, il a *cru* qu'il l'avait fait, comme l'ont démontré dans une brillante analyse J.-D. Lewis-Williams et T.-A. Dowson (1989 : 48 : 50) en démontant le mécanisme d'une recherche alors nouvelle et prometteuse, mais où la subjectivité intervient néanmoins, quasi inévitablement, à toutes les étapes. L'acceptation des dires de A. Marschack et leur application à d'autres objets du même type sans vérification préalable par une demi-douzaine au moins d'excellents chercheurs (cités par D'Errico 1989 : 498) a très certainement été favorisée par son usage du microscope qui prête une objectivité fallacieuse à des observations que d'autres analyses par les mêmes méthodes battent en brèche (*op. cit.*).

Autre exemple, qui prouve bien que la mode du scientisme et les réactions qu'elle génère sont des plus répandues et pas seulement propres à la France : lorsque B. Vandermeersch et O. Bar-Yosef ont attribué les sépultures d'Hommes modernes de Qafzeh à une époque très ancienne, évaluée à environ 100.000 ans à partir d'arguments archéologiques solides, l'incrédulité du milieu scientifique a été totale et leur évaluation fut très généralement récusée. Alors que, quelques années plus tard, quand furent connues pour le même site des dates de l'ordre de 90.000 à 100.000 ans obtenues par la thermoluminescence (Valladas *et al.* 1988), l'acceptation par la communauté des spécialistes fut immédiate, pour ne pas dire enthousiaste.⁵ Est-ce à dire que les méthodes de datation physiques sont reconnues comme indiscutablement plus objectives et fiables que les méthodes archéologiques ? C'est là un point qui mériterait discussion. On constate cependant que ce n'est pas toujours le cas, à en juger par la façon dont les archéologues procèdent lorsque des datages par le radiocarbone donnent des résultats jugés "aberrants" par rapport à ceux des autres méthodes. A chaque conflit de résultats on invoque alors des contaminations vers le haut ou vers le bas, et c'est le radiocarbone qui est souvent mis au ban des accusés. Ces attitudes sont contradictoires et malaisément explicables. Risquons une hypothèse : leur raison profonde pourrait être que le radiocarbone est devenu beaucoup plus familier que les autres types de datation absolue. Comme la palynologie, la sédimentologie et la paléontologie animale, il fait partie de l'arsenal des méthodes couramment employées et a perdu toute aura. la méthode a pour ainsi dire été digérée et est passé dans les moeurs, malgré quelques aberrations dans le comportement de certains de ses usagers (cf. *supra*). Tandis que les datations TL ou ESP ou autres pour les périodes très anciennes tombent encore comme des verdicts ou des oracles et sont parfois acceptées comme tels. Précisons que ce n'est pas mettre en cause leur

⁵ Faits rapportés par L. Meignen lors de sa communication au Congrès Préhistorique de France (Paris, novembre 1989).

validité que de signaler des ambiguïtés troublantes dans la manière dont sont acceptés ou non les résultats des diverses méthodes.

Nous venons de voir que l'empirisme dominant et l'objectif affiché d'une archéologie totale ou globale tendent à induire des effets de mode, entre autres dans l'hyperminutie pour la recherche de terrain, ou l'application plus ou moins mal à propos de certaines méthodes dévoyées de leurs buts premiers dans le domaine de la fouille et des analyses, ou dans l'interprétation de ces dernières, enfin dans la vogue du scientisme.

La mode naît aussi, très souvent, en réaction contre un passé abusif, qu'il s'agisse d'une voie de recherche trop exclusive ou d'une mode véritable qui sera ainsi remplacée par une autre. Cette cause ("le retour du balancier", déjà cité) est déterminante dans une majorité des cas et la mode nouvelle sera d'autant plus vivace et, partant, d'autant plus dangereuse pour l'objectivité de la recherche que celle qui précède aura été puissante et bien enracinée. Ces phénomènes de mode se manifesteront à un triple niveau; celui du vocabulaire, celui de la recherche, celui surtout de l'interprétation.

Le vocabulaire n'est évidemment pas neutre et ses modifications recouvrent des concepts divers. Ainsi, la vogue assez amusante du terme de "problématique", que l'on retrouve depuis quelques années mis à toutes les sauces, se veut une réaction à l'absence, souvent regrettée, d'objectif défini et de théorie explicite de la recherche. Or, lorsque, comme c'est si fréquemment le cas, la problématique exprimée est des plus vagues ("étudier le peuplement et l'implantation humaine à telle période dans telle vallée, dans son contexte paléoenvironnemental"), on ne dépasse pas le stade du suivi d'une mode. Notons à ce propos qu'il est de bon ton de nos jours d'émailler ses articles ou rapports de certains termes employés à plus ou moins bon escient : taxonomie, saisonnalité, archéologie spatiale, processus d'anthropisation, diachronie, biomasse, etc. etc. Les "modèles" et "attributs", après l'avoir été, sont beaucoup moins *in*.

A l'inverse, d'autres expressions deviennent quasiment taboues. Il en est ainsi des "fossiles directeurs" qui jouirent jadis d'un si grand succès. "Les termes de fossiles indicateurs ou de marqueurs chronologiques leur sont désormais préférés" (Camps-Fabrer 1988 : 23). La réaction, dans ce cas, va à l'encontre d'un système trop rigide où l'équivalence d'un type d'outil avec une civilisation ou une période était peu à peu devenue un dogme. Néanmoins, il existe sans aucun doute des outils exclusivement caractéristiques d'une culture définie. Qu'on les appelle comme l'on voudra, il s'agit bien de nos anciens fossiles directeurs, et ce qui importe, c'est qu'ils sont toujours utilisés comme tels, par exemple lorsque H. Camps-Fabrer constate que "les sagaies d'Isturitz (...) constituent un bon marqueur chronologique du Périgordien V à burin de Noailles" (*op. cit.*). Ce type de remarque est des plus courants, au point que, la notion de fossile directeur correspondant à une réalité archéologique certaine et présentant encore quelque intérêt pratique (cf. les pointes à cran solutréennes, les harpons du Magdalénien supérieur, les décors des céramiques campaniformes ou chasséennes, etc.) nombreux sont les auteurs à qui il arrive (par inadvertance ?) d'employer ce terme, et qui, ayant conscience d'avoir enfreint un interdit, jugent alors indispensable de s'en excuser... Ce fut le cas à diverses reprises lors de la session "Paléolithique supérieur" du Congrès Préhistorique de France (Paris, Novembre 1989)

(cf. aussi Utrilla 1987). ⁶ Le changement de vocabulaire sans changement corrélatif du concept correspond donc à un effet de mode certain.

Le discrédit, plus apparent que réel, des fossiles directeurs, va de pair avec celui de la typologie, surtout d'ailleurs, assez curieusement, de la typologie lithique, car la recherche en matière de typologie osseuse, grâce sans doute à l'impulsion de H. Camps-Fabrer, se porte très bien (nombreux colloques, parution de fiches analytiques, etc.), de même que celles sur la typologie métallique ou céramique.

“Les tendances les plus récentes de l'archéologie n'accordent guère d'attention à la typologie, quand elles ne l'ont pas simplement renvoyée au rayon des accessoires démodés. Qu'elles soient écologiques, processuelles ou les deux à la fois, ces tendances paraissent considérer que la typologie visait à résoudre des questions qui ne sont pas le but de l'archéologie, avec une méthode empruntée au modèle biologique du XIXe siècle” (Cleuziou 1983 : 100). Ce n'est alors plus seulement le vocabulaire mais une voie de recherche qui est en cause. Une chansonnette de D. Roe et P.-J. Watson exprime à la fois ce discrédit et illustre à merveille l'importance et les voies de la mode :

*“I am the very model of a modern Archaeologist :
A geoethnoarchaeoeconomobiologist. (...)
I've hired a taphonomer and fired my typologist.”*
(Cité in N.A. 1985-1986, n° 22 : 21).

Or, le développement de nouvelles techniques (études des types d'usure, chaînes opératoires, remontages et leurs implications) ne périme pas pour autant la typologie lithique morphologique (Camps 1979 : 138), car ces recherches ne poursuivent pas les mêmes buts. D'ailleurs, la typologie a été défendue sur un plan général par un éminent théoricien de l'archéologie (Klejn 1982). Le commentaire de S. Cleuziou (*op. cit.*) sur les réactions défavorables susceptibles d'être causées par la parution de cet ouvrage est éclairant : “On risque fort de lever les bras au ciel dans certains milieux archéologiques, devant ce qu'on considère comme un formalisme outrancier et dépassé. On aura tort. Klejn ne nie pas la valeur des approches économiques, ou des études processuelles, mais des notions comme type et culture sont vraisemblablement présentes jusque dans ces travaux, même si elles sont ignorées.”

Il est évident que ce commentaire dénonce des phénomènes de mode à deux niveaux; celui, négatif, de la prévisible réaction instinctive, irraisonnée (“lever les bras au ciel” n'a jamais remplacé un argument) devant une opinion à contre-courant; mais aussi l'absence de lucidité qui fait ignorer ou rejeter des notions de base considérées comme dépassées.

En matière de lithique, depuis une dizaine d'années, l'analyse fonctionnelle a très largement supplanté l'analyse typologique, même si celle-ci reste indispensable. C'est l'analyse fonctionnelle qui apparaît comme une recherche de pointe. Ses buts, ses moyens et ses limites

⁶ “Le moment n'est pas favorable pour parler des fossiles directeurs, depuis que, à partir des années soixante, de nombreux chercheurs se sont employés à les démythifier. (...) Cependant, bien qu'on ne puisse affirmer de façon absolue qu'il y ait des thèmes exclusifs d'une culture déterminée, on en est venu à observer, ces dernières années, qu'il existe des conventions techniques de représentation qui s'attachent de préférence à des époques concrètes. C'est à ces conventions que nous allons nous référer sous le terme de “fossiles-directeurs”” (Utrilla 1987 : 85).

ont, chose rare, été définis et exposés avec une parfaite clarté presque dès le début des recherches (Cahen, Karlin, Keeley, Van Noten 1980). Les remontages, dont les deux visées premières étaient l'organisation de l'espace (dispersion des éléments d'un même nucleus) et les restitutions des techniques et d'une suite de gestes, ont permis de mettre en évidence, à Etiolles, plusieurs niveaux d'expertise et de distinguer des tailleurs excellents, d'autres moins habiles, et certains très peu expérimentés (Pigeot 1983; Olive 1988).

Ces derniers ont particulièrement retenu l'attention, en raison des hypothèses d'ordre sociologique qu'ils permettaient. Des études spécifiques leur ont été consacrées, à partir d'observations faites à Pincevent : "L'apprenti d'Etiolles est à Pincevent, si l'on accepte l'observation des ethnologues qui associent chez les chasseurs-cueilleurs niveau d'apprentissage et classe d'âge, un *jeune* (...). Ainsi entrent en concordance un niveau de technicité et une tranche d'âge." (Bodu, Karlin, Ploux, *sous presse*; cf. aussi Ploux 1989). Le problème est dans ce cas le glissement de la notion de maladresse à celle d'apprenti puis à celle d'enfant, si plausibles que soient ces hypothèses. Mais elles sont ensuite considérées comme des vérités acquises et autorisent des conclusions beaucoup plus larges; "La présence des enfants suggère qu'il s'agit d'une cellule familiale composée d'hommes et de femmes, et non, comme cela avait été envisagé, un campement de chasse uniquement masculin" (*op. cit.*).

Les travaux mentionnés, menés avec une grande rigueur, apportent beaucoup à notre connaissance des Magdaléniens dans leurs activités journalières. On peut simplement se poser la question si, au niveau des *interprétations*, nous ne sommes pas confrontés à la constitution d'une mode, qui serait d'une part la recherche systématique de l'individu aux dépens des conventions et des traditions de l'époque (cf. dans le même ordre d'idée les travaux de J. Apellaniz sur l'art pariétal), d'autre part et surtout celle des enfants. La ligne de recherche visant à découvrir des enfants, en effet, n'est pas fortuite et ne découle pas directement des observations matérielles. Elle répond à une volonté d'étude sociologique qui, elle-même, constitue une véritable mode. Ainsi, quand une trace charbonneuse isolée, "entourée de huit éclats de silex et de quelques fragments osseux" est interprétée comme l'action possible de "deux ou trois enfants (...) qui se sont regroupés pour grignoter, entre copains, quelques fragments de carcasse" (*op. cit.*), parce que les éclats proviennent de différents amas de rejets et sont apparemment peu aptes à l'utilisation (cf. aussi Karlin, *sous presse*), il est permis de s'interroger...

Lors du Congrès Préhistorique de France (Paris, 1989), se remarquait parmi les posters une série de documents, au demeurant très bien présentés, sur les fouilles de F. Lebrun-Ricalens à Hui (Lot-et-Garonne), où la détermination de certains débitages par des enfants-apprentis était signalée. Peut-être un fait jusque-là occulté est-il ainsi révélé ? Peut-être aussi assistons-nous à la naissance d'une mode motivée par le comparatisme ethnographique et le biais sociologique déjà signalé ? Laissons à l'avenir le soin de trancher.

Les phénomènes de mode, nous le voyons, trouvent leurs champs d'action favori dans le domaine de l'interprétation, ou du moins c'est là qu'ils apparaissent avec le plus d'évidence lorsque des réalités archéologiquement identiques ou très proches suscitent simultanément ou successivement des explications complètement divergentes par simple référence à un modèle qui a changé avec l'air du temps. Cela se traduit sur le plan pratique par la minoration de certains aspects des données, alors que d'autres sont non moins systématiquement privilégiés. Cette adhésion à une mode, suivant les cas, peut être le fait d'une majorité des archéologues (mode

généralisée), d'une catégorie particulière (mode spécialisée), ou même ne concerner que quelques chercheurs influencés à titre personnel (mode ponctuelle).

Les modes généralisées, par leur effet d'entraînement, jouent un rôle majeur. Nous en citerons deux, parmi bien d'autres. Comme dans de nombreux cas, elles sont nées de réactions à des abus antérieurs.

Pendant longtemps, tout ce qui n'était pas immédiatement explicable était qualifié de "rituel". Il en fut ainsi de l'ocre. Puis, "l'irrationnel devient malséant, et c'est pourquoi l'ocre, entaché d'une lourde hérédité à cet égard, se voit quelque peu délaissée (...). Elle est discréditée (...)" (Audouin et Plisson 1982 : 40). On ne saurait mieux exprimer l'effet nocif d'un mouvement de la mode pour un certain domaine de la recherche. Aujourd'hui, les travaux des auteurs cités, ceux de C. Couraud, de G. Onoradini, de C. San Juan et de quelques autres ont réhabilité l'étude de "l'ocre", mais la notion de rituel reste toujours suspecte. Il est vrai que ce terme a longtemps servi à couvrir des ignorances et qu'il faut se défier de son emploi hors de propos. Mais de nos jours le danger serait plutôt dans son abandon quasi systématique au profit d'explications matérielles ou pratiques. Or, il n'est pas de société sans rites, et ceux-ci laissent souvent des traces, fussent-elles ténues. Avant de conclure à une activité rituelle, il est certes nécessaire de déterminer si des gestes utilitaires ne pourraient rendre compte des observations effectuées.

"Présente une connotation rituelle (...) toute activité ayant un caractère répétitif (...) qui ne s'explique pas par les besoins de la survie matérielle, et qui par conséquent ne relève ni du domaine technique, ni du domaine économique." (Gallay 1986 : 214). Il est certain que, mis à part sa répétitivité, les critères de détermination du phénomène sont négatifs, de sorte qu'il est toujours loisible d'évoquer un but pratique dont la raison nous échappe actuellement. C'est l'attitude la plus communément adoptée de nos jours. Ce fut la nôtre (Bégouën et Clottes 1981 : 183-184) au sujet d'esquilles osseuses plantées par les Magdaléniens dans de nombreuses fissures des parois dans la grotte d'Enlène. La prudence est nécessaire, mais trop de circonspection risque de nous faire passer à côté d'une réalité dont les innombrables exemples ethnologiques confirment l'importance. C'est ainsi que les dépôts de vestiges divers signalés dans les grottes ornées furent invariablement qualifiés de cachettes abandonnées ou d'objets perdus, avant que l'observation argumentée de leur caractère délibéré et répétitif ne fasse exclure ces hypothèses, celle de dépôts rituels devenant beaucoup plus vraisemblable (*op. cit.* : 182-184; cf. aussi Bégouën et Clottes 1982).

Le diffusionnisme fut une de ces modes généralisées, sous l'impulsion de Gordon Childe, mais aussi parce que "le recours explicatif à la diffusion a pour lui la facilité; c'est souvent un refuge, un procédé pour fuir le problème : on suppose une source étrangère" (Guilaine 1986 : 18). Les deux manifestations majeures de cette mode furent les migrations (ou invasions) et l'origine étrangère, généralement proche-orientale, de toute découverte et de tout progrès. J. Guilaine, dans le texte cité, estime que "la Préhistoire n'a pas complètement extirpé les vieux démons du diffusionnisme. Pourtant, plus nombreux sont les chercheurs qui refusent de se laisser entraîner dans des explications migratoires ou par trop événementielles."

En fait, sous les coups déjà anciens de spécialistes comme C. Renfrew, le diffusionnisme a bien passé de mode (Gallay 1986 : 61), et le danger actuel serait plutôt inverse, avec la recherche

à tout prix d'une évolution sur place des cultures (Binder et Courtin 1987 : 491) ⁷ ou le postulat trop exclusif du polygénisme des techniques et inventions (Camps 1987 : 211-212). ⁸ Après tout, dès que des textes existent, ils nous confirment la réalité des migrations et des invasions et leur rôle considérable dans le processus historico-culturel, ainsi que divers auteurs l'ont rappelé (Klejn 1980 : 290). La croyance en des vagues d'invasions suscita jadis des explications trop uniquement défensives pour certains sites néolithiques où l'on ne vit que grottes-refuges, enceintes, voire "châteaux" (le Lébous, dans l'Hérault). Maintenant, c'est l'inverse, les enceintes deviennent des parcs à bestiaux et les grottes-refuges des sites "spécialisés", à juste titre dans bien des cas sans doute, ⁹ mais la trop grande exclusivité de ces interprétations socio-économiques peut inspirer une crainte légitime.

D'autant que, si les phénomènes migratoires apparaissent avec l'Histoire, nous les retrouvons dès qu'on passe dans le Paléolithique, où les cartes de répartition (ou de mouvements de population) avec des flèches indiquant des sens de diffusion sont légion. Par exemple, dans un ouvrage tout récent (1989) sur le *Magdalénien en Europe*, pas moins de quatre articles contiennent des phrases qui seraient honnies si elles étaient consacrées à des périodes un peu plus tardives. ¹⁰ Si les paléolithiciens ont apparemment moins horreur du diffusionnisme que leurs collègues néolithiciens, c'est certainement en premier lieu parce que cette mode à son apogée fit moins de ravages dans le Paléolithique, et aussi en raison des comparaisons, explicites ou non, avec les civilisations historiques de chasseurs-collecteurs à qui il arrive de se déplacer sur d'assez grandes distances. Quoiqu'il en soit des causes, la mode anti-diffusionniste actuelle, bien qu'assez générale, est beaucoup plus marquée parmi les spécialistes de l'Holocène.

D'autres modes, de moindre ampleur, sont également *spécialisées* et occasionnent des divergences d'interprétation radicales selon que l'on est paléolithicien ou néolithicien. Ainsi, en octobre 1988, à l'occasion du Colloque de Chancelade sur le Magdalénien, le Dr. J. Gaussen avait organisé une exposition sur ses recherches dans la vallée de l'Isle et une visite de certains sites où plusieurs dizaines de spécialistes purent voir consécutivement les plans détaillés et les photographies de plusieurs structures magdaléniennes ainsi que des exemples *in situ*. Ces structures, généralement de forme rectangulaire et de tailles très diverses, étaient faites d'ensembles compacts de galets rubéfiés parfois sur la face supérieure et parfois sur la face opposée, ce qui indiquait que la rubéfaction ne résultait pas de simples feux à leur surface (cf. à ce sujet Gaussen 1980 : 122, 169, 188, 273-274). Aucun trou de poteau n'a jamais été découvert autour de ces structures, que le fouilleur estime constituer des assainissements de cabanes ou de

⁷ "Par réaction contre les excès diffusionnistes (...), on a insisté au-delà de toute mesure sur les capacités d'adaptation autonome des populations, face aux contraintes d'un environnement en constante évolution" (Binder et Courtin 1987 : 491), ce qui a conduit, entre autres, à multiplier indûment "les épi-ceci et les proto-cela" (*op. cit.*).

⁸ "Après les excès de la théorie diffusionniste érigée en système d'explication automatique de toute évolution culturelle et technique de l'humanité préhistorique, il ne faut pas, aujourd'hui, tomber dans les excès contraires et tout aussi ridicules fondés sur le polygénisme et l'évolution sur place" (Camps 1987 : 211-212).

⁹ Le lecteur constatera que je fais des efforts méritoires pour ne pas trop aller à l'encontre des modes actuelles ...

¹⁰ Par exemple, le Magdalénien de la Belgique est "intrusif", répond à un "mouvement migratoire", à une "colonisation de territoires" et une "diffusion" (Otte 1989 : 66-67; cf. aussi Pl. 10, p. 77 carte). Le Nord-Est de la France "aurait donc connu une alternance de phases d'occupation et d'abandon", avec "un Magdalénien classique venu du Sud" (David et Richard 1989 : 113). "On voit bien une culture orientale (...) arriver dans la vallée du Loing avec ses traditions culturelles", le "sens de la migration" est mentionné, les Hambourgiens auraient connu "une migration lente de 800 km" (Allain 1989 : 200). Dans le Bassin Parisien, on parle de la "pénétration magdalénienne", d' "incursions" (Schmider 1989 : 219), de "l'arrivée des Badegouliens" (p. 220), d'un "itinéraire de migration est-ouest" (p. 228).

huttes. Cette interprétation, habituelle pour ce type de vestiges (*op. cit.*, : 49-51), n'a pas été contestée, ni depuis la parution de l'ouvrage ni lors du Colloque de Chancelade.

Or, à peu près à la même époque où sortait le livre du Dr. Gaussen, étaient publiées des structures rectangulaires à galets rubéfiés, d'aspect assez voisin, mais néolithiques cette fois, mises au jour à Villeneuve-Tolosane (Haute-Garonne). Deux explications possibles furent alors avancées et discutées : soit des fonds de cabanes (hypothèse classique, admise jusque-là pour des structures identiques et de même époque, dans la même région, à Saint-Michel-du-Touch), soit des aires de combustion, sortes de fours polynésiens (Clottes, Giraud, Rouzard, Vaquer 1981 : 126-128).

Par la suite, aucune des deux hypothèses ne put être ni prouvée ni contredite.¹¹ Néanmoins, dix ans plus tard, il existe un assez large consensus parmi les néolithiciens pour admettre une utilisation exclusive comme fours de ces structures chasséennes : "Les nombreuses structures découvertes dans ces enceintes, rondes ou rectangulaires et empierrées de galets chauffés sont actuellement considérées comme des structures de combustion, et non comme des 'fonds de cabane' " (Gutherz 1989 : 121; cf. aussi Vaquer 1981; Vaquer et Claustre 1989 : 19), alors que "paradoxalement (...) on ne sait presque rien des habitations elles-mêmes" (Gutherz 1989 : 121), ce qui pourrait au moins laisser la place à quelque doute. En outre, selon un processus que nous avons mentionné plus haut, l'hypothèse admise comme un fait acquis sert ensuite de tremplin à d'autres hypothèses d'ordre sociologique : "La présence de grandes structures de cuisson destinées probablement à de grands repas cérémoniels" est l'une des idées majeures qui incitent les auteurs à envisager l'existence de "chefferies" (Vaquer et Claustre 1989 : 19). Le glissement est patent, depuis des structures dont l'interprétation "reste toujours problématique" (Guilaine et Demoule 1986 : 449) à des fours avérés, puis à de grands repas cérémoniels, puis à des chefferies.

La nature réelle de ces structures n'est pas notre sujet et nous n'en discuterons pas. Quelle qu'elle soit, l'effet de mode est d'autant plus remarquable qu'il fonctionne dans les deux sens, puisque les paléolithiciens qui ont mentionné *ces mêmes structures de Villeneuve-Tolosane* en sont restés, eux, à l'hypothèse de "surfaces d'occupation » pavées (Delporte 1984 : 104) et de fonds de cabanes (Bahn 1983 : 201-203) !...

Ainsi, non seulement la communauté des préhistoriens se divise devant des structures très proches les unes des autres selon qu'elles sont magdaléniennes ou chasséennes, mais en outre ces dernières sont interprétées différemment en fonction des traditions de pensée des divers auteurs, et cela bien que l'hypothèse des fours puisse aussi bien s'insérer dans des modèles ethnologiques pléistocènes : on verrait alors des sites "d'aggrégation" saisonniers et des

¹¹ J. Vaquer a fait des expériences de chauffage de galets, d'où il résulte qu'ils peuvent fort bien cuire diverses catégories de nourriture : "Il a pu être démontré que ces dispositifs fonctionnaient très efficacement en tant que fours" (*in* Guilaine 1986 : 180). En toute rigueur, cela signifie :

1. que cette hypothèse est envisageable;
2. mais cela ne *prouve* aucunement que cette utilisation fut celle des Néolithiques;
3. cela n'exclut pas un autre type d'utilisation pour certaines au moins de ces structures, dont les formes divergent fortement;
4. l'hypothèse inverse, c'est-à-dire l'adéquation de ces structures à l'assainissement de fonds de cabanes, n'a pas été testée expérimentalement. Si elle l'était et qu'il soit prouvé, comme il est probable, que les galets brûlés assainissent bien le sol, cela ne signifierait pas davantage que c'est cette dernière hypothèse qui doit être retenue et que celle des fours polynésiens doit être rejetée.

rassemblements magdaléniens autour de repas communautaires... L'acceptation, ou non, de ces idées répond donc bien dans un cas à une tradition de pensée, dans l'autre à une mode récente.

Il existe aussi des effets de mode qui ne concernent pas une communauté mais sont d'importance plus réduite, à l'échelle de quelques chercheurs seulement, voire d'un seul. Deux exemples de ces *modes ponctuelles* seront cités. Ils ont tous deux trait au mégalithisme et à ce que l'on pourrait appeler le "mirage atlantique", car les dolmens de l'Ouest de la France, par leur variété, leurs dimensions, leur ancienneté, leur nombre, mais également la qualité des recherches dont ils ont été et sont l'objet, font figure de pôle du mégalithisme national. Cette position exerce un effet attractif et suscite des imitations.

Le Dr. J. Arnal, qui fut un excellent préhistorien et à qui l'on doit le renouveau des études néolithiques dans le Midi de la France, ne voulut jamais démordre, pendant trente ans, de l'antiquité des dolmens méridionaux qu'il continua toute sa vie à attribuer systématiquement aux Chasséens, désirant obstinément leur prêter une ancienneté comparable à celle des dolmens atlantiques, alors qu'aucune découverte ou datation pendant toutes ces années n'est venue corroborer ses dires.

Dans le domaine des architectures, B. Pajot s'est lancé sur une voie semblable. Dans son cas, l'effet de la mode est plus évident encore, car il a modifié sans explication et pour les besoins de la cause son interprétation des constatations qu'il avait faites lui-même sur ses propres fouilles. En effet, ayant mis au jour des murettes bâties autour de la chambre sépulcrale dans le tumulus d'un dolmen du Bas-Quercy (le Frau 3, à Cazals, Tarn-et-Garonne), il constatait en 1984 que "ces structures sont toujours étayées par un remblai périphérique, qui conditionne la forme externe du tumulus" (Pajot 1984 : 92). Elles avaient donc un but pratique aisé à comprendre; empêcher l'effondrement rapide des murettes. Or, trois années plus tard, "cette masse n'est pas due à un apport intentionnel, mais à l'éboulement progressif des matériaux du tumulus" (Pajot 1987 : 39). La raison de ce changement d'optique est que l'auteur a décidé que les murettes qui circonscrivent les chambres devaient être des structures entièrement apparentes (alors qu'il se posait seulement la question d'une apparence partielle en 1984), et cela à l'instar des monuments parementés du Centre-Ouest. Il montre à ce propos le bout de l'oreille lorsqu'il écrit qu'il a élucidé "localement un problème déjà formulé et résolu ailleurs, notamment en Bretagne et en Vendée" (p. 6). Le désir d'imitation a joué et, sans apport archéologique qui aurait pu justifier un tel changement, puisque la fouille était depuis longtemps terminée et que les dolmens fouillés entre-temps ont révélé des structures rigoureusement identiques à celles du premier, les observations princeps effectuées au moment de la fouille et de la première publication s'en sont trouvées profondément modifiées.

On le voit, les conséquences d'une mode ne sont pas minces, puisqu'elles peuvent amener un même spécialiste à contredire ses propres observations de terrain. Incidemment, cet exemple illustre aussi l'illusion d'une archéologie empirique et positiviste où les vestiges parleraient d'eux-mêmes. Le chercheur cité, B. Pajot, procède à ses fouilles avec toute la minutie voulue et sa compétence de fouilleur n'est pas en cause. Or, le mode de dépôt des matériaux amoncelés pour étayer une murette est différent d'un éboulement de structures bâties et cela ne peut manquer d'être remarqué sur le terrain, ce qui fut fait initialement. C'est l'évolution du modèle explicatif qui a entraîné, *a posteriori*, une modification radicale de l'observation elle-même. Il va de soi que, une fois la mode bien implantée et le modèle prenant alors valeur de dogme, les

observations à venir, à la fois de l'intéressé et de ses imitateurs éventuels, risquent fort d'en être sensiblement affectées.

La mode produit ainsi un effet cumulatif dont il est difficile de mesurer les conséquences, puisque cet effet s'étoffe avec le temps et se renforce constamment, chaque découverte ou interprétation nouvelle allant forcément dans le sens du modèle initial et le confortant.¹² L'aboutissement ultime d'une mode, dans le domaine de l'interprétation, est une réalité faussée, ne serait-ce que parce que des hypothèses alternatives sont occultées, minimisées ou délibérément rejetées, souvent sans examen sérieux et en quelques mots méprisants, puisqu'il s'agit d'explications "anciennes", donc forcément périmées.

En d'autres domaines, le second effet négatif majeur est le gaspillage, en temps et en argent qui auraient pu plus utilement être dépensés ailleurs, dans le cas d'analyses abusives ou de fouilles à l'intérêt scientifique douteux, qu'il s'agisse d'hyperminutie injustifiée ou de recherches onéreuses sur des sites dont le seul mérite est que les frais de la fouille sont à la charge de l'aménageur.

La mode, c'est son grand avantage, finit toujours pour tourner, mais non sans avoir causé des dégâts qui, dans le cas des gaspillages, sont irréversibles.

En revanche, il ne faudrait pas, sous prétexte de mode, s'accrocher à une conception figée de la recherche qui ne conduirait qu'à la stérilité. Les changements de perspectives et de méthodes, l'apport raisonné de l'archéométrie concourent à un développement souhaitable de notre discipline, dont les nombreuses facettes sont tour à tour mieux éclairées.

Remarquons à ce propos que, si la mode affecte bien des chercheurs, sous de multiples formes et les moins légitimes, assez paradoxalement les institutions les plus vénérables, telles le C.N.R.S. et l'Université, font preuve d'une incroyable inertie pour traduire dans les faits non point certes les modes au sens péjoratif du terme (on ne le leur demande pas!), mais les tendances les mieux assurées et les plus prometteuses de la recherche.

C'est ainsi que de nombreuses disciplines admises comme des auxiliaires indispensables de la recherche moderne (tracéologie (cf. Plisson 1988), malacologie, paléozoologie, anthropologie physique, etc. etc. sont mal loties ou pas du tout (Berthoud 1984), au point qu'il a fallu qu'en 1989 la Sous-Direction de l'Archéologie sorte quelque peu de son rôle et essaye de donner un exemple incitatif en créant un poste de spécialiste en anthropologie physique. Quant aux universités, la carence des postes en Archéologie et surtout en Préhistoire, alors que ces disciplines connaissent une vogue sans précédent dans notre société, est de triste notoriété publique. Dans ce cas, on souhaiterait une plus grande réceptivité et une meilleure adéquation aux tendances du temps...

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¹² A. Leroi-Gourhan a bien perçu et décrit ce phénomène, lourd de conséquences et qui engage l'avenir : "Expliquer prématurément, étant donné la facilité avec laquelle une hypothèse séduisante est admise comme un fait probant, conduit à créer des écrans qui masquent pour longtemps une réalité qui a pu échapper au premier chercheur" (Leroi-Gourhan et Brézillon 1972 : 261).

Le but de ce article ne fut jamais de dresser un panorama ou un état de la recherche archéologique en France, vue sous un angle uniformément critique. Dans l'ensemble, malgré les graves carences citées ci-dessus sur le plan institutionnel, elle fait fort bonne figure dans le contexte européen et même mondial, comme en témoignent les trois gros ouvrages de synthèse publiés à peu près en même temps à la fin de l'année 1989 : *Le Temps de la Préhistoire*; *De Lascaux au Grand Louvre*; *Archéologie de la France*.

Mais toute médaille a son revers, et c'est une petite partie de celui-ci qui a été analysée, partant du principe que la recherche, pour être scientifique, exige la lucidité.

En théorie, nous essayons tous de découvrir *la* réalité objective, alors qu'en fait nous interprétons des données au moyen de schémas mentaux qui varient d'une époque à l'autre en fonction de nombreux paramètres dont celui de la mode. Les schémas les plus modernes n'offrent pas nécessairement davantage de garanties d'objectivité que les schémas antérieurs ¹³, bien que chaque époque pense détenir *la* vérité ou du moins marquer un progrès certain dans son approche par rapport à ce qui précède, ce qui est loin d'être prouvé.

Améliorer nos processus interprétatifs et accroître leur objectivité nécessite un effort constant de rigueur et de distanciation pour ne pas être dupes des facilités de pensée, des commodités et confortables imitations que nous offre la mode, car elle intercale bien un véritable "prisme déformant" entre l'observateur et son objet d'étude, comme il fut dit en exergue, et elle génère des abus nombreux. Une prise de conscience de l'importance et de la généralité des phénomènes de ce type, dont les dangers nous menacent à tout moment, est le préalable indispensable à tout effort vers une objectivité accrue.

Janvier 1990.

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¹³ "The belief that recent theories are more realistic than old ones is simply untenable." (Tangri 1989 : 87).

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L'IMPACT DU CHAUVINISME DANS LES ÉTUDES PALÉOLITHIQUES

par

Marcel OTTE * et Lawrence KEELEY **

RÉSUMÉ

L'allergie aux théories migrationnistes-diffusionnistes a prévalu récemment dans les explications des changements culturels observés dans le Paléolithique européen.

Cette tendance a exagéré le facteur diachronique dans les séquences régionales et la notion d'évolution autonome.

Ces visions s'opposent radicalement à ce que l'on sait pour les périodes historiques où l'on connaît de nombreux mouvements d'expansion de toutes natures.

Par quelques exemples, les auteurs montrent l'existence de phénomènes diffusionnistes dès le Paléolithique.

ABSTRACT

An "allergy" against migrationist and diffusionist theories may be observed in the prevailing explanations for cultural change in the European Palaeolithic.

This tendency has exaggerated attention to diachronic factors in regional sequences and emphasized the notion of autonomous evolution.

Such interpretations are radically opposed to those accepted for the historic periods where one finds numerous movements of peoples, ideas and techniques documented.

The authors give examples that show the existence of diffusionist phenomena in the Upper Palaeolithic of Europe.

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INTRODUCTION

Les études de synthèse sur le Paléolithique de l'Ancien Monde sont souvent fondées sur les données d'une seule région, voire sur la séquence d'un seul site réputé caractéristique. La valeur diachronique de telles reconstitutions est donc à portée géographique limitée. La signification donnée aux modifications dans de telles situations tend à renforcer implicitement l'idée d'une continuité régionale en insistant sur des phénomènes locaux.

La tendance à la mise en valeur "d'évolutions locales" fut en outre exacerbée dans la phase "post-diffusionniste" de l'archéologie récente. Ceci implique le rejet d'explications de nature "historique" pour les événements préhistoriques, tels que les migrations ou les diffusions, et l'accentuation d'explications à caractère "processuel" soulignant les changements systématiques internes aux cultures préhistoriques. L'intérêt intellectuel n'était plus orienté vers les successions d'événements préhistoriques mais vers les processus et les *stades* d'évolution sociale. Sous-jacente à cette approche processuelle se trouvait l'idée de "progrès" offrant une coloration morale aux études archéologiques. Les explications fondées sur des changements sociaux d'une région dus aux invasions ou aux diffusions impliquaient l'idée de populations sans progrès et de cultures dérivées, stagnantes. Ainsi, implicitement, les réputations de nations ou de régions (autant que celles des archéologues qui s'y identifiaient !), devinrent-elles liées à l'histoire des cultures préhistoriques et à leur interprétation. L'assimilation d'hypothèses migrationnistes ou diffusionnistes aux doctrines racistes et impérialistes fut alors teintée d'un mépris intellectuel avec une opprobre moraliste (Rouse 1986:16-18, discute également cet aspect).

Tout à la fois, ces tendances méthodiques théoriques et psychologiques (ou morales) donnèrent aux notions d'invasion, de colonisation et de diffusion massive une "mauvaise presse" et les rendirent très impopulaires en tant qu'explications des changements préhistoriques.

Ceci conduit à la création d'une "ligne de fracture" dans les reconstitutions du passé européen par exemple. La *période préhistorique* est ainsi vue comme une succession de développements autonomes éventuellement secondés par des diffusions mineures.

La seule exception tolérée généralement est la migration des premiers agriculteurs ou la diffusion d'éléments-clefs au mode de vie agricole à travers l'Europe (bien que certains soutiennent l'idée d'invention autonome de l'agriculture en Europe).

Tout au contraire, la *période historique* de la même région, telle que les textes nous la révèlent, consiste en une longue succession de mouvements de peuples et de diffusion massive de procédés et d'idées de toutes natures : technique, religieuse, sociale, politique. Citons par exemple : colonisation grecque, phénicienne, romaine, Empire romain; diffusion du monnayage, des moulins à vent, de la cavalerie, de la poudre à canon, de la navigation en haute-mer; invasions des Germains, Slaves, Huns, Musulmans, Scandinaves; diffusion du christianisme, du monachisme, de la féodalité, de la chevalerie, des universités, de la Renaissance.

De façon très symptomatique, l'*histoire* de l'Europe continentale commence précisément, dans la Guerre des Gaules de César, par la tentative de migration de milliers d'Helvètes à partir de la Suisse à travers la France !

Cette apparente différence de nature entre l'histoire et la préhistoire de l'Europe est très probablement la conséquence du plus grand intérêt *des préhistoriens* pour les interprétations et leur dégoût des migrations et diffusions. Cependant, en évitant de telles explications, les archéologues ne tombent-ils pas dans l'ignorance d'une grande partie de la réalité préhistorique ? Même un anti-migrationniste déclaré tel C. Renfrew (1987:3) se demande "... a-t-on jeté l'enfant avec l'eau du bain ?". Notre réponse quant au paléolithique supérieur d'Europe occidentale est ici positive !

DÉMONSTRATION

Les interprétations classiques des origines et des significations des cultures paléolithiques occidentales illustrent très bien les tendances ci-dessus.

MOUSTÉRIEN

La célèbre controverse sur l'interprétation des faciès moustériens français (Bordes, Binford) ne s'est jamais enrichie d'une vision pan-européenne. Une telle ouverture du champ d'étude apporte pourtant une clef à l'interprétation de ce phénomène. Certains faciès moustériens définis par F. Bordes possèdent en effet des répartitions géographiques particulières tandis que d'autres sont liés à l'environnement. G. Bosinski (1967) a bien montré que les sites du "Moustérien typique" sont bien plus communs dans la plaine d'Allemagne septentrionale, où le matériau siliceux abonde, que dans les grottes méridionales où, par contre, les ensembles "Micoquiens" sont plus fréquents, reflet de la carence en matériaux accentué par la structure d'accueil des grottes favorisant la préservation des vestiges. Les industries à outils bifaciaux sont limitées à l'est et au centre de l'Europe et se distinguent des industries occidentales sur éclats à retouches unifaces (Gabori 1976). De telles répartitions régionales indiquent l'existence de *traditions* tandis que les faciès à valeur technique (denticulés, Levallois) reflètent l'importance d'activités variées (Geneste 1985). Récemment, l'évolution diachronique, limitée à une seule région, a été considérée comme le principal facteur de variation des "faciès" moustériens (Mellars 1969).

CHATELPERRONIEN

Ce Moustérien, également appelé "Périgordien inférieur", est considéré par certains préhistoriens comme un aspect indigène du Paléolithique supérieur ancien occidental (Gamble 1986). Sa forte ressemblance avec le "Moustérien de tradition acheuléenne" (pièces-supports et types d'outils) a depuis longtemps été soulignée (Bordes 1968; Leroi-Gourhan 1964) mais ses quelques traits leptolithiques (rares outils sur lames, très rares pointes osseuses et dents perforées) ont été exagérés. Bordes avait en effet prétendu qu'ils étaient le fait de l'homme moderne et non de Néandertaliens. Cet ensemble était en outre considéré à la fois par D. Peyrony et par F. Bordes comme le précurseur du techno-complexe gravettien, intégralement paléolithique supérieur (dénommé aussi "Périgordien supérieur") et donc constituait le terme de continuité entre les industries du Paléolithique moyen et celles du Paléolithique supérieur local ! Malgré la découverte de l'auteur *néandertalien* de cette industrie chatelperronienne (Vandermeersch 1989; Lévêque 1989), de ses évidents caractères moustériens et de l'absence complète d'un stade

“Périgordien moyen” formant transition avec le véritable Paléolithique supérieur local, il est encore considéré comme du Paléolithique supérieur autochtone et démontre ainsi l’emploi de cette technologie par les Néandertaliens (Farizy et Schmider 1985). Les quelques traits “modernes” du Chatelperronien sont plus facilement expliqués par une dérive à partir de traits empruntés aux immigrants aurignaciens que par inventions indépendantes (Klein 1973:116). Il n’est de plus pas exclu que perles et poinçons soient de fabrication aurignacienne et obtenus par échange.

AURIGNACIEN

Considérée comme un stade dans la chronologie d’Europe occidentale, l’apparition de l’Aurignacien n’y est en fait que l’aboutissement évident d’un mouvement dynamique déployé à travers toute l’Europe méridionale et centrale ¹. La technique aurignacienne est complètement du Paléolithique supérieur avec la production de lames-supports tirées de nucléus prismatiques, de nombreux burins et grattoirs variés. Une quantité d’autres innovations se trouvent liées à ces caractères aurignaciens : de nombreux objets en os ou en ivoire dont les pointes de trait, l’art mobilier, les pendeloques et autres pièces à fonction décorative, les premières preuves de la récolte de coquillages et de la pêche, qui impliquent par ailleurs de nouveaux comportements. En effet, cette modification culturelle, irréversible et drastique, est associée à l’apparition d’un nouveau type anatomique, l’homme moderne. Il semble incontestable que, pour l’Europe occidentale, les hommes modernes, auteurs de l’industrie aurignacienne, aient remplacé les Néandertaliens utilisant la technique du Moustérien final (Chatelperronien y compris).

Selon une vision diffusioniste, cette migration a aussi pu affecter le comportement des moustériens récents d’Europe septentrionale (telles les industries à pointes foliacées, le Szélétien etc ...) comme il le fit pour l’Occident.

La typologie de l’Aurignacien apporte aussi quelques-uns des plus clairs exemples de la focalisation sur le local par laquelle les préhistoriens régionaux ont obscurci la nature de la répartition aurignacienne. La même pointe ou lamelle retouchée connue comme une “Font-Yves” à l’ouest de l’Europe est appelée de “Krems” en Europe centrale, une “Arjenah” en Iran (où l’industrie est appelée “Baradostien”) et une “El Wad” au Proche-Orient levantin. De la même façon, la pointe osseuse à base massive de France a été dénommée de Mladec en Yougoslavie, Olchévienne en Autriche et de Mamutowa en Pologne.

GRAVETTIEN

Le cas du mouvement gravettien est moins simple. Les nombreux faciès définis jadis par trois générations de spécialistes peuvent être aujourd’hui clairement associés à des traditions successives dont les distributions à travers l’ensemble de l’Europe sont très variées.

¹ Les dates fort anciennes de l’Aurignacien espagnol récemment obtenues (Bischoff *et al.* 1989 ; Cabrera Valdes et Bischoff 1989) réalisées à l’aide de l’accélérateur ne sont donc pas directement comparables aux autres dates européennes obtenues par la méthode classique. Cependant, les auteurs cités démontrent que l’Aurignacien remplace rapidement le Moustérien en Espagne également.

Le faciès aux pointes de la Gravette ("à fléchettes" ou Périgordien IV de France) est identique à celui d'Autriche, de Bavière, de Souabe et manifeste un mouvement d'est en ouest. Les sites d'Europe centrale sont systématiquement plus anciens que ceux de l'ouest (fig. 1). Le faciès aux pièces pédonculées ou de "Font-Robert" (Périgordien V A du sud-ouest français) est limité aux régions occidentales et est clairement plus ancien au nord qu'au sud (Otte 1990). Le faciès noaillien (Périgordien V C) est par contre distribué davantage vers le sud (David 1973). Cette séquence est observée clairement dans les sites d'Europe centrale (Molodova, Willendorf) où chaque stade peut être mis en relation avec des traditions diffusées dans des régions variées. Chacun de ces faciès fut découvert en succession stratigraphique, donnant ainsi une clef de leur chronologie relative. Les variations dans les répartitions géographiques montrent ainsi les variations des mouvements d'expansion propres à chaque stade ou faciès (Otte et Haesaerts 1987; Otte 1981).

SOLUTRÉEN

L'origine de ce techno-complexe reste obscure et, à cause de son aspect particulier, est un sujet de controverse (Smith 1966). Son caractère original, par rapport à toutes les autres traditions leptolithiques antérieures ou postérieures d'Europe occidentale, réside dans sa technologie lithique, particulièrement quant à la production de minces pièces bifaces. Dans sa forme la plus accomplie, le traitement thermique des supports de pièces bifaces ou des pré-formes et la délicate retouche par pression sont utilisés pour produire une grande variété de types. Par ailleurs, les techniques des matières osseuses et celle de production des lames rappellent le Gravettien. Le Solutrén "classique" se trouve seulement dans le sud de la France, en Espagne et au Portugal. Mais il est impossible de découvrir un précurseur au Solutrén dans sa technique bifaciale dans la gamme d'outils des traditions aurignaciennes ou gravettiennes du sud-ouest européen.

MAGDALÉNIEN

Cette tradition montre également une expansion territoriale mais dirigée cette fois en sens opposé. Dans ses aspects techniques, elle évoque un amalgame de composantes découvertes dans différentes industries précédentes (Gravettien, Badegoulien, Aurignacien). Son développement est marqué par une succession d'influences issues de plusieurs régions européennes dont l'Europe orientale et les plaines septentrionales (Kozlowski 1985). L'apparente "évolution" magdalénienne est traversée par une série d'innovations qui, considérées à l'échelle européenne, représentent une succession de phénomènes d'acculturation. Les navettes, harpons, pointes à cran et microlithes sont autant d'exemples par lesquels on peut suivre une diffusion à large échelle. La diffusion de motifs iconographiques et de traits stylistiques est plus frappante encore : statuettes féminines, gravures d'équidés (Bosinski 1982). Ces innovations diffusées et celles résultant de contacts inter-ethniques se combinent pour donner l'apparence, dans chaque région, d'une évolution stadiaire.

Dans la phase médiane (Magdalénien "à navettes"), une traînée de sites s'étend dans la zone des collines de l'est de la Pologne à l'est de la France (fig. 2). Les dates les plus anciennes sont

occidentales et les traces de contact (matériaux, motifs décoratifs) témoignent de liens avec les plaines russes (Kozlowski et Desbrosse 1988).

Dans la phase finale, le Hambourgien récent, originaire d'Allemagne centrale, semble traverser la plaine d'Europe du nord (fig. 3) durant le Bölling, vers les Pays-Bas puis la France septentrionale (Otte 1990).

Le Magdalénien est donc un techno-complexe très composite, intégrant des traditions d'origine, de distribution géographique, de mode adaptatif et de stades de développements variés. Ne considérer qu'un seul aspect dans une seule région dissimule complètement le rôle et le sens des facteurs extérieurs qui ont pour effet la production de ces nombreuses variables.

CONCLUSIONS

Après avoir longuement dérivé vers la tendance à l'invention indépendante et l'évolution interne dans une réaction peut-être justifiée au migrationisme, les interprétations des préhistoriens devraient, à nos yeux, redevenir plus réalistes et plus concrètes. En particulier, il semble nécessaire de tenir davantage compte de l'évidente mobilité des groupes humains et de la diffusion rapide des traits culturels. Dans une perspective à longue échéance, les moteurs les plus fréquents aux variations successives de techniques, d'économies et d'aspects sociaux pour une région donnée peuvent être les échanges et les remplacements créés via les contacts, les conquêtes et les migrations.

Il ne nous semble ni logique ni pertinent de maintenir une nette distinction entre les modifications évolutives (augmentant la complexité ou l'échelle d'une entité économique, politique ou sociale) et celles apportées par diffusion, migrations et conquêtes. Les changements remarquables récemment observés dans la complexité, l'échelle et le champ de la technologie et de l'économie japonaise, reflètent à la fois une conquête et des contacts prolongés avec les américains. Cependant, l'économie qui en résulte et le contexte social sont nettement japonais et la population reste génétiquement, linguistiquement, religieusement et artistiquement authentique et nipponne. Le passage vers le Paléolithique supérieur d'Europe occidentale a pu se faire selon des directions similaires. Le Chatelperronien, demeuré "ethniquement" néandertalien, culturellement moustérien, acquit quelques composantes comportementales nouvelles seulement à travers les contacts avec les immigrants aurignaciens. Mais, dans le cas paléolithique, les immigrants remplacent complètement les indigènes et tous les vestiges de leur mode de vie. Les états nationaux ou impériaux de l'Europe doivent une large part de leur puissance à la diffusion de la poudre à canon chinoise et, par conséquent, leur conquête en tant que groupe régional ethnique ou linguistique sur de nombreux autres (considérez les différences culturelles entre les Comtés familiaux d'Ecosse et les Highlands, le Brandebourg et la Souabe, le Piémont et la Sicile; la Castille et la Catalogne). Les emprunts culturels, volontaires ou forcés, les immigrations et les conquêtes sont des composantes plutôt que des anti-thèses de l'évolution sociale. Les facteurs de migration et de diffusion doivent à nos yeux être reconsidérés dans les interprétations processuelles. Ils doivent être replacés parmi les explications du changement au même titre que les facteurs internes. Les migrations et les diffusions sont des processus à caractère récurrent et non simplement des accidents de l'histoire. Les mêmes réponses des indiens des plaines au nord et au sud des Amériques à l'introduction du cheval sont un exemple de tels processus récurrents.

Dans les deux cas, des horticulteurs sédentaires sont devenus chasseurs nomades et cavaliers. Il y a aussi des caractères récurrents aux migrations qui peuvent être intégrés à des explications processuelles. Certaines régions par exemple semblent être des sources de vagues de migrations répétées. Les régions semi-désertiques d'Eurasie centrale, du Mexique du Nord et peut-être du Sahel africain ont produit de nombreuses migrations. Ceci peut être mis en relation avec la forte variabilité à longue échéance du taux de précipitations dans ces zones. Ce taux est suffisant pour permettre aux populations de se développer mais les sécheresses périodiques poussent les habitants à choisir entre la famine et la migration vers des terres mieux arrosées. Les origines probables de l'Aurignacien et du Gravettien quelque part à l'est de l'Europe montrent que ces régions étaient une source de migrations autant préhistoriques qu'historiques.

Si les préhistoriens doivent réellement assumer leur tâche, ils ne peuvent admettre une telle myopie théorique, un chauvinisme régional et une concentration sur le local afin de diminuer ou obscurcir les considérations des processus à grande échelle quant à la mobilité humaine et la facilité d'emprunts intellectuels.

Sur le plan méthodologique, c'est l'absence d'une vision "continentale", voire intercontinentale, qui limite souvent les interprétations archéologiques et entrave leurs estimations scientifiques. L'archéologie, comme la géologie ou l'astronomie, n'est pas une science expérimentale mais doit se fonder sur des comparaisons contrôlées pour évaluer des hypothèses et isoler des causes. Lorsque ces comparaisons restent trop locales, le contrôle de nombreuses variables est perdu; particulièrement celles qui dépassent ou traversent les régions.

L'apparition des types d'outils lithiques et osseux du Gravettien, de nouvelles méthodes de production laminaire, des figurines féminines obèses, assez différentes de celles de l'Aurignacien précédent, ont poussé les préhistoriens du sud-ouest européen à modifier le sens du Chatelperronien. Ce phénomène est local en effet et, en diminuant ces traits archaïques, on l'élève d'un statut presque complètement moustérien vers celui de paléolithique supérieur ancien, postulant l'existence non avérée d'un "Périgordien moyen" qui permet de le mettre en relation avec le Gravettien, tandis que, dans la région opposée, vers l'est de l'Europe, des ensembles découverts avec les mêmes traits stylistiques datent également d'une période antérieure au Gravettien occidental. Seule la précision dans les chronologies locales à l'aide de nombreuses datations radiométriques et leurs comparaisons les unes aux autres nous permettra de distinguer les mouvements des peuples et des techniques.

Finalement, revenant à notre concept de "focalisation locale", la séquence du paléolithique supérieur d'Europe occidentale ne peut être scientifiquement comprise que lorsque ses caractères sont comparés à ceux du reste de l'Europe et même du Proche-Orient. D'une façon générale, les préhistoriens devraient s'interdire de considérer l'Europe occidentale comme une île plutôt que comme une partie d'un continent, "une partie d'un tout".

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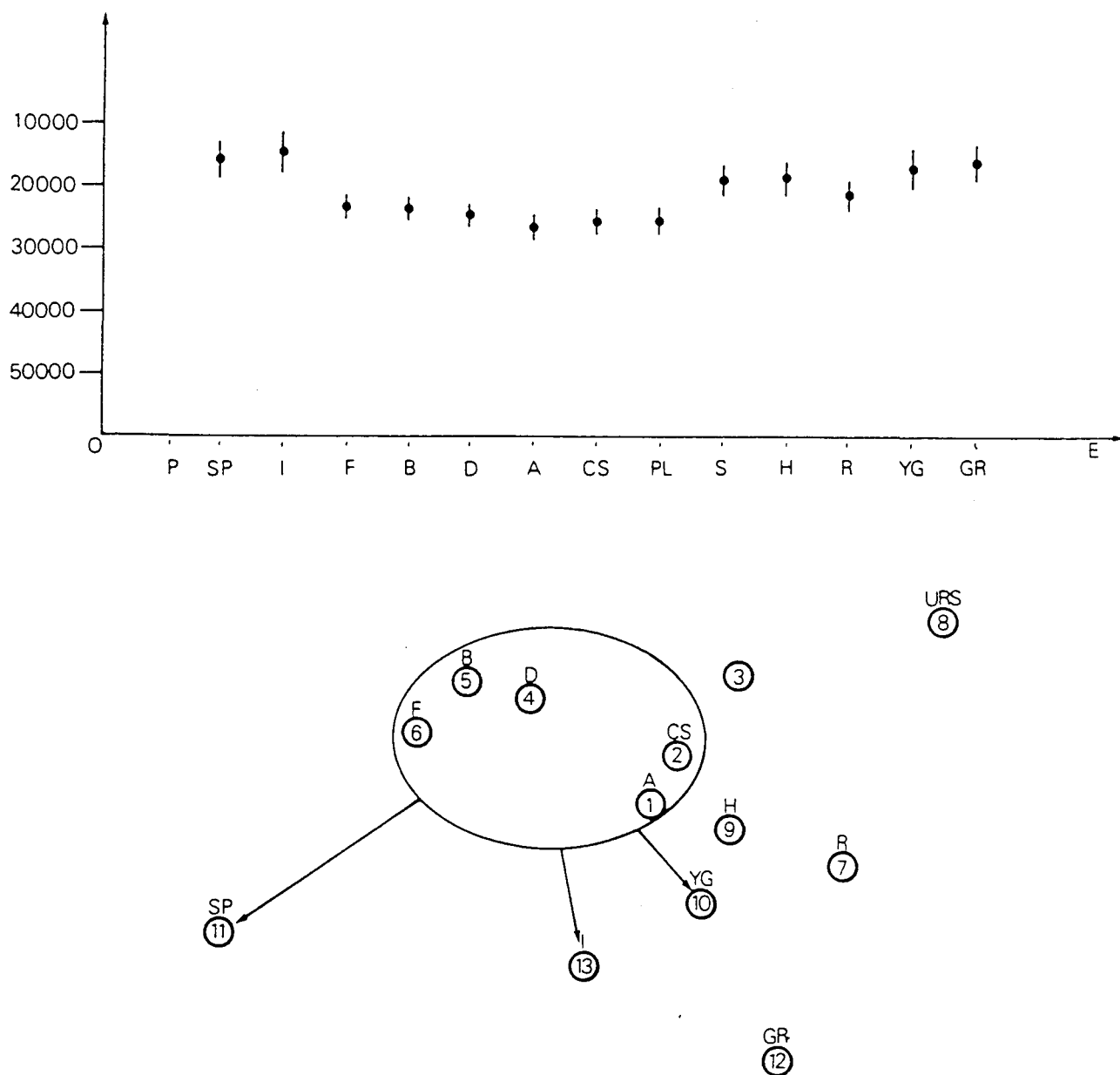


Figure 1 : Chronologie et répartition géographique du Gravettien (les lettres indiquent le pays, telles les plaques minéralogiques).

haut : les moyennes des datations C14 montrent une tendance à la diffusion à partir du centre vers l'est et l'ouest du Continent.

bas : sériation par ordre chronologique des différentes régions européennes où des sites gravettiens sont connus. Ici à nouveau, la zone centrale du continent semble la plus anciennement occupée par le Gravettien. Durant la phase la plus froide du dernier glaciaire, un mouvement de diffusion vers le sud-est et le sud-ouest semble se manifester.

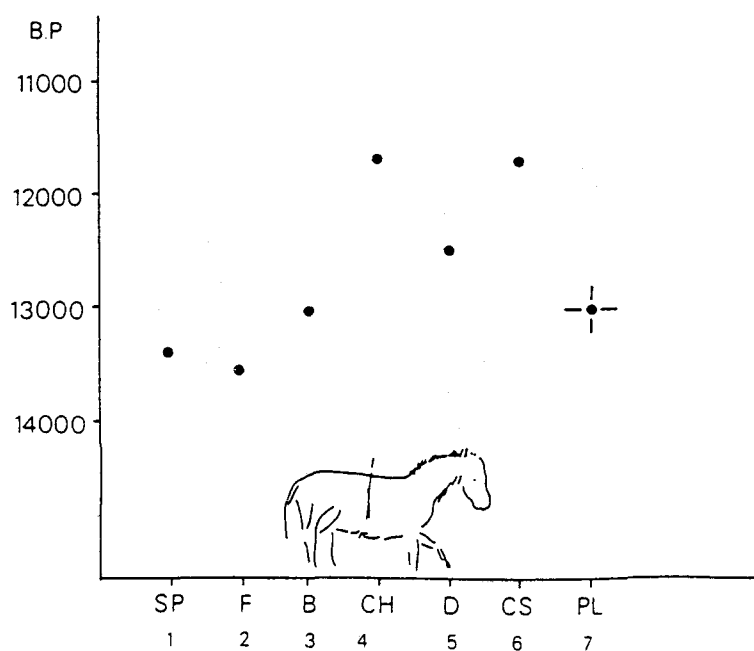
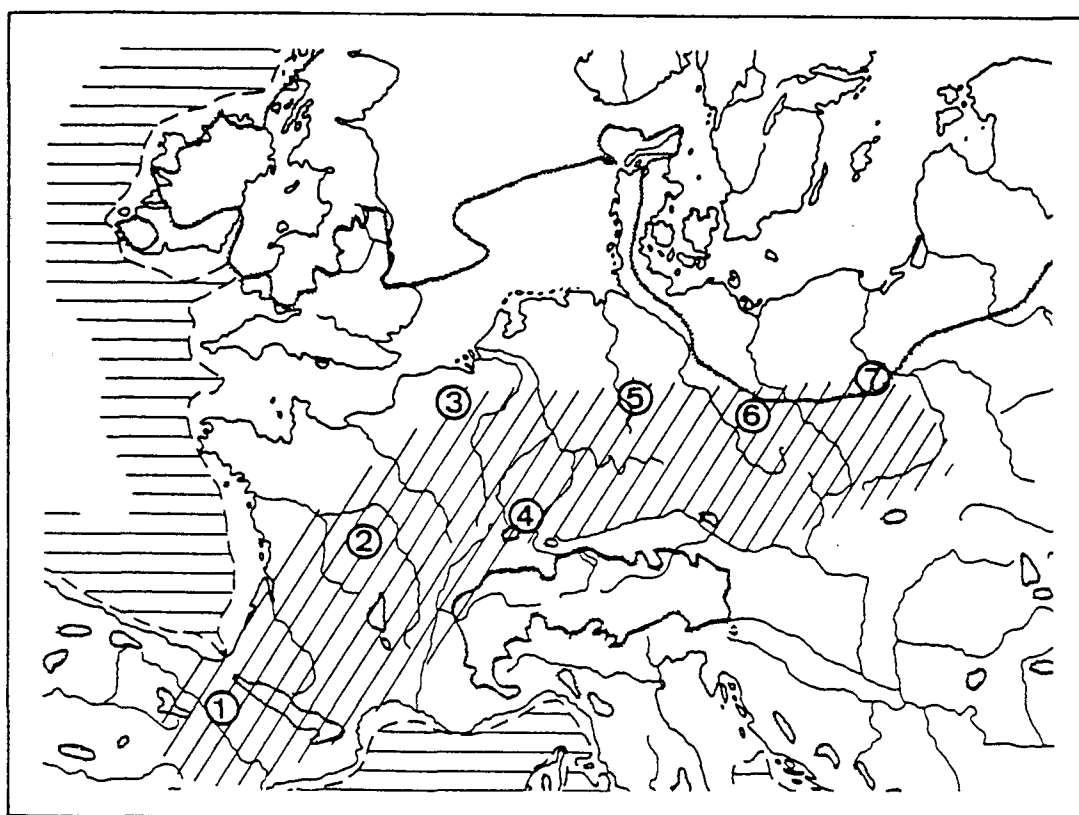


Figure 2 : Répartition géographique et chronologique du Magdalénien ancien et moyen à travers l'Europe (sigles = codes nationaux des plaques minéralogiques). Aires géographiques de la carte : 1 : Pyrénées, 2 : Sud-Ouest : 3 : Belgique : 4 : Suisse, 5 : Thuringe, 6 : Moravie, 7 : Pologne. Les dates montrent l'extension vers le nord-ouest de l'Europe à mesure du retrait glaciaire. La date polonaise (croix) est la plus ancienne et la plus orientale. Elle indique une composante distincte dans la formulation du Magdalénien supérieur (plaines russes : matériaux et motifs décoratifs).

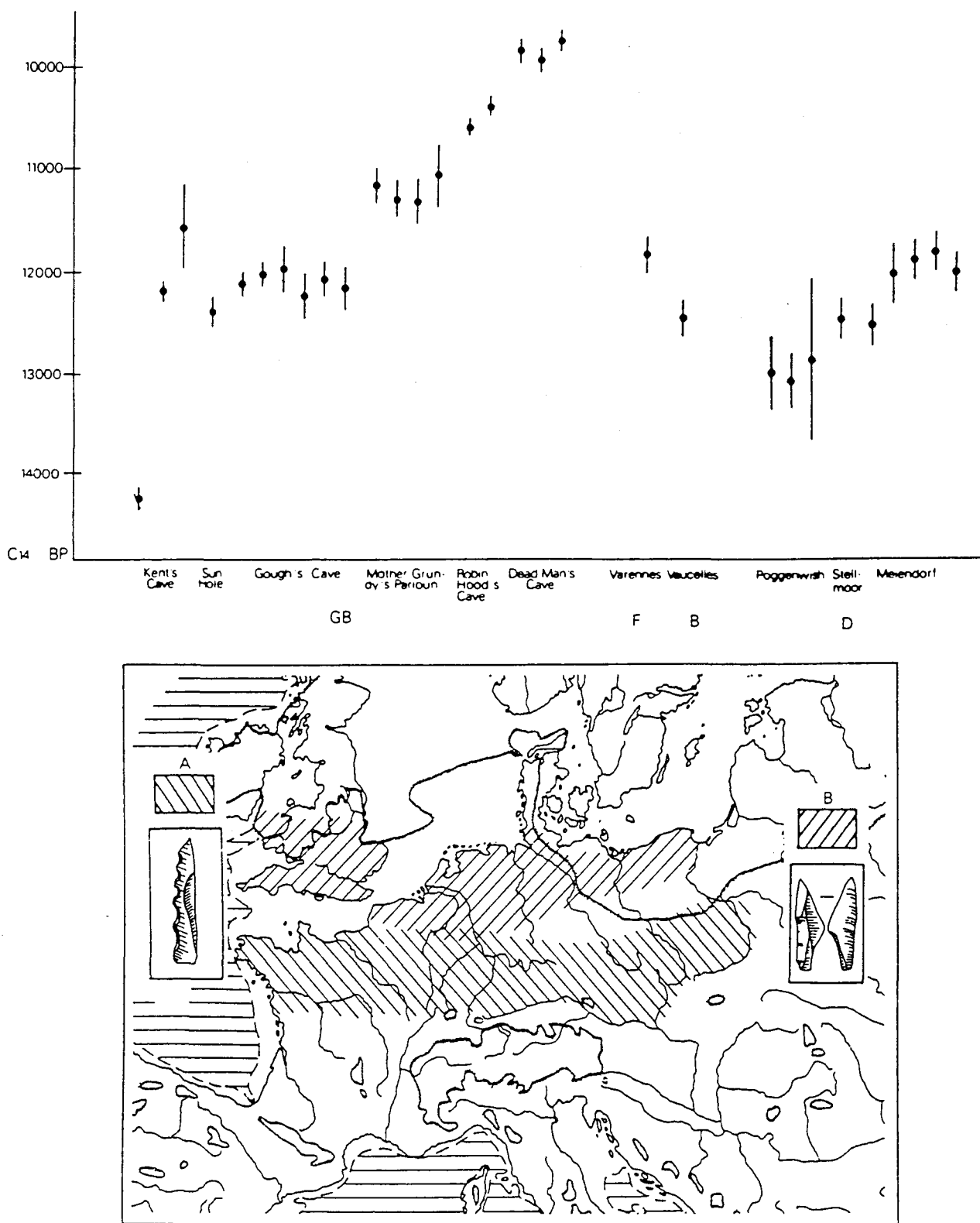


Figure 3 : Chronologie C14 et répartition géographique des industries septentrionales à pointes à cran (B) comparée au Magdalénien final (A). Cette carte fait suite à la phase évoquée à la figure n° 2. Au cours du Bölling, le Magdalénien, d'abord limité aux collines d'Europe moyenne, semble avoir donné naissance au Hambourgien des plaines d'Allemagne septentrionale. Les dates soutiennent l'idée d'une diffusion "à rebours" d'est en ouest dans la zone marginale au front glaciaire, à travers la Mer du Nord alors exondée. Ce mouvement donne naissance au "Creswellien" de Belgique et de Grande-Bretagne et influence ultimement le Magdalénien récent du nord et du centre de la France (Belloy, Cepoy, Marsangy).

PESSAH BAR-ADON PORTRAIT OF AN ARCHAEOLOGIST

by

Shoshana HEIMAN *

He burst upon the desert, like one possessed.

He rose early, strapped on a water-bottle, thrust some dry bread and a handful of olives into his knapsack.

He sped off in the direction of the beckoning wind. His eyes scanned each boulder protruding through the surface, seeking out every cluster of stones hidden from view as if it was signalling in some arcane code and attesting to the remains of an ancient settlement, human traces from bygone days. But mainly his sharp eyes followed the rhythms of the bare hills and mountains, discovering the entrances to the caves that snaked into the very heart of the mount.

From his youth, still at his studies, the spirit of the desert, of freedom, plucked at him. Free as a bird carried on the currents of air through the clouds he moved lightfooted in the trail of the bedouins, the wild tribes of the desert, until they adopted him as a son. Three full years he herded the flocks in the desert hills of the Hauran, learnt the dialects of the Bedouin tongue, became alert to the winds of the wilderness and the secrets of its paths. Later, the scorched Judean Desert set his imagination aflame. This desert is abundant in caves in which ancient parchment scrolls have been discovered, marvelously preserved in the dry air of the lowest desert on earth.

At the beginning of the 1960s he set out at the head of one of four archaeological expeditions to survey a number of caves in the desert valleys.

It fell to his lot to come upon a large cave in the valley wall overhanging Nathal Mishmar, and the approach to it was possible only by a rope ladder anchored to the rock over a deep chasm.

The dig in the cave lasted a whole season. They cleaned it out; lying on their bellies they removed from it an enormous amount of thick dust, clinging and suffocating. After the effort of an entire season the cave yielded up not a single find that might signify if it was worth going on

* Shoshana Heimann is a sculptor who for many years followed Pessah Bar-Adon in his desert endeavours and participated in some of his archaeological surveys.

for another season. The group of diggers, drawn to this radiant man whose ardour had sparked their imagination, broke up.

When the next excavating season came round he decided, contrary to the counsel of his archaeologist friends, following an inner, mysterious call, to return to the cave for another look. This time he went to the place at his own devices, again followed by enthusiastic volunteers, whose only payment was being in the presence of this special man, who shared with them his dreams.

They went back to that cave, clear of dust, back to peer into every crack and every fissure, and so it was that one of the volunteers came upon a slight space between two rocks. When together they started to shift one of the rocks they found in the murk of the pit an object looking like a wrapped parcel. Tremendously excited and with bated breath they gingerly extricated it and began to unwrap it. From it they drew out, one by one, four hundred and twenty-nine mysterious ritual vessels, of copper, ivory and haematic stone, stunning in their beauty and perfect workmanship. He, in a daze at the magnificence and size of the hoard, in his haste used the field telephone to call David Ben-Gurion, then Prime Minister, and declared with great emotion : "Here is yet another *kushan* (title deed) attesting to our right to this land..."

Dispersed among the splendid objects there were also grains of ancient wheat and olive and date stones, all saved from decay and destruction by the dry desert climate. The objects also testified to the existence of refugees, some six millennia distant in time, who as they fled hid the symbols of their spirit in that cave until the storm might pass.

The experience of discovery of the Cave of the Treasure drove him, in the years that followed, again and again, to the vastnesses of the arid desert, to its tortuous tracks, to examine every cave and cranny, as if his legs sprouted wings, and the everlasting dream, the hope of one more find to excite and reverberate across the world obsessed his soul.

Leaping from rock to rock, cave to cave, scraping away the heavy, sticky dust : perhaps hidden beneath is one more object or sign of another treasure. Each day darkness fell, and with it came weariness; but the next morning a new day dawned bringing fresh hope. Thus the weeks passed, the months, and years, but the longing and the perpetual expectation never grew dry.

One day he caught sight of the opening to a cave overhanging a deep gorge. There was no access to it, but this merely aroused his imagination more, here, in the heart of the desert, without water or shade.

He approached a friend, a senior army officer, for help and support in conducting a survey of the cave. His friend called in an entire unit with all its equipment, its tents, its small and large tools, a kitchen, and food, and a sentry roster.

The whole unit set off into the desert, to that awesome place, without shade, only sharp flint stones, fieldmice, wasps, yellow scorpions and the sun beating down without mercy.

Towards evening the unit pitched its tents near the cave. Early next morning, after some fruitless attempts, the ladder was fixed to the top of the rock, and the first men climbed down to

the cave entrance. The rockfall there was heavy and the thick, stifling dust blocked ingress to the depths of the cavern.

Ant-like, they slowly penetrated the interior, removing dust and rocks, returning to their tents at sunset worn out, grimy, exhausted.

The days followed each other, and they cleared away the rubble. They came upon a second chamber, and charred wooden beams suspended from the ceiling, on which were marks of fire and soot, of ancient times. But of other finds there were none.

The ritual of scraping away the dirt went on, day by day, each morning bearing its hopes, each evening its disappointments.

In a desert township, a few hours' journey distant along a track hardly passable, a television crew waited to document the resounding discovery of the finds...

Deep despair began to take hold, after excavations lasting almost three weeks without a single significant find. In the evenings Pessah tried to rally the drooping spirits, telling legends of the desert, but nothing could relieve the profound disappointment and the depression of the parched, unshaded spot, the birds of prey wheeling above, as though circling a carcass, just awaiting the camp to be struck, the flight of those who had invaded their realm.

The souls of the scrapers, stretched to the limits of their capacity, began to shrivel.

Three weeks later, exactly the span allotted to the task, the camp broke up, and in fragile silence the unit returned to its base.

The great hope evaporated with the winds of the desert and there remained only the shadow of Pessah's outstanding personality, like a monument in the desert around which tales are woven, motionless, unstimulating in that eerie place...

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