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Australian Rock Art Research Association (AURA)
and International Federation of Rock Art Organizations (IFRAO)

ROCK ART RESEARCH

Volume 8, Number 1 MAY 1991

Ibis Shelter, Quinkan Gallery, Site B(2), near Laura, north Queensland (the base for AURA congress field trips in 1992).
Photograph by G. Chaloupka.

The journal *Rock Art Research* is devoted to developing theory and methodology for the systematic and rigorous understanding of palaeoarts and related phenomena. Emphasis is given to communication across the various disciplines related to the study of global rock art, and to synthesising related subjects around the journal's focus: the surviving externalisations of early world views.

Contributions should be consistent with these general goals. Notes for contributors appear on the inside of the journal's back cover. All major articles submitted will be refereed. While final responsibility for the acceptance or rejection rests with the editor, responsibility for opinions expressed or data introduced is always the author's. In particular, the author is responsible for obtaining all approvals to clear text and illustrations for publication, especially with traditional indigenous custodians of rock art where these still exist.

Selected manuscripts will be sent to commentators and their reviews may be published in order to promote scholarly debate, in which case the author will be requested to respond to these comments. In addition to articles reporting original research of significance the submission of brief reports, conference reports, reviews and bibliographical entries is also invited.

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The principal objectives of the Australian Rock Art Research Association are to provide a forum for the dissemination of research findings; to promote Aboriginal custodianship of sites externalising traditional Australian culture; to co-ordinate studies concerning the significance, distribution and conservation of rock art, both nationally and with individuals and organisations abroad; and to generally promote awareness and appreciation of Australia's indigenous cultural heritage.

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The contents of this journal are listed in the *International Current Awareness Services* and in the *International Bibliography of Social Science*. Major papers are abstracted by international abstracting services.

KEYWORDS: **Rock art research - Discipline - Demarcation - Terminology - Pluralism**

A NEW NAME FOR A NEW DISCIPLINE

Osaga Odak

Abstract. It is argued in this paper that the established disciplines claiming rock art studies as part of their scope, such as ethnography, archaeology and art history, have treated this area as peripheral. Through their neglect they have contributed to its theoretical, methodological and technical backwardness. The author advocates the establishment of a new discipline, and the introduction of a new name providing this discipline with a distinct identity.

Although rock art studies have been conducted for well over a century, and despite its world-wide distribution, rock art has not until recently received sufficient attention. There are several reasons for this, but the most important is the lack of separate identity as a scientific discipline. Considering that a number of well-established disciplines have been involved in rock art studies it is ironic that so little progress has been achieved towards making it a separate field of enquiry.

With the recent interest in the subject, and the intensification of research efforts which has led to the discovery of rock art in nearly all countries of the world and to the realisation of the subject's universality, the need for further and systematic study of rock art has immensely increased. Thus there has emerged a need to understand the nature of rock art and its relationship to other aspects of human behaviour - besides understanding its function, both for present-day people, and for those who produced it. Accordingly, the systematic study only begun recently has registered amazing achievements, both in data collection and perfection of methodological approaches, coupled with formulation of theoretical principles for guiding further research. This impressive achievement is seen in the number of sites discovered, the inventory of individual figures registered world-wide and the number of people involved in rock art research. In the wake of these developments there have been improvements in method and in the analytical tools applied. New theories specifically concerned with explaining rock art-related phenomena are being proposed, among others.

Nevertheless, in the face of these achievements, there still remain enormous problems to be solved. Rock art is still known only to the few specialists involved in its study. One reason for this is the mechanism of informing the public about the subject, which is entirely inadequate. Rock art studies have not been introduced into the academic programs of many countries, and one gains access to it only via selected established disciplines. This is because it is not an independent scientific discipline, teachable at various levels in the world's educational programs. This state is likely to continue not only until the discipline has an all-embracing name that reflects its subject matter, but also is an independent scientific discipline removed from the covers of well-established allied disciplines in which it is currently couched.

This paper aims at examining some of the issues responsible for the inadequate awareness about rock art of the world's public, and offers suggestions on some measures to be taken so that it can take its rightful place among other disciplines.

The irrelevance of the term 'rock art'

In one of my papers presented at the 1988 First AURA Congress in Darwin, Australia (Odak 1988), I argued for the need for an independent status of rock art studies (RAS) as a scientific discipline. I noted that the word 'art', which is associated with the subject of our study, is illusive. The word comes from the Latin word *ars*, which means skill. The Chamber's Encyclopaedia (1955) defines *art* as including 'all phenomena which are due to the exercise of human ability as opposed to those in man which man in no way controls'. This equates the word art to culture, which White (1959) has defined as 'extrasomatic continuum'. Art, in this sense, excludes anything to do with human physical constitution and is not part of the physical environment. So art is equivalent to culture. But as we know, our preoccupation is with only one aspect of human behaviour, and not the totality of it. The Encyclopaedia Americana (1977: 382) is more specific: art, as commonly used today, 'means visual arts, those areas of artistic creativity that seek to communicate through the eye'. To what extent the subject matter of our study aims specifically at communicating through the eye is debatable. Even if we admit that the term art as defined has the connotation of 'achievements of human skills the aim of which is to give pleasure rather than utility' (Chamber's Encyclopaedia 1955), this does not help us much in associating our subject to the term *art* since we are not sure that all these paintings and engravings were meant merely to provide pleasure rather than other forms of utility. But all results of human skill are part of culture and so the definition does not distinguish our subject from culture, nor what is generally known as art. In contrast, the reasons why modern paintings, sculpture and graphic art (e.g. woodcutting, etching, engraving) are produced are known, and these products can be dubbed art in a conventional sense - if by that we mean those aspects of human creativity with the fundamental role of providing

pleasure to the human eye.

Secondly, the phrase 'rock art' literally associates the subject of our study with rock; one would justifiably define it as 'art executed on rock surfaces'. But we also study engravings on bone, antler or mobiliary stone artefacts, as has been extensively done by Marshack (e.g. 1969, 1972, 1979a, 1979b, 1983) and d'Errico (e.g. 1987, 1988, 1989). Some of the bao games (Townshend 1979: *bao* is a Swahili word for a game board with series of holes, for a game which is very popular in east Africa) are not executed on a rock surface, but on mobiliary wooden surfaces, or the games could be played out of holes hollowed out on the surface of soil. Painting or engraving executed on the walls of mud houses (Cory 1953) are legitimately part of the phenomena we study, yet they are similarly not rock art.

Accordingly, the phrase 'rock art' does not cover the entire scope of our subject and its conceptual dimensions, and so must be rejected as inadequate.

Some allied disciplines

Rock art is almost unknown outside of certain allied scientific disciplines. It is studied as part of general history, history of art or cultural history. In Kenya, conventional or art historians have paid little attention to the study of rock art beyond its mere mention in passing, under the name 'prehistoric art'. The general concern of art historians with aesthetic interpretation does a great disservice to the subject, limits its scope, distorts its objectives and deprives it of appropriate and relevant method. It neglects dimensions of the subject that may not be amenable to aesthetic interpretation. This is besides the fact that conventional historians cursorily mention it as one of the early stages through which artistic evolution must have passed.

However, what is known as rock art is not just a stage in the evolution of art, nor just a representative of a period in cultural history, but part of a cultural system of all peoples irrespective of the levels of socio-economic development. Otherwise there can be a temptation to regard the art created by certain contemporary peoples as standing at a lower level of cultural progress within an evolutionary continuum - an assertion which would be erroneous, considering the relative uniqueness of every culture and parts of it, with all the functions the latter perform within the culture itself.

Ethnology

In ethnology, one of the disciplines dealing with rock art, the term 'primitive art' is often applied to the rock paintings and petroglyphs of present-day or recent peoples. 'Primitive art' also includes sculpture and other forms of art produced by non-Western peoples, such as nearly all African art, the art of the Australian Aborigines, the art of Amerindians and of several peoples in some parts of Asia and Oceania. These art forms, of which rock art is an aspect, have specific functions within the socio-cultural environments of these societies. The art is executed under certain conditions and performs particular social functions, as exemplified by religious, magical, social and creative significance. Certain rock paintings or petroglyphs are directly associated with particular customs of the people (Culwick 1931), wherein they are an integral part of those customs. According to Muensterberger (1971: 11), 'the life of Aboriginal Australians points up to the interaction between mores, economic demands, religious ceremonials with artistic expression'. In a way, modern and historical ethnography of these peoples provides the context within which art is executed. In their study of different rituals, ethnographers see the so-called 'primitive art' as part of their subject as this provides them with supplementary information for enriching their description of the concerned people's culture. So the concern of ethnographers is principally with documenting the attitudinal and contextual parameters as viewed by the present population (Odak 1980) or the population of the recent past. Ethnographers not only describe the rituals associated with rock art but also the art itself, and so provide the context in which the art was executed. Since art per se is not seen by them as a central focus of study, but largely as a means of obtaining additional information, ethnographers pay only lip service to the study of rock art. The fact that they are interested in the study of the subject within the contemporary or recent sociocultural matrix is an argument against the use of 'prehistoric art' as the name for our discipline. Hence they have a claim on one branch of rock art studies; i.e. the recent, ethnographic art as opposed to 'prehistoric art' which is of interest to archaeologists.

Archaeology

Archaeology has been the main claimant for rock art. Its major interest is the mentioned prehistoric art which provides it with information that cannot be easily obtained through conventional archaeological methods. Prehistoric art, in that capacity, serves as an artefact providing information on past human cultures. Rock art sites in Kenya, for instance, were reported and preliminarily investigated mainly be archaeologists. Archaeologists, in studying rock art, have seen and treated it as part of their discipline. However, the main focal point has been prehistory, that is, pre-literate periods of human history. Both rock paintings and petroglyphs executed by modern or recent peoples - the domain of ethnography - pejoratively labelled 'primitive art' (there are journals whose titles still retain 'primitive' or 'prehistoric'), have been published in journals dealing with 'prehistoric and primitive art' by archaeologists. In these journals rock art is treated as representing lower stages of artistic development, of which modern, aesthetically interpreted visual art is seen as the highest stage of that development.

In regarding rock paintings and petroglyphs as branches of archaeology, attempts are made to employ archaeological methods. However, the success has been largely limited as is evidenced by the fact that one of the major archaeological techniques, excavation, has not succeeded in providing an understanding of rock art. Excavation cannot be successfully employed in the study of rock art.

Furthermore, dating has been a great topic in archaeological investigation and this concern with providing a chronological framework for the artefacts has been applied to rock art. When a new rock art is discovered the first question normally asked concerns the time period to which it relates. Accordingly a great deal of time and resources have been expended on attempts to answer the question of the age, as if this were of fundamental consequences for rock art studies (RAS). Excavations carried out in the floors of rockshelters and caves are expected to answer the time question of the art on the walls. It is not accidental that excavation results in Namibia by Wendt (1976), which revealed the date of portable art in Apollo

11 Cave, were received with great enthusiasm by archaeologists the world over. Yet RAS need not take dating as a major focus of their investigations.

Being a fundamentally cognitive field of enquiry, the thrust of RAS must be concerned principally with the understanding and interpretation of artistic phenomena as a basis for tracing the evolution of human cognitive processes, intellectual and emotional development rather than focusing on temporal parameters of execution. More specifically, rock art study needs to strengthen its position by borrowing from other fields, including archaeology, without necessarily being subordinated to them.

The historical association between RAS and archaeology has unfortunately not yielded a coherent methodological approach that would meaningfully integrate RAS to archaeology. Instead, archaeologists have been satisfied with superficial descriptions of rock art without providing it (and archaeology) with a stable theoretical status. Not surprisingly, Bednarik argues that archaeological methods to which rock art is being subjected are themselves questionable in reference to the degree to which they can be regarded as scientific (Bednarik 1990). The same is the case with theory. According to Bednarik,

> ecological prehistory [i.e. archaeology; O.O.] can afford to be subjective or parochial in its interpretations, because its ultimate purpose is to interpret subjectively. Cultural or cognitive archaeology [i.e. rock art research; O.O.], on the other hand, which operates primarily through the study of prehistoric arts, must strive for relevance by adopting a global, universal approach: its theoretical constructs must be falsifiable, and they must relate to, and be reconcilable with, any frame of reference (Bednarik 1990; see also for further arguments on the weaknesses of archaeology as a scientific sub-discipline).

In other words, in the field of theory archaeology itself has not developed well-grounded theoretical paradigms. This is, however, possible in RAS. If this weakness is detectable in the 'parent discipline', archaeology, then one cannot expect fields depending on it for guidance to go beyond the capability of the parent. Similarly, in reference to methodology, Bednarik (1990) correctly asserts that 'archaeology lacks adequate methods of testing or falsifying interpretive archaeological hypotheses'. Accordingly RAS, regarded by archaeologists as an appendage of archaeology, cannot make any headway in both theory and method unless it breaks away from archaeology.

Recent intensification of research in rock art by scholars coming from various disciplines has demonstrated the developmental potential of RAS as an independent and viable discipline outside archaeology. Recording techniques, such as tracings of pictographs; microscopic study of engravings on mobiliary artefacts; stylistic analysis of figures on rock art panels and similar strategies are some of the techniques RAS can usefully perfect for its further development. But further progress in this direction cannot be possible if it still has to rely on archaeology for inspiration in further research.

History of art

If rock art is an aspect of human culture, a study of its history must span the whole length and breadth of human cultural history. Nevertheless, evidence for the existence of rock art has been suggested to go back as far as 40 000 years ago in the African continent (Anati 1986). Thus an art historian utilises this long history of art production to elicit a history of art from it.

But history of art is part of the general history of humanity. Since rock art reflects the relationship between man and environment, it is an important source of historical information reflecting on the social, economic and cultural activities of humans in various periods of history. But its relationship with history stops at that. From there they part company, since the methods employed by history differ substantially from those employed in RAS. If rock art is part of history, insofar as it is also one of the issues with which history is concerned as a source of historical information, history has more or less the same claim to rock art as do archaeology and ethnology.

By virtue of the interdisciplinary methods applied in RAS, other disciplines (e.g. fine arts, musicology, geology) could legitimately lay claim to it. But since no scientific discipline is entirely autonomous nowadays, RAS, like any other discipline, can equally demand identity as an independent scientific field, despite interaction with other disciplines. It has its own subject matter (paintings or engravings executed on surfaces such as cave walls, rockshelters and house walls, on exposed rock outcrops or on mobiliary artefacts of wood, stone, antler, bone etc.), its emerging theory as evidenced by recent contributions (see e.g. Davis 1984; Groenfeldt 1985; Redfield 1959; Redman 1977; Ucko 1962; Wolfe 1969) and methods (e.g. Anati 1977). These major characteristics of any discipline can develop more freely under the conditions of independence rather than subordination.

Rock art researchers

At present rock art researchers come from many disciplines. There are historians, ethnologists, artists, archaeologists, linguists etc. Some of these researchers see rock art as a sub-topic in their mainstream disciplines. So they come to it to fill in the gap in their respective disciplines. Such researchers meet colleagues from other disciplines who are also interested in the subject. In this interaction, the unifying factor is interest in the study of rock art. When it comes to theory and methods, each scholar approaches it from the theoretical and methodological perspectives of the discipline from which they come. While the archaeologist's concern is to include excavation of the floors of rockshelters or caves for dating evidence, the ethnologist is more often interested in examining writings of early explorers, missionaries or colonial administrators reflecting upon the art and the rituals involved in its production. Because of the different methods employed, interpretation offered will inevitably differ and this situation is not limited to ethnology and archaeology.

The convergence of different interests, viewpoints etc. in RAS is perhaps one of the latter's strengths, since each specialist contributes to it in his or her own way, thereby enriching the emergent discipline. The new ideas from various sources brought into the discipline, under the guidance of its theories and methodologies, strengthen the discipline itself. But this situation is unlikely to emerge in a condition where all contributors come from outside and none from the discipline itself.

Hence, even though researchers from diverse sources have laid the foundation for RAS, it is now time for the latter to have its own researchers to guide those from outside. Otherwise researchers from outside would become a hindrance to the discipline's continued development, particularly if the status quo were to continue. In that case there would be theoretical and methodological eclecticism, a stage through which the currently well-established disciplines passed but which cannot remain forever if the new discipline is to progress.

The problem of a name

Every discipline has a name which describes its objectives, functions and subject matter. The name is also necessary to provide the discipline with an identity. The current name *RAS* is inadequate, for reasons already given. But the name of the scientific discipline which studies what has hitherto been called rock art needs to be distinguished. There is no controversy over what the subject matter constitutes. The rock paintings and petroglyphs need to be defined by a term setting them apart from other forms of art (e.g. music, dance, sculpture, modern paintings). What is likely to be controversial is the name for the science that studies these cultural phenomena.

Traditionally, new disciplines have been named to reflect the subject matter through the use of Classical European languages (i.e. Latin and Greek), as exemplified by 'anthropology', which combines the two Greek words *anthropos* and *logos*; or 'ethnology', which has *ethnos* and *logos* combined. If this practice were to be extended to RAS we would probably have a word that combines the already formed technical terms which refer to the general nature of the subject, namely *pictography* and *petroglyphics*. A combination of these would produce *pictopetroglyphics*, and the science obtaining could be *pictopetroglyphology*. But this name would attract several objections. Firstly, it is too complex and what we need is a simple word. Secondly, we do not necessarily have to introduce a Classical European language. For disciplines whose names derive from one it is easy to see why those languages became relevant. Thus early researchers were Europeans who obtained the terms from a past from which the rudiments of the disciplines originated. But rock art has been studied by people from different backgrounds and the subject itself is not rooted in European learning, and so not directly relevant to European history and heritage. Accordingly a new, fitting name for RAS will have to be neutral in its connotation, besides reflecting the subject matter and scope. The question is where to find a neutral and apt name for our discipline.

The new identity

For historical reasons and because some rock art specialists have an archaeological background, the phrase 'cognitive archaeology' has been suggested as a substitute for RAS. The fact that archaeologists have suggested 'cognitive' to describe it as a separate area of enquiry is an admission of RAS' theoretical and methodological exclusiveness within archaeology. The methods and theories relevant to rock art are difficult to integrate into mainstream archaeology. Whereas archaeology, treated as an auxiliary historical discipline, sees chronology as a major topic of enquiry, the emerging thrust of rock art studies is cognitive rather than chronological. This shift in emphasis easily explains the necessity of including the word cognitive to describe what is typically the domain of RAS as opposed to the major preoccupations of mainstream archaeology. The archaeologist's main field technique is excavation, which is essentially inapplicable in RAS. Whereas mainstream archaeology is concerned principally with establishing chronological sequences in human technological process, RAS is mainly interested in emotional and intellectual evolution, besides evolutionary changes in belief systems. Since archaeology has not been able to produce, on the basis of its fundamental methods of enquiry, reliable information on non-material culture, RAS has been relied upon to provide additional information unlikely to be received through the conventional archaeological techniques. RAS has thus been treated as an auxiliary topic in archaeology's quest for additional information.

This subordinate status attributed to RAS by archaeology has perpetuated the former's theoretical, methodological and technical backwardness. Hence, 'cognitive archaeology' must be rejected on the grounds that it subordinates RAS, thus preventing their free development.

What is said of archaeology relative to RAS also refers to 'cognitive anthropology', or any other name that could lead to a similar result.

In my search for a new name for the discipline (Odak, forthcoming) I had suggested the already mentioned *pictopetroglyphology* but now I reject it for the reasons advanced above. Giriraj Kumar (pers. comm. August 1990) has suggested the name *purakala*, a term originating from India which means early art or palaeoart of all forms, which has been given to the journal of the Rock Art Society of India. However, there are two major objections to this term being employed to designate RAS. Firstly, it is parochial in that it has been used only in India. Since our subject has been studied by scholars from all parts of the world we need a neutral name. Moreover, since the word *purakala* refers to ancient art of all forms it does not distinguish rock art (which can be ancient and recent) from all other early art forms. *Purakala* would probably be nearer to 'prehistoric art', which smacks of the influence from archaeology.

Recently, Bednarik (pers. comm.) suggested 'cognitive epistemology' as an alternative, which has the support of a number of scholars, including Tang Hui-sheng of China. This term seems appropriate. But the reasons I have advanced above against cognitive archaeology or cognitive anthropology also apply to cognitive epistemology. Epistemology, a branch of philosophy concerned with the theory of human knowledge is itself subordinate to philosophy. If RAS are to be called cognitive epistemology, it would subordinate the new discipline to epistemology. Rock art studies would become a branch of epistemology, itself an appendage of mainstream philosophy - something we are trying to avoid.

In another contribution, Bednarik (1988) suggested 'cognitology', which he considers to be synthetic and so appropriate. Compared to the previous suggestion, 'cognitology' is more acceptable, it does not subordinate RAS to an established discipline. However, its drawback lies in its heavy inclination toward interpretive strategies. The fact is that one of the major differences between RAS and archaeology is that the former have much to do with human perception, and with intellectual, symbolic and emotional domains. The term 'cognitology' thus seems to be more idealistic and also appears to be limited in its coverage of the range of RAS and is likely to sideline the objective sociocultural realities as reflected in rock art, and as well focuses on the psychological and philosophico-intellectual realms. Compared to cognitive archaeology, cognitive anthropology and cognitive epistemology, I see 'cognitology' as being appropriate, but only if no better term can be found. But I think there are better terms.

One of these comes from Giriraj Kumar (pers. comm.), India, who suggests taking the first letters of the names of the phenomena collectively studied by RAS. Thus,

according to Kumar, P would represent pictographs, E would stand for engravings on rocks, bones, ostrich egg shell and other surfaces, and F for figures on bone, stone and other materials. By assembling the first letters of these words we arrive at PEF. But since the first letter represents two words, pictographs and petroglyphs, I suggest that there be a double P. So the discipline that studies these phenomena would be 'ppefology', or 'pefology' (with one 'p') for ease and convenience.

Kumar's suggestion is important in that it shows the way to be followed in our efforts to establish a new and neutral name for the discipline. Employing analogous procedures one can arrive at several other synthetic terms. But there would be no reason for that if Kumar's suggestion can be acceptable. However, here is a challenge for rock art specialists to provide suggestions and new insights into a new possible name for our emerging independent discipline.

The advantage of the name 'ppefology' is that it embraces all areas which are the subject of RAS; it is independent of the existing disciplines; it does not involve the all-embracing term art, with which the emerging discipline has long been associated; and it imposes no temporal or spatial limit. So it is well suited to cover all objects of the discipline, and to replace such terms as rock art, prehistoric art and primitive art studies.

Conclusions

I have argued here that the word art, which is used to define rock art, is ambiguous and incapable of embracing its whole subject matter. I have also shown that we study in fact more than just rock art, so the name rock art studies is irrelevant for our discipline and should be rejected.

A cursory look at this discipline reveals that it has as long a history as its main claimants (e.g. archaeology and ethnology) but this has not prevented the latter from seeing it as part of their mainstream disciplines, although in reality it only receives peripheral treatment, being mentioned in passing as an auxiliary source of information for the mainstream disciplines.

Since each of the disciplines that has laid claim to what is known as rock art studies has dealt with only one segment of the whole subject of rock art (e.g. archaeology - 'prehistoric art'; ethnology - 'primitive art'), certain aspects of it are normally left out.

Although ethnographers have largely been concerned with the study of what they call primitive art, which includes rock art, they have only succeeded in providing a context for contemporary or recently executed rock art. This legitimises their claim to this segment of rock art studies. Archaeologists in turn have had much to do with what they call prehistoric art. Though the history of rock art studies goes back as far as the history of archaeology, archaeologists have all along regarded prehistoric art as part of their discipline, despite treating it as a source of information that cannot be obtained from the fundamental archaeological methods.

By espousing rock art as part of its sub-discipline, archaeology has, to a certain extent, been responsible for retarding the development of rock art studies as a potentially independent discipline with its own subject matter, methods and theories. This is more so since in integrating rock art, archaeology has at the same time not evolved a suitable method that can be used to further rock art studies. Instead it has persuaded, if not forced, rock art researchers to employ archaeological methods which have not yielded substantial results.

Moreover, archaeology is generally a subjective or a parochial discipline. Its hypotheses cannot be tested by scientific methods and so the outcome of archaeological investigations are largely unscientific, or nearly so. On the other hand rock art studies, a cognitive discipline, are promising to have hypotheses which can be tested by scientific methods, made accessible by recent work such as that of Marshack and d'Errico, among others.

Art history, which begins by examining prehistoric art, has not been open-minded in its interpretation of the subject as it has viewed rock art from the same perspective it uses for the study of visual arts, focusing on aesthetic interpretation.

All the students that have touched upon rock art have, nevertheless, contributed towards laying the foundation for the emerging discipline. But as the new discipline grew, these early positive contributions gradually became retrogressive as they tied the discipline to the theories and methods emanating from various sources. Innovation became difficult, and a need emerged for 'neutral' rock art specialists who would receive inspiration from the discipline itself, with the freedom to explore new ground favouring the theoretical and methodological development of rock art studies.

For independence as a discipline, rock art studies require a name which precisely describes its subject matter as well as the methods and other features that characterise it. Arguing against 'cognitive archaeology', 'cognitive epistemology' and 'cognitology', I consider Kumar's suggestion of 'pefology' appropriate. It is my hope that contributions from other colleagues will lead to a name that will be most appropriate, should this one not be so.

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COMMENTS

By EMMANUEL ANATI

Odak's paper is useful in that it awakens a debate. Odak expresses concern about the name to be given to rock art studies. It seems to be a matter of identity for the discipline. The main problem is that of contents, and their definition may help indicating its name.

For two generations rock art studies have focused on aspects of art history, archaeology, ethnology, palaeoclimate and environmental studies. Recently two new types of investigation practices have been growing, one being concerned with the mythology, beliefs and practices revealed by rock art, and is improperly considered as part of 'prehistoric religions'. The other is concerned with the reconstruction of the events, the economy, the social structure and the daily life reflected by rock art. It is considered part of 'history' - a history of pre-literate and non-literate societies. Actually, one of the main catalysing aspects of rock art is in what it reveals of the human viewpoint in tribal societies, indicating attitudes towards the individual, the group, nature and the 'supernatural'.

None of these various approaches appear, however, to include all aspects of rock art studies. We can therefore postulate the emerging tendency to a diversification of

approaches and of aims in rock art studies. The contents of each research depend to a large extent on its approaches and aims, and each research may well deserve a different name.

Beyond all possible sophisms, every term which is widely accepted is acceptable, provided it is well defined. New terms pose the difficulty that, in order to be acceptable, they must first be accepted. Terms are, after all, means of communication, and are often introduced and institutionalised in processes dependent upon their power of communication.

The ambiguity of terms is one of the refinements of our culture. Rarely one term has just one meaning. Even such simple terms as 'chair', 'bed', 'light' or 'art' have several meanings, according to the content and context of the phrase in which they are included. 'Rock art' appears to me as an appropriate and relevant term for rock art, provided that we know what we are talking about. And, why not, 'pefology' may once become appropriate as well, provided that the meaning of 'pef' is evident to us all.

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By JOHN CLEGG

One of the clearest and most important results of the Darwin AURA Congress in 1988 was the realisation that the activity of studying rock art has become a discipline. This is an appropriate time to reconsider its name. At present the name refers to 'rock art'. Professor Odak correctly states that the focus of our attention is not limited to rock, nor does it fit easily with any of the specialist meanings of 'art', some of which, especially 'primitive art', are offensive. The term 'rock art' is therefore inappropriate.

These points worried me for a long time, and I tried to do something about it by substituting the term 'prehistoric pictures'. I did not want to allow selection of the 'best' examples (as I feared the word 'art', which emphasises excellence, might). I wanted to ensure that there was no pre-judgment of questions of representation, iconicity or meaning. For me the word 'picture' includes abstract art. Once I had started using the term 'prehistoric pictures' I discovered that, in diametric contrast to what I meant, the word 'picture' implies representation to many people.

This experiment showed me that the word 'pictures' is no better than 'art' for my purposes. Both words can have misleading connotations.

I chose the word 'prehistoric' to focus on my particular field of study, prehistory. I was (and still am) most interested in what can be inferred about the prehistoric past from rock art. Before the Darwin congress my study was a branch of prehistory, itself related to archaeology and anthropology. After Darwin, my work is in the prehistory branch of our new discipline. I no longer wish to designate our studies with the limitation 'prehistoric'. I can continue to be a prehistorian who studies pictures, or rock art, but there are many other equally valuable orientations within the field.

Ours is a polythetic (Clarke 1968: 37-8; Davis 1990: 19-23) discipline, to be nurtured as a diverse entity, mutually supported by contributors from different fields with diverse interests and techniques. Polythetic entities are notoriously difficult to define, although they may sometimes be easily distinguished from other entities. Our name, being a name, should designate, rather than define or limit our study.

For all its faults, some of which Odak correctly identifies, the word 'art' has advantages. It connects our studies with other art studies, where many germane problems are under thorough, on-going discussion: are we concerned only with *deliberate* artefacts? Do non-human artists make art? What about by-products of other activities (axe-grinding grooves, chopping marks, footprints)? For most of our activities, such considerations are relevant, but not worth bothering with. When recording, we note natural as opposed to artificial marks, but try to distinguish between them and include them in the recording, for they might have had some influence on the picture.

The present usage designates and communicates well, if imperfectly; a very great number of people have a generally correct notion of what 'rock art' is. (Although an acquaintance of mine was referred for 'rock' to the record section of an English shop when he asked for Grahame Walsh's book *Australia's greatest rock art*.)

There is another consideration raised by Odak's paper: what is the nature of the entity to be named? At the moment there are two distinguishable entities: the artefacts, whatever they may be called, and the study or discipline 'rock art research', 'rock art studies' or whatever. Is the study yet a discipline? One view might be that it is not, consisting rather of the activities of a diverse set of people with nothing in common but their interest in 'rock art'.

By the other view we have a disciplined focus. We are concerned to distinguish observable fact from fancy; most of us are aware that we need to make assumptions in order to proceed, and we feel that those assumptions should be made as knowingly, and stated as clearly as possible. Investigation can start from the rock surface, and proceed through examination, recording, analysis, and hypothesis testing; alternatively it can start with a problem and work towards the objects studied. We are probably agreed that descriptions of marks and their location can be established reliably. There is less reliable information about how the marks were made, less still about when they were made, and their style and resemblance to other examples, much less information about their meaning, use or interpretation.

If there is general agreement about such things, we have a discipline, which we should continue to work at and enjoy. Perhaps there is no such agreement, and these ideas are relevant to only some of us. In that case, we may not have a single discipline, but we still have a subject, a diverse study, which we should continue to pursue and enjoy. At the moment this journal calls it 'rock art research'. I do not know what it should be called - but I do not think the name matters much. I think of the humble peanut, which, I understand, is botanically neither a pea nor a nut, but admirably and simply called 'peanut'. I think we may be stuck with 'rock art', although some instances of what we study may not be on rock, nor be art.

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By MARIO CONSENS

In his paper, Odak's final hope is that our contribution would lead to a new name for rock art studies. The following comments will disappoint him: a name will not necessarily answer all the interesting questions he raises.

I share several of his ideas and basic purposes, but cannot agree with some other assumptions and conclusions. I shall try to briefly illuminate my original position. As rock art researchers we are trying to secure information from faint material remains that are *part* of the evidence of symbolic activities. I cannot conceive that these remains are the sole result of these activities, or that they are the only or main depository of the symbolic activities of a culture.

Theory and methodology required to obtain information from these meagre remains must come primarily from the disciplines that have their main objectives in those ancient activities. I see anthropology and archaeology as major disciplines to recover and understand the human past. Basic techniques and methods must be taken from these experienced disciplines, but must also be specialised by rock art researchers. They cannot be devoted only to recording of design remains, even if this task must be their principal effort, in accordance with international resolutions proposed by themselves.

To be 'scientific' rock art researchers must go one step further: they must propose sound relations not only between rock art designs, but also between rock art and other remains of past human activities. Techniques to obtain these goals can differ, methodologies could be shared. Theories, inferences and hypotheses must be almost the same if the activities of humans (and not just partial and isolated behaviour of them) are the basic purpose of the research. From this perspective, research must be viewed as a complex and interrelated process within which it is quite difficult to separate and identify the different contributions of related disciplines.

Current research is based on team efforts. A rock art research team can be quite a strange assortment of photographers, semiologists, designers, psychologists, artists, data base engineers, biologists, chemists, physicists and some other people who share with them their love for rock art. And we can include archaeologists in this team, without disturbing its objectives.

For these reasons it is difficult to understand how rock art studies could try to be suddenly an independent scientific discipline, when research processes are so concatenated. Archaeology did not force us to employ its techniques and methods. We were unable, at least until the last two or three decades, to develop our own particular recording and registration techniques. And excavation, one (but not the only one) technique of recovering material remains and its relations, is a must for rock art researchers and cannot be denied because it is 'archaeological'.

Excavation also clarifies a second factor in rock art research: relations. If we cannot obtain relations, we can only describe forms. There is no special complexity to describe, and perhaps we are not the best-qualified for it; we could then be *artisans*, but not necessarily scientists.

When we address relations we think of other material remains, of faint marks of other kinds of cultural activities and of dating. It is difficult to think of rock art or any other form of human activity trace without attempting to know its archaeological matrix and chronological position.

Rock art researchers must try to use all the techniques in the archaeologist's arsenal to avoid (and to impede) the emergence of universals. These awful etic abstractions outrage scientific approaches.

We can use archaeological techniques and methodologies and we can still continue to be rock art researchers: specialists on particular archaeological remains that ought to be seen within a whole. If archaeology, with its large and strong body of experience and results, is considered by Odak to be a 'subjective and parochial discipline'. I strongly fear that rock art, isolated from its natural context and relationships, should receive more drastic adjectives. I will not take on in this Comment the defence of archaeology and its roles. Others are better qualified for this than I am. But I cannot accept that archaeological 'hypotheses cannot be tested by scientific methods'!

If we have arrived at the point that some of us claim to belong to an independent discipline it is just because archaeology has grown so far and so fast. It has achieved outstanding success in contrasting and testing hypotheses within epistemological issues, methodological advances and also quite incredible recovery techniques. This is a landmark that for us, rock art researchers, is today a remote goal.

Seen in this perspective, I cannot understand the importance of declaring rock art studies a separate discipline, independent from archaeology. Perhaps we work with all those specialists for ceramics, lithics, textiles, hide, feathers or metal, who do not claim to be independent from archaeology, even if they work with specific techniques and instruments. And this is so because artefacts made of these materials embrace symbolic activities, as does rock art. As do dance, music, abstinence, ceremonies or rites. And individual social relations or political and religious institutions. Or subsistence and settlement, one of the major areas to provide symbolic remains.

Thus we arrive at the question of a name. Does a name really describe the 'objectives, functions and subject matter' of a discipline? I think this is not so evident. Names for a discipline sprout from a precise juncture in the evolution of human knowledge and frequently reflect the political, social or religious circumstances that made it possible. Then a discipline's name can be only a reference to historical conditions without any precise information about its content.

But the necessity to *give* a name can reflect - in an anthropological approach - another necessity: the need to obtain some kind of knowledge about the unknown, or to bring order to chaos. This is a human requirement, but not necessarily a scientific rule.

I also accept Odak's assumption that rock art is unquestionably a term that does not explain the current state of the art of our research field. After his careful analysis I am even more sceptical that a new name can serve the purpose as he perceives it.

Turning to another of Odak's concerns, about the inadequate public awareness of rock art, I try to imagine myself announcing a conference on 'New trends in ppefology'. What kind of public should come, and what should they expect to hear?

I share Odak's criticisms of the partial and even discouraging proposals made on rock art by researchers from other disciplines. I believe that Odak's goal in this matter could be seen as how we must manage and

distribute accurate information about rock art research to the public. This is a very important problem that we commonly are not prepared to deal with.

Thanks to Odak's stimulating paper we have had the chance to refloat ideas, to shake off implicit accepted frameworks and to re-analyse some basic assumptions. Rock art research sometimes seems to be a very ample blanket under which we can find different concepts and sometimes quite divergent aims. Odak's paper contributes to a necessary self-criticism.

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REPLY

By OSAGA ODAK

I am thankful to the commentators for participating in this discussion which I consider to be a milestone in the development of our discipline.

I am particularly encouraged to note that the Comments are generally favourable to my central line of thought, namely that our discipline needs to be independent, or already is so. However, there appear to be some differences of opinion as to the need to provide a new name for the new discipline. I wish to address in my Reply the areas of possible disagreement.

Firstly, the issue of involving various disciplines in studying what has hitherto been known as rock art is the focus of this paper. In an independent discipline concerned solely with studying rock art, as is now emerging, those aspects which were the preoccupation of well-established disciplines such as history, archaeology, ecology, religion, palaeoclimatology etc., would be grouped together under the theoretical and methodological orientations of the newly independent discipline. This avoids the eclectic situation of the past, where there are no clear-cut methods and theories peculiar and specific to rock art studies (rock art research, ppefology and rock art studies or RAS are here used interchangeably; so is 'pef' which interchanges with 'art' or 'rock art'). The said grouping, if sufficiently comprehensive, could constitute a synthesis whose achievements could benefit other disciplines (e.g. archaeology) in borrowing any new findings from it. This has been the situation among the already well-established disciplines.

Professor Emmanuel Anati notes the possible tendency towards diversification of approaches and aims resulting from the diverse backgrounds of rock art researchers, considering that the contents of each research depend upon the approaches and aims. Accordingly, he cautions against the possibility of there being many different names on the basis of a variety in approaches and aims. Actually, what I am advocating is to bring together those diversities and aims under one roof so that they are governed by a common methodology and common theories developed by the new discipline. After all, established disciplines are not free from such diversities since each research project under the overall umbrella of each particular discipline has its individual aims and approaches, but operates under the central guidance of the parent discipline.

Noteworthy, however, is Anati's view that 'every term which is widely accepted is acceptable provided it is well defined'. This is precisely the reason for introducing 'ppefology' and 'pef' which I have given precise definition and description, covering the areas traditionally known as 'rock art studies' (cf. Pant 1991), and the detailed discussion of the meanings of 'pef' and 'pefology' is meant to avoid the ambiguity that has reigned under the concept of 'art'. Since we are now laying the foundation of the discipline there is no compelling reason why ppefology cannot become appropriate, considering the clarity of its meaning. The history of ppefology started at Darwin in 1988, when the scope of the new discipline was almost precisely defined.

I fully share John Clegg's view that 'the activity of studying rock art has become a discipline' traceable back to the First AURA Congress. Although this discipline is polythetic, that characteristic is not unique to it. Up till now this discipline has been nurtured from diverse sources with enrichment coming from various fields, most of which have laid claim to it. This enrichment, resulting from convergence of various disciplines, will continue into the future. All disciplines develop along similar lines. Archaeology, for example, has been enriched by such diverse fields as geology, physics, architecture etc., and what is now known as archaeology has less of its own identity than it has borrowed from diverse disciplines as a result of interaction and mutual feedback. Its stability as a discipline is, however, due to its independent status.

Clegg argues that one of the advantages of the word 'art' is that it connects our studies with other art studies and that many people have a correct idea about what rock art is. This may well be so. But I have argued that the word 'art' as currently used is synonymous with 'culture'. Culture is studied by nearly all disciplines of humanities and social sciences. So, all such disciplines are in one way or another dealing with culture, and hence related to other art (read: culture) studies. Yet, unlike RAS, these disciplines have their own respective identities, their names and their independence.

When I began writing this paper I thought about the possible resistance likely to come from those disciplines that have for long laid claim to rock art. Professor Bedekar (1991) compares such possible resistance to the situation that obtained when museology was striving to become a separate discipline, independent of anthropology. The resistance was great but in the end museology gained its status.

I expected such resistance, but I am happy to note that the initial reactions, as manifested by the Comments of Clegg, Anati, and in India Bedekar and Pant, among others, are not as negative as I expected. Some rock art researchers from those disciplines that are used to seeing RAS as their component sub-discipline are bound to be shocked to hear that there is talk about the breakaway of rock art studies. Consens, for instance, appears to have read my paper with some surprise. He seems to believe that rock art should be perceived in the same way as three-dimensional artefacts, such as ceramics, lithics etc., the traditional objects of archaeological preoccupations, the 'material remains' of past human activities. He even includes textiles, feathers and other objects, which are primarily the concern of ethnology, a discipline distinct from archaeology. He mentions dance, music etc., which,

like rock art, are cultural phenomena but which are studied by disciplines independent of archaeology and RAS. Further, he refers to 'material remains of the past' as if this were the only concern of RAS. Yet 'prehistoric' art, which is what he seems to be referring to, is only one aspect of RAS which falls within the archaeological preoccupation with a chronological framework. Clegg understands this well when he sees himself as working within the 'prehistory branch' of RAS.

I find it difficult to understand Consens' apparent notion that RAS are restricted to the recording of design remains. I am not at all surprised that he equates rock art with the so-called prehistoric art, when he states that we are trying to 'secure information from faint material remains that are part of the evidence of symbolic activities'. In like spirit he sees it as mandatory that the role of theory and methodology must be to obtain information from the meagre remains, by those disciplines that have their objectives in those activities. Here again rock art is conceived as synonymous with prehistoric art. Consens thus ignores those aspects of rock art which are concerned with the activities of modern or recent populations, and which may not necessarily relate to 'archaeological remains'.

Consens believes that, to qualify as science, RAS must consider the relations between designs and archaeology. If the concern is with the relations with other aspects of human culture, then RAS must necessarily be scientific even without reliance on the relations with archaeological remains. Moreover, although social sciences share certain methodologies and technical procedures, there are methods and procedures which are specific to particular di ciplines and depend upon the phenomena being investigated.

On the issue of team work it is true that much benefit is derived when a research project involving specialists from various disciplines is conducted as a multi-disciplinary effort. However, this team-work does not mar the disciplinary independence of participating disciplines. Just as rock art specialists can participate in any anthropological research project, along with biologists, geologists, photographers etc., so also these specialists can participate in any similar rock art research project.

Consens observes that detailed rock art recording and other techniques have been delayed until two or three decades ago. But he does not say why this has been so. The reason for the delay, I submit, is that for a long time RAS have been claimed by archaeology as a sub-discipline. Recent advances in rock art research appear to have resulted from the work of other disciplines in RAS, which has challenged the hitherto assumed monopoly by archaeology - something which led to efforts by RAS to break away. The loosening of archaeology's grip has provided RAS with relative freedom of methodological and theoretical creativity, as well as the opportunity of imagination and experimentation.

I do not understand how Consens could assert that excavation is a 'must forr ock art researchers', since we do not excavate the exposed rock outcrops on which we find petroglyphs, or the ceilings of rockshelters with paintings. Palaeontologists, geologists and other scientists also undertake excavation as one of their field research procedures, and such excavations are not necessarily archaeological. So excavation is not a monopoly of archaeology; but it is its major field research method which archaeology is arbitrarily trying to impose on RAS. These rock art specialists emphasising excavation as mandatory to ppefology must themselves be archaeologists. I, for one, do not undertake excavations since I do not see the need for them, and considering my disciplinary background which is not archaeological. It is regrettable that Consens did not state in what ways excavation clarifies relations in petroglyphs. How do we clarify relations between rock art and archaeological artefacts in a rockshelter whose floor is bare rock?

I personally see it easy to conceive of 'pef' executed in modern times but which does not have any archaeological association. To me chronology is fundamental to historians and archaeologists, but not necessarily to all rock art specialists. The reality is that Consens is trying to force ppefologists to indulge in archaeological research techniques - even if they happen not to be archaeologists.

RAS need not be part of archaeology, and this is the thrust of my paper. It is pointless to put a condition to rock art researchers that they either become archaeologists, or stop researching rock art!

The views expressed in Consens' Comment convey the impression that he exemplifies those archaeologists who claim rock art as an integral part of archaeology. His assertion that archaeology has a 'large and strong body of experience' is without qualification and must be denied. I could strongly argue against it, with a wealth of evidence, were it not for lack of space.

It is regrettable that, in defending archaeology's 'right' to keep rock art, Consens refuses to accept an obvious fact that archaeological hypotheses cannot be tested by epistemologically sound scientific methods. Perhaps his objection could have been more credible if he could have thrown in a sentence to refute that statement. Nor does he accept RAS as a possible independent discipline. Instead he would like to see it continuing to suffer under the archaeological tutelage, by incorporating as colonised the non-archaeologists into the archaeological empire. Yet what is clear is that I advocate the rebellion against archaeology not because I regard archaeology as having outgrown its boundaries, as Consens suggests it has, but rather because rock art researchers found themselves strange bedfellows with archaeology as a discipline, and became dissatisfied with it for not obtaining any hope for further progress.

Concerning the name of the new discipline, I do not agree with the view that a discipline's name can only be a reference to historical conditions without precise information about content, as is held by Consens. Examples in my favour include ethnology, geography, archaeology, anthropology etc. All these disciplines reflect content in their names. But I agree that names originate in the evaluation of the subject, and at particular stages under differing historical, political and cultural circumstances.

This explains the Latin and Greek origins of the names of many of the disciplines currently being studied. The names reflect the historical development of those disciplines which are generally rooted in European learning. The unique conditions under which RAS are evolving are that nearly all regions of the world are involved in studying rock art. This is different from the disciplines mentioned whose early periods were almost exclusively monopolised by Europe and, to some extent, North America. This argues for the uniqueness in the circumstances under which ppefology is emerging.

I assure Consens that it *is* possible to announce a conference on 'New trends in ppefology', but this

announcement must be made in the comparative historical perspective of origin and development of the discipline being announced.

A few decades ago it would have appeared strange to announce a conference of such a title, but it is not so now, in view of the current level of awareness and popularity of our discipline. We have to start from somewhere to reach a certain level, and that somewhere is the emergence of a new and independent discipline with a new name. The trend that started some decades ago should continue, involving research, documentation, dissemination and promotion of ppefology and related activities. Seminars, conferences, workshops, mass media and other channels should be brought into sway.

In this context it would be in order if the forthcoming Second AURA Congress in Cairns could include a session on the name and articulation of strategies for continued development of RAS as an independent discipline. I also propose, as a matter of urgency, that this be discussed at the forthcoming International Rock Art Conference in South Africa, as well as in the forum of IFRAO. During such discussions it could be considered to rename all such journals which currently have words such as 'primitive art', 'rock art', 'prehistoric art' etc. appear in their titles or subtitles. Through such efforts our new discipline will continue to develop from strength to strength.

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Résumé. *On soutient dans cet article que les disciplines établies qui déclarent que les études en art rupestre forment part de leur champ d'activité, comme l'ethnographie, l'archéologie et l'histoire de l'art, ont traité ce champ d'une manière périphérique. Cette négligence a contribué à leur arriération théorique, méthodologique et technique. L'auteur recommande la fondation d'une nouvelle discipline, et l'introduction d'un nouveau nom qui fournirai une identité distincte à cette discipline.*

Zusammenfassung. *Es wird in diesem Artikel erörtert, dass die eingesessenen Disziplinen, die Felskunststudien als Teil ihres geistigen Gesichtskreises sehen, wie Ethnographie, Archäologie und Kunstgeschichte, Felskunst als peripherisch behandelt haben. Durch ihre Vernachlässigung haben sie zur theoretischen, methodischen und technischen Rückständigkeit von Felskunstforschung beigetragen. Der Verfasser befürwortet die Begründung einer neuen Disziplin, und die Einführung eines neuen Namens der dieser Disziplin eine deutliche Identität verleiht.*

Resumen. *Se argumenta en este artículo que las disciplinas establecidas que alegan que los estudios de arte rupestre forman parte de su campo, tales como la etnografía, arqueología e historia del arte, han tratado esta materia como periférica. A través de su descuido han contribuido a su atraso teórico, metodológico y técnico. El autor aboga el establecimiento de una nueva disciplina, y la introducción de un nuevo nombre proporcionando a esta disciplina con una identidad distinta.*

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REFERENCES

ANATI, E. 1977. *Methods of recording and analysing rock engravings.* Studi Camuni No. 7. Edizioni del Centro, Capo di Ponte.

ANATI, E. 1986. *Les premiers peintures du monde.* L'Aventure Humaine. Publication Internationale, Paris.

BEDEKAR, V. H. 1991. Comment on 'Towards the teaching of rock art in academic institutions'. *Purakala* 2(1): 8-9. [OO]

BEDNARIK, R. G. 1988. Art origins. Paper presented in Symposium K, First AURA Congress, Darwin. In press in W. Davis (ed.), *The very possibility of meaning*, University of New Mexico Press, Albuquerque.

BEDNARIK, R. G. 1990. Global perspectives of rock art. *Purakala* 1(1): 2-6.

Chamber's Encyclopaedia 1955, Vol. 1, George Newnes Ltd, London.

CLARKE, D. 1968. *Analytical archaeology*. Methuen, London. [JC]

CORY, H. 1953. *Wall paintings by snake charmers in Tanganyika.* Faber and Faber, London.

CULWICK, A. T. 1931. Ritual use of rockpaintings at Bahi, Tanganyika Territory. *Man* 31: 41.

DAVIS, N. 1984. Representation and knowledge in the prehistoric art of Africa. *African Archaeological Review* 2.

DAVIS, W. 1990. Style and history in art history. In M. W. Conkey and C. A. Hastorf (eds), *The uses of style in archaeology*, pp. 18-31. Cambridge University Press, Cambridge. [JC]

D'ERRICO, F. 1987. Nouveaux indices et nouvelles techniques microscopiques pour la lecture de l'art grave mobilier. *Comptes Rendus de l'Academie des Sciences de Paris.*

D'ERRICO, F. 1988. A study of Upper Palaeolithic and Epipalaeolithic engraved pebbles. In S. L. Olsen (ed.), *Scanning electron microscopy in archaeology*, pp. 169-84. British Archaeological Report (BAR) International Series No. 452.

D'ERRICO, F. 1989. Memoires et rhythmes au paléolithique: le myth de calendriers lunaires. In G. Giacobini (ed.), *Hominidae. Proceedings of the 2nd International Congress of Human Palaeontology*, pp. 519-22. Jaca Books, Milan.

Encyclopaedia Americana 1977, Vol. 2, Americana Corporation, New York.

GROENFELDT, D. 1985. The interpretation of prehistoric art. *Rock Art Research* 2: 20-47.

MARSHACK, A. 1969. New techniques in the analysis and interpretation of Mesolithic notations and symbolic art. Valcamonica Symposium, Actes du Symposium International D'Art Prehistorique 1968, pp. 479-94. Capo di Ponte.

MARSHACK, A. 1972. Palaeolithic notation and symbol. *Science* 178: 817-32.

MARSHACK, A. 1979a. Upper Palaeolithic symbol systems of the Russian Plain: cognitive and comparative analysis of complex ritual marking. *Current Anthropology* 20: 271-311.

MARSHACK, A. 1979b. Upper Palaeolithic symbol systems. *Current Anthropology* 20: 605-8.

MARSHACK, A. 1983. European Upper Palaeolithic - Mesolithic symbolic continuity: a cognitive comparative study of ritual markings. *Valcamonica Symposium 3*, pp. 111-9. Capo di Ponte.

MUENSTERBERGER, W. 1971. Roots of primitive art. In C. M. Otten (ed.), *Anthropology and art: readings in cross-cultural aesthetics*, pp. 106-28. Natural History Press, New York.

ODAK, O. 1980. Archaeological methods and ethnographic analogy: evidence from a pastoral society in Kenya. In B. Ogot and R. Leakey (eds), *Proceedings of the 8th Pan-African Congress of Prehistory and Quaternary Studies*. Tillmiap, Nairobi.

ODAK, O. 1988. Current status of rock art research in Kenya. Paper presented in Symposium A, First AURA Congress. In press, M. Lorblanchet (ed.).

ODAK, O. forthcoming. Kenyan rock art studies and the need for a discipline.

PANT, S. 1991. Comment on 'Towards teaching of rock art in academic institutions'.*Purakala* (in press). [OO]

REDFIELD, R. 1959. *Aspects of primitive art*. Museum of Primitive Art, New York.

REDMAN, C. 1977. The 'analytical individual' and prehistoric style variability. In J. Hill and J. Gunn (eds), *The individual in prehistory*. Academic Press, New York.

ROE, D. 1972. *Prehistory*. University of California Press, Berkeley.

TOWNSHEND, P. 1979. Mankala in eastern and southern Africa: a distributional analysis. *Azania* 14.

UCKO, P. J. 1962. The interpretation of prehistoric figurines. *Journal of the Royal Anthropological Institute* 92.

WHITE, L. A. 1959. *The evolution of culture*. McGraw Hill, New York.

WOLFE, A. 1969. Social structural bases of art. *Current Anthropology* 10.

KEYWORDS: **Non-iconic petroglyphs - Spatial analysis - Statistics - British Midlands**

THE PETROGLYPHS OF WEST YORKSHIRE
Explorations in analysis and interpretation

Jack Steinbring and Maurice Lanteigne

Abstract. The phenomenology of rock art is made difficult by the diverse and often conflicting perspectives applied to its investigation. These range from intuitive, aesthetic, cultural-historical, to clinical empiricism. The community of rock art scholarship, not yet ruptured by the antagonisms of 'mature' disciplines, still constitutes a special blend of all. The maintenance of this synthetic centrality offers opportunities for the pursuit of knowledge not open to the isolates. This investigation of the petroglyphs in West Yorkshire attempts to avoid conceptual boundaries and steers directly to the core of the rock art, the straightforward business of 'making a mark'.

Introduction

Previous attempts in the analysis and interpretation of petroglyphs and petroforms in western Europe have relied almost exclusively upon regularities in orientation with reference to cosmic behavioural patterns (e.g. Baudouin n.d.; Brinckerhoff 1976; Hadingham 1975, 1981; Lockyer 1909; MacKie 1977; Moir et al. 1980; Thom 1971). As such, these studies have tended to circumvent the social and symbolic infrastructures inherent in human behaviour as they are expressed in the petroglyphs and petroforms themselves.

It has been demonstrated that the geographical positioning of rock art panels may have significant implications as to the content of the figures (Emory 1969: 17; Steinbring 1987: 16, 1988: 9) and, as such, the context of human behavioural processes associated with these panels (Lanteigne 1989a, b). The positioning of figures in relation to each other and to 'physical space' may have significance in the interpretation and understanding of general behavioural patterns associated with the development of the human mind in addressing 'symbolic space'. This aspect of the 'generalised human condition' (Steinbring, Granzberg and Lanteigne 1988) is just recently coming of age as an integral facet of rock art analysis known as archaeopsychology (Bednarik 1988; Chippindale 1988; Davidson and Noble 1988, 1989; Davis 1987; Deregowski 1988; Dibble 1989; Faulstich 1988; Halverson 1988; K. Hedges 1988; Lanteigne 1989b; Lewis-Williams and Dowson 1988, 1989; Marshack 1988a, b, 1989; Olsen 1988; Sehgal and Benson 1988; Steinbring, Granzberg and Lanteigne 1988; Steinbring and Lanteigne 1988, 1989; Striedter 1988).

The rocky moors of the British Midlands offer a vast and seemingly random dispersion of usable surfaces. The distinction between used and unused surfaces can be sought on the basis of spatial patterning, facial orientation, elevation etc., with morphological and metric dimensions added as a stream of analysis for all variables. The specific area chosen for this study is Rombalds Moor, near Ilkley (Figures 1, 2). A recent report on this area (J. D. Hedges 1986) provides a large amount of data, some of which allows for the testing of various hypotheses. These in turn, as they are tested, lead to refinements and new

Figure 1. Location of study area in Great Britain (after Hedges 1986).

Figure 2. Location of study area in West Yorkshire (after Hedges 1986).

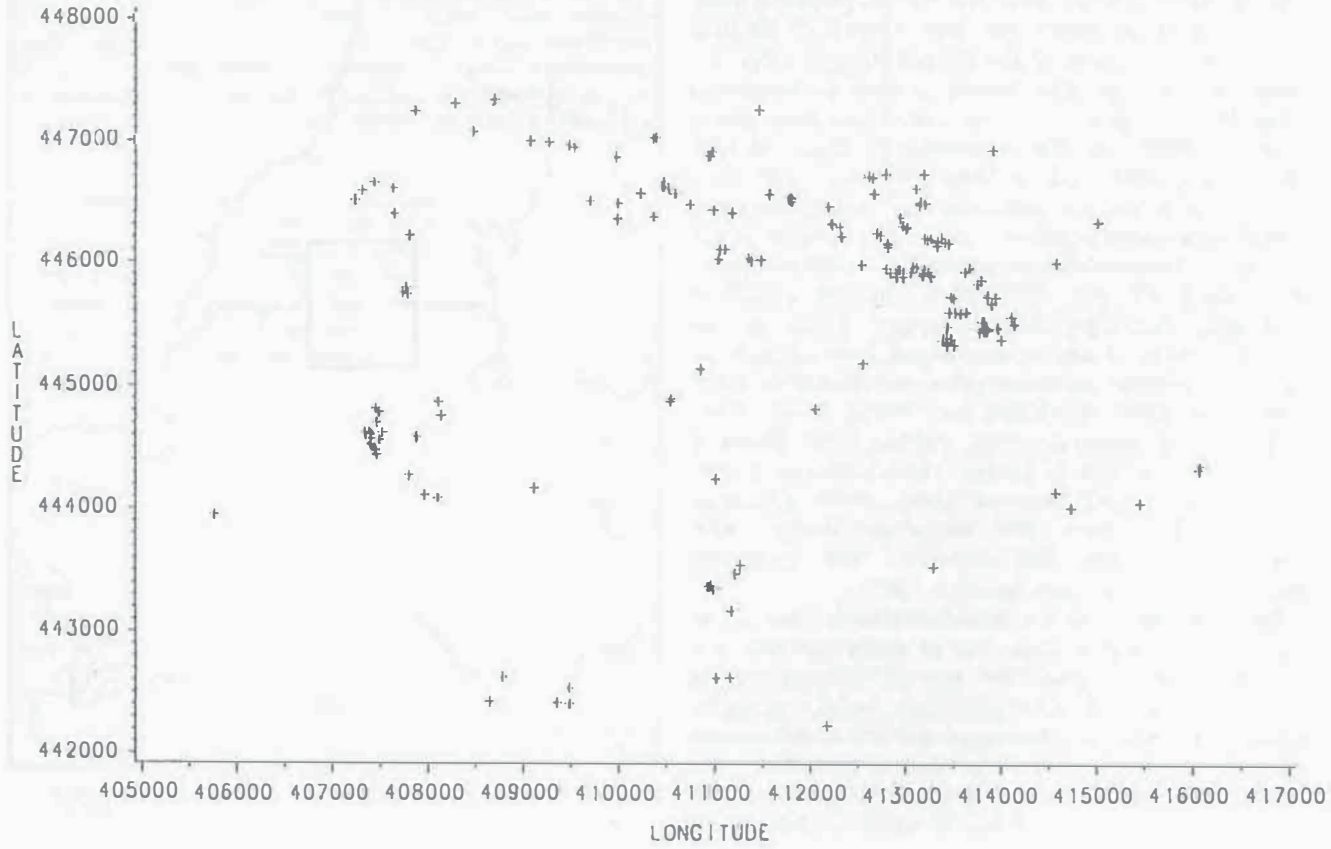
hypotheses. As the process becomes ever more demanding, technical properties of the rock art are often found to be inadequately reported - despite the fact that the Rombalds Moor report is one of the best efforts on record. The exploration of mid-range data levels becomes the only alternative to renewed, and exceedingly advanced, on-site data collection. Apart from that, the following study and its tentative results provide an example of a hopefully balanced 'rock art perspective'.

Problems in methodology

Of the 297 rock art panels catalogued in the regional gazetteer appended to the Rombalds Moor report, only the 230 panels from the main complex of the Rombalds Moor (Figure 3) were selected for analysis. Of these, 143 (62.2%) had been illustrated, and 115 (50.0%) were provided with magnetic orientation. Extremely complex panels, fragmented panels, panels illustrated out of context, portable panels and panels known to have been moved were deleted from this analysis. Conclusions for this study are based, therefore, upon only 78 (33.9%) of the panels recorded for Rombalds Moor. As such, it needs to be clearly stated that the sample used in this report does not represent a true statistical sample (see Lanteigne 1989c), as the criteria for selection were based not upon standard sampling procedures, but upon conditions conducive to the intended analysis. Notwithstanding, since the sample was taken from a known population size, and represents fully one-third of this population, the inferences generated are deemed highly probable for the main Rombalds Moor complex (circumspective if externally applied).

Several other biases are associated with this study. During the initial field recording, all markings had to be agreed upon as intentional markings. These confirmed markings were outlined in chalk (see Bednarik 1987; Hendy 1975). The panels were photographed at 1 m^2 intervals, reduced to $^1/_{10}$, or $^1/_{20}$, and then the markings were recorded. If the markings had been drawn in at the 1 m^2 size and then reduced, one could have greater confidence in the results. As it stands, one cannot differentiate between intentional or natural interruptions of linear arrangements. What appears to be one continuous figure may in fact be several unconnected but closely associated figures. What seems to be a circle with an eroded section may be a circle with an intentional gap, the directionality

Figure 3. Rombalds Moor. Petroglyph panel distribution in metres, as referenced to Ordinance Survey Grid.



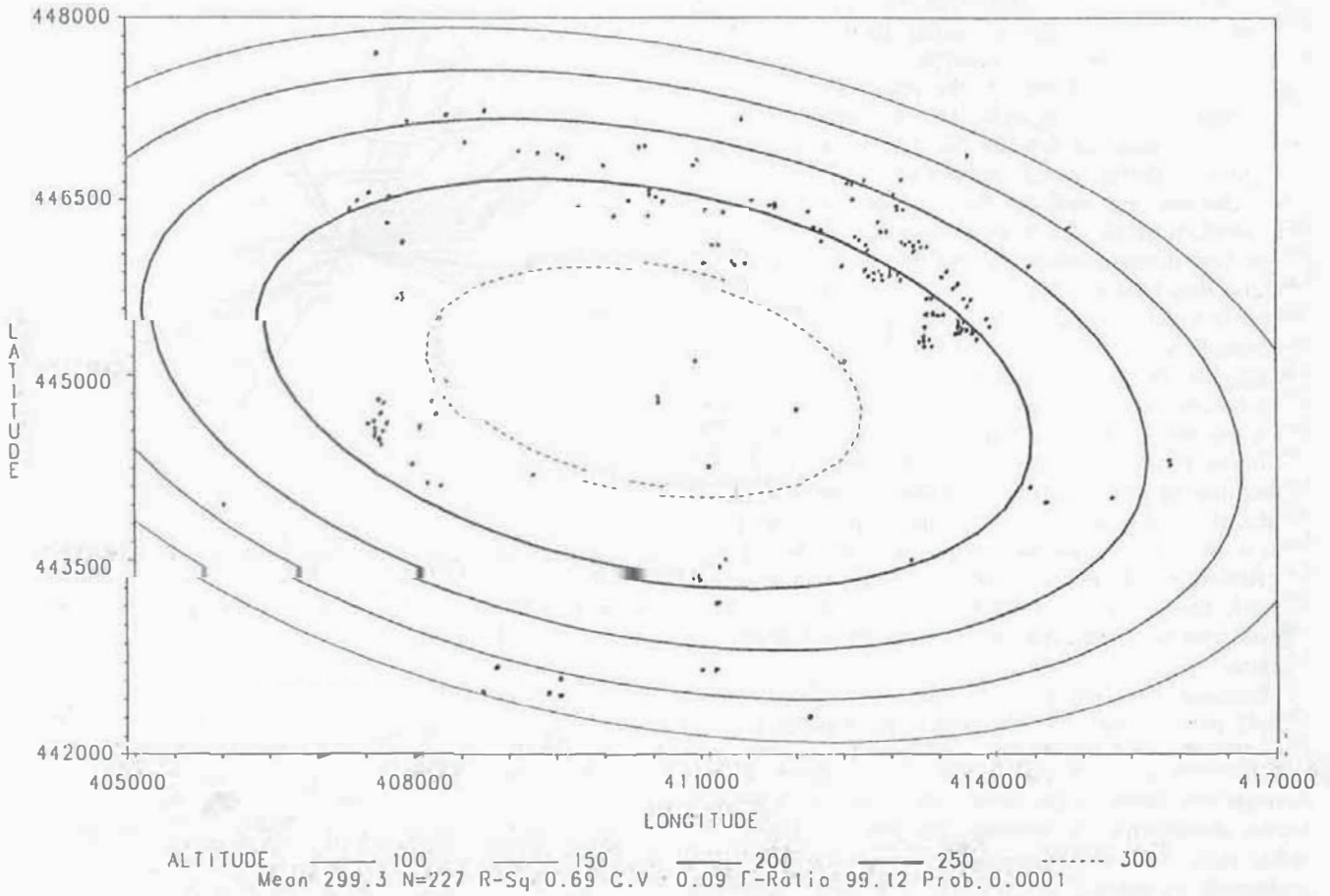


Figure 4. Rombalds Moor. Petroglyph panel distribution in metres, contoured for altitude in metres above sea level.

of which may be extremely significant. And since all markings had to be agreed upon prior to recording, not all markings were recorded.

The approximate centre of the panels selected for the present analysis was determined by means of triangulation. The centre of every figure was measured relative to the panel's centre and to the panel's edge, as well as to the polar co-ordinates. Every figure was also measured as to its maximum diameter or length. The observer's perspective on the sloping angle of the rock face is based upon the assumption that the vertical cross-sectional view of the rock panel, from which the sloping angle was obtained, is not illustrated out of context from the magnetic orientation given for the plan view.

Spatial analysis

A quadratic response surface regression procedure (SAS 1985: 725-34) was applied to the main Rombalds Moor petroglyph complex (Fig. 4). Contoured for altitude in metres ASL, we find that the majority of panels tend to congregate along the north-east to south-west flanks of the Moor. The relatively flat central plateau, at least in relation to its slopes, is generally avoided. On-site inspection indicates that a large portion of potentially usable panels are scattered throughout the Moor. Although these were not spatially quantified, it is posited that the central plateau of the Moor was deliberately avoided by the petroglyph manufacturers, not due to the lack of potentially usable panels, but because this central position did not fit into the culturally-defined function of the site.

This may be especially pertinent in the interpretation of general site use. From a distance at night, a procession of fire brands wending its way among the panels scattered across the Moor would be much more spectacular when viewed obliquely than at a perpendicular. If the cupules were indeed filled with oil and floating wicks (as some researchers have hypothesised; see Morris 1979), then each cupule 'candle' could be seen individually, arranged into large patterns to mirror the night sky or even to represent different ritual symbols. Whether any of these scenarios contributed anything to the main purpose of the site remains to be tested by other independent, corroborative procedures. (It should be noted that the prevailing winds are much stronger on the plateau than on the slopes. Optimising shelter from the elements also may have played a role in the panel selection process.) Regardless of the rationale, it is instructive to note that the selection of panels was not haphazard, but in relation to each other and their surrounding geophysical context. As shall be demonstrated, this general observation also holds true for the placement of individual petroglyphs on the panels.

Because of the large focus upon archaeoastronomical significance which has permeated petroform feature analyses in Great Britain during the past several decades, an attempt was made to ascertain whether or not orientational preferences were statistically demonstrable for the Rombalds Moor complex. Specific distribution patterns of the motifs on the panels may indicate positional preference for the initial manufacturers in their general foresight perspective relative to geophysical (or even cosmological) contexts. Recognising the great deal of controversy which has been generated by hidden assumptions in foresight/hindsight perspective in archaeoastronomical research (Burl 1980; Ruggles 1982), the present study is

concerned with general tendencies in culturally-defined human behavioural patterns rather than with specific alignments to cosmological patterns.

For example: in Figure 5, the placement of a major concentration of petroglyphs on the panel's southern sector is assumed to indicate that the manufacturer was positioned off the panel immediately adjacent to the concentration, and that the foresight perspective was to the northern sector. Such an assumption clearly ignores panel size dissimilarities, the possibility that the manufacturer may have reached across the panel to the opposite sector in order to produce the petroglyphs, or even sat in the middle of the panel facing the opposite direction. The validity of the primary assumption lies in its statistical corroboration. An understanding of generalised behavioural tendencies, whether active (individually understood) or latent (culturally defined), is the desired objective rather than precise occurrences. Although many variations in the physical positioning of the manufacturer are possible, if specific orientational preferences are indeed valid for British petroforms (as many researchers independently assert), then general orientational distribution patterns should also be statistically recognisable among petroglyph features.

Foresight measurements were taken of 197 petroglyphs (N=49 panels) relative to the centre and adjacent edge of each panel. Since the combined foresight perspective of any concentration of petroglyphs on a panel passes through the centre of the panel, this results in a conical scatter distribution of foresight perspective (Figure 6), rather than a parallel concentration characteristic of the traditional cylindrical perspective. The latter approach generates a 'best-fit' scenario, whereas the conical model generates a 'worst-case' scenario. For the purpose of statistical validation, the acquisition of a high significance value in a 'worst-case' scenario is always more desirable, as such a model would be statistically rejected more often than validated. Its validation, therefore, would be deemed more significant than if a 'best-fit' scenario acquired the same level of validation.

In testing foresight tendencies with multivariate statistical procedures, one is faced with a major problem in determining whether the dependent variable (foresight) is a linear or non-linear function of the independent variables (spatial co-ordinates). If linear, then the model being tested assumes that there is a direct (causal) relationship between changes in the independent and dependent variables, and that these changes are of a continuous (non-discrete) characteristic. For the Rombalds Moor complex, such a model (in layman terms) should demonstrate how the petroglyph manufacturers were positioning themselves in relation to their geophysical (or cosmological) contexts. For example, orientational patterns on the north-facing slope of the Moor should be the same as (or reversed to) the south-facing slope (if the same, or reversed, cosmological patterns were determining orientation), reversed (if major geophysical contours were determining orientation), or completely absent (if neither were directly responsible).

Figure 7 demonstrates that a good-fit linear equation cannot be formulated which could adequately account for changes in orientational preferences, relative to panel positioning on the site. Although a general orientational contour plot is generated, the low R-square and F-value vectors indicate that the equation is statistically non-significant.

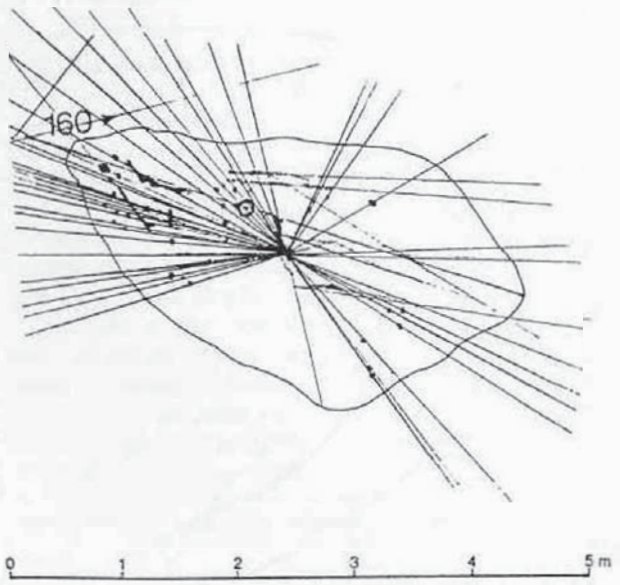


Figure 5. Major concentration of petroglyphs, foresight perspective analysis.

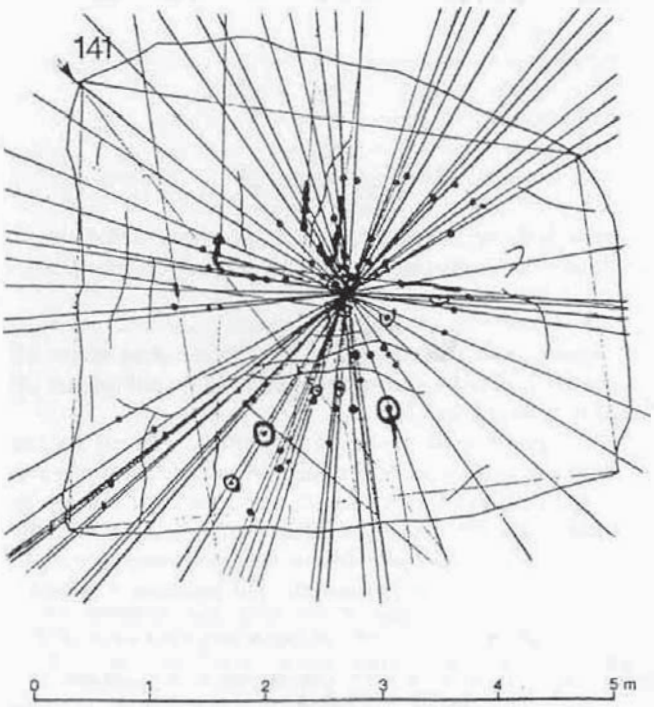


Figure 6. Conical scatter distribution of foresight perspective.

For the Rombalds Moor complex we are left with a non-linear approach. Such a model posits that the relationship between orientational preference and panel positioning on the site is discretely determined, indicating temporal discontinuity between general site-usage events, cultural discontinuity, or a combination of both. Within such a model, orientational preference would be culturally defined (not cosmologically, nor geophysically). Major spatial discontinuity in orientational preferences should indicate discontinuity in the oral transmission of culturally-defined values for these preferences. Such discontinuity could serve as temporal markers for site usage patterns.

Assuming a non-linear relationship exists between

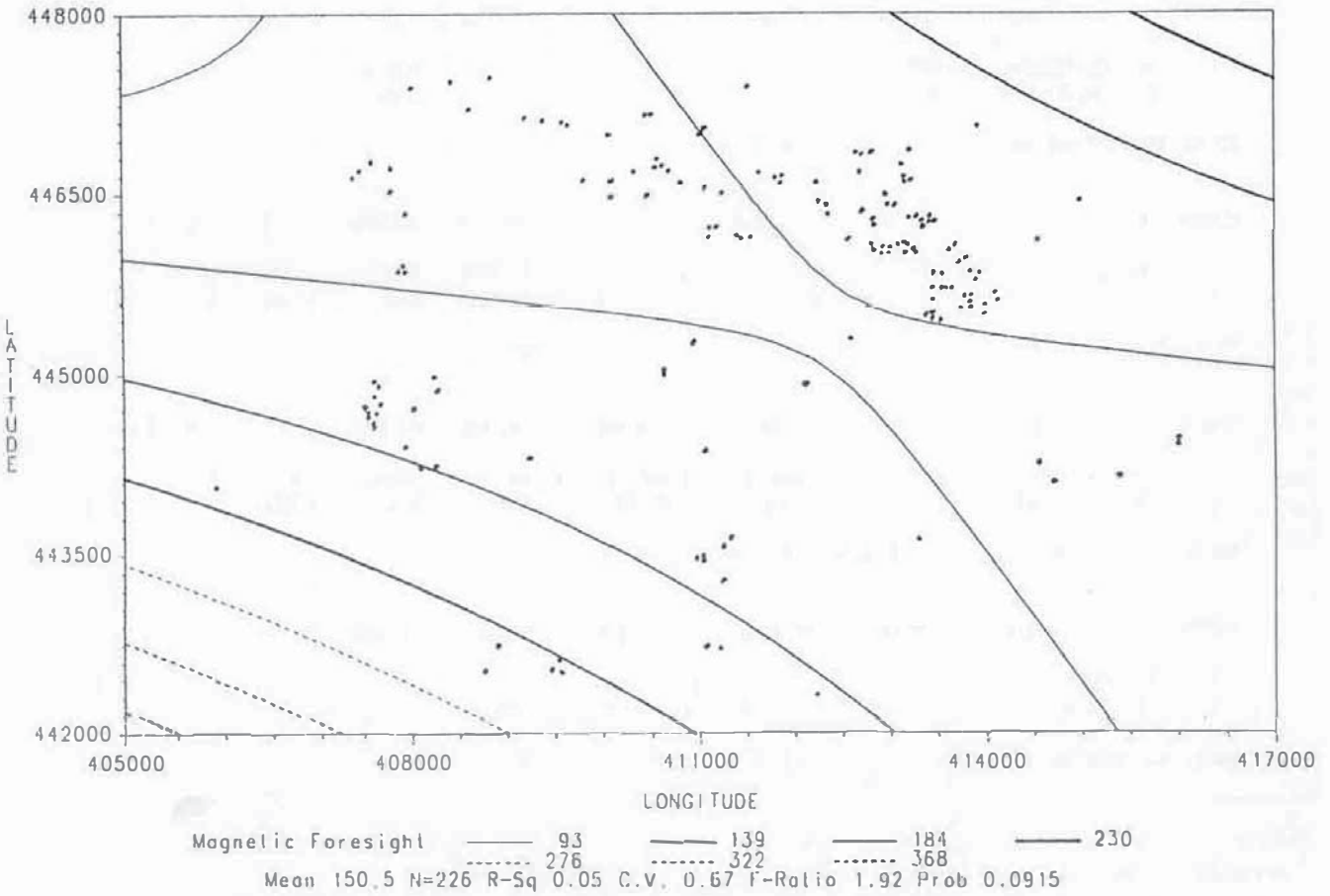


Figure 7. Rombalds Moor petroglyphs. Observer's foresight tendency perspective, assumptions of a linear model.

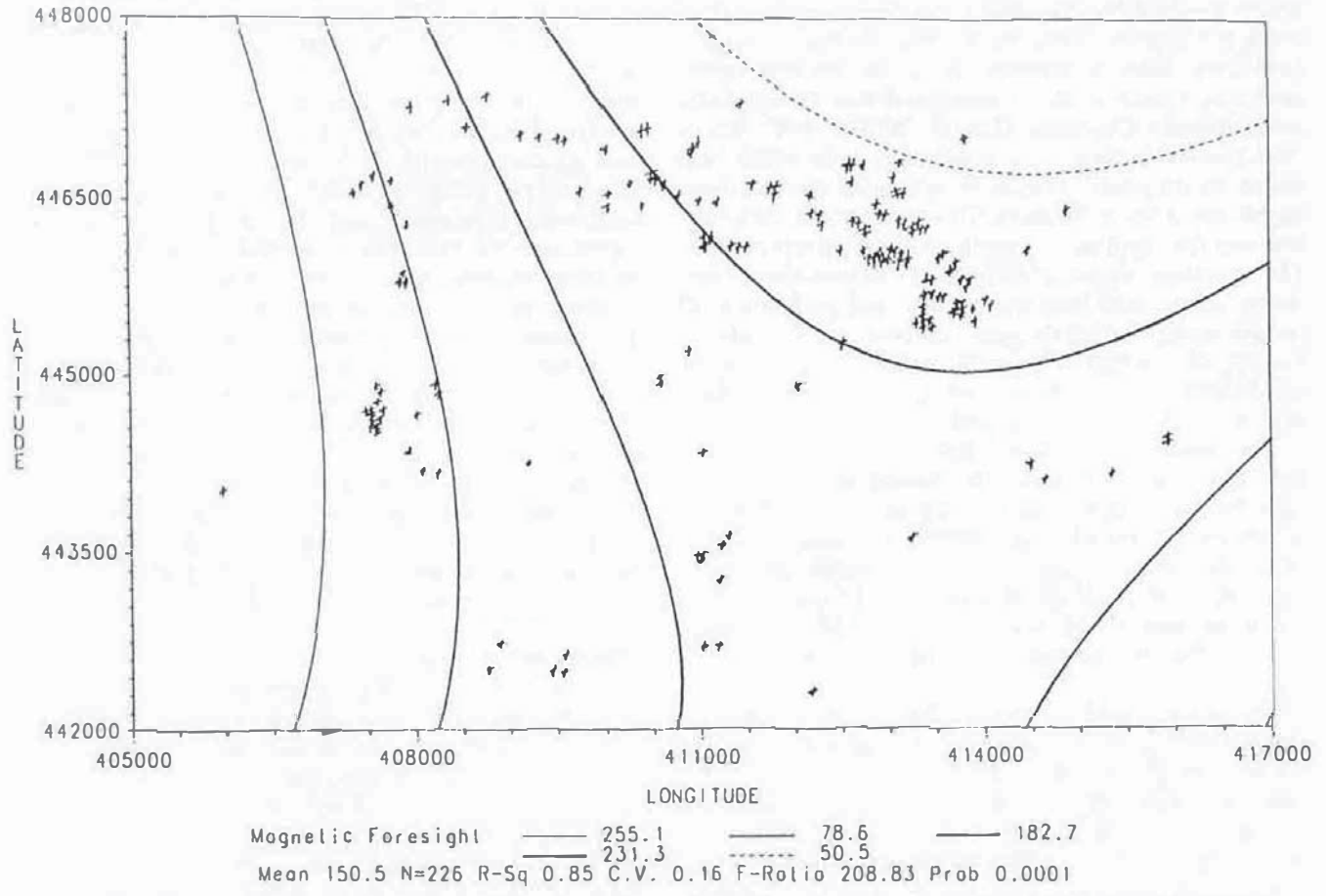


Figure 8. Rombalds Moor petroglyphs. Observer's foresight tendency perspective, using the Average Clustering Method.

CLUSTER	N	MEAN	STD DEV	STD ERROR	MINIMUM	MAXIMUM	VARIANCES	T	DF	PROB > |T|
4	64	231.28125000	67.05753719	8.38219215	134.00000000	359.00000000	UNEQUAL	19.6694	87.4	0.0001
5	61	50.47540984	29.46840486	3.77304262	3.00000000	107.00000000	EQUAL	19.3502	123.0	0.0001

FOR HO: VARIANCES ARE EQUAL, F'= 5.18 WITH 63 AND 60 DF PROB > F'= 0.0001

CLUSTER	N	MEAN	STD DEV	STD ERROR	MINIMUM	MAXIMUM	VARIANCES	T	DF	PROB > |T|
3	18	182.66666667	87.70874261	20.67314889	23.00000000	294.00000000	UNEQUAL	-2.1793	22.9	0.0399
4	64	231.28125000	67.05753719	8.38219215	134.00000000	359.00000000	EQUAL	-2.5328	80.0	0.0133

FOR HO: VARIANCES ARE EQUAL, F'= 1.71 WITH 17 AND 63 DF PROB > F'= 0.1280

CLUSTER	N	MEAN	STD DEV	STD ERROR	MINIMUM	MAXIMUM	VARIANCES	T	DF	PROB > |T|
2	30	78.56666667	44.99017773	8.21404507	8.00000000	185.00000000	UNEQUAL	-4.6797	22.5	0.0001
3	18	182.66666667	87.70874261	20.67314889	23.00000000	294.00000000	EQUAL	-5.4404	46.0	0.0001

FOR HO: VARIANCES ARE EQUAL, F'= 3.80 WITH 17 AND 29 DF PROB > F'= 0.0016

CLUSTER	N	MEAN	STD DEV	STD ERROR	MINIMUM	MAXIMUM	VARIANCES	T	DF	PROB > |T|
1	24	255.08333333	60.62996581	12.37603995	149.00000000	346.00000000	UNEQUAL	11.8836	41.4	0.0001
2	30	78.56666667	44.99017773	8.21404507	8.00000000	185.00000000	EQUAL	12.2804	52.0	0.0001

FOR HO: VARIANCES ARE EQUAL, F'= 1.82 WITH 23 AND 29 DF PROB > F'= 0.1290

Table 1. *T-test analysis of adjacent spatial groups, according to magnetic foresight.*

orientational preferences and relative panel positioning within the complex, the model was first applied to Hierarchical Clustering using the Average Method (a squared Euclidean distance criterion of a dissimilarity type), otherwise known as the 'Unweighted Pair Group Arithmetic Average Clustering Method' (Wishart 1987: 90). A classification criteria of a continuous relationship was output for all panels, thereby transforming the non-linear model into a linear function. This transformed classification was then applied to a quadratic regression procedure. The resulting equation (Figure 8) demonstrates very strong congruence between orientational preferences of specific panel loci and their internal positioning relative to the site as a whole. This relationship is of a dramatic discontinuous nature, demonstrating very clear modifications in general orientational patterns.

The question to be asked at this stage of the analysis is: how statistically discrete are the discontinuous groupings, such that they can be said to represent 'temporal phases' of site use? Acknowledging that relative sample size for each discrete classification may not be adequate (see Lanteigne 1989c), in the absence of additional sampling the Studentised T-test (SAS 1985: 795-800) was chosen as a corroborative procedure due to its greater effectiveness in such situations.

The results (Table 1) indicate that all paired adjacent groupings may be statistically recognised as being discrete from each other. As such, the hypothesis that these mathematically defined groupings represent major temporal phases of site use may be posited with some degree of confidence. The hypothesis is conditioned, however, to the statistical method and the variables (latitude, longitude, foresight) employed. Since it is based upon a mathematical equation, alterations in variables, cluster size, sample frequency or classification criteria will alter the results.

Although the model is not as extant as that which could be applied to a stratified sequence in an archaeological site (see Conkey 1980), in the absence of superpositioning it is (we believe) to be the 'next best thing'. Unfortunately, the model is not able, by itself, to distinguish in which order of the temporal sequence the various groupings occupy. Are there five major phases, each occupying the next adjacent position on the site, or two major phases of three and two groupings each? (The former representing a south-west preference, and the latter a north-easterly aspect, with the second phase regulated to positions external to the discrete clusters of the first phase?)

These questions are currently open, and will remain so until a concerted effort is made to spatially analyse other major regional rock art concentrations (representing various stages of site usage patterns). Once a clear understanding is achieved on how human behavioural processes influence general panel selection patterns, the ability to generate and test comprehensive evolutionary models on the human cognitive perception of 'symbolic space' will be realised for this region. The potential application, of the mathematical spatial model described above, to other major concentrations of rock art in the world is unlimited.

Morphological analysis

Whereas the foregoing section focused upon generalised human behavioural patterns as evidenced for the complex as a whole, vis-a-vis their interrelationship with the panel selection process, the following section will concentrate upon generalised behavioural patterns as expressed within the panels.

Part of the problem in the proper application of an empirical methodology in the morphological analysis of rock art is the degree of complexity in figure-element association (Lanteigne 1989b). The rock art of Rombalds

C	1064	cupule
CCCCCR	1	5 cupules enclosed by 1 ring
CCCCR	1	4 cupules enclosed by 1 ring
CCCR	1	3 cupules enclosed by 1 ring
CCR	1	2 cupules enclosed by 1 ring
CR	66	1 cupule enclosed by 1 ring (cup&ring)
CD	2	cupule at head of ladder
CK	3	cupule connected to crescent
CL	32	cupule connected to line
CLC	5	cupule-line-cupule
CLCLC	3	cupule-line-cupule-line-cupule
CLCR	1	cupule-line connected to cup&ring
CLCRT	1	cupule-line connect to cup&ring, w/tab
CLLC	1	cupule-line-line-cupule
CLR	9	cupule-line enclosed by ring
CLRT	4	cupule-line enclosed by ring w/tab
CLRTT	1	cupule-line enclosed by ring w/2 tabs
CLRR	3	cupule-line enclosed by 2 rings
CLRRRTTT	1	cupule-line enclosed by 3 rings w/3 tabs
CRC	1	cup&ring with 1 cupule on ring-groove
CRCCC	1	cup&ring with 3 cupules on ring-groove
CRCCCC	1	cup&ring with 4 cupules on ring-groove
CRCR	1	cup&ring ajoining cup&ring
CRCRT	1	cup&ring ajoining cup&ring w/tab
CRT	16	cup&ring with tab from ring
CRTT	7	cup&ring with 2 tabs from ring
CRTRRD	1	cup&ring with tab, 2 rings w/ladder
CRR	6	cupule enclosed by 2 rings
CRRD	1	cupule enclosed by 2 rings w/ladder
CRRT	1	cup&ring enclosed by ring w/tab
CRRR	1	cupule enclosed by 3 rings
CRRRD	1	cupule enclosed by 3 rings w/ladder
K	23	crescent
L	77	line
LCLCL	1	line-cupule-line-cupule-line
LL	1	line-line
RCC	1	ring w/2 cupules on ring-groove
RRRRT	2	4 rings w/tab from outer ring
A	1	triangle
U	1	u-shape

Table 2. Figure classification and frequency.

Moor is no exception (Table 2). 77.2% (N=1064) of the figures are isolated cupules, and 7.3% (N=100) are isolated lines and crescents. The remaining 15.5% (N=214) are figures composed of up to 36 different combinations of these primary elements. Only in three examples is there a ring, or a series of rings, without a central cup. The probability of this occurring as an integral aspect of the symbolic infrastructure is less than 0.0001 (Df=1 Chisq 120.273). As such, in a directional relationship of symbolic development, rings are said to be associated with cups, not cups with rings.

Univariate analyses of figure orientation (Table 3), relative to the sloping nature of the rock face and to the rock's edge, seem to suggest that specific sets of rules

	Ladder	Crescent	Line	Nat-Line	Ring-Tab	Cup-Line
Down Slope	0	8	-	-	21 0.008	32 0.001
Upslope	5	2	-	-	3	5
Cross Slope	0	5	17	3	4	9
With Slope	5	12	96 0.001	9	24 0.004	65 0.001
Away Edge	5 0.001	2	2	0	2	2
To Edge	0	13 0.001	79 0.001	2	24 0.001	34 0.001
Parallel Edge	0	0	11	0	3	7

Table 3. Slope frequency and probability significance levels of figure classes (based on goodness of fit in One nominal variable tests).

apply not only with the orientational placement of the manufacturer vis-a-vis specific clusters of panels, but also with specific figure types within the panels. For lines, ring-tabs and cup-lines, evidence indicates that down-slope orientation, with-slope orientation, and rock edge orientation are the norm in the execution of these figures within the total symbolic infrastructure. Tentatively, crescents face down-slope, with-slope, and to the edge. Ladders face up-slope, with-slope, and away from the edge. Rocks with natural lines tend to be selected with such lines running in accord with the slope.

Of all the lines emanating from the cups (N=37) only five point upward. Four of these five are found on Panel No. 41 (Figure 9). In all probability, this rock was moved onto its side during the construction of a boundary wall. Panel No. 99 is the only other rock which contains ladder figures. But since it is known to have been moved, no comparison is possible to determine whether the five upward-sloping ladders of Panel No. 92 are deliberate (part of the symbolic infrastructure) or accidental (the rock had been moved).

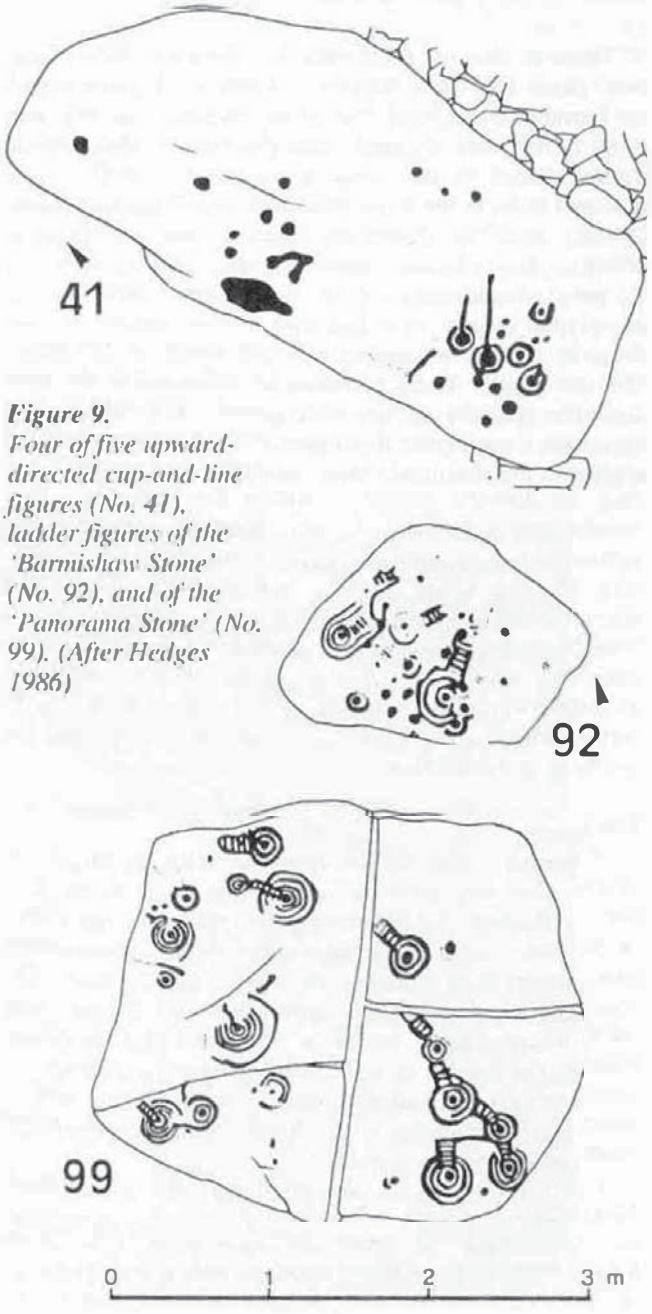


Figure 9. Four of five upward-directed cup-and-line figures (No. 41), ladder figures of the 'Barmishaw Stone' (No. 92), and of the 'Panorama Stone' (No. 99). (After Hedges 1986)

FREQUENCY ROW PCT					
Direction	Spatial Quadrants				
	25%	50%	75%	100%	TOTAL
North	48 13.87	86 24.86	132 38.15	80 23.12	346 21.34
East	27 7.71	91 26.00	154 44.00	78 22.29	350 21.59
South	61 12.84	99 20.84	182 38.32	133 28.00	475 29.30
West	60 13.33	141 31.33	154 34.22	95 21.11	450 27.76
Total	196 12.09	417 25.72	622 38.37	386 23.81	1621 100.0

Table 4. Spatial distribution of all elements. Magnetic direction by spatial quadrants.

There is obvious preference for the outer 50% of the rock panel (N=1008, 62.18%) (Table 4). Figures which are found in the central 25% of the rock face are assumed to be in the least desirable area (N=196, 12.09%), while figures found in the outer third-quarter (50-75%) are assumed to be in the most preferred area of the rock panel (N=622, 38.37%). Within the operating concept of spatial priority, glyphs located within the third-quarter sector of the panel should represent the first glyphs, with succeeding glyphs falling on either side of this region, as they compete for the remaining optimal space on the panel. The last glyphs would therefore be relegated to the least desirable position on the rock panel - the centre. It is important to recognise that this is only a general rule as it applies to the Rombalds Moor complex as a whole. There may be specific instances within the complex where another rule in the symbolic infrastructure takes over (e.g. optimal surface conditions, slope conditions etc.). Generally, this rule holds true for cups, cup-lines, lines and rings, but not ring-tabs. The latter element clearly departs from the normal curve to the centre of the panel (Table 5), indicating that the optimal space for its execution was already occupied. It is therefore suggested that ring-tabs were among the last elements to be incorporated into the symbolic infrastructure.

Discussion

A basic premise of the space-factoring approach, as employed in this study, is that the placement of the first figure influences the placement of every additional figure on the panel, with every subsequent figure incorporating this influence in relation to all earlier figures. The assumption underlying this approach is that figures were added incrementally, not all at once, and that the placement of the figures is influenced by specific patterns of human behaviour in approaching the rock panel as well as observing any configurations already present on the panel (both contrived and natural).

For the most part, the petroglyph patterns on Rombalds Moor can be viewed as non-compositional. An attempt has been made to avoid the imposition of a linear arrangement concept based upon specific cupule patterns, as this could initiate a significant theoretical error. A

FREQUENCY ROW PCT					
Element	Spatial Quadrants				
	25%	50%	75%	100%	TOTAL
Cup	138 10.95	347 27.54	486 38.57	289 22.94	1260 78.46
Crescent	1 3.57	8 28.57	8 28.57	11 39.29	28 1.74
Cup-Line	8 12.70	11 17.46	24 38.10	20 31.75	63 3.92
Line	15 20.83	8 11.11	24 33.33	25 34.72	72 4.48
Ring	21 14.48	34 23.45	56 38.62	34 23.45	145 9.03
Tab	15 39.47	8 21.05	9 23.68	6 15.79	38 2.37
TOTAL	198 12.33	416 25.90	607 37.80	385 23.97	1606 100.0

Table 5. Spatial distribution of elements by spatial quadrants.

'connect-a-dot' approach can develop any pattern the subjective mind chooses. The error is in presupposing that the initial glyph maker had in mind a specific linear arrangement when the particular cupules were placed. Only in specific cases is there any indication of cupules being perceptually associated with a particular linear arrangement. Although a groove encloses a linear arrangement in Panel No. 157 (Figure 10), there is nothing to suggest that linear arrangements were the initial intent. Cupules may have been on Panel 157 in random clusters and a linear arrangement was later generated by 'filling in' the intervals with additional cupules. Without on-site microscopic and physico-chemical tests of differential patination, order cannot in such cases presently be established.

It appears evident that general random clustering was the norm during the initial stages of glyph production. This random clustering was influenced by the 'physical' approach to the rock panel and the panel's relationship to the surrounding panels currently in use. Embellishment of existing cupules and lines with single and multiple circles, enclosing grooves, non-random cupules and ladder arrangements developed through time, naturally obfuscating the social meaning associated with the initial manufacture of the basic set of symbols (cupules and lines). The subsequent polymorphism of the panels, however, illuminates changing behavioural patterns toward pre-existing glyphs through the superpositioning of new conceptualisations upon earlier ones. From this perspective one can more meaningfully view the modern-day phenomena of graffiti, initials and dates (Figure 11), as impositions of individual will and proclamations of social intent.

With the acceptance of this view, an understanding of randomness in the placement of initial glyphs may lead to an elucidation of developmental sequences in the symbolic infrastructure of subsequent sets of glyphs.

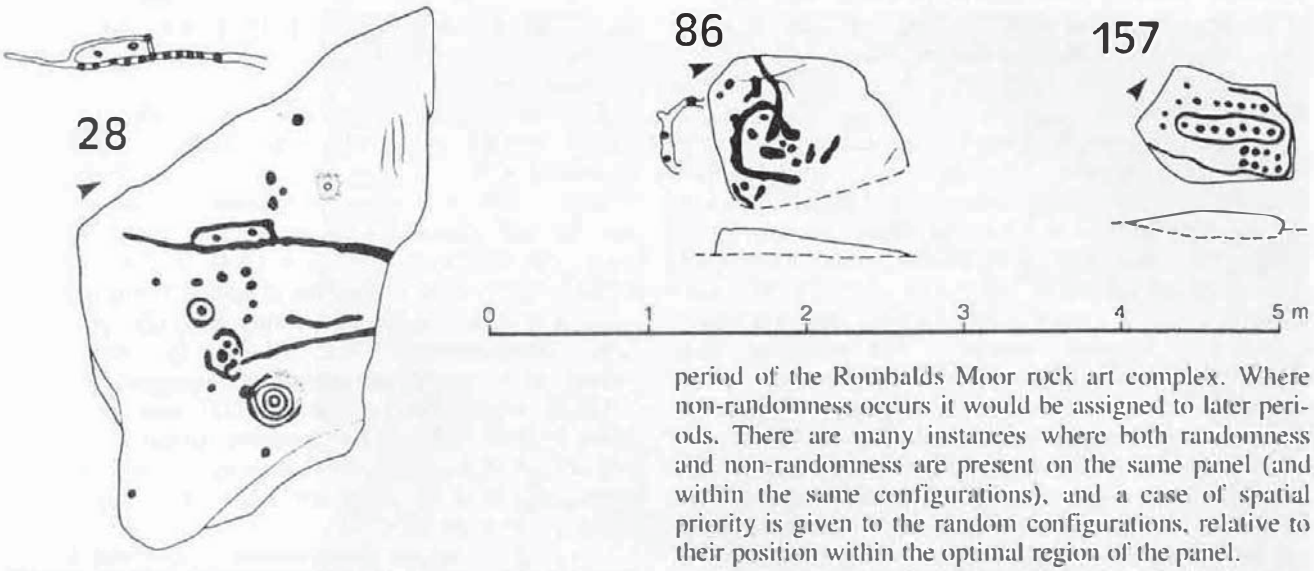


Figure 10. Cupules arranged within grooves (Nos 28 and 86), and enclosed by a groove (No. 157). (After Hedges 1986)

Randomness, and the superpositioning of complex elements upon existing cupules and lines, may be expanded into a socio-behavioural model as the rock art panels assume their present-day historic configurations.

It appears that specific panels may also generate their own particular sets of internal rules. Although complicating the interpretation of the symbolic infrastructure of the glyphs, it does allow an appreciation for the complex dynamics associated with human behavioural patterns in rock art execution. Where random spatial clustering is present among cupules, it would be assigned to an early period of the Rombalds Moor rock art complex. Where non-randomness occurs it would be assigned to later periods. There are many instances where both randomness and non-randomness are present on the same panel (and within the same configurations), and a case of spatial priority is given to the random configurations, relative to their position within the optimal region of the panel.

For example: on Panel No. 92 (Figure 9), the cupules are arranged in a tight cluster with multiple circles and ladders superposed upon the cupules. The circles and ladders take on a distorted appearance as they compete for the limited space available between the cupules. On Panel No. 99, the cupules, circles and ladders are in a more even spatial pattern, with adequate space for the full arrangement of all the figure elements. Interestingly, the ladders of Panel No. 92 point in (lead from) the general direction of Panel No. 99, but the latter had been moved from its original location during the Historic Period. Due to their relatively even distribution, the cupules, rings and ladders of Panel No. 99 appear to have been executed at about the same time. The more clustered cupules in No. 92, however, were executed before the rings and ladders were superposed upon them, and possibly before No. 99. The latter may have been selected for its general directional relationship to No. 92, a relationship which could indicate

Figure 11. Graffiti at Ilkley Moor, West Yorkshire. No vestiges of prehistoric art were seen at this locus.

a cyclical round of ritual activity in which entire figures, or elements thereof (a rung, or a ring) were added during each visit, finalising that stage of the ritual cycle.

Interpretation

An attempt has been made to consolidate most of the '104 theories' (Morris 1979: 15-29) put forth to explain the 'cup, line and ring' phenomena, within the constructs of symbolic anthropological theory (Granzberg and Steinbring 1988; Steinbring and Granzberg 1986; Steinbring, Granzberg and Lanteigne 1988). One of the most consistent cross-cultural parallels which arise is that of a binary conditioning between movement and rest, and their symbolic use in controlling the life forces of nature.

The complex morphological sequences (Figure 12) which follow from these primary elements (cups and lines) maintain a consistency within the two parameters of stasis and movement. The origination of the ring around the cup (a theme which has been overly emphasised by previous researchers) would appear as a natural progression from the two primary elements. Within the symbolic infrastructure being posited, the initial binding of the two primary elements into one would result in the motif of 'a line joined to a cup'. From this, the ensuing concept would be the invention of a third primary element which could embody the properties of the first two, yet be distinguishable from either. The most universal way to physically represent both movement and stasis within the same element is to draw a straight line as referenced to a stationary point. The result is a circle (or ring). Similar developments in stylistic variations on this theme (ellipses, spirals, meanders, boxes, mergers etc.) are characteristic not only of the rock art but also of the petroform features throughout Great Britain (Burl 1973). The relevance of such conceptual developments among petroforms is evidenced by the petroform 'ellipse' - a feature which is nothing more than a straight line as

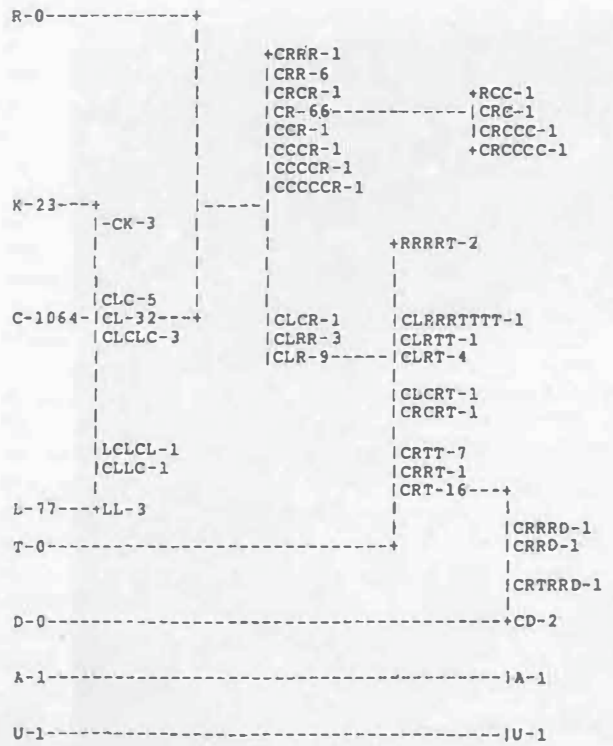


Figure 12. Hypothesised stages of morphological development (refer to Table 2 for element description).

referenced to 'two' stationary points; the petroform 'egg' - a straight line as referenced to 'three' stationary points; the petroform 'box' - a straight line as referenced to 'four' stationary points; etc.

The social and intellectual implications of such 'inventions' are interminable. One need only turn to the visualisation of the notation 'zero' in mathematics, a concept which was alien to Western culture until its presence was discovered among Islamic mathematicians during the Middle Ages (Struick 1969: 2). They, in turn, are believed to have discovered it among Hindu scholars in India sometime in the 9th century A.D. (Hooper 1948: 22-6; Youschkevitch 1976: 16-25). Its independent 'invention' by the Mayan culture of Mesoamerica (Long 1918) is evidenced at Tikal at least 600 years earlier still (Daniel 1978: 137). The apparent simplicity of the concept, at least in historical retrospect, belies the enormous infrastructural processes required to support its logical progression.

Does this mean that Great Britain's megalithic people had mathematicians? Or that they engineered megalithic computers for complex astronomical or calendrical calculations? These questions are certainly open to debate (see Atkinson 1975; Burl 1983; Ellegard 1981).

The model being posited by this study currently indicates that among relatively mobile hunting and foraging peoples, the basic symbolic infrastructure (cups and lines) could develop quite naturally and independently within a life cycle constantly punctuated by periods of movement and rest. Their world view may become predisposed to the metaphorical imitation of nature and life in abstract terms. The evolution of such primary elements eventually may take on the structural complexity of 'language', with the capacity to articulate specific events in the life cycle of individuals and/or groups: the transference of power or authority from progenitor to offspring; a union between two independent family groups through marriage, in which a transference of goods, services and obligations occurs; 'rites de passage' such as puberty rituals, death rituals; etc.

The general avoidance of the centre of the rock panel may acquire social significance if goods, ritual objects or even individuals are on (or around) the panel during glyph execution. If associated with any of the basic stages of the life cycle, the rock art itself comes to represent 'symbolic space' in which individuals or groups of individuals move to, and from, during ritual events. Rock art panels become part of the group history upon which ritual events of the individual member, family and group are validated and remembered.

It is not being suggested here that particular types of motifs of the Rombalds Moor complex consistently transmitted the same recognisable meaning throughout the entire cultural history of the complex. Periods of discontinuity in the oral transmission of certain culturally defined infrastructural values are certainly indicated. Discontinuity in the transmission of other cultural values are, therefore, also posited. It is instructive to note, however, that even with such basic primary elements (cups and lines), their metaphorical potential is such as could enable the articulation of complex symbolic meaning and intent for any circumstance within any iconographic system of any culture - including our own. (A modern day example is the binary bit-code in computer-related applications, a symbolism which has revolutionised our society and way of thought.)

Figure 13.
Exceptional density of cupules around a cup and ring motif at the High Banks Site near Kirkcudbright, Scotland.

Summary

In many ways the West Yorkshire rock art rather perfectly reflects the main rock art idiom of Great Britain. Until very recently all known rock art was abstract or geometric, never in the form of animals or humans, or other recognisable things. Cupules form the most numerous petroglyphic manifestations in Great Britain. There may be only a few small cups on an isolated rock, or there may be a great mass of them crowded onto a single rock, or many of them occurring on several rocks in a cluster (Figure 13). The 'cup and ring' form constitutes an elaboration of the cupule through the addition of a ring. These are far less numerous than simple cupules, but they are more conspicuous and thus more commonly cited. Cupules and cup and ring forms may occur together on the same panel, or they may be entirely separate. Where they are together, this study strongly suggests historical priority for the cupules (Figure 14).

It is difficult to speculate on which is more 'fundamental' in the procedure of 'marking', the simple cupule (a dot), or a groove (line). Both occur in nature, and both can attract the process of repetition. Data pertaining to either case is present on Rombalds Moor. While not too frequent, the occurrence of natural vesicles is noted for several formations, and the occurrence of lines produced by weathering along seams and sheer planes is well known. In a few instances, it appears that vesicles may have been enlarged or even 'ringed'. It is apparent, however, that natural lines may have formed the prototype for intentional ones. There are instances where natural lines may have been enlarged or enhanced.

The high percentage of isolated cupules reflects a commonality of human behaviour relative to sites containing cupules the world over: Scotland (Morris 1977), Denmark (Glob 1969), France (Germond 1980; Guiraud 1964, 1970), Near and Middle East (Ahlstroem

Figure 14.
Extremely eroded, random cupules on Ilkley Moor. Such manifestations may reflect the initial stages of petroglyph development by mobile hunters and foragers on the West Yorkshire Moors.

1978; Grebenart and Pierret 1966; Wreschner 1976), Rhodesia (Chaplin 1959, 1964; Clark 1958), South Africa (Evers 1981); Huffman 1979), California (Baumhoff 1980; Baumhoff, Heizer and Elsasser 1958; Baumhoff and Orlins 1979; Bertsch 1986; Castleton 1978; Ewing 1985, 1986; Fleshman 1975; Hedges 1980, 1983; Heizer 1953; Heizer and Baumhoff 1962; Heizer and Clewlow 1973; Hudson and Conti 1984; Minor 1975; Nissen 1975; Nissen and Ritter 1986; Parkman 1984, 1986, 1988; Payen 1968; Weinberger 1980), Mesoamerica (Gay 1973), Colombia (Hornell 1925), Peru (Thiermann 1977), Chile (Breton 1910), Bolivia (Querejazu Lewis 1990), British Columbia (Meade 1971), Alaska (Clark 1971), Polynesia (Cox and Stasack 1970; Steinbring and Steinbring 1983) and Australia (Flood 1987; Mountford 1960).

The universality of cupules relates firstly to their simplicity in the fundamental process of marking. The thesis entertained here is that basic cupules are the most likely form susceptible to re-invention or parallelism. The isolated forms need not be explained through diffusionistic formulae for another reason. All areas of the world provide natural prototypes. Vesicles, pits naturally formed by abrasion or impact, or both, abound throughout the world, and may be selected by humans as a basic sign, the symbolic meaning of which may vary. The phenomenal numbers of them, and their diffuse occurrence does, however, satisfy one basic tenet of diffusionism - that the most widespread phenomena are the oldest. In the instance of cupules, however, the view that basic marking, derivative from visual-to-symbolic process, represents a fundamental human process denies doctrinaire diffusion of a complex idea. This view favours the universal perceptive capacity to select nature's most fundamental forms at a very early point in human developmental history. It is well known that this history is highly variable, often interrelated and subject to reversals. As a result, the timing of cupule sites may vary dramatically, and the initiation of elaborative processes may vary accordingly where continuity can be safely inferred.

The Puuloa site on the Big Island of Hawaii contains over 30 000 cupules (Cox and Stasack 1970: 22), yet it is well established that Polynesian migrations to Hawaii probably did not occur earlier than A.D. 500 (Jennings 1979: 3). The 'Pit and Groove Style', several variations composed exclusively of cupules, is thought by some to be the earliest form of rock art in the Americas (Grant 1981: 152) and over 10 000 years old. The occurrence of cupules (or pigment dots) in Palaeolithic rock art is also well documented (Leroi-Gourhan 1967: 137; Marshack 1972: 193, 198).

The diffuse character of isolated cupule distribution in regions, and in site-regions, may reflect another attribute in the socio-cultural factoring of the phenomenon. If the hypothesis of early developmental selection is tested against distribution, high mobility, and essentially random, non-formalised transient settlement patterns (Kelly and Todd 1988) (excepting broad ecological parameters) emerge as a probable condition. This, in turn, suggests a non-agricultural dimension in the overall adaptation. While not universal, it seems more than just plausible that random, isolated cupule execution is aligned with mobile hunting and foraging societies - a conclusion which reinforces the developmental hypothesis. It is important to bear in mind, however, that many societies of the world, after becoming sedentary and perfecting technologies and economies allowing for intensive settlement, perpetuate symbolism of their less stable historical past.

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COMMENT

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I commence with a general point. The authors claim that most work on the petroglyphs of western Europe and of Great Britain has been concerned with astroarchaeological significance. This seems to be rather an exaggeration. The main problem with the more recent work on the petroglyphs of the British Isles is that, although there has been some extremely important thorough recording, such as Morris (1970, 1977, 1979, 1981) for Scotland, Hedges (1986) for West Yorkshire and Beckensall (1983) for Northumberland, there has been a great lack in interpretative studies in general. Some exceptions are Walker's (1970, 1974) computer study, and his use of cup and ring petroglyphs as 'archaeological finds', Beckensall referring back to the Mother Goddess theory, and a few ideas from Hadingham (1974). The authors do refer to Morris's (1977) semi-humerous idea of taking over a hundred possible theories and giving them each a score out of ten for strength of conviction.

Astroarchaeological work has been far more concerned with stone circles, only making reference to petroglyphs when they are associated with them. Thom's paper of 1968 looking at the geometry of the cup and ring petroglyphs is an exception.

It is against this background that one welcomes the paper of Steinbring and Lanteigne, and the new directions it points to.

The bias underlying their statistical approach is made explicit. A one-third sample is used, after excluding stones which were seen as problematic for one reason or another. Unfortunately in our field of study there are always unattractive or problematic examples - the broken fragments, the obviously disfigured rock, or the moved item. The question of how we deal with these in order to maintain maximum statistical rigour is often a problem. A stone from Galloway, Scotland (Figure 15), is not at all untypical of the sites in the area. It is broken and possibly included other symbols in its original state. Whether one included it or not might well depend on the nature of the study, but 'perfect' stones are very hard to come by.

The negative result that the authors achieve when they attempt to test any correlation between site choice and geophysical or cosmological orientation is a useful step

Figure 15. Petroglyphs at Townhead I, Galloway, Scotland.

forward. I am not so comfortable with the assumption that the resulting spatial discontinuity in orientational preferences, even when grouping is defined statistically, can serve as temporal markers; it would seem to be only one of a number of possibilities. As the authors themselves indicate, culturally defined criteria could be equally valid, with functional variation; this could well be combined with a temporal factor, possibly of very short duration, days, weeks, or months.

I find the approach to a morphological analysis more inspiring - surprisingly little has been done along these lines with the British Isles petroglyphs. I myself have tried analysing the more complete cup and ring marks and groupings using elements of the work of Munn (1966, 1973a, 1973b) and Faris (1972).

Since Steinbring and Lanteigne take the basic cup mark as the beginning of the system, I think it is important to draw attention to a particular characteristic of those in the British Isles. Although simple cup marks can be dated earlier than the cup and ring marks at Newgrange (Piggott 1973), many symbols now appearing as cup marks could have originally been cup and ring marks. The ring will often erode away long before the much deeper engraved cup mark in the centre. One can see this process by examining certain rocks where the ring is only just visible in certain lights, but the cup mark perfectly clear. Morris (1979) claims that on some sites the ring had disappeared completely when he revisited the site after a few years. Thus if one looks at the well-known site of Old Bewick in Northumberland, with its very worn symbols - in the course of time, and depending on the resistance of the rock, it could look more like the authors' Figure 14, or the Pancake Rock in the same area. This is a large, flattish but very eroded and indented rock which has many cup marks well enfolded in its textured surface. The process is also illustrated on the rock from Dobrudden, Rombalds Moor, where the rings are wearing away quicker than the cup marks. Therefore in some cases what now appears to be a grouping of random cup marks could have been a rock face with more complex symbols. Certainly at the moment cup marks do appear to predate the cup and ring symbols - see Piggott (1973) where the date of approximately 4000 B.C. corrected is given. Whereas the earliest date given by association for the cup and rings is from those found on the backs of the kerbstones and on a passage roofstone at Newgrange (O'Kelly 1982: 230), where an uncorrected date of 2475±45 b.c. is claimed for the charcoal infill for the roof of the passage. This has been the subject of some discussion but I think illustrates my point for this occasion. The dating is generally supported by Eogan's dating of charcoal at the base of Knowth to 2455∓35 b.c., where there are cup and rings on some of the graves. The authors' morphology is useful but needs to be viewed with caution if it is to be accepted as a temporal progression. Variation in the complexity and use of symbols or the spacing on the surface can be culturally or functionally defined, as well as being seen as a temporal progression.

Cup marks can be found in association with the more complex groupings of petroglyphs in the British Isles as well as isolated. Visually they often seem to play an important part in the total grouping with the more complex forms, but as the authors indicate, at times they may well have preceded the other forms; each example needs its own analysis and discussion. The site of High

Banks (Figure 13) speaks for itself, the cup marks are an essential part in the total rich assembly, and this is repeated in a similar way on the walls of the megalithic tomb of Dowth.

The points I have made help to show that the cup mark is the most enigmatic symbol to deal with. It seems quite logical to commence a morphological development with it and to progress to complexity. However, I suggest that we are on far from certain ground if we then assume a similar temporal progression on the rock surfaces. A morphological typology can be useful for our own classification, or for comparison, but more evidence is necessary to take us farther than this.

Referring to the rock in their F92, where a ladder and rings looks as though it may have been imposed upon a group of cup marks, the authors claim that this supports their idea of a sequential development of form, a temporal development. It of course may indicate this, but it could also be a change or correction at the moment or over a short period of time, using separate culturally defined vocabularies of form with differing functions.

When looking at the distribution of forms on the rock surfaces, Steinbring and Lanteigne claim that the outer edge of the rock is preferred, and the centre the least desirable. What their statistics may show is that most symbols are placed near the edge of the rock, the reason for this is another matter. It could well be that the centre has some sacred high level status and the edges are more residual. From this the reason for the ring and ladder form being in a solitary state in the centre of the surface may not be that it came late and found no room on the edge, but that it was a high-status symbol appropriate for the centre on this occasion.

The authors take the common path of assuming that the symbols 'can be viewed as non-compositional' on Rombalds Moor. One can certainly delve into semantics about 'composition', but I think there is often a confusion about groups of symbols non-symmetrically placed, with variations in spacing and scale. It is frequently assumed that this indicates a lack of firm intention in placing and scale, a casual, haphazard approach. From various small-scale tests with groups of students and children, I have found that the placing of forms on a flat surface tends to show far more regularity in spacing when no intention or thought is involved, than when the subjects are invited to think very carefully where they wish to place the forms. This leads me to suggest that in many cases of cup mark or cup and ring groupings we may be looking at a situation of intentional randomness, a specific intentional way of using the space.

This leads me to suggest another approach to a structural spatial analysis. Where cup and ring forms are found the space within the symbol is controlled, precise and finite; this can be placed against the 'intentional' use of random spacing on the rock surface, thus a binary structure is suggested, which is supported by alternative means to those suggested by Steinbring and Lanteigne. If one then compares the infinite space in which the stones or rocks are set, frequently with panoramic views, one can extend the structure to a triadic system - three variations on the use of space. As we see from Lévi-Strauss there can be an ambiguity between the binary and the triadic structure (1972: 134-5, 149-52). The lines which enter the cup and ring forms or cup marks form a linking system between the two spaces, and are capable of further analysis. I develop these ideas elsewhere, but offer them here as a support to the authors' approach.

Finally I must question the paper's assumption that cup marks were formed by 'mobile hunting and foraging societies'. One of the great problems of all rock art is dating or chronology, this is especially true in the British Isles. Steinbring and Lanteigne do not delve into this complex question, thus it would seem incorrect to make assumptions about the societies that gave rise to the symbols. Most evidence we have at the moment would not point to origins earlier than the Neolithic.

In conclusion, the paper introduces very interesting new directions for the study of British petroglyphs. It is to be hoped that others will follow and take farther these important pointers.

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[Jack Steinbring and Maurice Lanteigne will reply in the next issue.]

Résumé. *L'étude de le phénomène de l'art rupestre a fait difficile par les perspective divers et souvent discordantes. Ceci s'étendre de l'intuition, de l'esthétisme, de la culture-historique, jusqu'à l'empirisme objectif. Le monde savoir de l'art rupestre, pas jusqu'ici fragmenté par les antagonisme qui existe entre les autre discipline, constitue toujours un mélange spécial de tous. Le entretien de cette centralisation synthétique offre des occasion pour la poursuite de connaissance pas ouverte à les isolé. Cette investigation de les gravures rupestre de Yorkshire Occidentale essaie d'éviter les frontière concepnuel et gouverne tout droit à le trognon de l'art rupestre, les affaires franc de 'se faire une marque'.*

Zusammenfassung. *Die Phänomenologie von Felskunst wird oft erschwert durch die verschiedenen und vielfach widersprüchlichen Perspektiven die in ihrer Erforschung angewandt werden. Diese reichen vom rein Intuitiven, Ästhetischen, Kulturhistorischen, zu klinischem Empirismus. Die Gemeinschaft der Felskunst-Gelehrten, die derzeit vom Antagonismus der 'reifen' Disziplinen noch nicht zersplittert worden ist, vereinigt nach wie vor eine spezielle Mischung aller dieser Zweige. Die Erhaltung dieser Zentralität bietet Gelegenheiten für Wissensermittlung, die den Einzelformen nicht möglich sind. Die vorliegende Untersuchung von Petroglyphen in West Yorkshire versucht, konzeptuelle Grenzen zu vermeiden, und steuert direkt zum Kern der Felskunst, der direkten Frage der 'Zeichen-Herstellung'.*

Resumen. *La fenomenología del arte rupestre se hace difícil por las diversas y, a menudo, conflictivas perspectivas aplicadas a su investigación. Estas varían desde un empiricismo intuitivo, estético, histórico-cultural, hasta clínico. La comunidad estudiosa del arte rupestre, aún no fracturada por los antagonismos de disciplinas 'maduras', todavía constituye una especial combinación de todos. La conservación de este sintético centralismo ofrece oportunidades para la búsqueda de conocimientos no disponibles a las personas aisladas. Esta investigación de los petroglifos en West Yorkshire intenta evitar límites conceptuales y se dirige directamente a la esencia del arte rupestre, el hecho mismo de 'hacer una marca'.*

REFERENCES

AHLSTROEM, G. W. 1978. Wine presses and cup-marks of the Jenin-Megiddo survey. *Bulletin of the American School of Oriental*

Research 231: 19-49.

ATKINSON, R. J. C. 1975. Megalithic astronomy - a prehistorian's comment. *Journal for the History of Astronomy* 6: 42-52.

BAUDOUIN, M. n.d. L'orientation des mégalithes funeraires et le culte solaire a l'époque Neolithic: les rochers a Sabots d'Equides et la théorie de leurs légendes (sculptures vraies et depressions naturelles). Congrès International d'Anthropologie et d'Archéologie Préhistoriques.

BAUMHOFF, M. A. 1980. The evolution of Pomo society. *Journal of California and Great Basin Anthropology* 2: 175-87.

BAUMHOFF, M. A., R. F. HEIZER and A. B. ELSASSER 1958. *Lagomarsino petroglyph site, Storey County, Nevada.* University of California Archaeological Survey Reports 43(2). Berkeley.

BAUMHOFF, M. A. and R. I. ORLINS 1979. *An archaeological assay on Dry Creek, Sonoma County, California.* University of California Archaeological Research Facility Contribution No. 40. Berkeley.

BECKENSALL, S. 1983. *Northumberland prehistoric carvings.* Pendulum Publications. [PJ]

BEDNARIK, R. G. 1987. The chalking of petroglyphs: a response. *La Pintura* 14(2-3): 12-3.

BEDNARIK, R. G. 1988. Art origins. Paper presented in Symposium K of the First AURA Congress, on 2 September, Darwin (in press).

BERTSCH, H. 1986. Hollows and incised grooves in Chaco Canyon rock art. In K. Hedges (ed.), *Rock Art Papers* 3(20): 201-6. San Diego Museum Papers, San Diego.

BRETON, A. C. 1910. Seventeenth International Congress of Americanists, Buenos Aires, May 16th to 24th, 1910. *Man* 10: 141-4.

BRINCKERHOFF, R. F. 1976. Astronomically-oriented markings on Stonehenge. *Nature* 263: 465-9.

BURL, A. 1973. Dating the British stone circles. *American Scientist* 61(March-April): 167-74.

BURL, A. 1980. Science or symbolism: problems of archaeoastronomy. *Antiquity* 54(212): 191-200.

BURL, A. 1983. *Prehistoric astronomy and ritual.* Shire Publication, Princes Risborough.

CASTLETON, K. B. 1978. *Petroglyphs and pictographs of Utah.* Volume One: The east and northeast. Utah Museum of Natural History, Salt Lake City.

CHAPLIN, J. H. 1959. The Munwa Stream rock-engravings. *The South African Archaeological Bulletin* 14(53): 28-34.

CHAPLIN, J. H. 1964. Rock-engravings at Nyambwezu, northern Rhodesia. *The South African Archaeological Bulletin* 19(73): 13-4.

CHIPPINDALE, C. 1988. The great transformation in prehistoric art: from A under B to C. Paper presented in Symposium I, of the First AURA Congress, on 31 August, Darwin.

CLARK, D. W. 1971. Petroglyphs on Afognak Island, Kodiak Group, Alaska. *Anthropological Papers of the University of Alaska* 15(1): 13-7.

CLARK, J. D. 1958. The Chifubwa Stream rock shelter, Solwezi, northern Rhodesia. *The South African Archaeological Bulletin* 13(49): 21-4.

CONKEY, M. W. 1980. The identification of prehistoric hunter-gatherer aggregation sites: the case of Altamira. *Current Anthropology* 21: 609-30.

COX, J. H. and E. STASACK 1970. *Hawaiian petroglyphs.* Bishop Museum Press, Honolulu.

DANIEL, G. (ed.) 1978. *The illustrated encyclopaedia of archaeology.* Macmillan London Limited, London.

DAVIDSON, I. and W. NOBLE 1988. The archaeology of perception: traces of depiction and language. Paper presented in Symposium K, First AURA Congress, on 2 September, Darwin.

DAVIDSON, I. and W. NOBLE 1989. The archaeology of perception: traces of depiction and language. *Current Anthropology* 30: 125-55.

DAVIS, W. 1987. Rock art and 'archaeopsychology' - rationale for a symposium. *Rock Art Research* 4: 162-3.

DEREGOWSKI, J. B. 1988. The immutable eye and the changing styles. Paper presented in Symposium I, of the First AURA Congress, on 31 August, Darwin.

DIBBLE, H. L. 1989. On depiction and language. *Current Anthropology* 30: 330-1.

ELLEGARD, A. 1981. Stone Age science in Britain? *Current Anthropology* 22: 99-125.

EMORY, K. P. 1969. *The island of Lanai: a survey of native culture.* Bernice P. Bishop Museum Bulletin No. 12, Honolulu (reprint).

EOGAN, J. 1986. *Knowth.* Thames and Hudson. [PJ]

EVERS, T. M. 1981. 'Maps on rocks': some Iron Age engravings. *South African Archaeological Society Newsletter* 4(2): 1-2.

EWING, E. 1985. Tinjay Yubay: preliminary report on an unrecorded rock art site in central Baja California. In K. Hedges (ed.), *Rock Art Papers* 2(18): 1-18. San Diego Museum Papers, San Diego.

EWING, E. 1986. Pintadita: a newly discovered painting complex in the Sierra de San Luis, Baja California. In K. Hedges (ed.), *Rock Art Papers* 3(20): 51-74. San Diego Museum Papers, San Diego.

FARIS, J. C. 1972. *Nuba personal art.* Duckworth. [PJ]

FAULSTICH, P. 1988. Massaging the Earth: a psychoarchaeological interpretation of Pleistocene parietal art. Paper presented in Symposium K of the First AURA Congress, on 2 September, Darwin (in press).

FLESHMAN, G. 1975. *Pit and groove rocks and cupules in San Luis Obispo County.* San Luis Obispo County Archaeological Society, Occasional Paper 9: 89-115. San Luis Obispo, California.

FLOOD, J. M. 1983. *Archaeology of the Dreamtime.* University of Hawaii Press, Honolulu.

FLOOD, J. M. 1987. Rock art of the Koolburra Plateau, north Queensland. *Rock Art Research* 4: 91-126.

GAY, C. T. E. 1973. Olmec hieroglyphic writing. *Archaeology* 26(4): 278-88.

GERMOND, G. 1980. *Inventaire des mégalithes de la France.* Gallia Préhistoire 6, Éditions du CNRS, Paris.

GLOB, P. V. 1969. *Helleristninger i Danmark.* Jutland Archaeological Review Publications, Vol. 12, Gyldendalske Boghandel, Copenhagen.

GRANT, C. 1981. *Rock art of the American Indian.* Outbooks, Galden Co.

GRANZBERG, G. and J. STEINBRING 1988. The line, tree and circle in rock art and pictography: some selected examples of transcultural usage among Southwest and Northern Indians. Paper presented in Symposium B of the First AURA Congress, on 29 August, Darwin.

GRIEBENART, D. and B. PIERRET 1966. Traits polis et cupules de l'Oued Zireg (province de Taza-Maroc). *Libyca* n.s., *Série Anthropologie, Préhistoire, Ethnographie* 14: 329-35.

GUIRAUD, R. 1964. Cupules et gravures dans la commune de Combes (Herault). *Cahiers Ligures de Préhistoire et d'Archéologie* 13: 125-37.

GUIRAUD, R. 1970. Nouvelles gravures et cupules dans le Massif Caroux-Espinouse-Saumail. *Cahiers Ligures de Préhistoire et d'Archéologie* 19: 39-58.

HADINGHAM, E. 1974. *Ancient carvings in Britain, a mystery.* Grimstone Press.

HADINGHAM, E. 1975. *Stone circles and standing stones.* Heinemann, London.

HADINGHAM, E. 1981. The lunar observatory hypothesis at Carnac: a reconsideration. *Antiquity* 55(213): 35-42.

HALVERSON, J. 1988. Palaeolithic art and cognition. Paper presented in Symposium K of the First AURA Congress, on 1 September, Darwin (in press).

HEDGES, J. D. (ed.) 1986. *The carved rocks of Rombalds Moor. A gazetteer of the prehistoric rock carvings on Rombalds Moor, West Yorkshire.* Ilkley Archaeology Group, West Yorkshire Archaeology Service, West Yorkshire Metropolitan County Council, Wakefield.

HEDGES, K. 1980. Cupule sites in San Diego County, California. Report prepared for Westec Services, Inc., San Diego, California. Unpubl. MS in Scientific Library, San Diego Museum of Man.

HEDGES, K. 1983. A re-examination of Pomo baby rocks. In F. G. Bock (ed.), *American Indian Rock Art* 9: 10-22. American Rock Art Research Association, El Toro, California.

HEDGES, K. 1988. Images of the spirit world. Paper presented in Symposium K of the First AURA Congress, on 1 September, Darwin.

HEIZER, R. F. 1953. Sacred rain-rocks of northern California. *University of California Archaeological Survey Reports* 20: 33-8.

HEIZER, R. F. and M. A. BAUMHOFF 1962. *Prehistoric rock art of Nevada and eastern California.* University of California Press, Berkeley.

HEIZER, R. F. and C. W. CLEWLOW 1973. *Prehistoric rock art of California.* Ballena Press, Ramona, California.

HENDY, M. H. 1975. Photographing rock art without chalking and how to transfer a rock art design suitable for framing. *The Wyoming Archaeologist* 18(3-4): 14-6.

HOOPER, A. 1948. *Makers of mathematics.* Vintage Books, New York.

HORNELL, J. 1925. The archaic sculptured rocks and stone implements of Gorgona Island, South America [Columbia]. *Man* 25 (July): 104-7.

HUDSON, T. and K. CONTI 1984. The rock art of Indian Creek: ritual sanctuary of the gifted Chumash. In *Papers on Chumash rock art.* San Luis Obispo County Archaeological Society Occasional Paper 12: 47-88. San Luis Obispo Archaeological Society, California.

HUFFMAN, T. N. 1979. Test excavation at Chamabuefua, southern Mashonaland. *The South African Archaeological Bulletin* 34(129): 57-70.

JENNINGS, J. D. 1979. *The prehistory of Polynesia.* Harvard University Press.

KELLY, R. L. and L. C. TODD 1988. Coming into the country: early Paleoindian hunting and mobility. *American Antiquity* 53(2): 231-44.

LANTEIGNE, M. P. 1989a. The Manitoba rock paintings: evidence for territorial demarcation. Report presented to the Midwest Archaeological Conference, 13-15 October, Iowa City.

LANTEIGNE, M. P. 1989b. The Manitoba rock paintings. Report prepared for the Manitoba Heritage Federation, Inc., Winnipeg.

LANTEIGNE, M. P. 1989c. Further Comment on M. F. Nobbs and R. I. Dorn, 'Age determinations for rock varnish formation within petroglyphs: cation-ratio dating of 24 motifs from the Olary region, South Australia'. *Rock Art Research* 6: 145-9.

LEROI-GOURHAN, A. 1967. *Treasures of prehistoric art*. H. N. Abrams, New York.

LÉVI-STRAUSS, C. 1972. *Structural Anthropology*. Allen Lane, Penguin Press. [PJ]

LEWIS-WILLIAMS, J. D. and T. A. DOWSON 1988. The signs of all times: entoptic phenomena in Upper Palaeolithic art. *Current Anthropology* 29: 201-45.

LEWIS-WILLIAMS, J. D. and T. A. DOWSON 1989. Theory and data: a brief critique of A. Marshack's research methods and position on Upper Palaeolithic shamanism. *Rock Art Research* 6: 38-50.

LOCKYER, N. 1909. *Stonehenge and other British stone monuments astronomically considered*. MacMillan and Co., London.

LONG, R. C. E. 1918. The Maya and Christian eras. *Man* 18(August): 121-6.

MacKIE, E. W. 1977. *Science and society in prehistoric Britain*. St. Martin's Press, New York.

MARSHACK, A. 1972. *The roots of civilization*. McGraw-Hill, Toronto.

MARSHACK, A. 1988a. Rock art: the range, variability and modes of study. Paper presented in Symposium K of the First AURA Congress, on 2 September, Darwin.

MARSHACK, A. 1988b. On the entoptic hypothesis and shamanistic context of Palaeolithic art. Paper presented in Symposium K of the First AURA Congress, on 2 September, Darwin.

MARSHACK, A. 1989. Methodology in the analysis and interpretations of Upper Palaeolithic image: theory versus contextual analysis. *Rock Art Research* 6: 17-53.

MEADE, E. 1971. *Indian rock carvings of the Pacific Northwest*. Gray's Publishing Ltd., Sydney, British Columbia.

MINOR, R. 1975. *The pit and groove petroglyph style in southern California*. Ethnic Technology Notes No. 15. San Diego Museum of Man, San Diego.

MOIR, G., C. RUGGLES and R. NORRIS 1980. Megalithic science and some Scottish site plans. *Antiquity* 54(210): 37-43.

MORRIS, R. B. W. 1970. The petroglyphs at Achnabreck, Argyll. *Proceedings of the Society of Antiquaries* 103: 33-56. [PJ]

MORRIS, R. B. W. 1977. *The prehistoric rock art of Argyll*. Dolphin Press, Poole.

MORRIS, R. B. W. 1979. *The prehistoric rock art of Galloway and the Isle of Man*. Blandford Press. [PJ]

MORRIS, R. B. W. 1981. *The prehistoric rock art of southern Scotland*. B.A.R. British Series No. 86. [PJ]

MOUNTFORD, C. P. 1960. Simple rock engravings in central Australia. *Man* (October): 145-7).

MUNN, N. D. 1966. Visual categories. An approach to the study of representational systems. *American Anthropology* 68: 936-50. [PJ]

MUNN, N. D. 1973a. The spatial presentation of cosmic order in Walbiri iconography. In A. Forge (ed.), *Primitive art and society*, pp. 193-220. Oxford University Press, London. [PJ]

MUNN, N. D. 1973b. *Walbiri iconography*. Cornell University Press, Ithaca. [PJ]

NISSEN, K. M. 1975. A new style of rock art in the Great Basin: the Stillwater facetted style. *Nevada Archaeological Survey Reporter* 8(2): 10-1.

NISSEN, K. M. and E. W. RITTER 1986. Cupped rock art in north-central California: hypothesis regarding age and social/ecological context. In W. D. Hyder, H. Crotty, K. Sanger, F. Bock (eds), *American Indian Rock Art* 12: 58-75. American Rock Art Research Association, El Toro, California.

O'KELLY, J. M. 1980. *Newgrange*. Thames and Hudson. [PJ]

PARKMAN, E. B. 1984. A statement concerning cupule petroglyphs. Paper presented to the Society for California Archaeology, Salinas, California.

PARKMAN, E. B. 1986. Cupule petroglyphs in the Diablo Range, California. *Journal of California and Great Basin Anthropology* 8: 246-59.

PARKMAN, E. B. 1988. California dreamin': cupule petroglyph occurrences in the American West. Paper presented in Symposium B of the First AURA Congress, on 29 August, Darwin.

PAYEN, L. A. 1968. A note on cupule sculptures in exogene caves from the Sierra Nevada, California. *Caves and Karst* 10(4): 33-9.

PIGGOTT, S. 1973. The Dalladies Longbarrow, N.E. Scotland. *Antiquity* 47: 32-6. [PJ]

QUEREJAZU LEWIS, R. 1990. Rock art investigations in Bolivia. Paper presented to the Canadian Archaeological Association's 23rd Annual Meeting, Session 'Rock Art Research: into the 1990s', 9-13 May, Whitehorse, Yukon.

RUGGLES, C. L. N. 1982. A reassessment of the high precision megalithic lunar sightlines 1: backsights, indicators and the archaeological status of the sightlines. *Archaeoastronomy* 13(4): 21-40.

SAS INSTITUTE, INC. 1985. *SAS user's guide: statistics, version 5 edition*. SAS Institute, Inc., Cary.

SEHGAL, L. and A. BENSON 1988. Climbing Jacob's ladder: symbolism of a fantastic journey along the Milky Way. Paper presented in Symposium K of the First AURA Congress, on 2 September, Darwin.

STEINBRING, J. 1987. The Sassaginnigak Lake rock painting site, EjKv-P3, Manitoba. *Manitoba Archaeological Quarterly* 2(1): 3-47.

STEINBRING, J. 1988. Rock art site classification: part 2. *La Pintura* 14(2-3): 9-11.

STEINBRING, J. and G. GRANZBERG 1986. Ideological and cosmological inferences from North American rock art: an exploratory discussion. In K. Hedges (ed.), *Rock Art Papers* 3(20): 207-20. San Diego Museum Papers, San Diego.

STEINBRING, J., G. GRANZBERG and M. LANTEIGNE 1988. Elemental analysis in pursuit of function in rock art. Paper presented in Symposium K of the First AURA Congress, on 2 September, Darwin.

STEINBRING, J. and M. LANTEIGNE 1989. Archaeopsychological process in rock art: entoptic phenomena or relative recurrence of elemental forms? Paper presented to the Rock Art Symposium, San Diego Museum of Man, on 4 November, San Diego.

STEINBRING, J. and S. STEINBRING 1983. Manipulative factors in Hawaiian rock art. *American Indian Rock Art* 9: 89-95.

STRIEDTER, K. H. 1988. Ideograms in north African and Saharan rock art. Paper presented in Symposium K of the First AURA Congress, on 1 September, Darwin.

STRUICK, D. J. (ed.) 1969. *A source book in mathematics, 1200-1800*. Harvard University Press, Cambridge.

THIERMANN, U. 1977. The dots of Pantiacolla [Peru]. *South American Explorer* 1(1): 4-5.

THOM, A. 1971. *Megalithic lunar observatories*. The Clarendon Press, Oxford.

WALKER, M. J. 1970. An analysis of British petroglyphs. *Science and Archaeology* 2+3: 30-61. [PJ]

WALKER, M. J. 1974. Schematised rock carvings as archaeological evidence. In P. J. Ucko (ed.), *Form in indigenous art*, pp. 452-69. Duckworth. [PJ]

WEINBERGER, G. 1980. Cupules and their context: some Southern Valley Yokuts site examples. Paper presented at the annual meeting of the Society for California Archaeology, Redding, California.

WISHART, D. 1987. *Clustan user manual: cluster analysis software*, fourth edition. Computing Laboratory, University of St. Andrews, Fife.

WRESCHNER, E. E. 1976. The potential significance of the pebbles with incisions and cupmarks from the Yarmukian of Sha-ar Hagolan, Israel. *Bulletin de la Société Royale Belge d'Anthropologie et de Préhistoire* 87: 157-65.

YOUSCHKEVITCH, A. P. 1976. *Les mathématiques arabes* (8e - 15e siècles). Traduction française de M. Cazenave et K. Jaouiche. Collection d'Historie des Sciences, 2 - C.N.R.S. Centre d'Histoire des Science et des Doctrines. Librairie Philosophique J. Vrin, Paris.

KEYWORDS: **Rock paintings - Dating - Historical interpretation - Southern China**

ROCK ART AT HUASHAN, GUANGXI PROVINCE, CHINA

Li Fushun

Abstract. A major site of rock paintings is described from one of the southern-most regions of China. Aspects of its historical interpretation in terms of written records are considered, the dating of the art is briefly mentioned, and iconographic conventions used in the paintings are described and discussed.

The Guangxi Zhuang Autonomous Region, in the southern-most part of the Chinese mainland, is occupied by many ethnic groups. Among them, the Zhuang number over 13 million inhabitants. To the south of the region lies the Gulf of Tonkin, and on the south-west it borders the Vietnamese Democratic Republic. The scenery of Guilin in Guangxi has long been renowned throughout the world, but the rock art of the region has remained unknown outside of China.

Ningming County lies in the south-western part of Guangxi, close to the Vietnamese Democratic Republic (Fig. 1). Huashan Hill is situated on the eastern bank of the Ming River, in the north-west of Ningming County, and rises to 345 m above sea level. The Ming River approaches it from the south-west and at the foot of the Huashan Hill it turns north-west. So the Huashan Hill is just at the bend of the river.

The rock pictures are in the lower part of the 230 m-high cliff. The entire rock wall forms a large overhang which protects the rock art from rain. The painted panel is 221 m long and up to 40 m high, and it consists of 1819 figures (Fig. 2). The attitudes of some of the human figures suggest dancing and sexual acts, there are humans aboard boats, and images resembling dogs, ducks, brass drums, brass gongs, knives, swords and other objects. The largest anthropomorphs are about 3 m in height, while the smallest measure in the order of some dozens of centimetres. The panel is densely covered with figures, and its grand scale is of a magnitude rarely seen elsewhere in the world.

According to recent analytical work by the Archaeology Department of Beijing University, as well as the view of most contemporary Chinese rock art researchers, the Huashan paintings date from between 2370 and 2115 years BP, i.e. sometime between the Warring States period and the West Han Dynasty (Qin Shengmin et al. 1987: 230-2). Dating was secured from the radiocarbon content of stalactite (reprecipitated calcium carbonate) physically related to the art. (The dating work was conducted by Yuan Sixun, Chen Tiemei, Ma Li, Meng Qingping and Hu Yanqiu, Laboratory of the Archaeology Department of Beijing University.) The artists were of the Luo Yue tribe (ancestors of today's Zhuang people). It is suggested that the production of the paintings was connected with sacrificial practices (e.g. offerings to the God of Water, or to ancestors). For instance, a grand dancing scene may have been depicted in order to curry favour with the gods or to secure blessings (op. cit.: 171). Even in 1985 I observed offerings of sacrifice to the God of Water at the site: two brothers, having survived a collision in which their boat sank, placed a basket with a chicken and some burning joss sticks under the paintings on the morning after the accident, in token of their gratitude to the deity.

The practice of offering sacrifices to the God of Waters extends well into the ancient past. According to records on the *jia gu wen* (characters found on turtle shells and animal bones), females were sacrificed to the God of Rivers as far back as the Shang Dynasty in the 16th century B.C. (Zhu Tianshun 1982: 183). In the 2nd century B.C., Si Maqian (1973: 3211) recorded the prac-

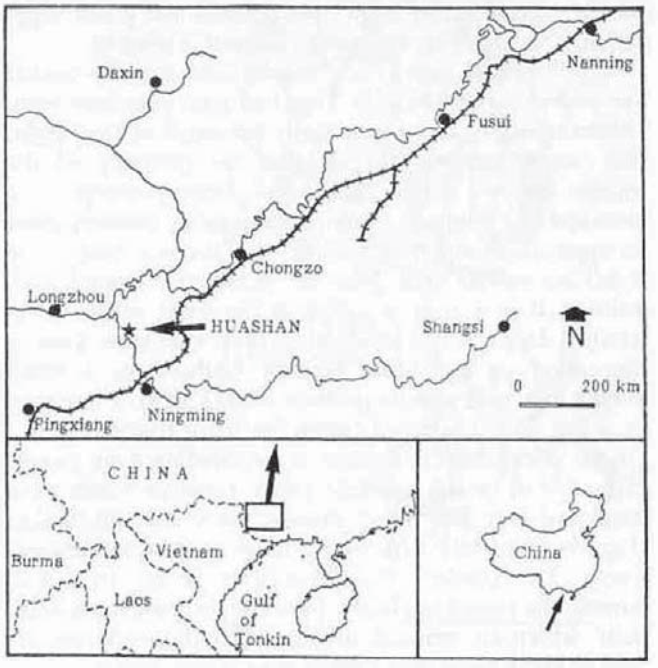

Figure 1. Location of Huashan in Guangxi Province, China.

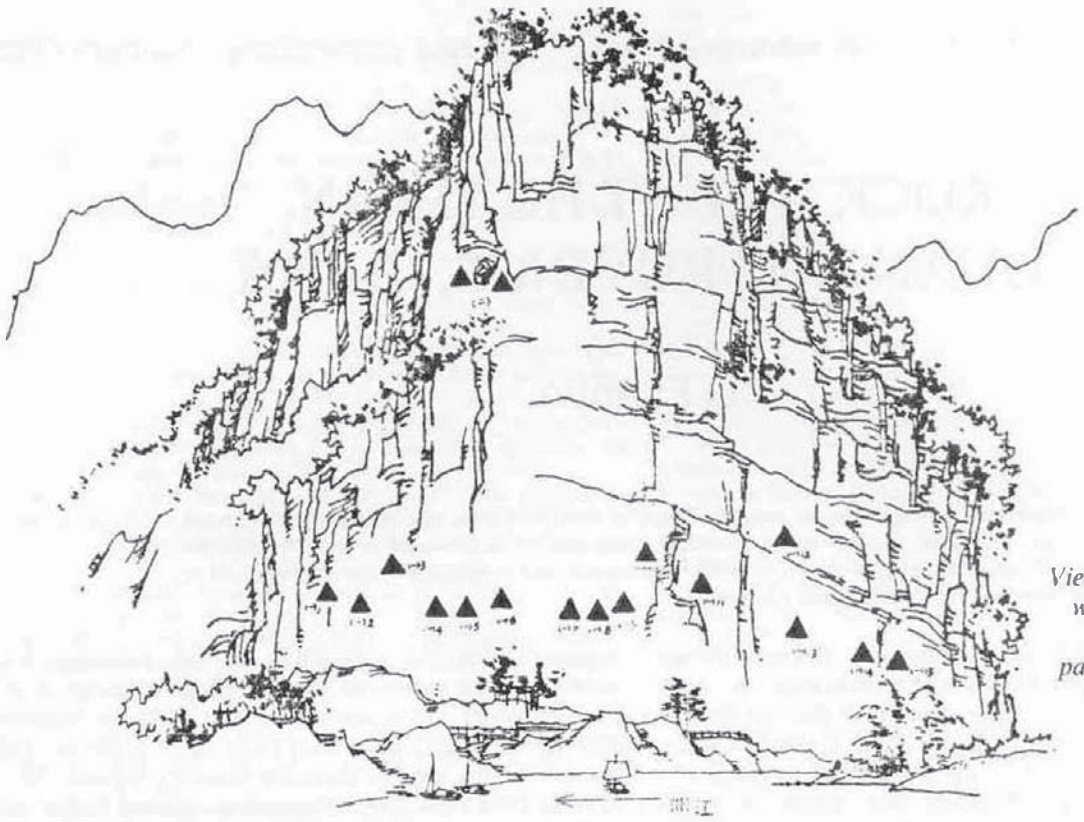

Figure 2. *View of Huashan Hill, with the positions of the major rock art panels indicated. The cliff is 230 m high.*

tice of sending young girls as brides to the God of Rivers (i.e. throwing them into the river so that their souls would follow the deity).

The Huashan panel of rock paintings exhibits the following prominent features:

(1) *The images are densely crowded, large and impressive.* Scarcely any room is left between the nearly 2000 images covering the surface of this huge rock, over 200 m wide and 40 m high. Such a monumental assemblage of rock paintings is not found in other provinces or regions throughout China. As the figures are all painted in a single colour, red, and the surface of the rock is a yellowish white, surrounded by brown stone walls and green vegetation, a strong effect of colour contrasts is evident.

(2) *Principal figures are emphasised and the figures are orderly arranged.* The Huashan rock paintings seem indiscriminately and haphazardly arranged at first sight, but careful analysis reveals that the grouping of the images follows certain rules. The anthropomorphs are arranged in a number of groups, each group centring upon an apparently important personage. This principal figure is tall and solidly built, generally depicted in frontal 'frog' attitude. It may have a sword at the waist and a short-handled dagger in the hand, and it may wear some kind of decoration on the head, perhaps feathers, or a small animal may appear to be perched on it (Fig. 3). There may be a dog-like quadruped below the main figure (Fig. 4). On the sides of this personage or surrounding it are groups of people in frontal and side views, some of which have long hair and protruding breasts, some are ithyphallic (Fig. 5), evidently depicting females and males respectively. The gender of other figures is not indicated. Among the crowd are brass drums, brass gongs and brass bells, which are musical instruments traditionally associated with the sacrifices.

Dogs were regarded as divine creatures by the ethnic minorities of southern China. Li Yanshou (7th century B.C.) reports:

> The Liao nation held dogs in high respect. When there was a fight between a father and his son, and the son killed his father accidentally, escaped and hid himself, he could be pardoned by his mother provided he could get a dog and bring it home to offer apology to his mother. (Li Yanshou 1974: 3154)

The historian Fan Ye (5th century B.C.) describes how a fattened dog was asked in incantations to lead the soul of a dead warrior to the Red Hill, before it was cremated together with a warrior and his horse (Fan Ye 1973: 2980). Dogs were regarded as the protectors of the dead. Their images can also be found on the walls of the burial caves of Gongxian County, Sichuan Province, in which coffins were suspended from the cliffs.

A striking aspect in the grouping of the Huashan images is that the central figures are usually of much larger size. This method of depiction was often employed in various arts as a graphic convention of expressing importance or beauty. For instance, in the sculptures and paintings of ancient Egypt, the images of rulers are always considerably larger than those of their subjects. The ancient sculpture of Laocoon (Trojan priest, killed with his two sons by two sea serpents after warning the Trojans of the wooden horse) also shows that the serpent-entwined father is about twice the size of his two sons. Among the Indian and Chinese Buddhist sculptures, the image of Buddha is always much larger than those of his disciples and the bodhisattvas at his side. The phenomenon of considering largeness as 'beautiful' was an aesthetic concept of many early peoples. Largeness can also represent authority or social position. Such an artistic method of using the relative sizes of the images to distinguish principal from secondary components has been in use in China for scores of centuries. It has also existed in other parts of the world. In Europe, the method remained in use until some time after the Renaissance.

(3) *Extensive use of the method of contrasting.* One of the factors rendering the Huashan rock art rich in artistic

Figure 3.
Red painting of anthropomorph with small animal perched on head and dog-like quadruped below. Huashan Hill, China.

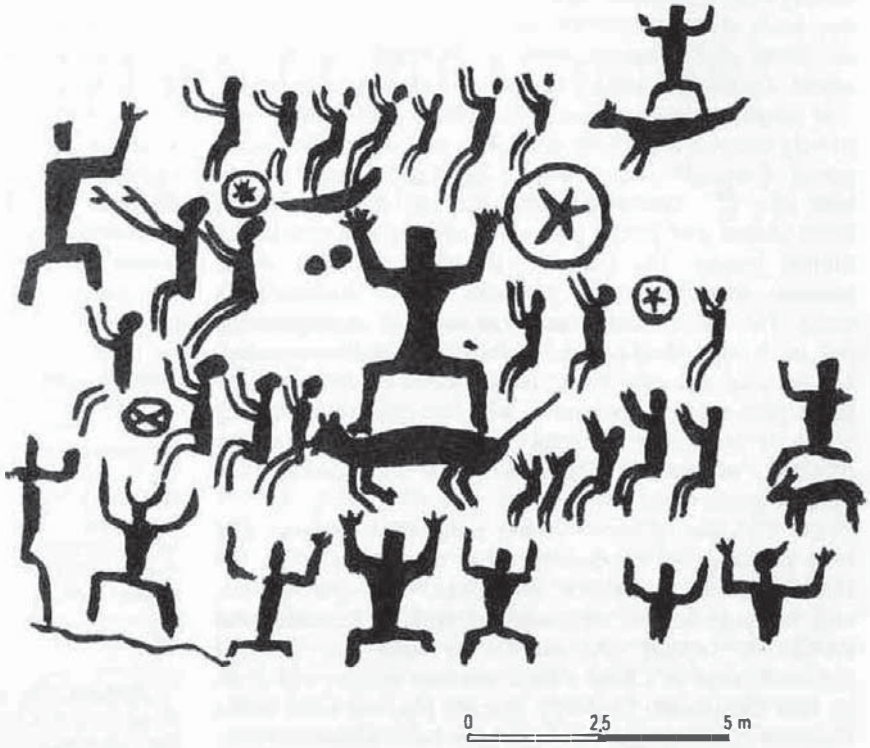

Figure 4.
Group of anthropomorphs, in both frontal and profile views, and some quadrupeds. Huashan.

Figure 5.
Some of these anthropomorphs are ithyphallic, others seem to have long hair. Huashan.

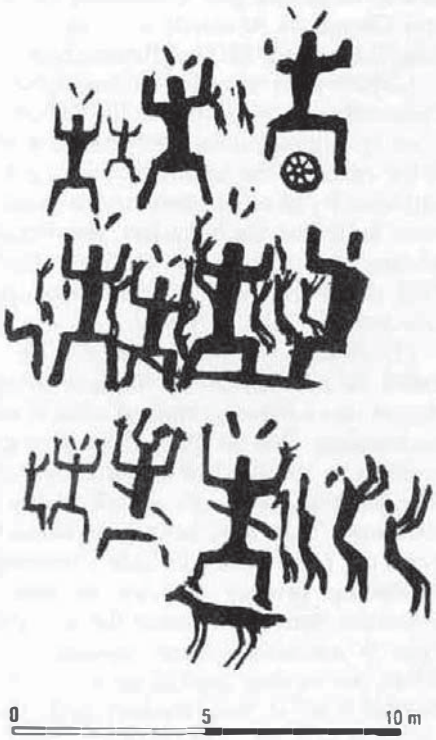

Figure 6.
Typical group of anthropomorph paintings, Huashan Hill, China.

appeal is the extensive use of contrasting. Among the images, there are contrasts of sizes, frontal and side views, attitudes etc.; the principal figures are large, tall and mostly seen in frontal view, while the secondary figures are small, short and mostly depicted in side view. Among the forms of the images, there is a contrast of square and round, the straight and crooked, still and moving, complex and simple, sparse and dense etc. Humans in frontal view mostly assume a position like  , and are mostly composed of straight lines; those in side view assume an attitude like  , composed mainly of curves. The circular brass drums and gongs provide a striking contrast to the human figures. The latter are distinctly animated while musical instruments and weapons appear appropriately static. The entire mural consists of compact arrangements, but each individual image is most succinctly executed. Concerning colours, there is the contrast between the large, pale-coloured precipice, which is especially striking when viewed from the distance, the red of the pictures, the azure sky of southern China, the green vegetation and the blueish-green water of the river.

(4) *The use of monochrome painting technique*. The rock paintings so far discovered in China, including the Huashan panel, are almost all painted in single colours, and the polychrome art found in Africa, Australia and Europe is lacking. As shown by analyses, the red pigments used in Chinese rock pictures are derived from an iron compound. Similarly, the red pigment used in the Huashan paintings has been found to be of mineral origin, by the Guangxi Chemical Research Institute. While I conducted investigations at the site in 1985, as a member of the Zuojiang River Valley Rock Art Investigation Corps, a team member discovered lumps of pigment in a cave above the rock paintings. When mixed with water and daubed onto rock, it produced the same effect as the ancient rock paintings. The binder of the pigment has been identified as an animal protein compound consisting mainly of animal glue (identified by the Guangxi Industrial Chemicals Research Institute under the direction of Long Gangqiang; cf. Qin Shengmin et al. 1987: 233).

Objects excavated by archaeologists indicate that the Palaeolithic people living on the Chinese mainland 30 000 years ago already decorated articles with ground iron ore. In the caves on the summits of hills at Zhoukoudian, then inhabited by cave dwellers, ochre-covered small pebbles, stone balls, fox teeth, antlers, fish bones and ornamental articles have been found (Chinese Encyclopaedia 1986: 432). (I saw these many years ago, while attending nearby Zhoukoudian Middle School.)

(5) *Schematisation of images*. By schematisation is meant to summarise the images of a certain kind of objects into a basic formula of relative uniformity. It is the intermediate link to pass from pictographs to abstract symbols in the field of pictorial art. The images in the Huashan rock paintings are all highly abstracted representations. They have been generalised into two perspectives, the frontal and the side ('dancing') attitudes. The abstracting process has led to their resemblance of geometric forms. The reason for such treatment is that the focus is not on the iconic content of the images themselves, but on their implied meaning. On the one hand, the original role of the Huashan rock painting panel was probably to provide an object for worship to the people offering sacrifices, and on the other the art was a token offered to curry favour with the gods. It had to be capable of eliciting communal sympathy, so it abounds with mystique. In accordance with their main purpose, the images had to be striking, conspicuous and divine, and had to arouse a sense of sublimity among the people.

The Huashan rock paintings are rich in content, and of a unique style. They offer excellent potential for studying various cultural aspects of the people they refer to, their religious beliefs and ideas of aestheticism. The art is therefore of great historical and artistic value. It is introduced here for the benefit of rock art scholars world-wide, as a valuable source of information enabling comparative research.

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Résumé. *On décrit un site important de peintures rupestres d'une région de l'extrême sud de la Chine. Certains aspects de son interprétation historique sont considérés vis-à-vis des rapports écrits, la datation de l'art est brièvement mentionnée, et les convenances iconographiques observées dans les peintures sont décrites et discutées.*

Zusammenfassung. *Ein sehr grosses Fries von Felsmalereien aus einer der südlichsten Gegenden von China wird beschrieben. Gesichtspunkte ihrer geschichtlichen Deutung auf Grund von schriftlichen Überlieferungen werden erörtert, die Datierung der Kunst wird kurz erwähnt, und die in den Malereien verwendeten ikonographischen Regeln werden beschrieben und besprochen.*

Resumen. *Un sitio grande de pinturas rupestres de una de las regiones más meridionales de la China es descrito. Aspectos de su interpretación histórica, en términos de registros escritos son considerados, la datación del arte es brevemente mencionada, y las convenciones iconográficas usadas en las pinturas son descritas y discutidas.*

REFERENCES

CHINESE ENCYCLOPAEDIA 1986. Chinese Encyclopaedia Publishing House, Beijing.

FAN YE 1973. Biography of Wu Huan. In *Book on the Later Han Dynasty*. Zhong Hua Book Store, Beijing.

LI YANSHOU 1974. Southern nationality. In *History of the north*. Zhong Hua Book Store, Beijing.

QIN SHENGMIN, QIN TSAILUAN, LU MINFEI and YÜ JUYÜ 1987. *The investigation and research of the cliff and mural paintings of the Zuojiang River Valley in Guangxi*. Guangxi National Printing House, Nanning.

SI MAQIAN 1973. *Shih Ji* (Chinese history). Zhong Hua Book Store, Beijing.

ZHU TIANSHUN 1982. *Preliminary exploration of the Chinese ancient religion*. Shanghai People's Publishing House, Shanghai.

KEYWORDS: Percussion petroglyphs - Cupules - Raisen - Bhimbetka - India

PETROGLYPHS FROM CENTRAL INDIA

R. G. Bednarik, G. Kumar and G. S. Tyagi

Abstract. The recording of the first prehistoric petroglyphs in the renowned rock painting region of central India is reported. Recent discoveries suggest that deeply patinated petroglyphs have previously not been recognised in the area. Their possible antiquity is briefly considered.

Since rock paintings were first noticed in India in 1867-68 by A. Carllyle (Smith 1906: 185-95) and recorded by Cockburn (1883, 1899), central India has become one of the richest known repositories of rock art in the world. Hundreds of site clusters have been located, many of which consist of dozens of individual shelters. Major concentrations of rock paintings exist on the Chambal and its tributaries, around Bhopal, Madhya Pradesh, and in the Mirzapur district of Uttar Pradesh, while rock art tends to become more sparse as one travels southwards from these focal areas. In comparison to this wealth of rock paintings, comparatively few petroglyphs have been located throughout India, and it is widely assumed that only the practice of rock painting has contributed to prehistoric rock art production in India's central regions.

Early records of Indian petroglyphs come from Fawcett (1901: 409-21), who discovered those of Edakal Cave in 1894-96; from Carnac (1877: 1-15), who located 'rows of shallow depressions' in the Himalayas; and from Henwood (1856: 204-5). In the 1880s, H. Knox found petroglyphs in Karnataka which were later described by Foote (1916: 87-9). Petroglyphs are quite well represented in southern India (Deccan region, Karnataka, Andhra Pradesh, Kerala) and in the north, especially in the Himalayas. Mathpal's (in press) substantial rock art study of the Uttarakhand region includes the recording of 56 petroglyph sites and stands out as the only major work in this area.

Thus the existence of petroglyphs in India has long been known, but most researchers have neglected them in favour of the more spectacular paintings. In the rock art region of central India, which occupies much of Madhya Pradesh, prehistoric petroglyphs have been claimed to be lacking almost entirely. Wakankar (1984) mentions only the occurrence of some faint scratchings on calcite encrustations at the vast rock painting complex of Bhimbetka, south of Bhopal, which are difficult to recognise. He was also aware of a group of cupules in shelter Bhim III F-24, but did not realise what they are, believing them to be use traces of 'musical stones'. True lithophones do occur in southern India (E. Neumayer, pers. comm. to RGB), and they occur of course in other parts of the world, such as Kenya (Odak 1988) and in the Palaeolithic of Europe (Dams 1985).

Following the Rock Art Seminar in Agra in 1990, a three-week field trip was undertaken by GK and RGB, concentrating on three regions: the Chambal valley, three major rock art clusters near Bhopal, and the Kurnool region in southern India. The latter two regions were found to contain petroglyphs that had not been previously recorded.

The most interesting find was perhaps the first one. Raisen is a small town about 30 km east of Bhopal (Fig. 1), in an area geologically dominated by low-relief quartzite sandstone escarpments with a distinct bedding that is inclined at 8-10°, overlooking broad alluvial plains and loess deposits. The sandstone hills are covered by semi-arid scrub vegetation, the valleys are used as agricultural land. The region's rock art, which is among the better preserved in India, is being subjected to an intensive study by GST.

On 3 March 1990 we examined a series of eleven painted rock shelters at Hathithol, Raisen. In the shelter

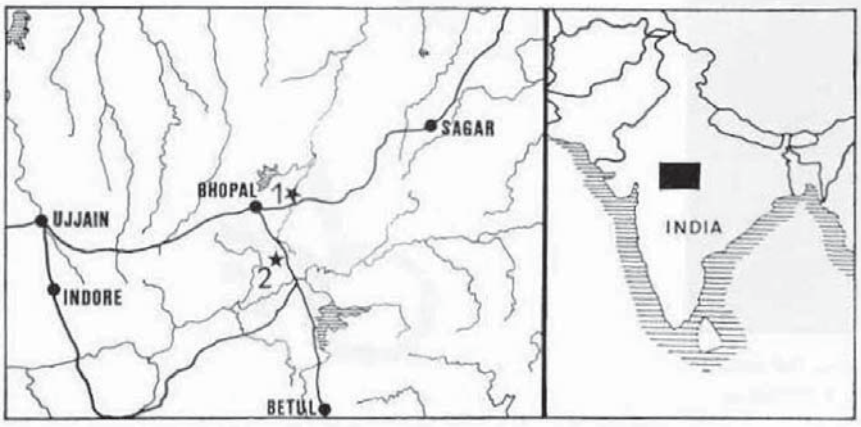

Figure 1. *The study area and its location in India. The two petroglyph sites are: 1 - Hathithol, near Raisen 2 - Bhimbetka*

next to the westernmost, RGB located the first recorded percussion petroglyphs identified in central India. (A percussion petroglyph is a rock art motif produced by a deductive process in which a percussion method was used, such as pecking, battering, bruising or pounding.) It is on a sloping floor panel of very dense and compact, buff-coloured quartzite that has been patinated to a light brown (Munsell Chart, No. 2.5YR-6/6) of a distinctly glazed appearance. There are few exfoliation scars on this rock, but none are in the vicinity of the petroglyphs. A polished grinding depression, oval in shape and measuring about 210 x 140 mm, is located about 200 mm from the main petroglyph and bears a similar patination (Fig. 2). The petroglyph consists of several hundred fine impact marks which form a circle ('enclosed space') with at least one, probably two, and possibly further cupule marks next to it. There are two radial lines attached to the circle which are less deeply pounded. In view of the extreme hardness and compactness of the quartzite, a very considerable effort was required to produce this motif (Fig. 3).

The outside diameter of the circle varies from 105-113 mm, the groove width from 20-26 mm, the maximum depth is 2.9 mm. The radial lines are 80 mm and 85 mm long respectively, and oriented at 54° of each other. The clear cupule is of 25 mm diameter and also up to 2.9 mm deep. Microscopic examination of two recent scratches in the grinding depression suggests that the case hardening present on the shelter floor is of a silica skin.

Just below the rock shelf bearing the petroglyphs is an artificial platform constructed of loose rock slabs, which could well conceal further petroglyphs. The back wall of the shelter and its roof above the petroglyphs is richly decorated with rock paintings, which have been claimed to be of the Mesolithic and Chalcolithic periods, on the basis of stylistic argument.

On 7 March, RGB and GK examined the 'rock gong' in Bhimbetka III F-24, and found it to bear seven clearly very early cupules (or cup marks). It is considered to be ineffective as a gong, and the cupules are no different from those found in all other continents, often in vast numbers. We consider them to be percussion petroglyphs, and not incidental marks, and we request that researchers refrain from damaging them further by trying whether the huge boulder bearing them would function as a rock gong. It would certainly lack the required acoustic properties, and the bruising occasioned by this vandalism will prejudice future dating attempts of the rock art.

Although they are completely sheltered inside the cave-like site, the cupules are deeply weathered and they predate extensive geomorphological deterioration of the rock. A few metres from them, on a large boulder at the base of a 2 m-deep excavation, is a cup-like depression and an adjacent meandering groove, which may also be artificial and were concealed by Palaeolithic occupation strata (Bednarik 1990).

Since then, GK has located extensive cupule patterns at Kanyadeh II and IIIA, near Kota, which include also pounded grooves, and are deeply patinated and covered by a mineral deposit; and he has found complex cupule patterns at Chattaneshwar IV, Chambal region, which precede patination and a sequence of exfoliation events. We therefore expect that further petroglyph sites will be found in central India in due course. Cupules are also likely to be more common elsewhere in India: prompted by our finds, N. Chandramouli (pers. comm. to RGB) has located a large cupule site at Chintakunta, Andhra Pradesh, although these petroglyphs are clearly very much younger. Rao has recently recorded cupules in the Vidarbha region (Rao 1989: 59-63).

Discussion

There are no clues to the age of the Raisen petroglyphs, apart from the patination of the pounded grooves, which is identical to that of the unmodified adjacent rock surface. The impact marks resemble those made with stone tools on similar rock in other countries. Contemporaneity of the petroglyph and the nearby grinding hollow is not assured. The latter is unlikely to be the result of axe or celt grinding, a distinct Neolithic with ground axes is not found in the region, only few polished celts have appeared in rather late Chalcolithic levels (E. Neumayer, pers. comm. to

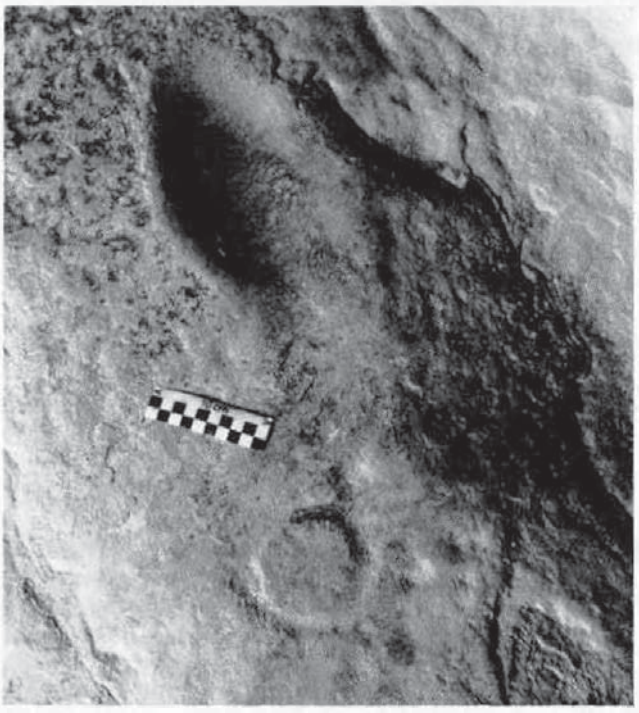

Figure 2. The first petroglyphs at Raisen, and the nearby grinding depression. Floor of painted rockshelter at Hathithol.

Figure 3. Recording of Raisen petroglyphs.

RGB). Thus the grinding surface more likely served for food preparation (e.g. cereal seeds) or for the preparation of mineral pigments. Microscopic examination revealed no extraneous material on the polished surface.

The perhaps most striking aspect of the petroglyphs is the similarity with archaic art in other continents, for instance in Australia and the Americas (Bednarik 1988). Early Indian rock paintings have close parallels in northern Australia (dynamic art in Arnhem Land, Kimberley), where some of this art certainly dates from the Pleistocene (Loy et al. 1990). The Pleistocene antiquity of the 'archaic linear petroglyph tradition' in Australia is well established (e.g. Nobbs and Dorn 1988; Morwood 1989).

In India, reliable evidence for a Pleistocene antiquity of any of the paintings has remained elusive. Based on his find of what he thought was a green pigment pebble in the sediment deposit of Bhimbetka III A-28, Wakankar (1983) has suggested that the green, dynamic paintings of central India are of the Upper Palaeolithic (Sali 1974: 152). GST has refuted his claim that the green 'S-figures' have precedence, having found many superimposed on red motifs, such as 'intricate patterns' (Tyagi 1988). Thus the existence of Upper Palaeolithic rock paintings remains a controversial issue which can only be resolved by further work, and the introduction of new methodologies. However, we propose that future discussion of this subject should take into consideration the possibility that non-iconic petroglyphs may have been produced during the Palaeolithic of India. Our enquiries suggest to us that many Indian rock art researchers may not have been able to recognise deeply weathered, worn or patinated petroglyphs until now, which means that there may have been a recording bias against older petroglyphs.

It is suggested that petroglyph production in India was not restricted to the north and south, but that further research efforts will locate many examples in the rock painting region of central India, in Madhya Pradesh and adjacent areas of Rajasthan. The finds reported here should not be seen as isolated instances, they relate to major prehistoric petroglyph traditions. Indian rock art, seen until now almost entirely as a succession of Holocene painting traditions, is to be reassessed in this light.

We emphasise that this is a preliminary paper, that the discussion offered is speculative and not to be cited as evidence, and that there is no intention of presenting in this brief note a comprehensive report about Indian petroglyphs. We concede, however, that such a major paper is much needed, and we will endeavour to provide same, or encourage others to do so.

Acknowledgments

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R. G. Bednarik, *RAR* Editor

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Résumé. *On présente un rapport sur les premières gravures préhistoriques sur roche de la région célèbre des peintures rupestres du centre de l'Inde. Des découvertes récentes suggèrent que les gravures extrêmement patinées n'ont précédemment pas été reconnues dans la région. Leur antiquité éventuelle est brièvement considérée.*

Zusammenfassung. *Die Registrierung der ersten prehistorischen Petroglyphen in dem bedeutenden Gebiet von Felsmalereien in Zentralindien wird berichtet. Neue Entdeckungen deuten an, dass tiefpatinierte Petroglyphen bisher in dem Gebiet nicht erkannt wurden. Ihr mögliches Alter wird kurz erörtert.*

Resumen. *El registro de los primeros petroglifos prehistóricos en la famosa región de pinturas rupestres de la India central es presentado. Descubrimientos recientes sugieren que petroglifos intensamente patinados no han sido previamente conocidos en el área. Su posible antigüedad es brevemente considerada.*

REFERENCES

BEDNARIK, R. G. 1988. Art origins. Paper presented in Symposium K of the First AURA Congress, Darwin, 2 September (in press).

BEDNARIK, R. G. 1990. The Palaeolithic art of Asia. Paper presented at 23rd Chacmool Conference, Calgary, Canada, November (in press).

CARNAC, H. R. 1877. Rough notes on some ancient sculpturings on rocks in Kumaon, similar to those found on monoliths and rocks in Europe. *Journal of the Asiatic Society of Bengal* 46(1): 1-15.

COCKBURN, J. 1883. On the recent existence of *Rhinoceras indicus* in the north western provinces, and a description of a tracing of an archaic rock painting from Mirzapore representing the hunting of this animal. *Journal of the Asiatic Society of Bengal* 52(2): 56-64.

COCKBURN, J. 1899. Cave drawings in the Kaimur Range, north west provinces. *Journal of the Royal Asiatic Society of Great Britain and Ireland* 31: 89-97.

DAMS, L. R. 1985. Palaeolithic lithophones: descriptions and comparisons. *Oxford Journal of Archaeology* 4(1): 31-46.

FAWCETT, F. 1901. Notes on the rock carvings in the Edakal Cave, Wynaad. *The Indian Antiquary* 30: 409-21.

FOOTE, R. B. 1916. *Madras Government Museum: the Foote Collection of Indian prehistoric and protohistoric antiquities, notes on their ages and distribution.* Government Press, Madras.

HENWOOD, W. J. 1856. Megalithic structures at Devidhoora in Almora. *Edinburgh New Philosophical Journal* (n.s.), pp. 204-5. Edinburgh.

LOY, T. H., R. JONES, D. E. NELSON, B. MEEHAN, J. VOGEL, J. SOUTHON and R. COSGROVE 1990. Accelerator radiocarbon dating of human blood proteins in pigments from Late Pleistocene art sites in Australia. *Antiquity* 64: 110-6.

MATHPAL, Y. in press. *Rock art in Uttarakhand*. Indira Gandhi National Centre of the Arts, New Delhi.

MORWOOD, M. 1989. The archaeology of Aboriginal art in S.E. Cape York: preliminary report on the 1989 fieldwork. *Rock Art Research* 6: 155-6.

NOBBS, M. F. and R. I. DORN 1988. Age determinations for rock varnish formation within petroglyphs: cation-ratio dating of 24 motifs from the Olary region, South Australia. *Rock Art Research* 5: 108-46.

ODAK, O. 1988. Ethnographic contexts of rock art in east Africa. Paper presented in Symposium H of the First AURA Congress, Darwin, 1 September (in press).

RAO, K. P. 1989. Identification of south Indian Megalithic complexes and their significance. *The Eastern Anthropologist* 42(1): 59-63.

SALI, S. A. 1974. Upper Palaeolithic research since Independence. *Bulletin of the Deccan College Research Institute* 34: Nos 1-4.

SMITH, V. 1906. Pygmy flints. *The Indian Antiquary* 6: 185-95.

TYAGI, G. S. 1988. The decorative intricate design patterns in Indian rock art. Paper presented in Symposium A of the First AURA Congress, Darwin, 30 August (in press).

WAKANKAR, V. S. 1983. The oldest works of art? *Science Today* 20: 43-8.

WAKANKAR, V. S. 1984. Techniques of prehistoric paintings and engravings. In K. K. Chakravarty (ed.), *Rock art of India*, pp. 196-200. Arnold-Heinemann, New Delhi.

KEYWORDS: Lightning Brothers - Rainbow Serpent - Victoria River District - Australia

A SURVEY OF ROCK ART IN THE VICTORIA RIVER DISTRICT, NORTHERN TERRITORY

Howard P. McNickle

Abstract. The major rock art body of the Victoria River District in the Northern Territory, Australia, has received little attention until the 1980s. Extensive survey work by the author and other researchers has shown the region to possess one of the largest and most diverse concentrations of rock art in Australia. Over 600 sites have so far been recorded, consisting of many painting and petroglyph styles. Particularly noteworthy is the occurrence of the world's largest known paintings, of composite painted/engraved figures, and of recent 'contact petroglyphs'. In this progress report, the author provides an overview of the distribution, stylistic forms, techniques and various motif types, and he considers also the region's unique stone structures.

The Victoria River District (VRD) is an area of over 300 km square [100 000 km^2, about the size of Hungary or Indiana], comprising about one-tenth of the area of the Northern Territory, Australia (Fig. 1). For the purposes of this report, the VRD boundaries can be defined as: the Fitzmaurice River to the north; Hooker Creek to the south; the Delamere Road to the east and the Western Australian border to the west. This region comprises the entire catchment of the Victoria River and its tributaries, including the West Baines and Keep Rivers, and also incorporates part of the catchment of the Daly and Fitzmaurice Rivers in the north-east sector and part of the catchment of the Ord River in the south-west.

Even though rock art was known to be located within this area since the time of the first European exploration, surprisingly little survey research had been carried out until the 1980s. Rock paintings were first recorded along Gordon Creek, a tributary of the upper Victoria River, during the 1856 expedition of the explorer Augustus Charles Gregory and they were illustrated by Thomas Baines. The well-known Lightning Brothers site on Delamere station was first recorded by Davidson (1935) in 1930, and the main figures were repainted about 1950. Some early archaeological excavations were carried out in this part of the VRD, including at the Delamere site, and also at the Ingaladdi Waterhole on Willeroo Station where, in 1966, the first minimum dates for petroglyphs in Australia were obtained by Professor D. J. Mulvaney (1969), who excavated a buried panel of abraded grooves from a layer dated at about 6000 years BP.

In spite of these early discoveries, the VRD as a whole was very largely neglected for many years. Among the reasons for this were the remote nature of most of the region and in particular of most art sites, the lack of any sizeable centres of population, and the somewhat trying climate (wet and humid in summer, mainly hot and dry for the remainder of the year). While detailed recording was conducted from 1960 onward in other rock art regions of northern Australia such as south-east Cape York Peninsula, western Arnhem Land, and parts of the Kimberley and Pilbara regions, very little further recording was undertaken in the VRD.

One of the first to carry out detailed rock art research in the VRD was Darrell Lewis who recorded a number of sites, mainly in the Victoria River Downs and Wave Hill areas in the south-east of the VRD. Lewis's project, beginning in the late 1970s, did not record a large number of sites, but did include detailed ethnographic work relating to several of these sites. Much of this work was carried out in conjunction with anthropologist Dr D. B. Rose, initially on behalf of the Northern Territory Museum of Arts and Sciences and more recently for the Australian Institute of Aboriginal Studies. George Chaloupka of the Northern Territory Museum also

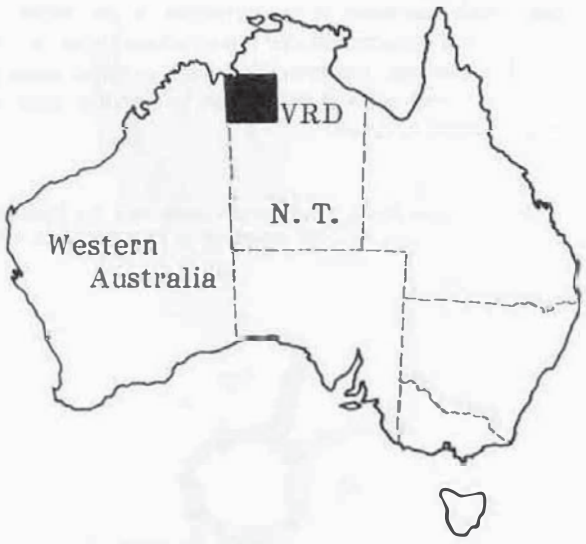

Figure 1. Location of the study area.

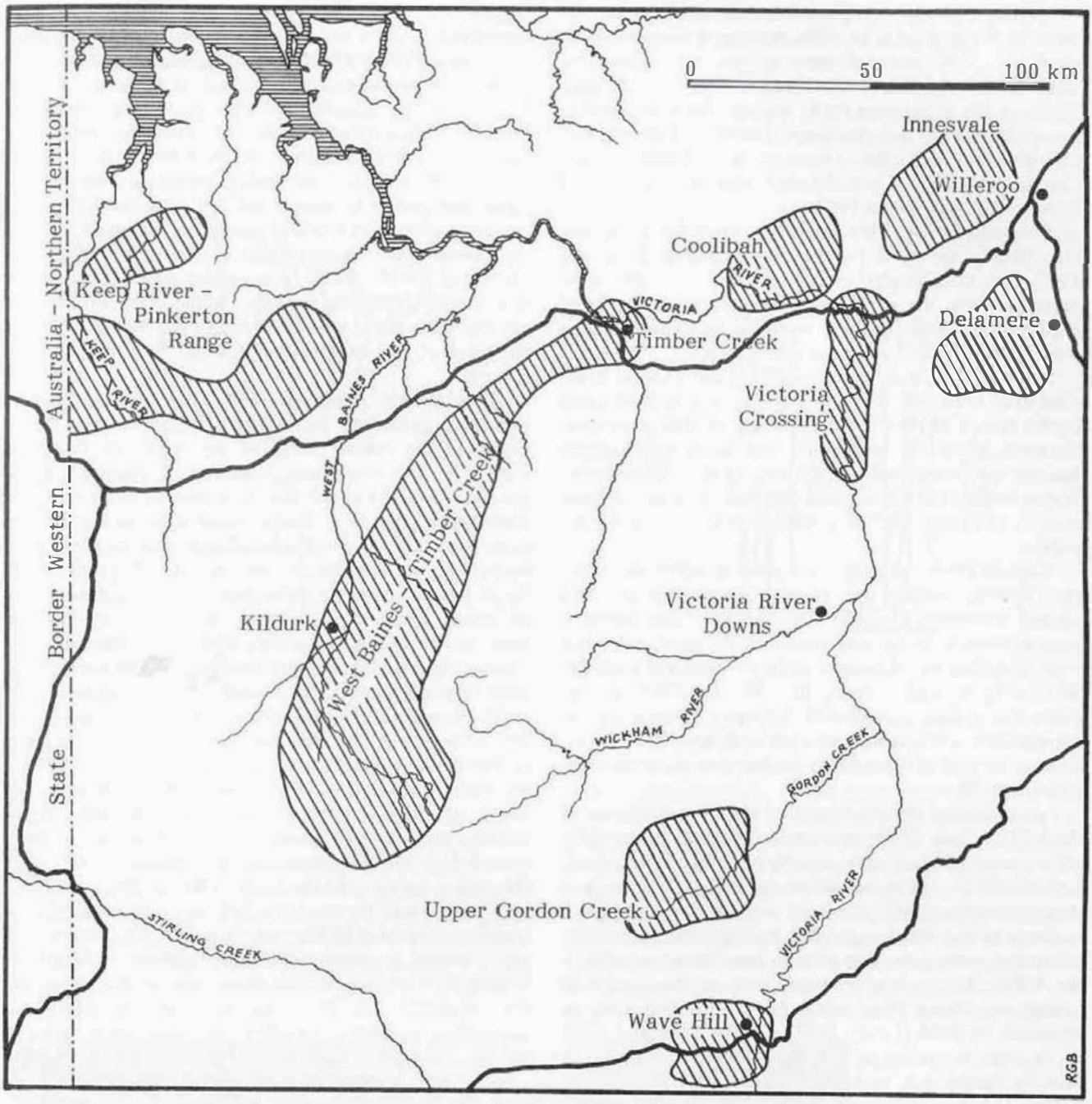

Figure 2. Map of the Victoria River District, Australia.

recorded a number of sites, including several in the Keep River area in the extreme north-west VRD, plus further sites in the Delamere-Willeroo area. Prior to 1984, however, no comprehensive overall survey of the entire VRD had been attempted, and no more than about fifty sites had been recorded throughout the region.

A project to survey the VRD as a whole was begun in 1984 by the author. This was carried out during 1984 and 1985 on contract to the Northern Territory Museum of Arts and Sciences, and continued during 1987, 1988 and 1989 by means of grants from the Australian Institute of Aboriginal Studies. The purpose of this ongoing survey, which has so far involved 20 months of intensive field work, has been to obtain a broad overview of this extensive rock art region, at a fundamental level. Detailed archaeological and ethnographic interpretation of specific sites has not been attempted.

Distribution of sites

The survey is far from complete but at the present stage, an overall picture of the distribution of sites and broad variation of rock art styles (throughout this paper, the word 'style' refers to the way I perceive style) of this vast region can be formed. The distribution of sites is by no means uniform, it relates closely to both topography and geology. In general, sites have been found to be concentrated within areas possessing favourable sandstone types and topographic structure (e.g. residual outcrops, lines of escarpment cliffs or outcrops overlooking deep creek beds, where occupational shelters are most likely to be located).

The main areas of the VRD in which either superficial or in-depth surveys have been carried out are listed below, together with the various researchers who have contributed to the recording of these areas (see Figure 2).

Keep River area. This region contains sandstone formations similar to those of the Bungle Bungle forma-

tions in the Kimberley, which are not found elsewhere in the VRD. Chaloupka recorded several sites in the far north of the area prior to 1984. Surveys by the author in 1984 and 1985 covered most of the area reasonably comprehensively. The southern sector, which incorporates the Keep River National Park, has also been recorded in some detail by the Northern Territory Conservation Commission, while Ben Gunn, as a consultant to the Commission, carried out detailed recording at selected sites during 1988 (Gunn 1988).

Pinkerton Range. This extensive region has been only superficially surveyed by the author during 1984 and 1985, due to difficulty of access. Some 15 sites were recorded along the escarpment face and on Bullo River Station. Additional sites will no doubt be located when a more comprehensive survey is undertaken.

Coolibah Station is an area north of the Victoria River which has been only partially surveyed during brief visits by the author in 1984 and 1989. Only 11 sites have been recorded, but a few outstanding individual sites suggest that the area has potential for future surveys. Grahame L. Walsh has carried out detailed recording at some of these sites in 1985 and 1989 as a follow-up of surveys by the author.

Victoria River Crossing is the point at which the Victoria Highway crosses the Victoria River, and the area centres within the Gregory National Park. The surveyed area includes a 50 km-long stretch of the river. Two sites were recorded by Chaloupka prior to 1984 and a further 40 sites by the author during the 1984 and 1985 surveys. Gunn has carried out detailed recording of rock art and ethnographic work in consultation with Aboriginal custodians at several of these sites, and has also recorded some additional sites.

Innesvale and Willeroo Stations in the far north-east of the VRD include four main concentrations of art sites plus other, more isolated sites, mostly on soft residual sandstone outcrops. They include some of the most outstanding examples of VRD rock art. One of these groups of outcrops is that which surrounds the Ingaladdi Waterhole, this being among the first sites to have been recorded in the VRD. The author recorded sites in the remaining complexes during 1984, while Lewis recorded sites on Innesvale in 1986 (Lewis 1987). During 1988 and 1989, an in-depth recording project has been carried out in this area by Earthwatch, under the direction of Dr Josephine Flood. This ongoing project includes detailed art recording, archaeological excavation and ethnographic consultation of Aboriginal custodians. It has covered all four major rock art site complexes within the territory of the Wardaman people, and members of this community have participated in all aspects of Flood's project - the most detailed study as yet of rock art sites within the VRD.

Delamere Station, to the south of the above region, includes the well-known Lightning Brothers site, first recorded by Davidson in 1930 (Davidson 1935), and a further three major art sites a few kilometres to the west in the vicinity of Gregory Creek, these having been recorded by the author in 1985. This area is also within the territory of the Wardaman and it is proposed to extend Flood's Earthwatch project into Delamere.

Victoria River Downs and Wave Hill Stations are two of the three largest cattle stations in Australia and occupy most of the south-eastern sector of the VRD. Much of this is flat or undulating plateau with little likelihood of rock art being found, but it does include some concentrations of abundant sites. The Gordon Creek sites located by Gregory in 1856 are within this region. The first researcher to carry out detailed studies was Lewis, who recorded a number of art sites and collected ethnographic information associated with several of these as early as 1975. This area includes the territories of three Aboriginal tribes, the Ngarinman, Bilinara and Gurindji, and Lewis has liaised with custodians of all three over a long period. During 1985 and 1987, the author completed a survey of a significant part of Gordon Creek and its network of tributaries, which dissect a belt of sandstone comprising one of the most prolific concentrations of sites in the VRD. Ken Mulvaney (1988) has also carried out research, in consultation with Aboriginal custodians in the Wave Hill region. Several other sites were recorded in the Wave Hill area but most of that extensive region has been only poorly surveyed.

West Baines sandstone belt. This is a geological formation extending for nearly 200 km, from Timber Creek in the central north of the VRD, to Waterloo Station in the south-west. The belt is dissected by a network of creeks which flow in a general north-westerly direction into the West Baines River. Sites in the Timber Creek area and close to Kildurk homestead were recorded during the author's limited surveys in 1984 and 1985, while the survey of the remaining areas occupied nearly the entire 1988 and 1989 survey seasons. Over 300 sites have been recorded in this region which has been given a reasonably complete overall coverage. Walsh carried out more detailed recording at some 20 of these sites in 1989.

Those areas of the VRD not included in the above (e.g. the catchment of the Ord River in the south-west) have generally been subjected to an overall reconnaissance by the author and scattered sites have been recorded, but many of these regions comprise mainly rock types unlikely to yield rock art sites. The total number of sites recorded by various researchers is in excess of 600, and the total number existing in the VRD is likely to be at least twice this figure. However, it is expected that a significant majority of the major sites are now recorded so that a realistic overview of stylistic variation can be given. It should be emphasised, however, that in this paper the term 'recorded' does not necessarily refer to detailed scale recordings; it merely describes sites that (while always having been known to the Aboriginal people of the region) have received some archival and photographic coverage. Only a small proportion of the known VRD sites have actually been recorded in detail.

Major stylistic painting elements

Both rock paintings and petroglyphs are distributed throughout the VRD and a wide range of styles and techniques are present in both art forms. The more outstanding techniques, subjects and styles are listed below, together with their recorded distribution within the region.

Large snake figures

Snake figures include the largest painted motifs in the VRD and, it appears, in Australia (Walsh 1988). Representing the Rainbow Serpent, these motifs occur throughout the VRD, but they are uncommon in the far north-eastern or Wardaman tribal regions. They appear to become progressively more common toward the west and toward the south. The most frequently depicted style of this motif possesses a mammalian head with ears (Fig. 3a). However, the few examples recorded to the east of

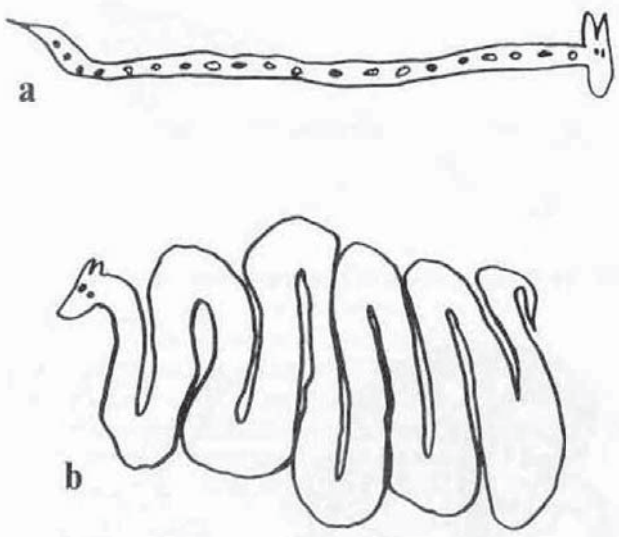

Figure 3. Rainbow Serpent motifs.

Coolibah Station do not appear to possess these ears, whereas the many examples to the west and south, with very few exceptions, do. The shape of the serpents take two common forms, a straight extended style and a meandering style resembling a coiled wave (Fig. 3b). The wave style is dominant in the Gordon Creek and Wave Hill regions, whereas the straight style is dominant in the West Baines area. In the rather poorly surveyed Pinkerton Range area, both forms are present and large examples of each may occur at a single site.

The largest recorded figure is an example of 24 m length in the Keep River region. With a body exceeding 1 m in width, this figure, though badly weathered in parts, appears to be the largest rock painting recorded in Australia to date. It is also the largest known in the world; the largest outside of Australia is in India, and is about 10 m long (R. G. Bednarik, pers. comm.). Two further, well preserved examples from the West Baines measure 12.3 m and 11.5 m respectively. Figures in excess of 4 m are quite common in the VRD, but examples of serpents of under 1 m also occur (Fig. 4).

Large anthropomorphs

The best-known VRD sites, at Delamere and Ingaladdi, each feature a pair of large upright anthropomorphs which Wardaman consultants describe as the 'Lightning Brothers'. At least seven further sites have been recorded in the Wardaman tribal region and neighbouring Nungali to the west which also feature pairs of upright anthropomorphs (Fig. 5). The figures in each pair are of similar size and body design, the majority exceed 2 m, and one of the Delamere figures is over 4 m. Most, but not all, feature a colour style of parallel stripes resembling that of the Ingaladdi figures.

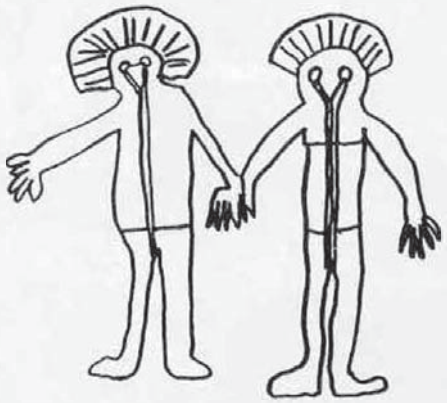

Figure 5. Lightning Brothers motif.

The north-east region of the VRD, comprised largely of the Wardaman and Nungali tribal areas is definitely the stronghold of the 'Lightning Brothers' myths and associated painting traditions. To the south-west of this region, a large area of the VRD is occupied by the Ngarinman tribe. At least four recorded sites within this area also feature pairs of almost identical upright anthropomorphs up to 2 m tall. One site lies on the western edge of the Gordon Creek drainage and the remaining three in the southern part of the West Baines. The figures at each of the four sites are quite different in style and colour design to those in Wardaman country and are different to each other, but all are in keeping with the localised styles of their particu-

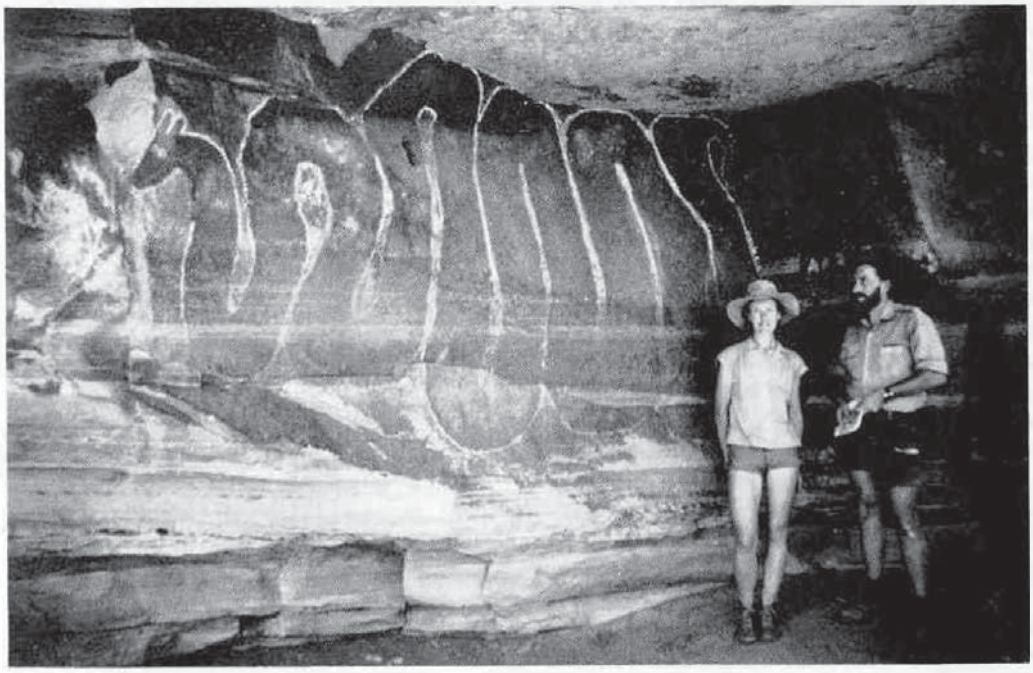

Figure 4. *Grahame Walsh and assistant Helen Clark beside a large Rainbow Serpent painting on Gordon Creek, VRD.*

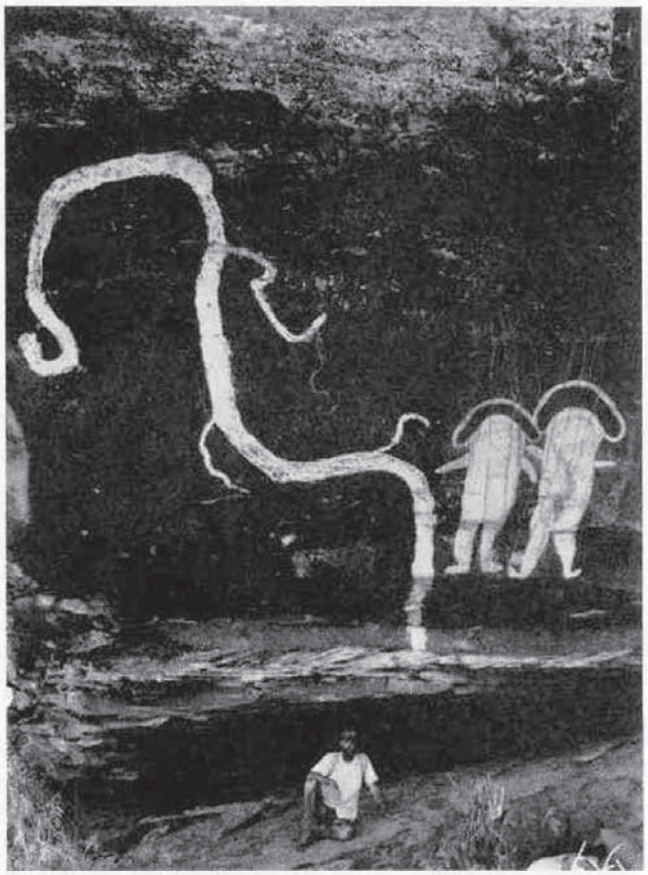

Figure 6. Lightning Brothers, Gordon Creek. The apparent bolt of lightning extends for 8 m on the sloping ceiling.

lar areas. There is no direct information from custodians that the Ngarinman examples also represent the 'Lightning Brothers' but the associated stories are well known in the area. At the site in the Gordon Creek network, a long meandering white motif extending for 8 m, next to two anthropomorphs, resembling a bolt of lightning (Fig. 6).

Within the Wardaman and Nungali territory, sites featuring a pair of anthropomorphs are the most conspicuous, but sites with single large anthropomorphs with striped internal design are also common. In this part of the VRD, sites of large anthropomorphs are considerably more common than those of large snakes, which are notably rare in Wardaman country.

Although paintings of Rainbow Serpents are common in the remaining regions of the VRD, large anthropomorphous figures also dominate many sites. In the northwestern areas which include the Pinkerton Range and Keep River, a widely distributed style is that of a long, extended and often snake-like or contorted anthropomorph with very small limbs in proportion to body length. Such figures are up to 7 m long and usually occur on low ceilings. In most instances, a bag-like extension has been added to one finger (Fig. 7). The distribution of this motif is known to extend westward into the Kimberley (W. Dix, pers. comm.), but apparently not into the West Baines region to the south.

The most widespread large anthropomorphs in the West Baines region are squat female figures with widespread legs and raised arms (Fig. 8). In most instances the spread of the legs (over 2 m in some examples), exceeds the body height. Several of the most recent and best preserved of these figures also feature up to four lines or

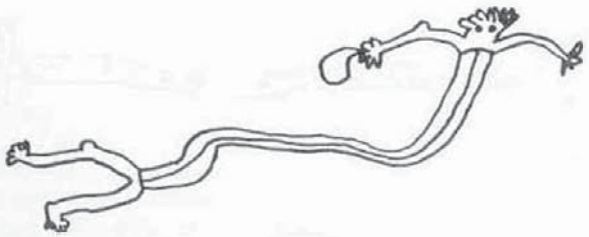

Figure 7. Long, snake-like anthropomorph motif.

Figure 8. Squat females with string arcs motif.

arcs joining the hands above the head. While the squat female motif extends beyond the West Baines to other VRD regions, the 'string-arc' version has so far only been recorded in the West Baines.

Another type of anthropomorph found in the West Baines, but with a far more restricted range, is that of a male, usually with two or three 'horns' as head appendages, and an extension to one finger, shaped like a stone axe (Fig. 9). These 'stone axe men' are confined, at least in this particular style, to some ten sites in the northeastern area of the West Baines (although several of the 'Lightning Brothers' figures in the Wardaman country also hold stone axes). More than half the West Baines sites possess two or more of these figures, but the figures themselves appear to be painted as individuals rather than in pairs or groups. Most of these figures are 1-3 m in height, the body and limbs are not generally distorted and in all but two instances, the 'stone axe' figures are painted on flat ceilings rather than vertical walls of shelters. Even in these two sites, the figures were placed in horizontal rather than upright positions. The figures are mostly painted in red with white outline but feature other colours such as yellow or black for internal central lines or chest and waist bars (cf. Huchet 1990), and these figures are among the most visually impressive of VRD motif types.

Figure 9. Anthropomorph with 'horns' and 'stone axe' appendage.

Some of the most magnificent VRD sites are those featuring rows of multiple anthropomorphous figures in identical or similar style. Such sites occur scattered throughout the VRD. Anthropomorphs range from simple white stick figures less than 50 cm high to large polychrome figures up to 2.5 m high, with elaborate head decoration and internal design. Such compositions may depict ceremony scenes and some of these sites in the eastern VRD are important to Aboriginal custodians. A photograph of one well-known site with some 80 medium-sized figures described as 'young lightnings', in the far north-east of the VRD, has been published (cover of *RAR* 4[1]), but some similar sites in the southern VRD are highly sacred, and thus of restricted access. Four southern sites feature from 6 to 30 anthropomorphs with heights ranging from 1.5-2.5 m, but sites featuring groups of more than two very large anthropomorphs have yet to be recorded in the north.

Contorted figures

Painted anthropomorphs with bodies in twisted or distorted shapes are common throughout most of the VRD, as they are in many of Australia's northern regions, and these figures cover a wide range of styles and sizes (Fig. 10). 'Contorted' figures usually possess twisted or 'agitated' bodies, with limbs usually bent or doubled back, while head, hands and feet are often grossly enlarged. The figures often appear to be flying or leaping, similar to the 'Woodstock' figures of the Pilbara, the *Namandi* and *Namarakain* figures of Arnhem Land, and some of the *Quinkans* of the Laura region.

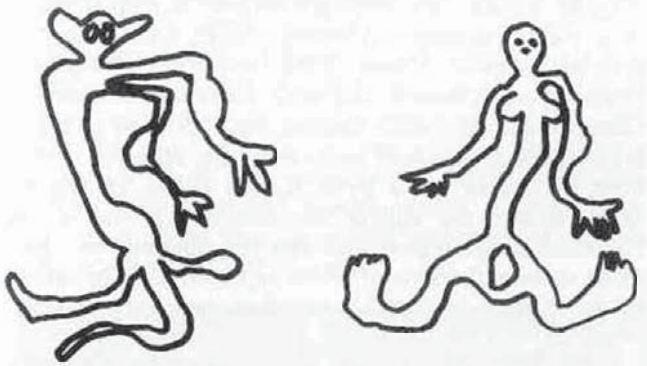

Figure 10. *'Contorted' anthropomorph motifs.*

One common interpretation given by Aboriginal consultants is that they represent 'sorcery' paintings or evil magic, serving the same purpose as 'pointing the bone' or 'singing' a person. This interpretation has been given to me by at least one custodian, and in a paper on the sorcery art of the Gurindji people (Wave Hill) Ken Mulvaney (1989) describes a particular site in that area as a sorcery site and this particular type of art as *Yarra Yarnarg*. The term, however, applied not only to contorted figures but also to anthropomorphs of other styles at the site and even to paintings of catfish.

Many of the contorted paintings may relate to sorcery, but those engaged in sexual union may well represent 'love magic'. The styles of this type of figure are too numerous to detail in this brief paper, but one deserves special mention and is described as follows.

Diwalliba figures

This type of contorted figure is characterised by a series of lumps or enlargements along the body, and can be either male or female. Sometimes the hands and feet are grossly exaggerated in size, occasionally male and female examples are joined sexually. The figures are commonly 1-1.5 m, ranging to 3 m. Most are monochrome, usually white, but yellow and orange ones occur, and a few bichrome examples are red with white outline.

The distribution of these figures appears to be mainly confined to the southern VRD, where it extends at least for 200 km from the West Baines to the Gordon Creek and Wave Hill region. In a variation found along a 14 km section of one West Baines tributary, from one to four small figures of dingoes seem attached to various parts of the body, as if biting or attacking the anthropomorph (Fig. 11). This distinctive variation is most prolific within its small area of distribution, occurring in at least 18 sites. While it has not been recorded elsewhere in the VRD, Walsh (1988) depicts the painting of a *Namandi* Spirit from western Arnhem Land, 700 km to the north-east, with body lumps and attached 'macropod'.

Figure 11. *Diwalliba figures.*

In 1984 Ginger Packsaddle, senior custodian of the Amanbidgee (Kildurk) community in the heart of Ngarinman country, took the author to a painted rock-shelter in the vicinity of Kildurk homestead. Most of the paintings, including an impressive panel of bichrome figures, were of the figures with lumps on the limbs and joints. Mr Packsaddle called the shelter and the short valley in which it is situated *'Diwalliba'*, stating that he had often camped there with his family as a young child. He could not give a definite interpretation of the contorted figures but suggested that they could perhaps be 'lightning figures', and he was certain that all the paintings had been there as long as he could remember, and had not been added to in his time. My subsequent surveys showed that these contorted anthropomorphs are widespread, but nowhere as numerous as at Diwalliba, where they were first recorded. During a more detailed follow-up survey in 1989, Walsh recorded a total of 52 partly or wholly preserved figures at this site. I propose that the name 'Diwalliba Figure' be adopted to describe this distinctive type of anthropomorph, which features lump-like growths along the body, limbs or joints.

In 1985, a senior Aboriginal custodian at Gordon Creek described a panel of these figures as 'sorcery' paintings to the author. However, at another site on the same creek bed, Lewis and Rose (1987) describe a similar figure as a 'Lightning Being', similar to the suggestions of Ginger Packsaddle. Perhaps most of the Diwalliba Figures are related to sorcery, but other interpretations may also apply for many of them, especially at the sites featuring large numbers.

Fauna

Paintings of birds, mammals, reptiles and fish are common throughout the VRD. These figures are often of considerable size, as those of Rainbow Serpents and anthropomorphs. The largest examples of the most commonly depicted creatures are salt-water crocodiles of 7 m, freshwater crocodiles of 5 m, saw-sharks of 5 m, macropods of 3.6 m and emus of 2.4 m. Contact art figures also reach considerable size, one painting of a bull reaching 4.5 m. Large macropods are often depicted having been speared, with several excellent examples in the West Baines. Salt-water crocodiles are generally found in the northern regions close to salt-water sources, and fish tend to be more common close to the major waterways, but otherwise there do not appear to be distinct concentrations of any particular species in any one region, with one possible exception. In those VRD regions in which the mammal-headed Rainbow Serpents are depicted it has been assumed that paintings of smaller snakes with naturalistic heads represent ordinary snakes, and this has been confirmed by Aboriginal consultants. Along one particular tributary of Gordon Creek, most sites are dominated by panels of from 2 to 5 snakes, all painted in the same shape, colour, style, and parallel to each other.

Contact art

Examples of contact art are found throughout most of the VRD, with a tendency to be more common in the northern regions. (In Australian rock art studies, the term contact art refers to rock art of a motif content which demonstrates that it postdates first contact with Europeans or Asians, e.g. Macassans.) A distinctive feature of VRD contact art is that it occurs not only as paintings, but also as petroglyphs. At the Ingaladdi Waterhole, the figure of a ship has been deeply abraded into relatively soft sandstone while in the south-west area of the West Baines, scratched line figures depict mounted horses and a helicopter.

Mounted horsemen are depicted in paintings, sometimes holding rifles, particularly in the northern regions. Sail-boats occur at Innesvale and Pinkerton Range sites in the north. In the central part of the West Baines, some 100 km from the coast, three sites feature a painted boat, one of which is 2.4 m long and painted in washing blue. The West Baines also possesses some of the largest and most elaborately decorated contact figures. There are several bulls or cows of over 2 m length, painted in white outline and red interior with white mottling. The largest of these measures 4.5 m. At least two figures of camels are painted in the Keep River region and an excellent bichrome example of over 2 m is located at the same site as the 24 m-long snake. Also at Keep River are stencils of what appear to be rectangular objects of European origin. Other objects are easy to identify, such as pistols, rifles and European policemen (Fig. 12).

***Figure 12.** Contact art, depicting Europeans, rifles and pistols. Monochrome over earlier paintings.*

Early paintings

Most of the sites recorded in the VRD prior to 1984 were on the relatively soft sandstone of the Willeroo, Innesvale and Delamere regions - a rock which provided suitable surfaces for deep petroglyphs but upon which even recent paintings, including contact art, are seen to deteriorate rapidly (Clarke 1978). During the 1984 season, Lewis recorded several sites in the far north on Innesvale, Coolibah and Bradshaw Stations which possess paintings in red pigment on a hard sandstone, quite different in style from the recent VRD styles (Lewis 1985). During the same season, the author also recorded a site in the Pinkerton Range region with dark red paintings on a hard white surface, the style of which appeared to more closely resemble early Arnhem Land art than the nearby styles of recent art.

The 1988 and 1989 field seasons showed that a considerable number of sites (at least 35) with early paintings occur in the West Baines. There is little doubt that the art is of an ancient origin although none of the styles can be considered to be identical with either Kimberley or Arnhem Land early paintings. Two distinct pigments are present, dark-red and black. Red is more common but the most widespread styles of this phase of painting include examples in both pigments, suggesting that the two were contemporary. This is confirmed by superimpositions of red upon black as well as black upon red, but bichrome figures have not been recorded. At five of these sites there are instances of figures painted in the black pigment which have undergone a colour change to light-green, either partially or fully.

The recorded sites include a range of styles and some of these are repeated throughout the area of distribution. One common motif is that of an anthropomorph with small head and limbs but a long or enlarged body with two parallel lines along each side of the body. The motif is usually less than 1 m long, but one specimen measures 3 m. It bears some resemblance to the Arnhem Land Yam figures. Another very common motif is that of a macropod

with very small head and limbs, a fairly naturalistic tail and a fat or large body (Fig. 13). This 'fat-bodied macropod' motif is found throughout the West Baines early paintings range, and its size ranges from less than 50 cm to over 2 m. A similarly shaped macropod is also depicted in several petroglyph sites. The shape resembles that of a possum more than that of a kangaroo, which poses the question whether the figure represents a stylisation, or depicts a now extinct species. Thus, in the VRD, we have a counterpart to the Pilbara puzzle of the 'fat-tailed macropod' common in early outline petroglyphs (Brown 1984; McNickle 1984).

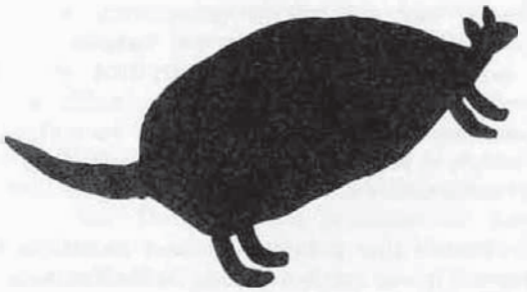

Figure 13. 'Fat-bodied macropod' motif.

Although quite a few of the early paintings exceed 1 m length, and one design resembling a plant reaches 7 m, the majority are relatively small. The echidna is commonly depicted, always in profile, while in recent art it is invariably shown in plan view. Three examples have been recorded of snake figures with small forelimbs close to the head, and two of these appear to have ears on the head, providing another possible parallel with the Yam figure tradition of Arnhem Land in which small, mammal-headed serpents with forelimbs are found. The West Baines examples also show the spiked tails of the Yam snakes, and grass prints are present at some sites. Lewis and Walsh are both of the opinion (pers. comm. 1989) that the West Baines early art, though ancient, is probably of more recent origin than the earliest preserved art of both the Kimberley and Arnhem Land.

Petroglyphs

Although paintings are the more conspicuous form of rock art in the VRD, the surveys have shown the region to be among the most prolific in Australia for petroglyphs. Indeed, the wide variety of different petroglyph techniques found probably rates the VRD as the most diverse of all Australian petroglyph regions.

Abraded grooves

These are often found on soft sandstone outcrops such as those of Innesvale, Willeroo and Delamere, and were thus recorded by the early VRD researchers. Sites such as those at Ingaladdi Waterhole often possess large numbers of deep grooves on the lower part of rear shelter walls, below large paintings, or on detached boulders beneath or close to rockshelters. According to Aboriginal consultants, these numerous short, upright grooves relate to rainmaking ceremonies. At some sites, extensive areas are almost completely covered with such petroglyphs. The great majority are straight, short grooves, but occasionally they are arranged to form simple figures such as concentric circles (Fig. 14), bird tracks and even stick-figure anthropomorphs. Deep impressions of human footprints and macropod tracks are common, some being larger than natural size. More complex figures made by the abrading method are rare, the ship at Ingaladdi being an exception.

Elsewhere in the VRD, where the sandstone is harder, abraded grooves are significantly less common, but they occur in great numbers at two sites of soft residual outcrops in the Keep River region. In the central VRD regions such as Victoria Crossing, Coolibah and West Baines, abraded grooves, where present, are much smaller and fewer in number, probably due to the harder rock surface. At two West Baines sites, however, detached boulders of over 1 m across are totally covered with long deep grooves up to 5 cm deep.

Pecked petroglyphs

Percussion is the most widely represented petroglyph technique in the VRD. It is possibly also the one most neglected during the surveys, because pecked petroglyphs

Figure 14. Petroglyphs at Jalijbang West.

are often located at sites well away from the obvious, favoured occupation shelters. Consequently a large number of such sites will have been missed by researchers, due to the selective nature of most survey projects. The recorded distribution of pecked petroglyph sites is considerably wider than that of abraded grooves, probably because the percussion technique is more effective than abrasion on harder sandstone, but this pattern of distribution is by no means uniform throughout the VRD.

Panels of pecked petroglyphs are often found at the same sites in which abraded grooves are also located. However, whereas the abradings are normally located on the lower rear walls of shelters, often with paintings superimposed onto them, peckings are much more likely to be found on detached slabs of rock, either below the shelter or outside of it, and sometimes at considerable distances away from the shelters or even on the summit of the shelter outcrops, if accessible. The peckings cover a wide motif range, both figurative and nonfigurative. Many panels contain only 'geometric' designs such as circles, ellipses, meandering lines etc., motif types that were once included in the so-called 'Panaramitee style'. Anthropomorphs and figures of birds, mammals and fish occur also at many sites. The larger figures tend to be executed in outline only, whereas smaller figures are often fully engraved. The largest pecked figure recorded by the author in the VRD is a 5 m outlined snake at Keep River which is located completely within the body of the 24 m-long snake mentioned above, the longest painted figure.

In the central northern regions of the Pinkerton Range, Coolibah and Victoria River Crossing, pecked petroglyphs are rare (although abraded lines and circular pits are occasionally present), and likewise in the Gordon Creek network, where only two sites with pecked figures have been found. In the West Baines, pecked petroglyphs are more common, occurring at over 10% of the recorded art sites. Here, the petroglyphs appear frequently on the upper surface of boulders on the floor of shelters, sometimes even on the shelter floors where these consist of bedrock. At some sites, totally unpatinated figures are found on the same panels as patinated ones, or even superimposed over them. Occasionally pecked figures are located at such low levels that they are regularly inundated by flood waters.

At least four of the early painting sites of the West Baines possess pecked petroglyphs on adjacent boulders. No petroglyphs have so far been recorded on any early painting sites in either the Kimberley or Arnhem Land, perhaps due to the hardness of the rock required to ensure preservation of old paintings.

Of the VRD regions to have been surveyed in any detail, those with the greatest distribution of pecked petroglyphs are the West Baines and Keep River regions, but the individual sites with the largest number of such petroglyphs are two sites in the southern VRD. At remote Kirkimbie Station, in the far south-west, several hundred pecked figures have been placed along a 100 m section of a sheer wall of a gorge, many of them so high that it is almost impossible to reach them. This may be the most prolific petroglyph site yet recorded in the Northern Territory. At Camfield Station, further to the east, one site features many detached blocks scattered over a wide area, bearing small, mainly nonfigurative motifs.

Scratched line engravings

Small numbers of figures that had been executed rather crudely, as if by drawing with a sharp stone, were first located in the Gordon Creek and Wave Hill areas in 1985. At two sites these appear to include contact figures of mounted horses. One of these is at the site recorded by Gregory in 1856. Scratched engravings were subsequently recorded at a large number of sites in the West Baines in 1988 and 1989, especially in the area to the east and north-east of Kildurk homestead. Most of these figures are small and crude, but there are also examples exceeding 1 m in which attention was given to detail. Some only consist of outlines, in others the interior is filled with parallel or arcuate lines. Motif subjects include the full spectrum of the more recent painted art, including mammal-headed snakes. Contact art includes several horses, and at one site a helicopter is depicted - evidence of a very recent date for this technique. A few of the larger and more detailed scratched figures have pigment added, usually in the form of a thin drawn outline in charcoal, but occasionally with the addition of central lines or bars in red.

The scratched line technique is most prolific in the West Baines. It was rarely recorded in the far northern regions of the VRD, although on occasion very small paintings appeared to have been outlined crudely by scratched lines.

Circular pits

At least 16 sites have been recorded in the VRD which feature clusters of deep circular pits ground into rock surfaces. The distribution range of these pits extends well beyond the VRD and previously illustrated examples have been described as 'grinding hollows', but, unlike true grinding hollows which are usually quite random, the regular size and clustering of the pits suggests that they are a form of rock art rather than of utilitarian origin. Unlike the distribution of other VRD petroglyphs, such as the abraded grooves, that of the circular pits shows no correlation at all to rock type. Sites appear to be equally common in regions of hard and soft sandstones. Almost half of the sites (7) are located on the high escarpment cliff faces in the Victoria River Crossing, Coolibah and Timber Creek areas which feature hard sandstone and in which petroglyphs of other techniques are quite rare. Several sites featuring such pits have been recorded by the author on the hard granite outcrops in the Pilbara region of Western Australia (McNickle 1984, 1985). Similar pits have also been recorded in the Kimberley (AURA field trip 1988), Katherine and Arnhem Land regions.

Recorded locations of circular pits are usually within occupational shelters which normally possess other forms of rock art. The pits may be ground into the rear wall of the shelter, but they occur more frequently on detached boulders. The number of pits at a site ranges from a half dozen to well over 100, the size is usually fairly constant at any one site, and at most sites the pits are quite evenly spaced. Most pits are of 6-8 cm diameter and about 1 cm depth. The depth, however, may be considerably greater, and at one site of quite hard sandstone near Timber Creek, a depth of 7 cm was recorded. At three sites the dimensions appear to have been proportionally increased, the diameter being approximately 12 cm. These larger pits bear more resemblance to grinding hollows, but the regular spacing suggests that they probably represent larger versions of the more common, smaller pits.

A boulder at a Keep River site, over a metre high, is covered almost entirely by pits. The pits on the vertical

sides are all small and regularly spaced, but on the upper surface a number of considerably larger pits have been ground, and smaller pits subsequently superimposed over these. This is the only instance of superimposition recorded within this technique of rock art.

Composite painted-engraved figures

Rock art figures comprising both petroglyph and painting techniques are extremely rare throughout Australia, and in fact lacking in most rock art regions. In the VRD, however, a considerable number of sites include figures which incorporate both techniques. They are by no means abundant even here, but the occurrence of such composite figures is one of the most distinctive features of VRD rock art.

The Delamere Lightning Brothers, recorded in 1930 (Davidson 1935), are typical examples of composite figures. The tip of the penis of each of these figures features a deeply abraded groove which has been painted over in red. This has been described by Aboriginal consultants as a subincision and it is claimed that the Lightning Brothers introduced this practice among the Wardaman. Composite rock art seems to be concentrated in the north-east region where large numbers of abraded grooves are common at many sites. Rock artists have utilised deeply engraved motifs to emphasise parts of large, painted figures. In most instances only a small part of a painting has been engraved, and then painted over. Further sites on Delamere, Innesvale and Willeroo also incorporate engraved 'subincisions' of the penis, and other composite examples include figures with engraved eyes, a macropod with engraved feet, figures depicted as abraded lines and human footprints that were deeply abraded and than painted over.

Composite art is less common outside the north-east region of the VRD. It has been recorded at only one Keep River site, where a flat rock surface appears to have been ground smooth, concentric ellipses were abraded into the prepared surface, which was then covered in red ochre.

Composite figures of similar techniques occur at three West Baines sites. At one location in that region an engraved, squat, stick-like male figure has been outlined in white. At another site, 12 large upright figures each have eyes deeply engraved and painted in black.

Another category of composite figures, combining also an additive and a subtractive technique, are scratched line engravings which have been outlined or augmented in wet or dry pigment. They differ from the first category in that engraving is their main component, the pigment component usually being minor. In at least two sites of the Gordon Creek-Wave Hill region scratched line figures include paint application, but only a small proportion of the numerous scratched line figures in the West Baines are composite.

At an exposed cliff face site in the Gordon Creek area occur a macropod, a snake and 8 anthropomorphs which appear to have been initially engraved. These figures became faint as repatination took place, and some were then pounded to renew the contrast. Most of these figures have subsequently been provided with a red outline, and two of them at least with a white outline as well. Due to the exposed location the white outlines have almost disappeared, and the red ones have faded. This combination of technique has not been observed elsewhere in the VRD.

Stone structures

Stone arrangements of the type and magnitude of those found in south-west Queensland have not been observed in the VRD. However, stone structures of at least two different categories, both of which may well be unique to the VRD, have been recorded. One type is hawk hunting hides, or 'hawk traps' as they are known locally. These are roughly circular walls of stone, usually up to 1 m high and 2-3 m across, and were used within recent times. The hide would be covered with branches to conceal the hunter, and bait would be placed on top of these. When a bird of prey descended to take the bait it would be quickly caught by the hunter. The hides are normally located on exposed escarpment tops or flat plains. Lewis (1988) has recorded several such structures which are found in various parts of the VRD.

The second type of stone structure appears to have a much more restricted distribution. With one exception it is found only within the West Baines region where it has been recorded at 35 sites. These structures are normally located within rockshelters which possess art and evidence of human occupation, but also, at times, on exposed ledges which would be less than ideal for occupation. The usual form of the structure is a straight or curved wall, up to 15 m long but usually much shorter and rarely more than 1 m in height. Often a deep, low, cave-like recess is partly walled off, but at some sites such recesses are elevated and quite difficult of access. At other sites, short walls may join two or more large natural boulders to effectively wall off an area of the shelter. At a few sites, structures are simply circular piles of stones, resembling cairns rather than elongated walls. Most sites feature a single wall or similar structure, but at some sites up to four separate structures have been recorded. Some of the larger and more elaborate walls include large wooden branches.

Aboriginal consultants in the region have not been able to provide information about the purpose of these structures. The wide range of shapes, sizes and type of location suggest possibly a ceremonial rather than a utilitarian function. The only site of this class recorded outside the West Baines region is on a tributary of Gordon Creek, but between these two areas lies the as yet unsurveyed region of the upper Wickham River.

Summary

In less than a decade, the VRD has emerged from being a poorly known region to one which can now be recognised a one of Australia's major rock art provinces. The most significant overall feature of the rock art is the wide variety of both styles and techniques utilised by the Aboriginal artists.

Both paintings and petroglyphs are well represented throughout most of the surveyed areas and even though paintings may be regarded as the dominating form of art, they do not dominate to the extent they do in Arnhem Land or the Kimberley. It is likely that the widespread co-occurrence of both techniques has inspired artists to occasionally combine them to create composite figures, which comprise one of the most distinctive features of VRD rock art.

Another distinctive attribute is the very large size of many painted figures. The largest rock paintings in Australia are found in this region. More emphasis appears to have been placed on great physical size than on complexity of internal design such as is found in Arnhem

Land. However, many of the larger VRD paintings feature quite elaborate internal decoration.

For many years, the only examples of VRD rock art widely known to researchers were the Delamere and Ingaladdi examples of the Lightning Brothers. The recent surveys have shown that figures of this tradition are indeed common in the north-eastern VRD, being less common in other areas. However, an even more widespread subject are the large paintings of snakes which represent the Rainbow Serpent. Other outstanding and widespread style include the long, snake-like anthropomorphs possessing extension to a finger found in the north-west, and the contorted Diwalliba figures of the south.

This paper has highlighted only the most outstanding and widespread of the VRD rock art styles and motif types. There are numerous localised styles and individual examples of rock art which have not been discussed for lack of space. Further surveys by the author and other researchers are continuing in the region, detailing smaller areas or analysing particular styles and techniques. The VRD has had to wait until this late stage before systematic surveys of its vast corpus of rock art commenced, but this work is now progressing at full pace.

Acknowledgments

During the VRD survey, begun in 1984, I have been given invaluable assistance by numerous individuals and organisations. In particular, the assistance and co-operation of station owners and managers, and that of the various Aboriginal communities, has made this survey possible. It is not practicable to list all individuals, but below are acknowledged those who have provided particularly valuable assistance over the project's duration.

The Northern Territory Museum of Arts and Sciences provided funding and a vehicle during 1984 and 1985. The Australian Institute of Aboriginal and Torres Strait Islander Studies provided funding by way of grants during 1989 and 1990. The Conservation Commission of the Northern Territory provided assistance to reach sites in 1984 during the survey of National Park areas in the VRD.

I thank all those residing on Kildurk Station, including the Buckley family and the Amanbidgie community, for providing assistance to reach sites in the important West Baines region; particular thanks to Ginger Packsaddle, Ngarinman elder, who gave permission to survey this region and accompanied me to several sites. The Dagaragu community at Wave Hill and elder Hoppy Mick assisted in the region of the Gurindji people, and the Victoria River Downs Station management and members of the Yarralin, Humbert River and Pigeon Hole communities on that station. Particular thanks are due to Pigeon Hole elders Hector and Anzac, the custodians of the Gordon Creek area. I thank Milton Jones and staff at Coolibah Station; the Fogarty family, operators of the Timber Creek store and campground; and the management and staff of Innesvale, Delamere, Limbunya and Auvergne Stations.

Grahame L. Walsh joined me on several occasions in order to carry out detailed follow-up recording of selected sites, and his unique means of transport made it possible to locate and record a number of additional sites. Finally, thanks are due to Darrell Lewis and Ken Mulvaney, for constructive criticism of an earlier draft of this paper, and to Lewis also for providing locations of sites along the lower Gordon Creek.

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Résumé. *Le corpus principal d'art pariétal dans la région de la rivière du Victoria dans le Territoire du Nord, Australie, a reçu peu de considérations avant les années 80. Le travail exploratoire approfondi de l'auteur et d'autres chercheurs a montré que la région possède l'une des plus grandes et diverses concentrations d'art pariétal de l'Australie. Plus de 600 sites ont déjà été enregistrés, représentant beaucoup de styles de peinture et de gravure sur pierre. Ce qui est particulièrement remarquable est la présence des peintures les plus grandes connues dans le monde, d'images composites peintes/gravées, et de récentes gravures historiques. Dans ce rapport sur l'état actuel de la recherche, l'auteur présente une vue d'ensemble sur la distribution, les genres de styles, les techniques et les différentes catégories de figures, et il examine aussi les édifices uniques de pierre dans la région.*

Zusammenfassung. *Das bedeutende Felskunst-Vorkommen des Victoria River Gebietes in Northern Territory, Australien, hat bis in die 1980er Jahre wenig Beachtung erhalten. Ausführliche Erschliessung durch den Verfasser und andere Forscher zeigte, dass dieses Gebiet eine der grössten und mannigfaltigsten Konzentrationen von Felskunst in Australien besitzt. Über 600 Fundorte wurden bereits registriert, die viele Stile von Petroglyphen und Malereien einschliessen. Besonders erwähnenswert sind: das Vorkommen der grössten Felsmalereien der Welt; die zugleich gemalten und gravierten Figuren; und die rezenten Petroglyphen aus der Periode europäischen Kontaktes. In diesem Progress-Bericht vermittelt der Verfasser einen Überblick der Verbreitung, stilistischen Formen, Herstellungsmethoden und Motiftypen, und erwähnt die ungewöhnlichen Steinbauten der Gegend.*

Resumen. *El conjunto mayor de arte rupestre del distrito del Río Victoria en el Northern Territory de Australia, ha recibido poca atención hasta la década de 1980. Extensivo trabajo de prospección por el autor ha demostrado que la región posee una de las mayores y más diversas concentraciones de arte rupestre en Australia. Más de 600 sitios, consistiendo en muchos estilos de pinturas y petroglifos, se registraron hasta la fecha. En este informe sobre el desarrollo del trabajo realizado, el autor proporciona un panorama general de la distribución, formas estilísticas, técnicas y varios tipos de motivos, y considera también las singulares estructuras pétreas de la región.*

REFERENCES

CLARKE, J. 1978. Endangered site - a famous example. In C. Pearson (ed.), *Rock art deterioration and conservation in the 'Top End' of the Northern Territory*. ICCM, Sydney.

DAVIDSON, D. S. 1935. Archaeological problems in northern Australia. *Journal of the Royal Anthropological Institute* 65.

LEWIS, D. J. 1985. Mimi on Bradshaw. *Australian Aboriginal Studies* 2: 58.

LEWIS, D. J. 1987. Engraved human figures and faces from Wardaman country, eastern Victoria River District, Northern Territory. *Australian Aboriginal Studies* No. 1: 67-79.

LEWIS, D. J. 1988. Hawk hunting hides in the Victoria River District. *Australian Aboriginal Studies*.

LEWIS, D. J. and D. B. ROSE. 1988. *The shape of the Dreaming. The cultural significance of Victoria River rock art*. Australian Institute of Aboriginal Studies, Canberra.

McNICKLE, H. P. 1984. Variation in style and distribution of rock engravings in the Pilbara region (Western Australia). *Rock Art Research* 1: 5-24.

McNICKLE, H. P. 1985. An introduction to the Spear Hill rock art complex, northwestern Australia. *Rock Art Research* 2: 48-64.

MOUNTFORD, C. P. and E. BRANDL. 1968. Aboriginal cave paintings and rock markings at Ingaladdi rock shelter, Willeroo, Northern Territory of Australia. *Records of the South Australian Museum* 15(3): 371-82.

MULVANEY, D. J. 1969. *The prehistory of Australia*. Thames and Hudson, London.

MULVANEY, K. 1988. Who paints for a killing? Sorcery art of the Girindji. Paper presented in Symposium D of the First AURA Congress, Darwin (in press).

TREZISE, P. J. 1971. *Rock art of south-east Cape York*. Australian Institute of Aboriginal Studies, Canberra.

WALSH, G. L. 1988. *Australia's greatest rock art*. E. J. Brill/Robert Brown and Associates, Bathurst.

WELCH, D. 1990. The bichrome art period in the Kimberley, Australia. *Rock Art Research* 7: 110-24.

RAR DEBATES

Rock Art Research 1988, Vol. 5, No. 2, pp. 91-107.

DELIBERATE ENGRAVINGS ON BONE ARTEFACTS OF HOMO ERECTUS

DIETRICH MANIA and URSULA MANIA

FURTHER COMMENT

A reply to Davidson on Mania and Mania

By ALEXANDER MARSHACK

I have not till now commented on the Bilzingsleben problem because the comments that have been published have begun an adequate discussion and evaluation. However, Davidson's two responses to the Manias (*RAR* 5: 100-1; 7: 52-6) pose a number of problems because they begin with a strongly stated a priori position and then, particularly in the second response, report on an attempt to validate that position by what I regard as a cursory examination of the problems and materials.

The a priori position is clear: 'I am sceptical of all claims for early appearance of particular abilities in the evolutionary emergence of our species'. That presumption has skewed Davidson's inquiry and encumbered the logic of his argument since, being an article of faith, Davidson cannot escape its influence. 1) For some two decades I have undertaken a similar investigation of the early materials, not to prove a hypothesis but to *test* possible hypotheses and to examine the problem posed by the possibility of pre-*Homo sapiens sapiens* forms of early symboling and problem-solving, and to do so by a careful study of the scattered and rare relevant materials. 2) It may be of value to indicate the differences in the nature of the investigative effort and the nature of the data obtained by the two methods.

I begin with the artefacts. I was the first researcher, more than a decade ago, to examine the marked elephant tibia from Bilzingsleben (Mania and Mania 1988: 98, Fig. 3). Using a microscope I found that the set of seven marks appearing at an angle at right in the Manias' Fig. 3 were *not* the straight lines that they have illustrated. The lines were clearly invaded by fractures and many had the inherently meandering form of a fracture for their entire length, including the irregular edging and the lack of a floor or bottom common to a fracture. I could not be sure that all of these lines were, in fact, engraved. The other strokes on the tibia were clearly tool made but posed a different type of problem from the one that either the Manias or Davidson are concerned with. I wrote a report for D. Mania suggesting that before publication he should have comparative microphotographs made of the lines at right and rethink the possibility of its being intentional marking. I assume that Mania has not presented the microphotographs because of a lack of technical equipment. His close-up photographs do, however, indicate the fractures. I was pleased to read that Bednarik (1988: 96) has also noted these fractures and that he thinks them to be later than the engraving. On the basis of the questions raised I would suggest a careful microscopic study is required of all the marked and worked Bilzingsleben materials with a comparison to similar production from other early sites. This process would be different from Davidson's eye-ball study, argument by presumption, and lack of documentation.

Davidson reports, for instance, that I had told him that the parallel lines on the elephant tibia 'could have been made on greasy bone using the bone as a support while cutting skins. It [AM's suggestion] seems plausible, but there is still a need to know why hominids might have been cutting skins in parallel ways.' Again Davidson is in error, on reportorial and presumptive grounds. I shall discuss 'why' skins may have been cut as well as why intentional marking may often involve more than meets the naked eye, below. When Davidson and I met I made no mention of a 'greasy bone' in referring to the Bilzingsleben marking but had *instead* presented Davidson with the microscopic evidence for the engraving of exceedingly long parallel lines on a relatively flat shoulder blade from the French Mousterian site of La Quina, one of the classical type sites of that period (Martin 1907-1910: 208, Fig. 8). At first glance the lines seem similar to the lines on Mania's Artefact 3, Fig. 1. However, microscopic analysis of the La Quina shoulder blade revealed that these absolutely straight lines did not vary in depth or cross-section from their start to their end (Fig. 1 a, b). This evidence differed dramatically from the cutting and work marks I found on other bones from La Quina and from the incised work marks I found in other Neanderthal sites. The ordinary work mark made on a cutting platform often begins with a point of impact and is followed by a rapid tailing out, while the accumulation is often dramatically 'unrhythmic', i.e., the marks are irregular in cross-section, scattered and overcrossing. This type of marking is well-known and addresses Davidson's question concerning the term 'rhythmic' (1988: 101).

I had suggested to Davidson that the only way that such absolutely straight lines of invariant depth could have been incised and maintained was probably by cutting through a hide or skin. I had also suggested that the *set* of lines may have been made on the shoulder blade when the skin that was being cut was moved slightly at the end of each stroke, but with the cutting continuing linearly on the hide itself. This kind of cutting can be done on flat ground, but it is not easily done on a hard stone surface or on the impacted, irregular surface of a rock shelter. The La Quina strokes, I told Davidson, were therefore *not* symbolic, i.e. they had no symbolic 'meaning' of their own, but, I suggested, they may have been part of a different type of symbolic process. I shall discuss this crucial differentiation below because my suggestion changes the nature of the question one can ask concerning symbolic production. As a result of the analysis of the La Quina shoulder blade, I suggest that a comparable microscopic analysis be conducted of Mania's Artefact 3, Fig. 1. The examination would include the point of impact, the nature of the stroke and the tailing out. If this examination should reveal data comparable to that on the La Quina shoulder blade, the questions that will need to be addressed will be of a different order from those posed by Davidson, the Manias or the other discussants.

I have for long argued that careful study of an artefact or image almost always reveals unexpected data. For instance, my microscopic study of the fossil nummulite from the early Mousterian site of Tata (c. 100 000 BP) with a cross on both faces indicated that one of the lines was a fracture that extended through the fossil. When a powerful light was directed through the partially translucent fossil it lit up only one half, admitting light to the fracture, with the half beyond the fracture abruptly dark. The light was stopped and reflected back from the vertical crack. This finding posed an interesting problem. An engraved line had apparently been made on each face of the fossil at a right angle to the natural fracture. The act was apparently intended to make the image of a 'cross' on each face, though a 'cross' is what we, in our culture, see, while in the Mousterian it

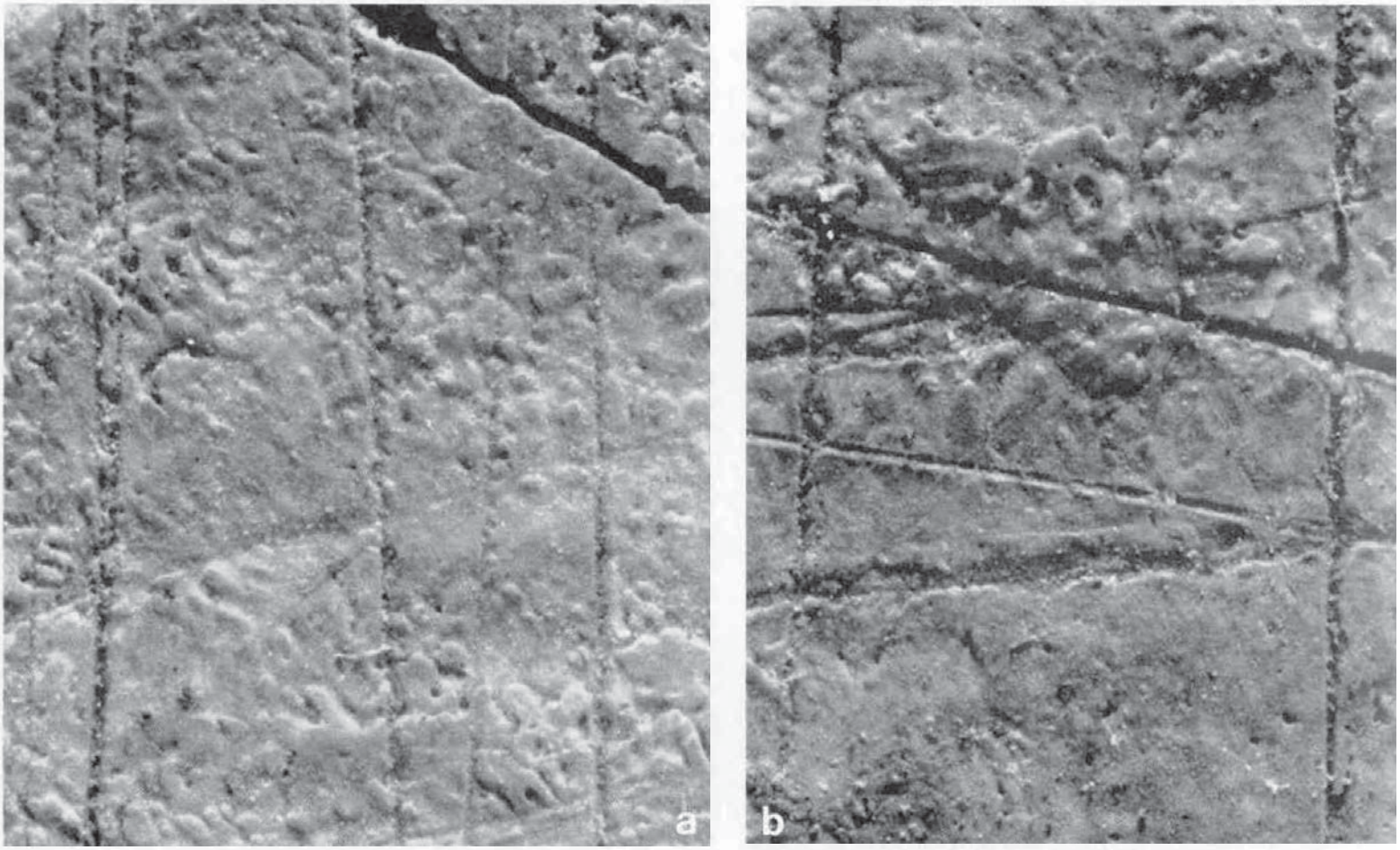


Figure 1 (a, b). La Quina, France (Mousterian). Close-up photographs of the incised lines on a bovid shoulder blade indicating the invariant depth and straight linearity of each line. This type of invariance could probably only be achieved by cutting through a skin or hide.

may merely have been an 'effect' that was intended, the double over-crossing of natural lines. That was only part of the problem. The fossil was a 'pure' circular exotic that may have been collected, kept and marked because of that fact. The rich brown, shining fossil has a sensuous look and feel. However, purely round, circular forms were also known to have been *manufactured* in the Mousterian. The presence in the Mousterian of worked round stone balls ('bolas') and discs has long been known. These are not discussed by Davidson. Henri-Martin, for instance, has published a beautiful example of a carefully shaped, *non-utilitarian*, large circular stone disc from La Quina (cf. Henri-Martin 1947). Merely at the level of production, categorisation and kinesthetic recognition, ball and circle were known entities in the Mousterian and the tiny fossil nummulite may have had relevance within some category that involved discs and balls. But there is an equally important and related question: how does one discuss such artefactual data in terms of Davidson's assumptions concerning the beginnings of 'depiction'? Depiction of what? Or do we simply ignore this category because it does not fit into a class of artefact in contemporary discussion? Have such objects become, in effect, 'non-data'? It is unfortunate that while Davidson discussed the Tata plaque (which he admits was intentionally shaped and *therefore* puzzled him), he did not discuss the fossil nummulite, the La Quina disc or the Mousterian 'bolas'. It may be that he did not know how to deal with this class of data.

Davidson also declares that he attempted to examine the supposed 'beads' from Bocksteinschmiede, c. 110 000 BP, but that one of the beads was lost and the other showed evidence of gnawing. I found a different set of data. Although the small vertebra had been 'lost', a cast of that bone was made before it disappeared and this was available in Ulm, while the original close-up photographs of the vertebra and hole were available at Bosinski's laboratory at Monrepos. Casts, despite their crudity, were once widely used in morphological and anatomical studies to disseminate basic measurements and features. A study of the cast indicated that the holes on each side were clearly funnel shaped. This is consistent with boring but would have been difficult to achieve by biting, gnawing or root action. I therefore located the original close-up photographs of the hole and found the funnel shape of the cast clearly documented (Fig. 2 a, b). On the basis of the cast and photographs the holes seemed to have been reamed. Davidson states that he was informed that the piece came not from the stratigraphy of a home-site floor but from the talus, apparently implying that even if it was genuine it might be Upper Palaeolithic; but the Bocksteinschmiede talus contained Micoquian artefacts and faunal materials. It did *not* contain the Upper Palaeolithic materials found at other excavations in the Bockstein and Lonetal area. Bocksteinschmiede is, in fact, the type site for the early Micoquian period in the southern German Swabian Alps (Narr 1951; Wetzel 1958; Bosinski 1967; Wetzel and Bosinski 1969).

Based on my study of the cast and the original photos I thought it necessary to test whether a Bocksteinschmiede type of tool could bore a hole of this size and shape in an animal vertebra of this size and shape. This was attempted by one of the researchers at Bosinski's laboratory in Monrepos, Germany, and in less than a minute a hole of precisely this size and shape was bored into each face (Fig. 3). This test did not prove the validity of the original 'bead' but it did 'warrant' its possibility. This 'warrant' would not, of course, suffice. I needed the broader warrant of context. Keeley (1977, 1980) had more than a decade earlier conducted microwear studies of tools from the earlier European Acheulian period, c. 300 000 - 250 000 BP, and had documented a tradition of working skins, fibrous plants and meat, including boring or reaming. 'To cite one further example

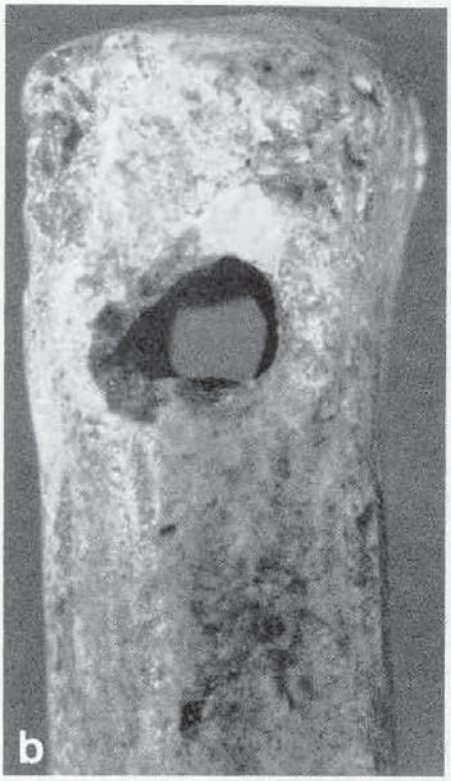


Figure 2. Bocksteinschmiede, Germany (Micoquian).
a) Close-up photograph of the funnel-shaped hole found on both faces of a wolf vertebra. Photograph by Wetzel (Wetzel and Bosinski 1969).
b) Close-up photograph of the cast of the vertebra showing the funnel shape. The cast has been chipped but the funnel shape is evident on both faces.

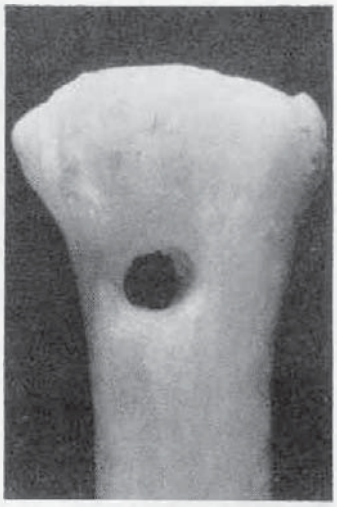
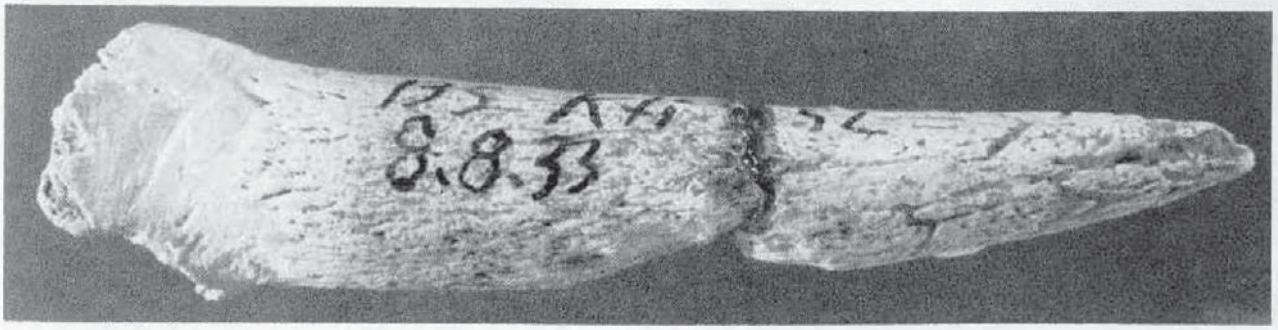


Figure 3.
Close-up photograph of the hole experimentally bored into the tail vertebra of a young fox with a Bocksteinschmiede point, indicating the same type of funnelling apparent on the wolf vertebra in Figure 2.

of microwear analysis, a bifacial tool from Clacton was found to show wood polish on its marking surface. Further examination revealed utilization damage that could only have come from a rotary motion such as boring. The tool had been turned in a clock-wise direction at the same time that downward pressure was being applied ...' (1977: 128). In fact, carefully shaped spears of yew wood, often more difficult to work than cooked or waste bone, were found from this period, at Clacton-on-Sea in England and Lehringen in Germany. The 'warrant' was, therefore, increased. Keeley's evidence for working a range of materials also tends to confirm Mania's evidence for similar work in this period. The process of boring was probably therefore known and possible at an early date at Bocksteinschmiede in Germany. In fact one of the bone artefacts excavated at Bocksteinschmiede represented an awl that had been laboriously cut and 'sawed' from the skull of a chamois (Fig. 4), suggesting that relatively large holes were probably being made in skins, but whether for containers, shelters or clothing is not known. I will discuss the relevance of the manufactured hole later.

There were other bone fragments with holes among the Bocksteinschmiede materials. They presented a different but even more interesting problem and set of data. These holes were made by organismic action, perhaps by a type of worm or larva or a root as suggested in the report by Wetzel and Bosinski (1969). These holes, however, had absolutely straight, parallel sides, with an abrupt right angle entry and exit and with the same diameter for the length of the hole, differing from the funnel-shaped hole in the vertebra (Fig. 5 a, b). 3) I was intrigued by this finding because by use of the microscope I had found similar holes among the early Aurignacian beads excavated from Blanchard in France, bored holes not made by a tool, but by an oceanic carnivore that preyed on shelled species. Such natural holes, even those created by wave and water action, could easily have suggested the use of shells for beads for the anatomically modern humans who inhabited European coasts during the early Upper Palaeolithic. It has apparently never been remarked that such natural models for a bead are common and can often be dramatically striking. One does not need the hypothesis of a sudden creation of human 'self-awareness' concurrent with the Aurignacian manufacture of bone and stone beads in Europe, as is being currently promulgated by White (1989a, b), in discussing the origins of personal decoration. When working at the Archaeological Institute in Kiev I was shown an artefact from a Mousterian level at the Ukrainian site of Prolom that had been published by a student of Semenov, the mid-century pioneer in analysis of Palaeolithic bone work, as a Neanderthal

Figure 4. Bocksteinschmiede. A chamois horn 'awl' cut from the skull. Such a natural tool may have been used to create holes in skins.



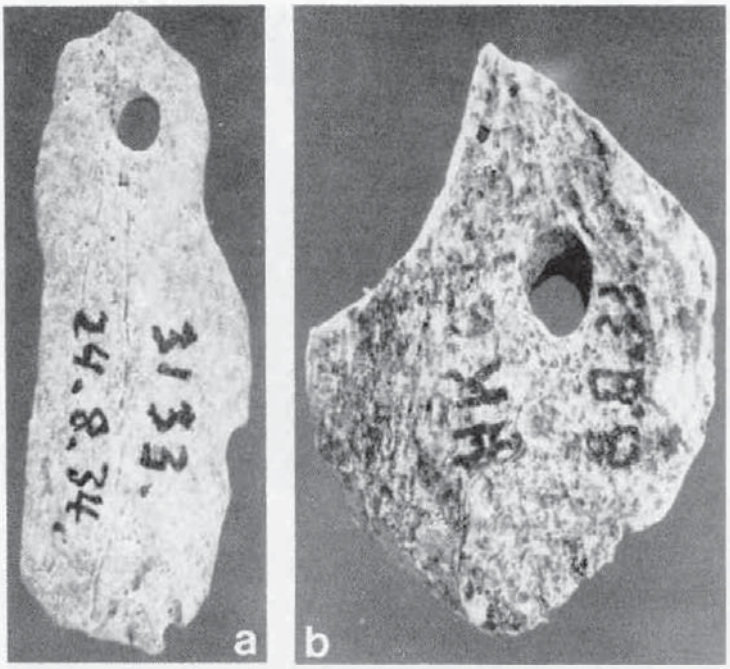
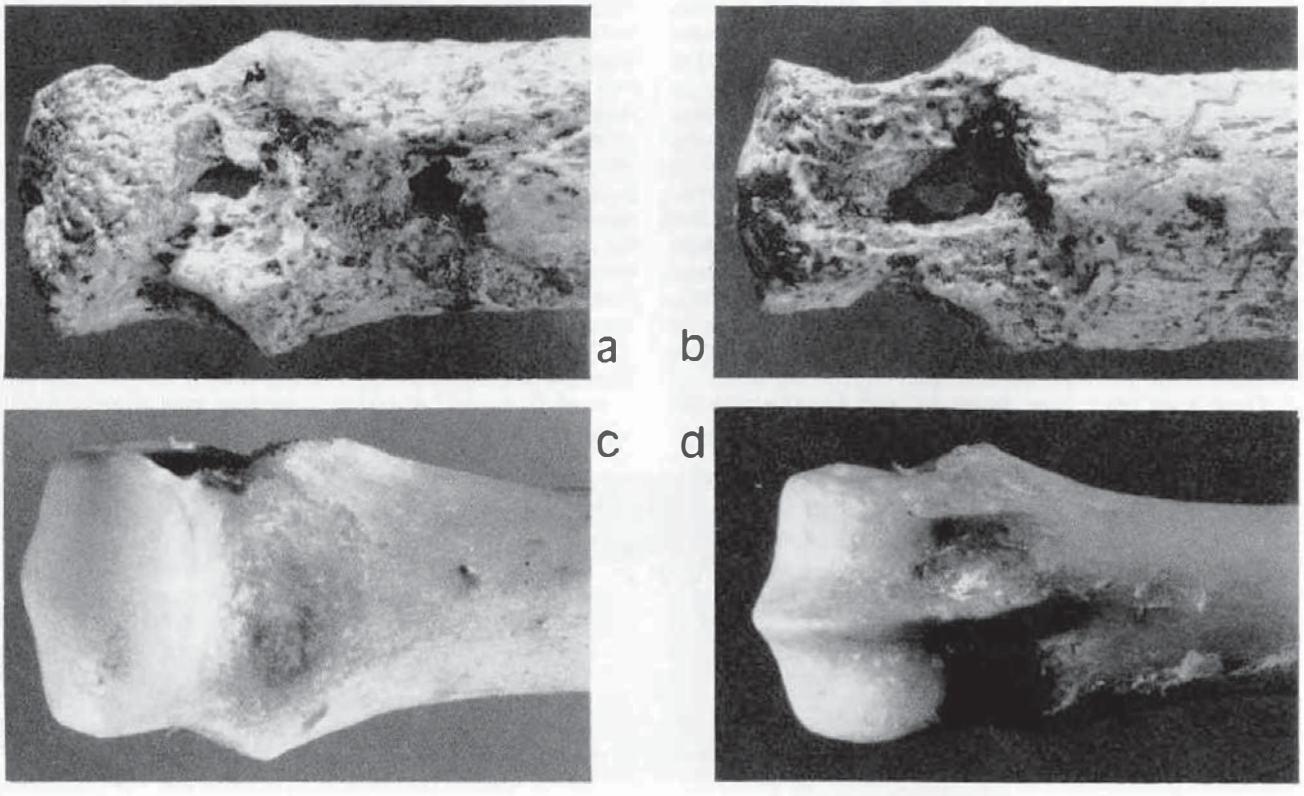


Figure 5 (a, b). Bockstein, Germany. Bone fragments with natural holes, perhaps bored by larvae, recovered from different Bockstein excavations. Such natural holes in bones, vertebrae, fossils etc. suggest the possibility of their use as bead or pendant.

Figure 6. Prolom, Ukraine (Mousterian). The ear-bone of a cave bear with a natural hole and tunnelling suggesting a possible pendant.

Figure 7 (a-d). Bocksteinschmiede, Germany (Micoquian). Close-up photographs of the holes on each face of a wolf metapodium (a, b) compared with an undeteriorated recent wolf matapodium (c, d), indicating the natural concavity on one face and the arched protuberance on the other. The deep gouging of a 'bowl' where the protuberance was does not seem to have been made by gnawing. A shelf at the bottom of the 'bowl' was apparently made by cutting and is seen in the original photograph of this area made by Wetzel.

pendant 'head' with a bored hole. Carefully cleaning away the soil accretions under the microscope I found that natural channels or tunnels ran in different directions through the artefact and that it had not therefore been 'manufactured' (Fig. 6). The artefact was the inner ear-bone of a cave bear and, because of its shape, may have been an 'exotic' collected and kept by a Neanderthal. It could have been worn as a pendant because of the natural hole, but it could equally have provided a conceptual 'model' for beads made of other materials. Artefacts such as the Bockstein bone fragments with natural holes could also have been collected and worn (Fig. 5 a, b) though there is no evidence for it. 4) To my knowledge there has never been an investigation of the possibility for a range of such natural 'symbolic'



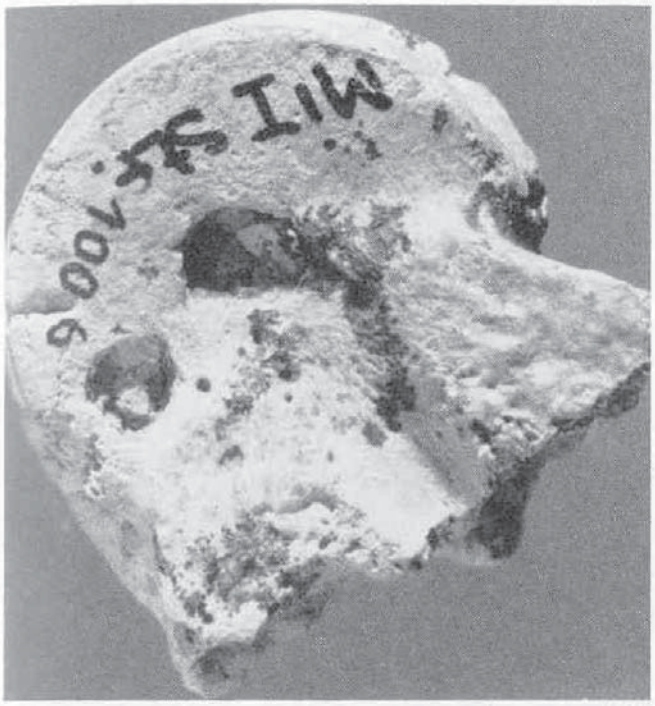


Figure 8. The recent bite marks made by a rodent into the vertebra of a deer, indicating the rounded hole and the collapsed material still in the hole.

objects in the early record. In a culture where 'holes' were used, the possibility, of course, existed. The Bocksteinschmiede problem, then, is not precisely as Davidson would have it, and more careful research and a broader, more careful inquiry is required than he has given it or considered. But I have only begun a discussion of the problems involved.

We come to the wolf metapodium that Davidson claims shows gnawing marks at the proximal end. He does not document this evidence for gnawing. The hole at the distal end, according to Davidson, 'can be accounted for by piercing with teeth ... the displaced portion of bone ... can still be seen there'. Davidson makes the statement without offering the documentation that would serve as a 'warrant'. My own study under the microscope (Fig. 7 a-d) revealed no portion of 'displaced bone' within the hole on either face. The tiny hole on the concave face could easily have occurred when the reverse face itself was drastically changed. The narrow hole shows no evidence that it was made as the result of a bite. Holes made by a tooth tend to be rounded with an irregular downward breakage occurring around the perimeter. The broken material is often present in the hole (Fig. 8). The long oval shape of this hole suggests puncture by a stone point more than it does puncture by a tooth. The reverse face, however, was more interesting. Microscopic examination found a relatively straight, slightly angled horizontal platform below the base of the original protruding arch (Fig. 7 c, d). The platform was deteriorated but it did not seem that the effect could have been created by gnawing. It seemed as though a horizontal cut had been made at the base of the protuberance in order to begin the reaming or scraping of the arch above. This could not be verified because of the deterioration apparent in the photograph.

Davidson states that 'gnawing' is apparent at the distal end. I thought the missing portion at this end was due to breakage and to the severe deterioration which the bone was clearly undergoing. I saw no evidence of gnawing. But 'gnawing' at the distal end would not explain the relatively untouched state of the concavity at the proximal end. There was no evidence of gnawing in or around this concavity; merely evidence of the general deterioration of the bone. This is in contrast to the deep bowl-like gouging evident on the reverse face. To indicate the nature of gnawing in contrast to the deeply gouged bowl that is present

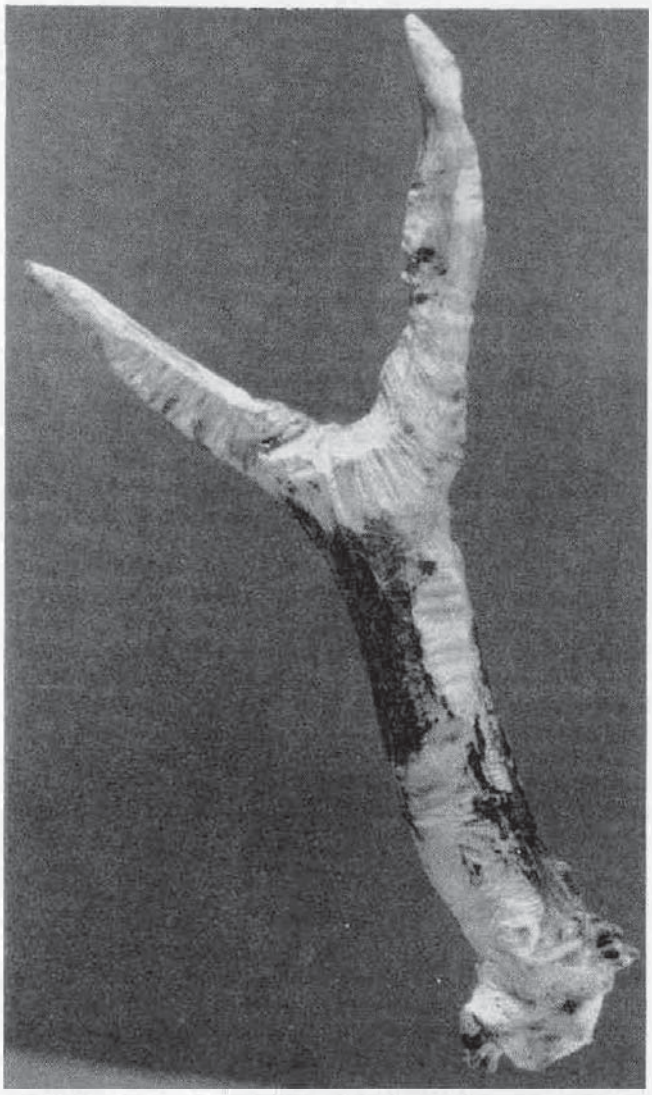


Figure 9. The gnawing marks made by a rodent on a recently shed roe deer antler indicating the planar nature of the gnawing action. Such gnawing would not create the effect of a deep bowl.

on the metapodium I illustrate a roe deer antler that has been gnawed by a rodent. The flat, essentially horizontal nature of the gnawing is in contrast to the 'bowl' on the metapodium (Fig. 9). This type of gnawing would not have created the linear 'platform' effect or the deep bowl. Bosinski, a careful excavator of the German Mousterian, had studied the Bocksteinschmiede material when he was helping to prepare the site publication (Wetzel and Bosinski 1967), and was of the opinion that the two objects were intentionally made. I could not 'prove' him right or wrong, but in the context of the accumulating and comparative data I felt that they could indeed have been intentional. Having, therefore, puzzled over the metapodium under the microscope, having seen the original close-up photos, having spoken with Bosinski, and having assayed the evidence for working a diverse range of materials in this period, having studied the making of holes and having seen what gnawing does to fresh bone, I am puzzled at Davidson's strong assertions both of careful examination and 'warrant'.

But statements of this type suffuse Davidson's argument. Davidson states that he saw the La Quina incisor with a hole, but that it was under glass and was 'difficult to see'. Nevertheless he states that the hole was 'certainly not made by boring, drilling or cutting'. The slight marks around the hole, he states, 'could be called bruises, and might correspond to tooth marks, though why an animal would chew a tooth is less obvious'. This is an extraordinary statement to be presented as part of an effort to

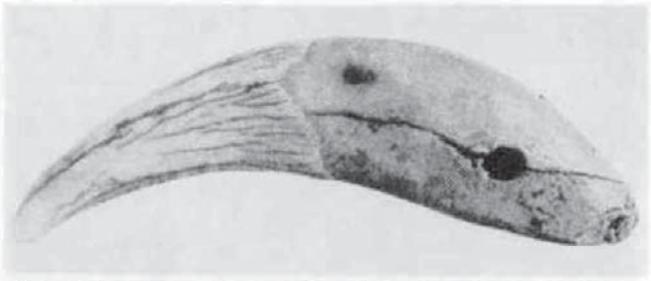


Figure 10. La Quina, France (Mousterian). A fox canine with a hole and fracture on one face. The hole does not seem to have been reamed but was apparently the result of an impact or pressure that also caused the fracture. An animal bite at this point on a canine is unlikely while an effort to create a bead with an initial puncture is possible.

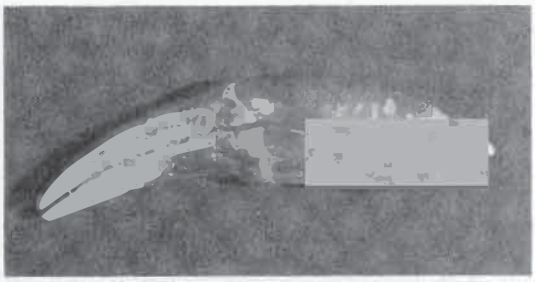


Figure 11. Arcy-sur-Cure, France (Chatelperronian/ Aurignacian). A canine bead with a reamed hole and a crack of the same type as on the La Quina canine. Fractures of this type created by the pressures of boring are not uncommon.

examine and evaluate the relevant materials. I would not argue so categorically on the basis of an examination made through glass.

There are other ways of approaching the problem. The original photograph by Martin (1907-1910) shows a vertical fracture descending from the hole (Fig. 10). I had suggested that impact or pressure during an attempt at piercing or boring, particularly in the Mousterian when the technique of boring into hard tooth material had not yet been perfected, may have created the fracture and that the canine may therefore have been abandoned. Could a fracture such as that on the La Quina canine result from the pressure or impact of an attempt at boring? Without any difficulty I found precisely this type of fracture in a number of canine beads from the early Aurignacian (Fig. 11). The fracture on the La Quina canine may, then, have occurred as the result of an initial attempt to puncture the canine. It is, in fact, not easy to either bite or bore a hole in a canine. An examination of the earliest bored proto-Aurignacian tooth beads, from Bacho Kiro, Bulgaria, c. 42 000 BP, presumably made by anatomically modern humans not much later than La Quina, dramatically evidences a clumsy effort to initiate such holes by a multiple crisscrossing at this point in a canine. The clumsy effort created a downward crack in one 'proto-Aurignacian' bead (Fig. 12 a, b). In the La Quina case, a human attempt to puncture a hole is therefore as likely as the bite of an animal. Like Davidson, I cannot imagine why an animal would bite into a canine, and if it did so would do so at this precise point. But of greater importance, my suggestion for human action had been made because of the awls in the Mousterian that were intended for boring, and the presence of one large intentional hole carved and bored into bone at the Mousterian site of Pech de l'Azé (Bordes 1969), a site in France not too far from the Mousterian site of La Quina. 5)

A better argument against the canine and phalange has come from researchers who have noted that the provenance of the two La Quina artefacts is uncertain because it came from an early excavation. This argument is usually made in the context of arguments against a Neanderthal symboling capacity. The problem of dating the materials from early excavations is generic, but this does not mean that we should not attend to the broad range of theoretical problems posed by these materials. In any case, the issue of pendants or beads in the Mousterian can be addressed from a number of other angles.

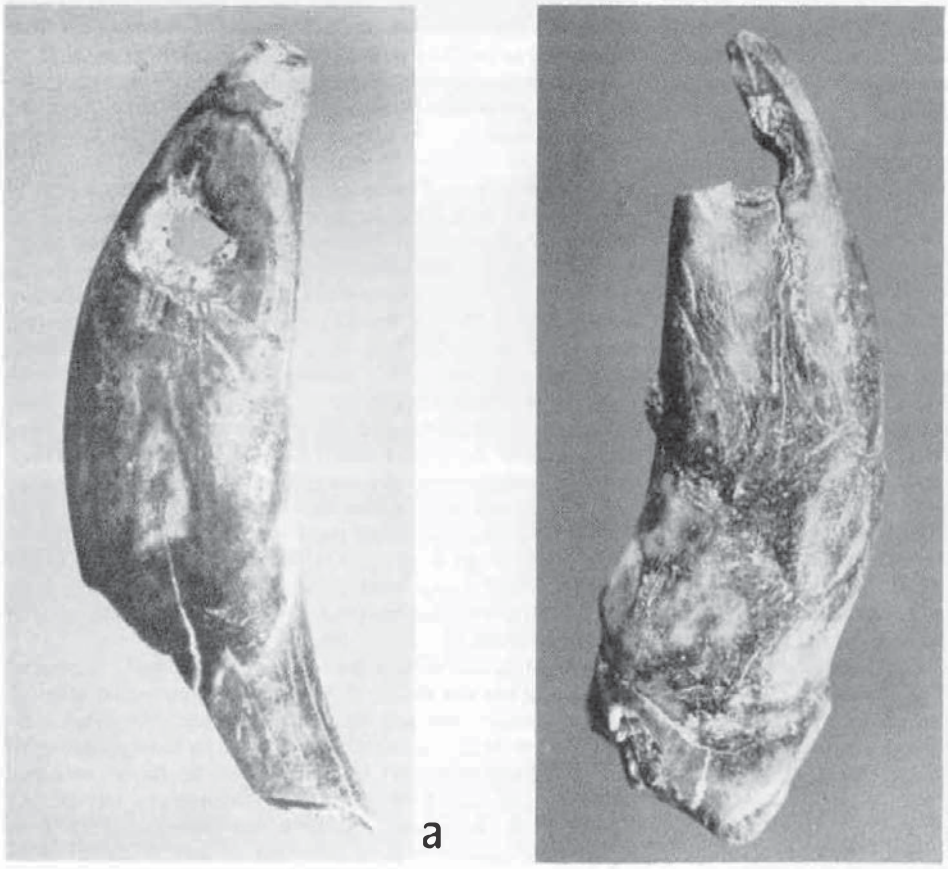


Figure 12 (a, b). *Bacho Kiro, Bulgaria (Proto-Aurignacian). Two crudely made canine beads.*
a) The bead at left indicates the repeated crisscrossing made to create the initial hole to begin the reaming.
b) The bead at right indicates the cracking caused at the hole by the pressure of boring or reaming. These crude early efforts at boring into a hard tooth material differ markedly from the skill in boring holes in canines and manufacturing beads evidenced thousands of years later in the true Aurignacian.

Figure 13. A model reconstructing a Mousterian Kebara, Levantine, type of point hafted to a wooden shaft by mastic and thongs. The reconstruction is based on the microwear studies of John Shea. (Photograph by J. Shea)

I have indicated, for instance, that the La Quina blade may have been used for cutting skins. This suggestion puzzles Davidson. The suggestion is, however, germane to the issue of capacity and possible symboling. Elsewhere, for instance, Davidson and Noble (1990b) have cited Holdoway (1989) as having expressed doubt about hafting in the Mousterian. The issue is important because hafting is a complex sequential activity that requires planning and the cojoint use of different techniques and classes of material. Holdoway was writing about the absence not of hafting but of hafted *projectile* points among those French Mousterian materials he had studied. There is accumulating evidence that the hafting of *hand-held* working tools may have been common in the Mousterian (Bosinski 1985; Beyries 1987; Anderson-Gerfaud and Helmer 1987). Shea (1988, 1989a, b), whom Holdoway challenged on hafting, has given strong evidence for the hafting of projectile points at the Mousterian site of Kebara in the Levant, besides providing the evidence for the Neanderthal working of a wide range of materials, including skins and wood. 6) Holdoway's lack of evidence for the hafting of projectiles in the French materials he studied does not disprove the evidence for projectile hafting *in the Levant*, nor does it disprove the hafting of other types of tools in Europe. These facts are crucial for a discussion of the problem of 'capacity'. Hafting often requires a use of mastic as well as thongs for binding. Resin with the imprint of a stone tool was found at the Mousterian site of Königsaue in Germany (Mania and Toepfer 1973) and resin was found in a Mousterian level at Bocksteinschmiede (Bosinski 1985). Shea has suggested a use of both mastic and of thongs in the Levant (Fig. 13).

It is possible, therefore, that it was thongs that may have been cut on the shoulder blade from La Quina and that such thongs were used in hafting. But thongs, once made, could also be used for personal decoration. In a major analytical paper (Marshack 1989b) I have shown that twined cords and thongs were commonly used as bracelets, necklaces, body bands, hip belts, collars etc. across much of Europe in the early and late Upper Palaeolithic. They apparently represented an ancient tradition and mode of 'personal adornment' that probably preceded, and was more common than, the use of beads with bored holes. The evidence for a use of personal decoration made of perishable material contradicts many recent presumptions, such as those by White (1989a, b) that Upper Palaeolithic beads represent the beginnings of both 'self awareness' and personal decoration, and that these beads suggest also the beginnings of complex culture and language. Echoing this entrenched presumption, for instance, Taborin, in a recent study of the Chatelperronian Arcy-sur-Cure materials (1990) states that: 'Ornament-making proceeds from two different fields: on the one hand from the sphere of reflexive thought and symbolism, on the other hand by technical skill for *shaping hard faunal raw-material*' (my italics). The presumption that Upper Palaeolithic bone, shell and stone beads represent the beginnings of personal decoration is clearly one that Davidson, like White, Taborin and even Binford, accepts, since it fits a presumption concerning the Neanderthal lack of capacity either for problem-solving or significant symboling.

It is such presumption again that limits what Davidson could see in his examination of the marks on the arm of the therianthropic lion-headed ivory figure from Hohlenstein-Stadel. I have published a major paper on the variability in such Upper Palaeolithic marking, including the marking on the Hohlenstein sculpture (Marshack 1990a). The carving, from the early Aurignacian, represents the earliest depiction of a human or an anthropomorphised spirit wearing an animal mask. It is the head, however, that is important. I have suggested that the use of animal parts and skins, which persists throughout the full Upper Palaeolithic and is common even in historic times, probably preceded the manufacture of beads and probably occurred in the Mousterian period before the Upper Palaeolithic. The skin of an animal, even if worn against the cold, would become 'symbolic', if only because it may have been killed by the wearer or because it represented a particular species etc. I need hardly enter into a discussion of the problem here. If animal skins, parts and thongs were worn in the Mousterian, that fact would dispute Davidson's and Noble's elaborate argument about the beginning of language with the supposed appearance of 'depiction' within the cultures of anatomically modern *Homo sapiens sapiens*. The skin of a feline could, of course, be as symbolic as the rendition of a feline. 7) In addition, if as is currently being argued, the Chatelperronian culture in France documents the contemporaneity of the Neanderthals with the Cro-Magnons for a period of more than a thousand years around 35 000 BC (Combier 1990; Debeneth 1990; Demars 1990; Gouedo 1990), and if some of the Arcy-sur-Cure beads were made by Neanderthals, or if the Neanderthals had communication and cultural exchange with neighbouring and contemporaneous Cro-Magnons, we are faced with the problem of dealing with a possible functional symboling and communicative capacity among the Neanderthals at a level that contradicts the model being proposed by Davidson. The easiest escape from such a problem is to deny a priori that the problem exists.

In this regard I should note that a number of the beads from Arcy-sur-Cure are not made in the typical 'Aurignacian' manner, i.e. with a bored hole, but instead have an encircling groove around the top that would have required tying (Fig. 14 a-d). The tool marks around this grooving are apparent. 8) This mode of tying is easily derived from the practice of tying that is often involved in hafting. 9) The conceptual process would not be far removed from the tying that would also have been involved in the use of skins and boots. In the Chatelperronian at Arcy an exotic fossil crinoid with a natural hole was found among the 'beads' by Leroi-Gourhan (1965). Conceptually a fossil with a

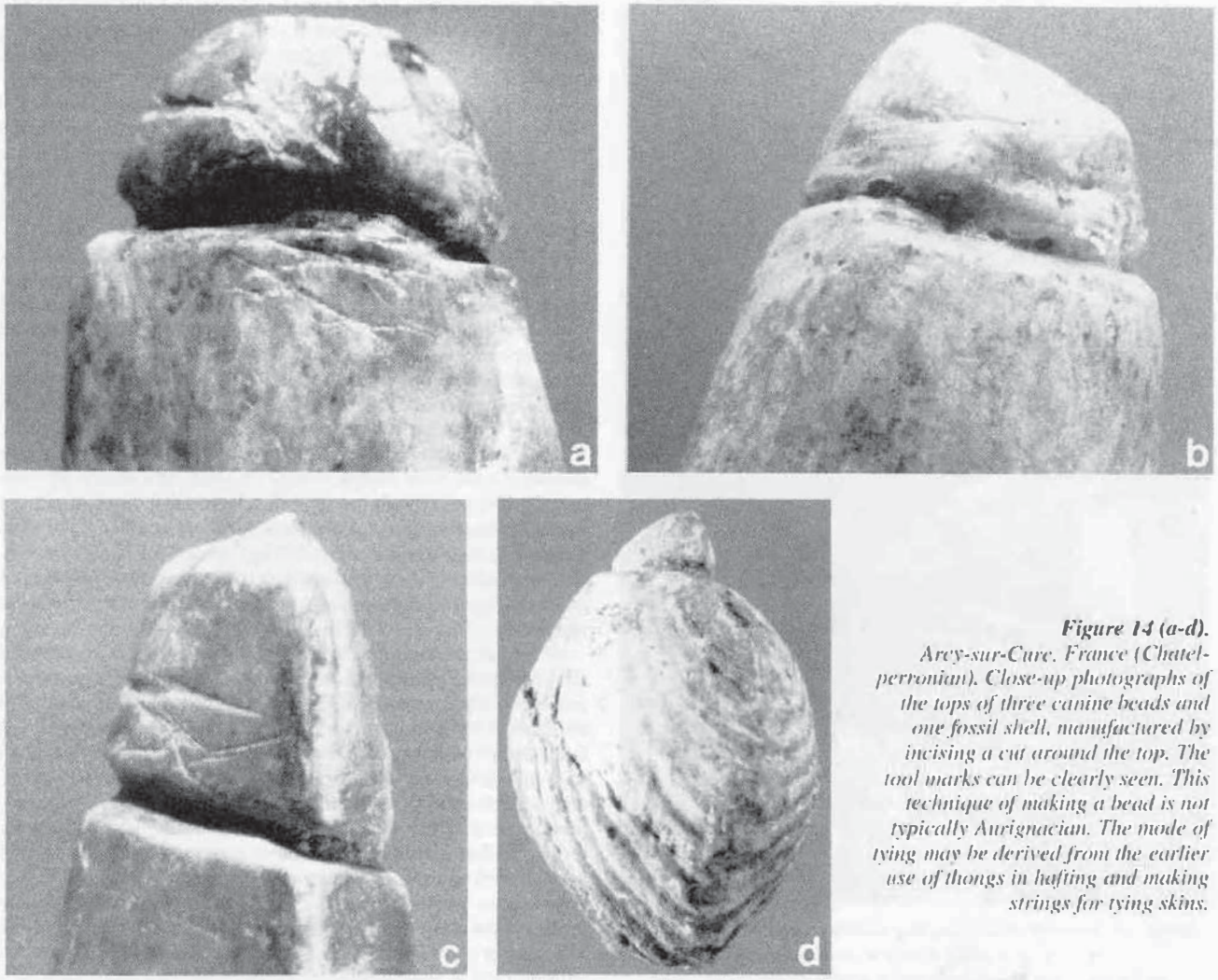


Figure 14 (a-d).
Arcy-sur-Cure, France (Chatelperronian). Close-up photographs of the tops of three canine beads and one fossil shell, manufactured by incising a cut around the top. The tool marks can be clearly seen. This technique of making a bead is not typically Aurignacian. The mode of tying may be derived from the earlier use of thongs in hafting and making strings for tying skins.

hole falls into that category of natural objects that could easily have served as a pendant or bead without any expenditure of effort except for the preparation of a string.

Davidson cites Binford on what appear to be 'root marks' among the marked Mousterian materials from Cueva Morín in Spain that Echegaray and Freeman have repeatedly claimed to be intentional, symbolic marking. When the Morín material was published (Echegaray and Freeman 1971) I made it a point to conduct my own microscopic analysis of the marking.

On May 1, 1975, I sent a long analytical report to Freeman:

> ... after speaking with you I cleaned the ... pieces carefully, laboriously, under the microscope. I examined them all and I was astonished ... The 'meanders' are *not* tool made. The full results are so extraordinary, and so important, they intrude a new phenomenon into the study of early bone industries ... The (enclosed) analysis may be important also for your China trip since apparently comparable marking comes out of Choukoutien. It would be helpful if you could check on this first hand while you are there ... From Pei's paper it seems possible that he is describing the same type of marking for Choukoutien that I have just analysed for Cueva Morín. I have prepared one set of analytic pictures and notes for you, perhaps to take to China. The full analysis of the bones is complex ... Incidentally, some of the markings on the Cueva Morín materials (are) tool made ... but the 'meanders' are not ... If the analysis I have enclosed is correct then it is possible that Valoch's 'meanders' from Kulna may also be of this type. I will be in Brno in late June to examine his example(s). It may also now be necessary to re-examine Bordes' bone from Pech de l'Azé ... I can now go back with a hell of a lot more insight into the analytic problems involved ... If you want a microscopic analysis of (the) various Mousterian 'marking' traditions for the Nice meetings, I could probably give a short paper ...

In my report to Freeman I remarked that 'we seem to have the phenomenon of a partial and limited deterioration of some of the bone surface under particular conditions of moisture, drainage, acidity ... chemistry, and perhaps bacterial action ... etc.'. I present a close-up of one of the tree-like, 'meander' patterns on the crucial Cueva Morín example of supposed intentional marking. Low-power microscope study indicated that some of the supposedly 'incised' lines dipped below the surface of the bone and then reappeared on the surface some millimetres ahead. The canals under the surface had in places been filled with what appeared to be black manganese and could be seen through the thin surface of the bone forming a linear connection to the section that surfaced ahead. One could track the incipient formation of other such branches within many of the tree-like 'meanders'. These are not tracks made by root action. One can compare this analytical photo from Cueva Morín with Binford's later, more generalised photos and discussion (Binford 1981).

Unlike Davidson's study, my two-decade-long inquiry has not been conducted to prove either the existence of pre-Upper Palaeolithic symboling or to prove an entrenched hypothesis. It has been conducted to determine the nature of the problem and the supposed evidence, without preconception or a priori presumption. **10)** When McBurney (1967), for instance, published what he claimed was a pre-Upper Palaeolithic whistle from the North African site of Haua Fteah, I attempted for years to have the artefact found and when it was located I flew immediately to Cambridge to study it. I found that there was no evidence for intentional reaming and reported to the museum that it was not a whistle and should not be labelled or filed as such. When I received word that a number of Acheulian carvings in stone had been excavated in Czechoslovakia, I went there to examine the stones and determined that the odd forms were natural and that there was no evidence of carving. However, unexpectedly, I found that there was evidence from that early site of

Figure 15. Cueva Morín, Spain (Mousterian). The lines on the surface of a bone supposedly indicative of intentional symbolic marking. The close-up details reveal that some of the broken lines descend below the surface and then reappear. Some of these tunnels are filled with soil and manganese.

Beçov, c. 150 000 BP, of a manufacture and use of red ochre. The quartzite rubbing stone, the abraded ochre and the profusion of ochre powder (Marshack 1976) came from roughly the same period as the Bilzingsleben material and the carved and shaped spears of hard yew wood from Clacton-on-Sea in England (Oakley et al. 1977) and Lehringen in Germany (Jacob-Friesen 1956). 11) When I was informed by Derek Roe that fossil shells (*Cocinopora globularis*), from the Lower Palaeolithic at Bedford, England, had been published as possible beads I went to Oxford and studied them by microscope. I did not find evidence for the intentional widening of the natural holes in these fossils, though the remnant organic material in the holes that run through the shells did suggest that they may have been strung on thongs or fibres (see also Keeley 1980: 164). The necessary test would involve an analysis of the dark inner, organic, seeming 'string' material. I did suggest such a test to the curators. This would be a test of symbolic usage by the secondary evidence of a string rather than by the primary evidence of reaming or boring. Keeley, as noted above, did find evidence for reaming or boring in this early period with at least an implication that such holes may have utilised strings or thongs.

Clearly, then, the problem of early capacity and the potential for complex problem-solving and symboling is not as Davidson would like to see it or has reported it. The careful, comprehensive and broad inquiry required cannot be conducted as Davidson has conducted it, as a rapid, cursory, eye-ball survey. My persistent request for methodological precision and care in the study of the extant materials and the problems they pose is, of course, what Lewis-Williams and Dowson have recently denigrated in this journal as Marshack's 'empiricism', in contrast to their own supposedly 'scientific' arguments, based not on a study of the materials, but on models derived from ethnography, selective aspects of psychology and simple eye-balling. After publishing their 'psychological' entoptic model and being criticised for theoretical presumption and their lack of familiarity with the materials, Lewis-Williams and Dowson, for instance, made a rapid trip of a few weeks through the Franco-Cantabrian caves in the effort to 'prove' their hypothesis. Davidson and Noble, in similar manner, have argued by recourse to a theoretical psychological model, this time that of Gibson, and, being criticised for lack of familiarity with the material ? Davidson has undertaken his own rapid, and relatively cursory, 'touristic' investigation of certain artefacts. Though such hypotheses and cursory examinations make for fascinating and often illuminating discussions, it is not the way that science works. The Bilzingsleben materials and problem are complex and are integrally bound up with the theoretical problem of the evolving and developing hominid capacity (cf. Marshack 1989a). A self-limiting theory, whether it is the Gibsonion psychology of Davidson and Noble or the entoptic psychology of Lewis-Williams and Dowson, may offer certain heuristic insights but it inevitably constrains far more than it enlightens or advances research and inquiry.

The problems raised by Davidson in his recent publications have been but touched on. The assumption, for instance, that language derives from 'depiction' (Davidson and Noble 1989) may seem to have a certain rational, logical validity. I have myself argued that the capacity for visual categorisation is a fundamental aspect of the capacity for language (Marshack 1984, 1985, 1987, 1989a). But that does not mean that all symbolic marks and images *must* have what Davidson and Noble naïvely term 'shared meaning'. The marks on the La Quina shoulder blade were *not* symbolic, but in context they may have been the product of a process that may have had symbolic relevance or reference. I have repeatedly indicated that many of the images in the caves may not have been 'symbols' or 'signs' intended to communicate a shared meaning. There are painted dots, finger marks and hand prints and animal images in difficult to reach alcoves and recesses that were clearly intended neither to be seen nor to be shared. They are often the 'product' of private, participatory symboling activity and behaviour. A mark that indicates that someone has touched a wall or image in a ritual gesture of participation is not necessarily a mark whose intent is to communicate a shared meaning. It is the *behaviour*, not the image, that may have been symbolically relevant in such a case, and the behaviour is gone while the mark remains. Such a mark may not have been seen again, in fact it may have been intended to remain unseen. We are therefore faced with a problem. It *may*, to us, today, represent evidence of symbolic *behaviour*, but it was not necessarily a 'sign' with a translatable or assigned 'meaning' in the original culture. It was not, therefore, 'depictive'. Such marks might be made with a tool, with colour, with a finger or with a hand print. The end products would be different, yet the *behaviours* might in each instance be symbolically comparable. A set or collection of such participatory marks, which are often found within Upper Palaeolithic caves, may have no intentional structure, or pattern. They often consist of a random scatter on a wall. Yet as an aspect of symbolic behaviour these can be studied and, in fact, patterns of *usage* can often be determined, if not patterns of 'meaning' or design. We, as analysts, inevitably see such marks as 'signs' or 'symbols' and classify or categorise them as forms or styles of 'depiction' or 'representation'. But were such marks necessarily seen as such by the makers? Would such *non-communicative*, participatory symboling behaviour and 'image making' fall into Davidson's and Noble's model of the 'depiction' that leads to language?

I close on a different note, one that approaches the problem of early capacity from another direction. There is a growing body of morphological evidence that the Neanderthals underwent similar stages and rates of ontogenetic development as those found in modern humans, challenging longstanding presumptions concerning foetal and ontogenetic, maturational differences between the Neanderthals and anatomically modern

humans (Minugh-Purvis 1988a, b, 1990; Tillier 1988). Laitman (1983) and Budil (1990) have suggested, on the basis of morphological reconstructions, that the Neanderthals could 'speak'. Geneste (1986, 1990) has indicated that the French Neanderthals had conceptual maps and models of the resources in their territory similar to those of the following Cro-Magnons. Studies in the Levant have indicated that there was no difference in the technology or subsistence strategies found among anatomically modern humans and those of contemporary Levantine Neanderthals (Bar-Yosef 1988). A recent report by Kuhn (1990) indicates that the late Neanderthals of the Italian coast, under the pressure of changing climatic conditions, changed their subsistence patterns to include the long-distance acquisition and local storing of stone resources and probably also shifted to planned collective ambush hunting. These behavioural changes required new conceptual maps and models that functioned in space and time. It is doubtful whether such changing schedules of communal, social activity could be instituted without aspects of referential 'language' and symboling. Despite a paper by Gargett (1989) opposing claims for Neanderthal burial, there is a strong belief among many Mousterian specialists that Neanderthals did bury their dead. **12)** These data and concepts, in concert, do not conform to that presumptive model of hominid evolution and that lack of pre-Upper Palaeolithic capacity being proposed by Davidson and Noble. The data do suggest that what is required is neither presumption nor cursory examination but careful, methodological investigation and a more comprehensive theoretical frame for modelling possible and potential hominid symbolic behaviour. **13)** The chronologically early and suggestive Bilzingsleben materials in particular would, in such a perspective, require far more careful, methodological analysis and far more probing theoretical and 'cognitive' consideration and discussion than has yet been given them.

NOTES

1) Apart from recent papers by this researcher (1984, 1985, 1988, 1898, 1990) that question that presumption, there have been other papers questioning the rigid 'species' concept and 'species' assumptions embedded in the Davidson and Noble model of hominid evolution and capacity (Smith et al. 1989; Cachel 1989; Lindly and Clark 1989; Wolpoff and Caspari 1990; Corruccini 1989, 1990). O. Bar-Yosef (pers. comm. 1990) states that the variability in the morphological data makes a clear determination of 'species' differences impossible. Until the many problems currently being debated concerning the hominid 'species' concept and the differences among late hominid species are clarified, it is presumption to attempt to prove the Davidson and Noble model by the simple non-contextual and selective eye-ball studies of particular materials and examples.

2) My own theoretical bias, which is contrary to that of Davidson and Noble, is that hominid evolution essentially involved the mosaic development of sets of 'potentially variable capacities' whose primary feature was that they were increasingly capable of performing diverse forms of visually mediated problem-solving, categorisation, and mapping and modelling (Marshack 1984, 1985, 1989a). In concert with these developing capacities, symboling and imaging systems (including 'language' at different levels of competence) were developed to mark and abet these cognitive processes; together these capacities fostered the development of variable, incremental stages of late hominid social and cultural complexity. I have suggested in this regard that Neanderthal capacity and that of anatomically modern humans were probably 'comparable if not precisely equal', and that what may have differentiated anatomically modern humans from the Neanderthals may have been an increase in the 'potentially variable capacity' for mapping and modelling behaviour and thought in time and space; not 'depiction', but this more generalised capacity is what makes language functional and adaptive. This model proposes a fundamentally different type of behavioural contrast among these late hominids than that proposed by Davidson and Noble.

3) Like the ear-bone in Fig. 6 such natural forms and holes could invite 'stringing' long before traditions of boring bone became archaeologically apparent. I do not know whether the problem of incipient or precursive traditions leading to the later bone-working traditions of the Aurignacian have ever been systematically addressed, except incidentally as I have in a number of papers. In large measure a priori assumption and a widespread belief in sudden 'origins' has made the raising of such questions difficult. Yet studies such as those of Keeley, Anderson-Gerfaud, Shea and Marshack are indicative of the studies and questions that must be addressed. My unpublished study of some of the incised and rhythmically marked plant rods from Border Cave in Africa, comparable to the type of marking on bone found in the Aurignacian in France, strongly suggests a widespread, precursive use of such materials. In Australia, the precursive rock art traditions documented by the marking in Koonalda and other caves and rock shelters pose comparable problems for study and theoretical inquiry.

4) Both as a child and an adult I, and thousands of others, have collected water-worn shells and stones with natural holes and have string them for wearing as pendants or beads. I have seen this process among indigenous cultures in many parts of the world. The suggestion that beads must have begun with intentional manufacture is an archaeological fallacy.

5) Not only has Keeley documented boring in a far earlier period within Europe, but since the presence of 'borers' are well known in the Mousterian I have addressed the theoretical problem of the 'hole' as both an early hominid concept and technology in a number of recent papers.

6) Keeley (1977: 128) has stated that microwear analysis of Acheulian tools has documented that 'most of the side-scrapers showed the characteristic polish of hideworking tools'.

7) Even skin boots or moccasins worn during a Palaeolithic winter might become 'symbolic' of that season if such boots were not worn in the summer. The preparation of winter boots could have become a 'symbolic' activity of the autumn season and perhaps even have been symbolic of the specialised activity of a class of makers. Such boots would probably have to be tied by thongs. The problem of the symbolisation of human products and human production, like many of the other problems raised in this paper, has almost never been discussed. If the Neanderthals did in fact wear skins during the winter, would these not inevitably become both 'depictive' and 'symbolic'? Would the 'style' of one maker or one group eventually differ from that of another?

8) The natural grooves in the bear incisors from Selayn (Gautier 1986) all occur at the opposite end of the tooth, not at the end that is intentionally grooved among the Arcy teeth.

9) The tying of hard materials that are difficult to bore is documented in the Upper Palaeolithic of Mal'ta, Siberia, where bone and tooth beads were bored, but where harder stone beads were tied upon a groove that was incised around their middle.

10) I examined the supposed Neanderthal carvings collected and once exhibited by Mathes in Germany; in Czechoslovakia I examined the seemingly intentional Mousterian engravings from Kulna found by Valoch and found them to be similar to the processes documented at Cueva Morín; in Italy I studied the supposed intentional Mousterian engraving from Riparo Tagliente published by Leonardi etc. The number of such instances and collections in every part of Europe, supposedly 'symbolic', and the number of items and papers sent to me for reading, would itself make an interesting analytical paper. In most cases I found the evidence to be suspect. In a few cases, however, the evidence for intentional 'image-making' seemed secure; this evidence included the manufacture of ochre at Beçov, the intentional incising of a series of zigzag motifs at Bacho Kiro, the carving of a plaque and the making of a cross at Tata, the vertebra at Bocksteinschmiede etc. I was struck during this long and careful inquiry not by the rarity of such evidence, but by the *variability* in the products and the processes that could be genuine.

11) These rare wooden artefacts confirm Keeley's microwear studies and indicate the capacity to work and shape exceedingly hard non-lithic, non-bone material.

Bednarik's recent note on an Acheulian haematite pebble with striations from the Hunsgi site complex in India confirms the early and widespread hominid use of ochre (Bednarik 1990: 75). Comparable evidence comes from the Middle Stone Age of Africa. How such widespread evidence fits the Davidson and Noble model of early symbolism and 'depiction' requires strong explanation.

12) Some burials at least (at Shanidar, La Ferrassie, La Chapelle-aux-Saints) apparently indicate a strongly flexed position, suggesting a possible secondary use of thongs in what may have been a primary 'symbolic' act. The *thongs*, in such a use, would not have been symbolic; the primary *behaviour* may have been. Because of the debate now going on it may be sufficient to add these suggestions of flexing and binding as a corollary of burial to the well-known and debated flowers found in the Shanidar burial. I add them to make a theoretical point, arguing neither for nor against their presence, but merely because flowers would not have been a product of symbolic manufacture, but their use may have been symbolic. Such suggestions open up the problem of the nature of symboling behaviour, which have been raised in different ways throughout this comment. These questions are perhaps as relevant as the debate over the validity of one or another datum.

13) For researchers in world rock art I note that wherever rock imagery occurs it almost always represents an aspect of ritual behaviour produced in conjunction with or in relation to various forms of 'perishable' symbolic, ceremonial and ritual thought and behaviour. It is not the image alone, therefore, that is relevant. Rock art images often

include representations or depictions from a wide range of symbolic systems or symbolic realms in the culture (few of which may be 'entoptic'). Rock art itself, therefore, merely represents one symbolic mode among the many that reside in a culture and must have accompanied or supported the rock imagery. As a cultural manifest, rock art would therefore be presumably rather late in human development, since it is usually an aspect of and a reference to other symboling systems. It occurs late even in the European Upper Palaeolithic where significant cave 'art', the model for so much theory and discussion about the beginnings of art and rock art during this century, appears in the Franco-Cantabrian region only after many thousands of years, long after other forms of ritual and symbol are in the record. In most parts of Upper Palaeolithic Europe, and in other parts of the world at this time, it never appears at all. That rock art does not appear among archaic or pre-anatomically modern cultures does not mean, therefore, that ritual, symbolic and ceremonial behaviour did not (or as Davidson would probably have it, *could not*) occur in other forms.

One must add to this the fact that other forms of 'rock art' probably preceded the production of true rock art imagery. The collection and curation of stones and exotics with odd or indicative shapes may have been precursive to later depictive traditions. Natural shapes and forms are richly mythologised in all 'primitive' cultures of which we have records and are, at times, more important in the culture than those manufactured by hand. As is well known, the Australian, American and Asian landscapes are dotted with a profusion of mythologised natural forms. The geography of a place is itself often a 'myth'; and the 'depiction' that is seen or read into such natural forms is richly mythologised even though there was no human production of the extant form. Ceremonies often occur at these places because of the indicative form and the myths that come to surround them. Can these natural places and forms be considered as 'rock art'? Or can they be considered as precursive of rock art? Can they be considered as 'depictive' in Davidson's and Noble's naive definition of referential imagery? If these natural forms are subsequently *used* by the addition of a handprint or other sign, does the relevant 'depiction' now occur in the handprint or sign? Or does it occur in the natural form or image to which the image is attached? Does the attached image have to be 'depictive'? Similarly, the stone shapes from Berekhat Ram and Beçov (like the fossil exotics from Tata and Arcy), even if they were not manufactured, may have been 'symbolic' within their cultural contexts. The beginning of such natural forms in hominid cultural contexts is itself worthy of inquiry. Unfortunately these questions and problems have never been systematically addressed. They have often, in fact, been considered to be 'non-questions', incapable of inquiry or dismissed as inconsequential. I do not see how the subject of rock art, for instance, can be addressed without some attention paid to aspects of natural metaphorical form, or to the placement, position or orientation of such forms in the real or cultural landscape. The positions, for instance, are often as important as the images subsequently placed on the surfaces of these natural forms. An entire subdiscipline of archaeology has in recent years developed around the orientation of rock art and the positional orientation of the surfaces and symbols they contain. Where would such natural forms or orientations fall in Davidson's and Noble's model of intentionally produced 'depiction'? Any student of cultural and symbolic variability is constantly faced with such at first seemingly indeterminate questions. A proper inquiry will address the cognitive problems involved in hominid symboling, not in terms of a priori presumption, but in terms of the inherent *variability* found in such behaviour.

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REFERENCES

BAR-YOSEF, O. 1988. The Early Upper Palaeolithic in Levantine caves. In J. F. Hoffecker and C. A. Wolf (eds), *The Early Upper Palaeolithic: evidence from Europe and the Near East*, pp. 23-41. BAR International Series 437.

BEDNARIK, R. G. 1988. Comment *Rock Art Research* 5: 96-100.

BEDNARIK, R. G. 1990. An Acheulian haematite pebble with striations. *Rock Art Research* 7: 75.

BEYRIES, S. 1987. Quelques exemples de stigmates d'emmanchements observés sur des outils du Paléolithique Moyen. In *Le main et l'outil: manches et emmanchements préhistoriques*. Table ronde C.N.R.S. tenue à Lyon, 1984, sous la direction de D. Stordeur. Travaux de Maison de l'orient, No. 15, pp. 55-62. Bocard.

BINFORD, L. R. 1981. *Bones: ancient men and modern myths*. Academic Press, London.

BORDES, F. 1969. Os percé moustérien et os gravé acheuléen du Pech de l'Azé II. *Quaternaria* 11: 1-5.

BOSINSKI, G. 1967. *Mittelpaläolithische Funde im westlichen Mitteleuropa*. Bohlau Verlag, Köln und Graz.

BOSINSKI, G. 1985. *Der Neandertaler und seine Zeit*. Rheinland-Verlag GmbH, Köln.

BUDIL, I. 1990. Functional reconstruction of supra-laryngeal vocal tract of fossil human at Petralona. Paper delivered at the NATO conference on 'The origins of language', Cortona 1989. In press, Mouton, The Hague.

CACHEL, S. 1989. The theory of punctuated equilibria and evolutionary anthropology. *Journal of Social and Biological Structures* 12: 225-39.

COMBIER, J. 1990. De la fin du Moustérien au Paléolithique supérieur - Les données de la région Rhodanienne. In C. Farizy (ed.), *Paléolithique moyen récent et Paléolithique supérieur ancien en Europe*. Ruptures et transitions: examen critique des documents archéologiques, pp. 267-77. Actes du Colloque International de Nemours, 1988. Mémoires du Musée d'Ile de France No. 3.

CORRUCCINI, R. S. 1989. The forgotten Skuhl crania and the 'neopresapiens' theory. Paper presented at the 1989 AAPA annual meeting, Miami.

CORRUCCINI, R. S. 1990. Metrical reconsideration of Skuhl crania IV and IX and Border Cave 1 in the context of modern human origins. Unpubl. MS.

DAVIDSON, I. and W. NOBLE 1989. The archaeology of perception: traces of depiction and language. *Current Anthropology* 30: 125-55.

DAVIDSON, I. and W. NOBLE 1990. Tools, humans and evolution: the relevance of the Upper Palaeolithic. Paper presented at Wenner Gren Foundation Symposium No. 110, Cascais, Portugal.

DEBENATH, A. 1990. Le Paléolithique moyen récent et le début du Paléolithique supérieur dans le Bassin de la Charente. In C. Farizy (ed.), *Paléolithique moyen récent et Paléolithique supérieur ancien en Europe*. Ruptures et transitions: examen critique des documents archéologiques, pp. 223-33. Actes du Colloque International de Nemours, 1988. Mémoires du Musée d'Ile de France No. 3.

DEMARS, P.-Y. 1990. Les interstratifications entre Aurignacien et Châtelperronien a Roc-de-Combe et au Piage (Lot). In C. Farizy (ed.), *Paléolithique moyen récent et Paléolithique supérieur ancien en Europe*. Ruptures et transitions: examen critique des documents archéologiques, pp. 235-9. Actes du Colloque International de Nemours, 1988. Mémoires du Musée d'Ile de France No. 3.

FREEMAN, L. and J. ECHEGARY 1971. *Cueva Morin: excavaciones 1966-1968*. Santander.

GARGETT, R. 1989. Grave shortcomings. The evidence for Neanderthal burial. *Current Anthropology* 30: 157-77, 184-90.

GAUTIER, A. 1986. Une histoire de dents: les soi-disant incisives travaillées du Paléolithique moyen de Sclayn. *Helinium* 26: 177-81.

GENESTE, J.-M. 1986. Systèmes d'approvisionment en matiéres premières au Paléolithique moyen et au Paléolithique supérieur en Aquitaine. Paper delivered at the international colloquium 'L'Homme de Neanderthal', Liège.

GENESTE, J.-M. 1990. Développement des systèmes de production lithique au cours du Paléolithique moyen en Aquitaine Septonrionale. In C. Farizy (ed.), *Paléolithique moyen récent et Paléolithique supérieur ancien en Europe*. Ruptures et transitions: examen critique des documents archéologiques, pp. 203-13. Actes du Colloque International de Nemours, 1988. Mémoires du Musée d'Ile de France No. 3.

GOUEDO, J.-M. 1990. Les technologies lithiques du Châtelperronien de la couche X de la Grotte du Renne d'Arcy-sur-Cure (Yonne). In C. Farizy (ed.), *Paléolithique moyen récent et Paléolithique supérieur ancien en Europe*. Ruptures et transitions: examen critique des documents archéologiques, pp. 305-8. Actes du Colloque International de Nemours, 1988. Mémoires du Musée d'Ile de France No. 3.

HENRI-MARTIN, G. 1947. Un disque en calcaire dans le gisement Moustérien de la Quina. *Société Préhistorique Française* 5-6: 1-4.

HOLDOWAY, S. 1989. Were there hafted projectile points in the Mousterian? *Journal of Field Archaeology* 16: 79-85.

JACOB-FRIESEN, K. H. 1956. Eiszeitliche Elefantenjäger in der Lüneburger Heide. *Jahrbuch des Römisch-Germanischen Zentralmuseums Mainz* 3: 1-22.

KEELEY, L. H. 1977. The function of Paleolithic flint tools. *Scientific American* 237(5): 108-26.

KEELEY, L. H. 1980. *Experimental determination of stone tool uses*. University of Chicago Press, Chicago.

KUHN, S. L. 1990. Late Mousterian technology and foraging patterns at Grotta Breuil, Italy. *Anthroquest* 42: 12-5.

LAITMAN, J. T. 1983. The evolution of the hominid upper respiratory system and implications for the origin of speech. In E. de Grolier (ed.), *Glossogenetics: the origin and evolution of language*, pp. 63-90. Harwood Academic Press, Paris.

LEROI-GOURHAN, Arl. and A. LEROI-GOURHAN 1965. Chronologie des Grottes d'Arcy-sur-Cure (Yonne). *Gallia Préhistoire* 17(2): 1-64.

LINDLY, J. M. and G. M. CLARK 1990. Symbolism and modern human origins. *Current Anthropology* 31: 233-61.

MANIA, D. and V. TOEPFER 1973. *Königsaue. Gliederung, Ökologie und mittelpaläolithische Funde der letzten Eiszeit*. Berlin.

MARSHACK, A. 1976. Some implications of the Paleolithic symbolic evidence for the origins of language. *Current Anthropology* 17: 274-82.

MARSHACK, A. 1984. The ecology and brain of two-handed bipedalism: an analytic, cognitive and evolutionary assessment. In H. L. Roitblat, T. G. Bever and H. S. Terrace (eds), *Animal Cognition*, pp. 491-511. Erlbaum, Hillsdale, N.J.

MARSHACK, A. 1985. *Hierarchical evolution of the human capacity: the Paleolithic evidence*. 54th James Arthur Lecture on 'The evolution of the human brain', 1984. American Museum of Natural History, New York.

MARSHACK, A. 1988. The Neanderthals and the human capacity for symbolic thought: cognitive and problem-solving aspects of Mousterian symbol. In M. Otte (ed.), *L'Homme Neanderthal: Actes du Colloque International, 1986, Liège*. Vol. 5, *La pensée*, pp. 57-91. Université de Liège.

MARSHACK, A. 1989a. Evolution of the human capacity: the symbolic evidence. *Yearbook of Physical Anthropology* 32: 1-34. The invited luncheon lecture presented at the annual meeting of the American Association of Physical Anthropologists, 1988, Kansas City.

MARSHACK, A. 1989b. The female image: a 'time-factored' symbol. (A study in style and modes of image use in the Upper Palaeolithic.) The keynote address presented at the conference 'Palaeolithic art', of The Prehistoric Society of the University of Oxford, 1989, Oxford University. In press. *Proceedings of the Prehistoric Society*, Vol. 59.

MARSHACK, A. 1990a. Early hominid symbol and evolution of the human capacity. In C. Stringer and P. Mellars (eds), *The human revolution: behavioural and biological perspectives on the origins of modern humans*, Vol. 2, pp. 457-98. Edinburgh University Press, Edinburgh.

MARSHACK, A. 1990b. L'Evolution et la transformation du décor du debut de l'Aurignacien au Magdalénien final. In J. Clottes (ed.), *L'Art des objets au Paleolithique. Tome 2: Les voies de la recherche*, pp. 139-62. Proceedings of international colloquium in Foix-le Mas d'Azil, 1987. Ministère de la Culture, de la Communication, des Grands Travaux et du Bicentenaire, Picard, Paris.

MARTIN, H. 1907-1910. *Récherches sur l'évolution du Moustérien dans le gisement de la Quina (Charente). Industrie osseuse*. Vol. 1. Schliecher Frères, Paris.

McBURNEY, C. 1967. *The Haua Fteah (Cyrenaica) and the Stone Age of the south-east Mediterranean*. Cambridge.

MINUGH-PURVIS, N. 1988. Patterns of craniofacial growth and development in Upper Pleistocene hominids. Unpubl. Ph.D. thesis, Department of Anthropology, University of Pennsylvania.

MINUGH-PURVIS, N. 1990. Ontogenic variation in the mid-face of Upper Pleistocene *Homo sapiens*. Paper presented at the 1990 AAPA annual meeting. Abstract, Program p. 270.

NARR, K. J. 1951. Alt- und mittelpaläolithische Funde aus rheinischen Freilandstationen. *Bonner Jahrbuch* 151: 5-47.

OAKLEY, K. P., P. ANDREWS, L. H. KEELEY and J. D. CLARK 1977. A reappraisal of the Clacton spearpoint. *Proceedings of the Prehistoric Society* 43: 13-30.

TABORIN, Y. 1990. Les prémices de la parure. In C. Farizy (ed.), *Paléolithique moyen récent et Paléolithique supérieur ancien en Europe*. Ruptures et transitions: examen critique des documents archéologiques, pp. 335-44. Actes du Colloque International de Nemours, 1988. Mémoires du Musée d'Ile de France No. 3.

TILLIER, A.-M. 1988. Croissance et developpement chez les Neandertaliens. Dossiers Histoire et Archéologie, special issue: *L'Homme de Neandertal* 124: 34-9.

WHITE, R. 1989a. Production complexity and standardisation in early Aurignacian bead and pendant manufacture: evolutionary implications. In C. Stringer and P. Mellars (eds), *The human revolution: behavioural and biological perspectives on the origins of modern humans*, pp. 366-90. Edinburgh University Press, Edinburgh.

WHITE, R. 1989b. Visual thinking in the Ice Age. Scientific American 261(1): 92-9.

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!SCIENCE, HYPOTHESIS TESTING AND PREHISTORIC PICTURES

DANIEL TANGRI

FURTHER COMMENT

By BERNARD M. J. HUCHET

Tangri has raised the important issue of hypothesis testing in rock art studies and, more generally, archaeology. I agree with the author that testable hypotheses should be formulated and that the kind of confirmation approach discussed by Tangri is inadequate. However, I believe that the most adequate approach is one which involves the use of multiple working hypotheses rather than refutation.

In his *RAR* article (1989a) and elsewhere (1989b), Tangri restricted his discussion of confirmation to an approach (one may conveniently call *strict confirmation*) involving the testing of one hypothesis only, the hypothesis deemed most probable following a consideration of prior probabilities. Tangri, however, did not discuss the nature of a second approach involving confirmation: the simultaneous testing of multiple hypotheses. This is most evident in his paper on confirmation (1989b) in which the examples of confirmation he gave involve the evaluation of a single hypothesis, namely, that Aboriginal brains were more primitive than those of other races. The time has come to discuss the advantages and problems associated with refutation and the use of multiple working hypotheses.

This last method is termed here the *C/R approach* (cf. Huchet 1990); C/R stands for confirmation and refutation. The rationale for proposing this term is that the approach leads to confirmation of the hypothesis deemed most plausible, as well as to the dismissal (or refutation) of all alternative hypotheses. Although the approach involves the testing of hypotheses against data that are most likely to *confirm* each hypothesis, the *aim* of confirming is regarded here as immaterial. Rather, the *result* obtained is what matters most: i.e. to *confirm* as well as to *refute*. Of course, with the C/R approach the path to refutation differs from that followed in Tangri's refutation approach insofar as, with the former, one is not looking for data that are most likely to refute an hypothesis. Nevertheless, the aim of researchers using a C/R approach is clearly to discriminate between plausible and inadequate hypotheses. As such it is improper to classify this approach solely under a confirmation label.

One may begin the examination of the pros and cons of the two approaches with a discussion of *logic* since much of Tangri's argument in favour of refutationism revolved around it. On logical grounds, Tangri argued that the only viable approach to hypothesis testing is one based on refutation. This argument is very reminiscent of Popper's (e.g. 1963) views. That logic assumes a paramount role in Tangri's argument is reflected in his criticism of confirmation on the basis that it involves reliance on analogy and, thus, induction in the consideration of prior probabilities (Tangri 1989a: 85). Tangri (1990: 72) also criticised the C/R approach on the basis of induction. Of course nobody would deny that, (a) it is impossible to logically derive conclusions using these two hypothesis testing approaches, and (b) in theory, refutationism is the only approach capable of yielding logically valid conclusions.

However, there are several practical problems in successfully applying this logic of refutation to test archaeological hypotheses due to the nature of archaeology and the complexity of humans and their society. That is, it is generally difficult or impossible to account for all relevant auxiliary hypotheses and antecedent conditions tied to an hypothesis. This stems from problems in identifying these two elements and adequately recording and

studying all relevant empirical data with minimal subjectivity (e.g. Salmon 1976: 377; Kelley and Hanen 1988: 79-80; see also Palmer 1989: 91). If one could ascertain that all relevant antecedent conditions and auxiliary hypotheses had been identified and that all relevant facts had been recorded and properly analysed, then one would be justified to rely on the strict logic advocated by refutationists, and be assured that refuting or altering an hypothesis on the basis of one or few disconfirming instances is the adequate thing to do. The problem is that we have no guarantee that the conclusions reached by refutationists on a given hypothesis are more reliable than those reached by confirmationists or via the use of the C/R approach. Considering the serious practical limitations involved in any hypothesis testing exercise, a single instance of unsupporting evidence should not be regarded as evidence that the hypothesis subjected to evaluation has been refuted. In practice there are too many ways to explain why data do not support the hypothesis. For instance, negative evidence could refute any of the premises on which the hypothesis is based or one of its auxiliary statements rather than the hypothesis itself (e.g. Ryan 1970: 63; Shanks and Tilley 1987: 41-2). Regardless of the approach used, what is at least as important as logic is the researcher's ability to identify auxiliary hypotheses and antecedent conditions, take these into consideration, and attempt to reduce subjectivity at all levels, from recording data through to the final evaluation of the results of testing. There are no strict rules of logic available to carry out such tasks. Thus, to view confirmation and C/R approaches as inadequate relative to refutation strictly on a logical basis is to subscribe to a form of scientific empiricism that is too restrictive for archaeologists.

Other aspects of hypothesis testing are also important. The *quantity of evidence* that supports or contradicts an hypothesis should play an important role in the acceptance of the results of hypothesis testing. It is unsafe, for various practical reasons discussed above, to maintain that a single piece of evidence refutes an hypothesis. Indeed, it may well be that the researcher who presented the evidence either deliberately twisted the facts or recorded and analysed them inadequately. The errors or dishonesty which led to the production of counter-evidence and refutation may not be detectable. A classic example is that of the Piltdown man forgery, which led to the refutation of the notion that Neanderthal man was a direct ancestor of modern man. It took 40 years to discover that the Piltdown skull was a forgery (Millar 1972). It is thus safer to refute or confirm an hypothesis on the basis of several pieces of evidence, particularly when the data have been gathered by many researchers over many years and from different geographic areas and fields of knowledge. If such is the case, it is unlikely that the integrity of all the researchers can be called into question.

It is also important to consider the contributions that the C/R and refutation approaches can make to our field. In his *RAR* paper, Tangri noted that refutationism can only be used as a tool for criticism. He sees the worth of refutation in its ability to diminish or negate the political power of hypotheses (Tangri 1989a: 93). Tangri (1989c: 62) also stated elsewhere: 'my concern was never with plausibility'. As such, he is not interested in attempting to identify and then test most likely hypotheses to explain archaeological data. By virtue of his strict epistemological impositions and the purity he finds in the logic of refutation, Tangri is only interested in what is false. 'This is because I do not believe there is any point in trying to detect the most likely or realistic hypothesis' (Tangri 1989c: 62). But we, as archaeologists, constantly need confirmed hypotheses to 'serve as lawlike statements in our explanations' (Kelley and Hanen 1988: 36). In addition,

> If science is to progress, we must be able to accept certain hypotheses, even if only provisionally; and surely it is appropriate that hypotheses worthy of acceptance should be those that are confirmed and corroborated to some appropriate degree. Our hope in science is that confirmed hypotheses will have explanatory power. (Kelley and Hanen 1988: 226)

Many aspects of archaeology must involve the establishment of plausible hypotheses. For example, it is necessary to build chronologies, reconstruct patterns of prehistoric settlement and subsistence, and so on. Contract and academic archaeologists are not so concerned with the falsification of hypotheses. Their business is to formulate plausible hypotheses for the benefit of developers, the scientific community and/or the general public, depending on the aim of the project. This does not mean that archaeology lies largely outside the realms of science. Would Tangri ever be able to write a thesis on a regional prehistory or a contract report using solely or primarily refutationism? If the answer is 'No', then refutationism is of little use to archaeologists. This is where the C/R approach has a distinctive advantage over refutation: not only does the simultaneous testing of hypotheses enable the dismissal or refutation of certain hypotheses, it also provides a means to establish, even if only provisionally, plausible hypotheses.

It is also relevant to note that one of the advantages of Tangri's refutationism is also shared by the C/R approach. Tangri (1989a: 88) advocated that when an hypothesis is refuted it must be *modified, reassessed or abandoned*. This can also be done with a C/R approach. One can always try to explain data that do not fit expectations set out in the implication statements. Also, Tangri (1989b: 31) pointed out, regarding refutation, that 'data that contradict an hypothesis are not deemed to be implausible or irrelevant if not outweighing data that support an hypothesis. Rather, they are deemed crucial'. With the C/R approach, the amount of unsupporting evidence, regardless of whether or not it outweighs confirmation data, is always taken into consideration when establishing which hypothesis is plausible and which hypotheses must be rejected.

A word is also necessary about Tangri's (1989a: 84) claim that the *interpretation* of data represents one of the main aims of the New Archaeologists. This assertion is incorrect. If one examines some of the sources Tangri quoted in support of his claim, one finds that the ultimate aim they advocated is the *explanation* of similarities and differences between 'cultures' (e.g. Binford 1968; Fritz and Plog 1970). For instance, Binford (1967: 10) considered the role that interpretation plays in archaeology as being rather different from, if not opposed to, that of explanation: 'I do not view interpretations, or syntheses of interpretations, as an end product of our investigations', and: 'It is maintained here that as anthropologists we have a task quite different; we seek to explain cultural differences and similarities'. In addition, the word *explanation*, not interpretation, appears in the title of several of the textbooks brought out by the New Archaeologists (e.g. Watson, LeBlanc and Redman 1971, 1984). Many other references could be cited to show that Tangri has misinterpreted the main manifestos of scientific archaeology (see Huchet 1990: 33). In archaeology, *explanation* has a meaning that is quite different from that of *interpretation*. The former is generally concerned with answering the question WHY. This can be done by relying on solid facts, scientific methods such as the use of law-like generalisations, and a minimal amount of interpretations. To interpret, however, implies to *signify* or *give meaning to*, and is associated with highly speculative or subjective assumptions rather than facts. Interpretations would no doubt be regarded as unscientific by the New Archaeologists.

In conclusion, Tangri argued that refutationism is the only viable approach to test hypotheses because it involves 'tight' logic. I have argued instead that there are other aspects besides logic which must also be considered in the choice of an approach to hypothesis testing. If one adopts a broader epistemological stance than Tangri's, it becomes clear that the use of the C/R approach is most appropriate. I agree with Palmer (1989: 92) that 'we must be content to follow less stern epistemologies' than Tangri's because, as Nordbladh (1989: 90) pointed out, a rigorous scientific attitude leads to failure. Rather than attempt to model archaeology on philosophy, it is time we asked what practical solutions can philosophy offer to archaeologists?

The C/R approach ought to be considered more often by archaeologists since, contrary to Malmer's (1990: 71) presumption, it has hitherto not been the major hypothesis testing approach used by Australian archaeologists (Huchet 1990: 97).

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REPLY

By DANIEL TANGRI

Huchet's Comment is a welcome addition to the recent debate in *RAR* on hypothesis testing in rock art studies. On the one hand, it adds to the more erudite sections of the correspondence. On the other hand, it brings up some points that appear to need clarification.

Huchet argues that I was criticising a 'strict' form of confirmation in which only one hypothesis is tested. He distinguishes this from a preferred method of confirmation based on the use of multiple working hypotheses. Actually, I was criticising the form of confirmation advocated by Salmon (1982) and Smith (1977). Both of these authors are committed to the use of multiple working hypotheses. Salmon is admittedly vague on the point, but does model her method around the testing of several alternative hypotheses (1982: 55-6). Indeed, her prior probability approach is based on the concept of proving one hypothesis more plausible than another (Salmon 1982: 42-5). As such, it clearly includes the use of multiple working hypotheses.

Smith makes his commitment to their use more explicit. He argues:

> For any defined problem area it is advisable, for a number of reasons, to formulate a series of ... alternative hypotheses ... Working with a number of conflicting alternative hypotheses is also necessary because in archaeological reasoning the issue is not and can not be one of establishing or confirming the strength of a single hypothesis, but rather attempting to demonstrate which of a number of alternative, tentative solutions seems most correct (Smith 1977: 610).

Consequently, Huchet's distinction between 'strict' confirmation as I discussed it and his more open confirmation is unfounded. It should also be noted that I connected Smith's confirmation with multiple working hypotheses in my initial essay (Tangri 1989a: 84). The major point, however, is that multiple working hypotheses are often used to make refutation difficult. Our assessment approaches are often framed within a systemic view, and as such various hypotheses are commonly seen to be interrelated. This is a curious form of reductionism, and may negate low-level refutation by forcing all hypotheses to be considered equally relevant to a particular problem. The use of alternative hypotheses might be improved by framing them within a hierarchical mode of explanation (cf. Fletcher in press a, b), in which hypotheses are related to specific aspects of a problem and all propositions are not seen to be crucially linked. To use an analogue, propositions about past human modes of settlement as evidenced by rock paintings may be assessed independently of propositions about the dating of the art. The latter are admittedly relevant in setting the limits of the question, but should not preclude refutation of higher-level hypotheses if unanswered.

This leads us to Huchet's method. Following Kelley and Hanen (1988), he argues that normal confirmation based on testing several hypotheses should be used to determine the most plausible hypothesis: 'the aim of researchers using a C/R approach is clearly to discriminate between plausible and inadequate hypotheses'. To deem hypotheses inadequate both confirmation and refutation would be used. Huchet terms his method the C/R approach, as it synthesises both methods.

Huchet's Comment is of necessity brief, and consequently certain issues are unclear. The precise working of the C/R approach are among these issues. Huchet noted that confirmation has several flaws, and that refutation, though difficult to implement, has some strengths. He argues his approach incorporates the strengths of both methods and allows one to determine plausible hypotheses. How it does this is unclear, as he notes that 'one is not looking for data that are most likely to refute an hypothesis'. One is entitled to wonder how plausible hypotheses are then to be discovered, given that Huchet argued that confirmation has major flaws. At the risk of misunderstanding that writer, it seems that he may have claimed that paying attention to disconfirming data will balance the weight of confirming data in hypothesis assessment. This is an accepted approach (cf. Kelley and Hanen 1988), but it negates the crucial point underlined by Murray and Walker (1988), that the assessment of disconfirming data should follow their binomial condition. This states that an hypothesis will be deemed applicable (rather than likely) if, and only if, it is not contradicted by data. Huchet did not indicate if hypotheses that are partly disconfirmed are still to be treated as likely. I would argue that such hypotheses need not be abandoned except in clear circumstances. They should, however, be modified, and theoretical limitations specified.

It is undoubtedly the case, as Huchet claims, that refutation is difficult (cf. Bednarik 1990). It is also a sociological reality that archaeologists are unlikely to modify or, less likely, abandon an hypothesis unless it is contradicted more than once. Indeed, archaeologists may ignore numerous contradictions unless that evidence is explained by an alternative hypothesis or theory. Even here, that theory may be ignored or downgraded if it is at odds with an established tradition within the academic discipline of archaeology. This is precisely why refutation is a useful tool for criticism.

This introduces Huchet's subsequent point, that I portrayed refutation as a mere critical device, with an interest only in what was refuted (I do not accept his claim that I was interested in what was false). Huchet used an out-of-context quote to make his point. The quote is from a passage discussing plausibility, and arguing that no testing method may actually allow us to define hypotheses as true or even likely. If we accept that our ideas are framed within ideological bounds, and that we cannot be sure how valid our ideas are because of this imposition of ideology, confirmation suffers as no one hypothesis can be soundly shown to be more plausible than others. Refutation, if used primarily as a critical tool, ignores the problem posed by plausibility in this regard.

This is not to say, however, that we must abandon hypotheses that are not yet refuted, or theories that have been modified. Archaeologists are likely to use hypotheses that have not been refuted, or that have been modified; refutation does not preclude such usage. I view the establishment of regional prehistories or chronologies as the result of an interplay between data and theory, with theories being modified and hypotheses being refuted or supported by new data (cf. Gräslund 1988). In this case refutation simply describes low-level archaeological methodology. It operates rather like confirmation, and both methods are commonly used in ordinary hypothesis assessment. Refutation, then, seems not to act as Huchet claims it does, and in that sense his criticisms are more semantic than sound.

Finally, Huchet claims that I portrayed the New Archaeologists as interested in the simple interpretation of data, rather than its explanation. Inasmuch as the New Archaeologists were interested in explaining patterns based on social or economic systems that had to be interpreted from the data, they were using interpretive hypotheses. Their aims are therefore incidental to this particular discussion.

Huchet's article has provided a welcome opportunity for me to clarify my arguments. *Rock Art Research* is to be applauded for providing the sort of forum in which such extended discussions can occur, thus permitting greater development and understanding of relevant views. Such a forum has been essential and invaluable in my case.

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REFERENCES

BEDNARIK, R. G. 1990. Comment on D. Tangri, '!Science, hypothesis testing and prehistoric pictures'. *Rock Art Research* 7: 69-70. [DT]
BINFORD, L. R. 1967. Smudge pits and hide smoking: the use of analogy in archaeological reasoning. *American Antiquity* 32: 1-12. [BH]
BINFORD, L. R. 1968. Archaeological perspectives. In S. R. Binford and L. R. Binford (eds). *New perspectives in archaeology*, pp. 5-32. Aldine, Chicago. [BH]
FLETCHER, R. J. in press a. Time perspectivism, annales and the potential of archaeology. In J. B. Knapp (ed.), *Archaeology, annales and ethnohistory*. Cambridge University Press, Cambridge. [DT]
FLETCHER, R. J. in press b. Refutation and tradition: an uneasy relationship. *Australian Archaeology* 32. [DT]
FRITZ, J. M. and F. T. PLOG 1970. The nature of archaeological explanation. *American Antiquity* 35: 405-12. [BH]
GRÄSLUND, B. 1988. *The establishment of prehistoric chronology*. Cambridge University Press, Cambridge. [DT]
HUCHET, B. M. J. 1990. Modelling and Australian prehistory. Unpubl. B.A. (hons) thesis, University of New England, Australia. [BH]
KELLEY, J. H. and M. P. HANEN 1988. *Archaeology and the methodology of science*. University of New Mexico Press, Albuquerque. [BH] [DT]
MALMER, M. P. 1990. Comment on D. Tangri, '!Science, hypothesis testing and prehistoric pictures'. *Rock Art Research* 7: 69-70. [BH]
MILLAR, R. 1972. *The Piltdown men*. Granada Publishing, London. [BH]
MURRAY, T. and M. J. WALKER 1988. Like WHAT? A practical question of analogical inference and archaeological meaningfulness. *Journal of Anthropological Archaeology* 7: 248-87. [DT]
NORDBLADH, J. 1989. Comment on D. Tangri, '!Science, hypothesis testing and prehistoric pictures'. *Rock Art Research* 6: 90. [BH]
PALMER, K. 1989. Comment on D. Tangri, '!Science, hypothesis testing and prehistoric pictures'. *Rock Art Research* 6: 90-2. [BH]
POPPER, K. R. 1963. *Conjectures and refutations: the growth of scientific knowledge*. Routledge and Kegan Paul, London. [BH]
RYAN, A. 1970. *The philosophy of the social sciences*. Macmillan, London. [BH]
SALMON, M. H. 1976. 'Deductive' versus 'inductive' archaeology. *American Antiquity* 41: 376-81. [BH]
SALMON, M. H. 1982. *Philosophy and archaeology*. Academic Press, New York. [DT]
SHANKS, M. and C. TILLEY 1987. *Re-constructing archaeology*. Cambridge University Press, Cambridge. [BH]
SMITH, B. D. 1977. Analogical Inference and inductive confirmation. *American Anthropologist* 79: 598-617. [DT]
TANGRI, D. 1989a. !Science, hypothesis testing and prehistoric pictures. *Rock Art Research* 6: 83-95. [BH] [DT]
TANGRI, D. 1989b. Early physical anthropology, confirmation, and Australian Aboriginal brains. *Australian Archaeology* 28: 26-34. [BH]
TANGRI, D. 1989c. The hand that mocked: a rejoinder to Murray. *Australian Archaeology* 29: 61-6. [BH]
TANGRI, D. 1990. Reply: Refutation and prehistoric pictures. *Rock Art Research* 7: 71-3. [BH]
WATSON, P. J., S. LeBLANC and C. L. REDMAN 1971. *Explanation in archaeology*. Columbia University Press, New York. [BH]
WATSON, P. J., S. LeBLANC and C. L. REDMAN 1984. *Archaeological explanation: the scientific method in archaeology*. Columbia University Press, New York. [BH]

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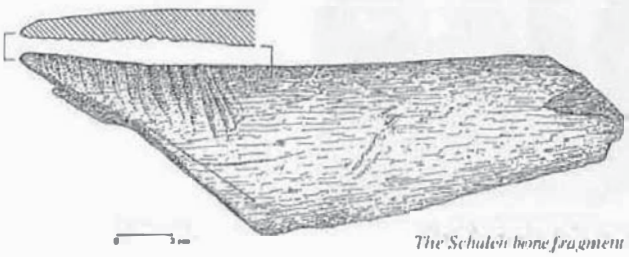

The Schulen bone fragment

Rock Art Research 1990, Vol. 7, No. 2, pp. 125 - 132.

MOUSTERIAN SKIFFLE? NOTE ON A MIDDLE PALAEOLITHIC ENGRAVED BONE FROM SCHULEN, BELGIUM

DIRK HUYGE

COMMENT

Carnivore traces or Mousterian skiffle?

By FRANCESCO D'ERRICO

The Schulen bone fragment studied by Dirk Huyge would have deserved a more systematic analysis, or more demonstrative illustrations. In particular, the interpretation of several traces visible on this fragment as the result of some human activity needs to be discussed.

The author claims that the oblique fracture on the top of the fragment was 'undeniably produced by human agency' and that a groove technique had been used to produce it. Yet no evidence is offered to support this claim. Examination of the photographs does not reveal to me any of the traces characteristic of this technique, which should be clearly visible on a 2 cm-thick bone. The edge does not seem to be at all different from the fracture edges of bone flakes found on both anthropic and non-anthropic bone assemblages lacking the mentioned technique.

Huyge states also that, 'aligned with the upper part of this fracture a slight cutting trace, obviously caused by a sharp-edged tool, is clearly visible on the outer surface'. The fine incision may well be due to a tool, but this cannot be determined from the photographs (see for instance Shipman 1981; Behrensmeyer et al. 1986; Olsen and Shipman 1988). No other microscopic evidence is supplied or described in order to support this claim.

Huyge also thinks that the anthropic origin of the grooves visible on the upper part of the object is 'beyond doubt'. Two reasons are given for this: they are 'in no way similar to the irregular carnivore gnawing traces that have been observed on bone material from recent hyena lairs and archaeological sites'; and they 'display in their centre multiple cutting scratches parallel to the direction of the grooves'. Nevertheless, these elements seem insufficient to claim an anthropic origin of the grooves and other possibilities must be taken into account.

The constant direction of the incisions is no proof of a human action: in fact the contrary can also be true. Holding and rotating a long bone fragment by a carnivore in its jaws may produce linear incisions which are often parallel or subparallel, and perpendicular to the main axis of the bone or the bone flake (see for example Binford 1981: 48). The direction of the incisions is the result of a transverse position of the bone relative to the motion of the carnivore's teeth. Because of their regular character and because they are very close, these incisions, called 'scoring' by Binford (see also Pei 1938: Pl. 15; Haynes 1980), can sometimes be mistaken for cut marks.

On the photographs published in Huyge's paper the incisions look fairly wide, with sloping sides and flat or rounded bottom. Such a morphology renders them more similar to traces produced by repeated action of a carnivore's teeth than to the action of a lithic cutting edge. The depth of the grooves on the Schulen bone is not systematically reached on bones gnawed by carnivores, but it is perfectly compatible with this action. To demonstrate this point I present a fragment of a long bone (Fig. 1) from Montgaudier Cave (Charente, France; in the collection of the Institut de Paléontologie Humaine, Paris). One end (Fig. 1B) shows rounded and polished edges as well as an extensive, slightly smoothed, pitting testifying to a carnivore's action. The centre of the fragment bears numerous 'microcups', probably due to chemical alteration. The other end (Figure 2A) shows

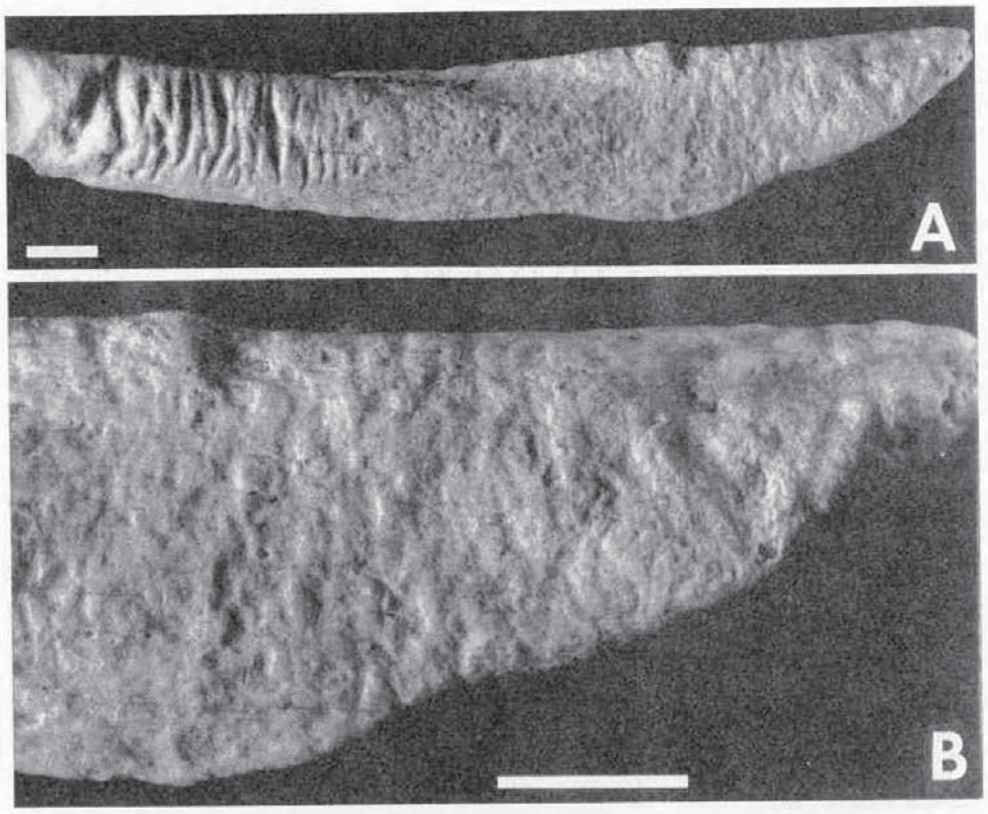

Figure 1.
A: Bone fragment from Montgaudier Cave (Charente, France; now in the collection of the Institut de Paléontologie Humaine, Paris). B: Surface showing an extensive, slightly smoothed pitting, indicating a carnivore's action. Scales 1 cm long.

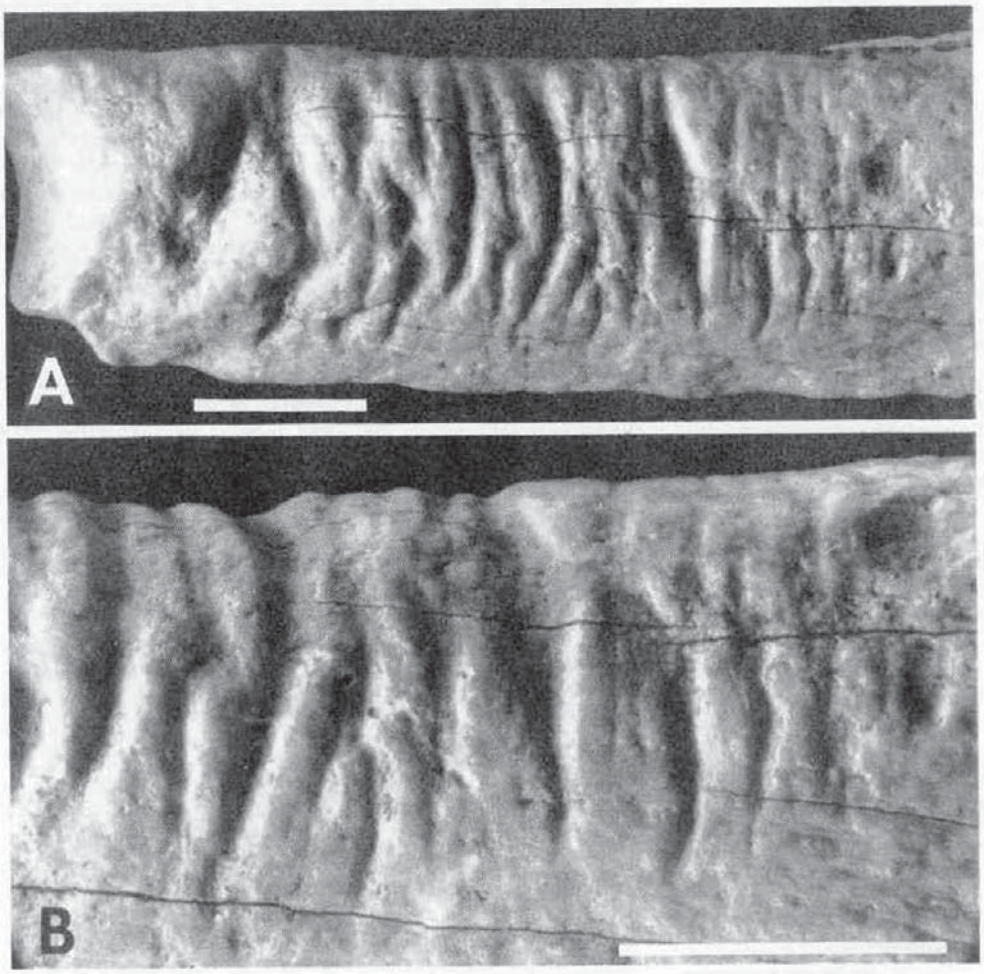

Figure 2.
A: Surface of bone fragment from Montgaudier Cave, evincing grooves subparallel and perpendicular to the main axis of the fragment. B: Detail showing the smoothing and polishing of this zone. Scales 1 cm long.

deep grooves which are subparallel and perpendicular to the main axis of the bone. These traces are probably the consequence of the to-and-fro action of a carnivore's teeth. Some pits, visible near the grooves, confirm this hypothesis. Striations parallel to the direction of the grooves are visible in the centre of some of them (Fig. 2B). Therefore the presence of these scratches, evoked by Huyge, does not necessarily demonstrate an anthropic origin of the grooves.

Concerning the shine and smoothing observed on the Schulen bone, both their aspect and distribution on the bone surface are perfectly compatible with the result of carnivore action. This polish is well known by specialists in taphonomic studies. 'In the event that a mashed edge is then licked repeatedly it may obtain a rounded polished appearance ... such rounded and polished edges are regular elements in both dog yard and wolf den assemblages' (Binford 1981).

On the Schulen specimen, the smoothing decreases from the end to the centre, which Huyge interprets as indicating re-use of a tentative 'scraped idiophone' as a 'beak-shaped tool' by Mousterians. However, the decrease in polish is typical of most of the bones gnawed by carnivores. It is probably due to the fact that carnivores effect a stronger chemical action on the terminal part of the bone.

The fact that intensive gloss, 'obviously caused by rubbing over the protruding ribs' in the opinion of the writer, is developed on these protruding parts is tentatively interpreted as wear produced by the use of the object as a 'scraped idiophone'. A similar wear can be observed at corresponding locations on the Montgaudier bone (Fig. 2B). This wear appears to be the logical consequence of a carnivore's action, but other factors may have contributed to produce this sheen on the Schulen bone.

Although the writer provides no information on the taphonomic context of the find (degree of fragmentation of bone assemblage, carnivore traces etc.), carnivore action seems to be compatible with the evidence from the site. The presence of flint artefacts, 'heavily patinated, wind or water polished', shows that bones could have been exposed on the surface and accessible to animals or other modifying agents.

In conclusion, it is not impossible that grooves and other traces on the Schulen bone may have an anthropic origin, but evidence presented to support this hypothesis seems largely inadequate. Using the data available in Huyge's article, grooves, smoothing and sheen may all be interpreted as the result of a carnivore's action. If this hypothesis were to be confirmed by a new analysis of the object, one would have to admit that the grooves near the extremity must have been completely smoothed at the time the bone was abandoned by the carnivore. Therefore the Mousterians could not have picked up the object after the carnivore's action and used it as an idiophone, because the grooves required for such use would have already disappeared almost entirely.

In my opinion, the analysis and interpretation of the object presented in this paper both show how it is possible to interpret as the result of human behaviour evidence that should not necessarily be attributed to this cause. The epistemological approach adopted in the course of an analysis and the subsequent paper requires utmost attention from the analyst. Often in the past, but also recently (as exemplified by a part of Huyge's bibliography), cases of wishful thinking were transmuted into 'scientific' reality without adequate preliminary criticism of available evidence.

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REPLY

Blades for a set of false teeth?
By DIRK HUYGE

I am grateful to Dr d'Errico for his profound and severe critique of my article on the Schulen bone; the more so as the discussion he initiates focuses on methodological (or 'epistemological') aspects, and not on the interpretation which is, after all (as clearly stated in my abstract), an exploratory suggestion. It is just an idea. (And one is allowed to have ideas, isn't one?)

However, I must say that d'Errico has not convinced me to write a *mea culpa* of a wishful thinker. In the first place, I do not quite see how the analysis of the bone could have been more systematic. Frankly, I suspect that d'Errico disapproves of the adopted (macroscopical) approach because his own *approche méthodologique* to the study of bone surfaces (by light microscopy and scanning electron microscopy on replicas and casts) has not been applied (see e.g. d'Errico and Giacobini 1985). I simply did not see the need for an elaborate microscopical study of the object as all the diagnostic features and traces are clearly visible to the naked eye. Magnifying them by means of a microscope would not at all have altered my ultimate interpretation. 1)

As d'Errico suggests, the article may indeed have required better illustrations. I thought that the selected photographs were sufficiently demonstrative. I must admit that the striations within the grooves, which are crucial to the argument, are not very clear in Figure 5 (*RAR* 7: 129). They are clearer on the original photograph than they appear in print (which is by no means a critique of the otherwise excellent technical quality of *RAR*).

When I claim that the oblique fracture on the top of the fragment has been undeniably produced by human agency, I refer of course to the cutting trace aligned with this fracture, which is essential to the argument. When d'Errico claims that it is not clear from my photographs that the incision is anthropic and caused by a tool, he is clearly playing the role of the devil's advocate. What else could have caused this deep trace with its V-shaped section, if not a sharp-edged tool? Can he offer a more plausible explanation for its occurrence? In his critique at least he does not do so, probably because there are no animals with blades for a set of false teeth which could have caused the mark. And is it not logical to suppose that this diagonal line is functionally related to the diagonal fracture, the upper part of which it is aligned with?

To refute my argument that the anthropic origin of the set of grooves on the bone surface is beyond doubt, d'Errico produces the example of the grooved Montgaudier piece. Clearly his main argument to prove the carnivore origin of the marks present is the occurrence of extensive pitting on one end and near the grooves. Such pitting, however, is completely absent from the Schulen bone. As d'Errico admits himself, the depth of the grooves on the Schulen bone is not systematically reached on bones gnawed by carnivores. This implies that the Schulen bone would have been held and rotated for quite some time in exactly the same position (without causing pitting near the grooves!). It is difficult to imagine a carnivore systematically rotating a bone in its jaws for the mere pleasure of rotating it.

In my opinion it is difficult to uphold a carnivore origin for the marks on the Schulen bone. Of course the grooves were not caused by a single action of a lithic cutting edge, but by repeated action. The multiple cutting scratches in their centre, which have V-shaped profiles, evince this. They cannot have been caused by carnivore teeth (unless, again, by the false set evoked above).

D'Errico's remarks with regard to the shine and smoothing observed on the Schulen bone and to the taphonomical context of the find are speculative (not to say wishful thinking advantageous to himself). I may add that repeated licking of bone by carnivores is usually restricted to the spongy parts (epiphysial extremities and interior surface) and not to the diaphysial outer

Figure 1.
A close-up view of the grooves on the Schulen bone, showing the multiple cutting scratches in their centre (photograph by L. Cleeren).

surface (Sutcliffe 1970). With regard to the taphonomical context of the Schulen find, it may also be of significance that carnivores such as wolf or hyena are neither represented by osteological remains nor by unambiguous gnaw marks (pers. comm. W. Van Neer, Koninklijk Museum voor Midden-Afrika, Tervuren, Belgium).

More fundamentally, I think, the epistemology of d'Errico's critique is as worthy of consideration as that of my article: without having first-hand knowledge of the object under consideration and judging only on photographic documentation, such a written comment leads to nothing but confusion. I therefore cordially invite d'Errico to study the bone object himself, according to his own methodology. I may ultimately not be able to convince him to write his *mea culpa d'un sceptique*, but at least we will be able to discuss on a fair basis.

In conclusion I would like to paraphrase d'Errico's concluding statement: often in the past, but also recently (as, I think, in d'Errico's critique), cases of valid archaeological reasoning were dismissed to the realm of fantasy (or wishful thinking) without a basic effort to obtain first-hand knowledge of the facts. I must say I feel like Boucher de Perthes and de Sautuola presumably felt, well over a century ago.

1) Dr Jean-Paul Caspar, a traceologist at the Laboratorium voor Prehistorie of the Katholieke Universiteit te Leuven (Belgium), confirms the anthropic character of the marks on the bone surface. According to him, the macroscopically visible evidence is amply sufficient proof. Microscopical magnification does neither substantially reinforce nor weaken the interpretation.

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REFERENCES

BEHRENSMEYER, A. K., K. D. GORDON and G. T. YANAGI 1986. Trampling as a cause of bone surface damage and pseudo-cutmarks. *Nature* 319(6056): 768-71. [FD]

BINFORD, L. R. 1981. *Bones, ancient men and modern myths.* Academic Press, New York. [FD]

D'ERRICO, F. and G. GIACOBINI 1985. Approche méthodologique de l'analyse de l'outillage osseux. Un exemple d'étude. *L'Anthropologie* (Paris) 89: 457-72. [DH]

HAYNES, G. 1980. Evidence of carnivore gnawing on Pleistocene and recent mammalian bones. *Paleobiology* 6: 341-51. [FD]

OLSEN, L. S. and P. SHIPMAN 1988. Surface modification on bone: trampling versus butchery. *Journal of Archaeological Science* 15: 535-53. [FD]

PEI, W. C. 1938. Le role des animaux et des causes naturelles dans la cassure des os. *Palaeontologia Sinica*. Geological Survey of China. Peiping, n.s. (7), 16 pp. [FD]

SHIPMAN, P. 1981. *Life history of a fossil. An introduction to taphonomy and paleoecology.* Harvard University Press, Cambridge. [FD]

SUTCLIFFE, A. J. 1970. Spotted hyena: crusher, gnawer, digester and collector of bones. *Nature* 227: 1110-3. [DH]

BRIEF REPORTS

SYMPOSIUM RATIONALES FOR THE SECOND AURA CONGRESS, August/September 1992, Cairns, Australia

The symposium rationales for several of the Cairns '92 symposia are presented here. They are essentially 'Calls for Papers', addressed to prospective participants and contributors. Readers who would like to contribute to this event and whose work relates to one of the following subjects are requested to prepare abstracts of less than 200 words, and submit these either to the AURA Editor; or to the Symposium Chair (the author[s] of the rationale), sending a copy also to the AURA Editor.

Subjects not covered by the following symposia will be included in further sessions which are still to be announced.

Rock art studies: the post-stylistic era. Where do we go from here?

MICHEL LORBLANCHET and PAUL BAHN

In recent decades, a situation developed in which structuralism and statistical studies established a homogeneous theoretical concept of 'sanctuaries' and rock art sites which, in a way, stifled the existence of a real rock art chronology. In the case of European cave art, every cave was considered an organised sanctuary, a homogeneous whole to be classed in one style or another, and each style was considered a well-defined pigeonhole. This clear and simple view sprang largely from Leroi-Gourhan's concept of structure which lumped everything together: in other words, we saw each cave as a homogeneous and contemporaneous unit which formed a composition. This academic vision of rock art was highly theoretical, and subsequent research has proved it false.

Chronologies based on style are becoming increasingly suspect, with the traditional view being undermined on a number of fronts:

A) Ever more detailed archaeological studies of context, to examine all the traces of sites' use.
B) Modern methods of tracing and recording, which attempt to be exhaustive and encompass all the motifs and traces on the rock surface.
C) New studies of the technology of portable art, involving microscopes and the SEM; and that of parietal art, using computers and video cameras.
D) Physico-chemical analysis of pigments which has become possible thanks to technical advances, and now requires such minute amounts of material to be removed from motifs that no damage is caused.

These avenues of research combine to write the history of decorated rocks, what one might call their 'biography'. We are now achieving a new, dynamic perception of sanctuaries and sites, we can see the sites' life more clearly. Some were used for a long time, others very briefly; there may be periods of intensive activity interspersed with periods of abandon, over centuries or millennia: the study of pictorial activity reveals that figures were added or retouched over time; some were integrated with older figures, others were transformations of older motifs. In short, the situation is now very different to Leroi-Gourhan's monolithic block with its neat pigeonholes. Moreover, the dates now being obtained directly from organic material in pigments from every continent's rock art are providing a more objective alternative to traditional dating methods, and often proving stylistic attributions to be wrong, or at least incomplete and simplistic.

So where do we go from here? Is style dead? Should we abandon it, or can it still be useful? We hope that our symposium will provide a forum for debate on these and related points. In particular we invite papers on the following themes:

1) Critiques - or defences - of traditional stylistic chronologies.
2) Style: its definition, and its utility in rock art studies.
3) Recent data on the use made of particular sites through time.
4) Studies of art using new methodologies (see C, above).
5) The contribution of pigment analyses to our understanding of rock art.

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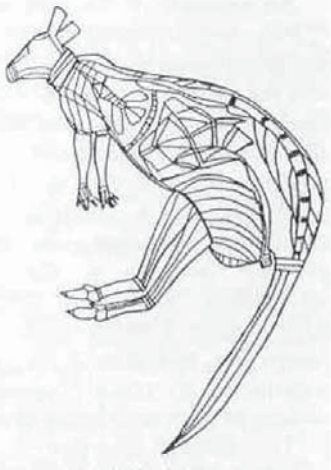

Djugerre - a female black wallaroo, Arnhem Land, Australia (G. Chaloupka)

Rock art and information exchange

CLAIRE SMITH

The manner in which information is acquired and distributed has been central to much anthropological and archaeological study: the concepts of diffusion, acculturation, enculturation and socialisation have all been developed to explain how information is conveyed within and between societies (see Moore 1983: 178). Information is a resource which is not equally accessible to all people: information exchange is basic to the functioning of all human societies. Information concerning how people perceive themselves in relation to those around them is encoded in their material culture. This information can be conveyed explicitly (by insignia of rank) or implicitly (through stylistic continuities or discontinuities), or even subconsciously (through artefactual manufacturing processes).

Until quite recently archaeological research into information exchange in past human societies was constrained by the positivist view that ideological/social features are beyond effective archaeological reconstruction. However, with the increased interest in structuralist studies in archaeology during the late seventies and early eighties, the processes involved in information acquisition and dissemination have been addressed more frequently. Early structuralists (e.g. Bahn 1982; Conkey 1978; Gamble 1982; Hodder 1982; Morwood 1979; Rosenfeld 1977; Wobst 1977) argued that the design structure of artefacts could be used to provide insights into socio/political facets of past societies.

While Wobst (1977) provided much of the theoretical grounding, Conkey's (1980) study of design structure among incised bone and antler artefacts at Altamira opened up the treatment of style in prehistoric art as a form of ritual communication. This study was part of a general theoretical movement in which style came to be consciously viewed as an active constituent of social process, rather than as a passive reflection of such process. This movement developed in at least two directions: assessing how constraints upon information exchange functioned to establish and develop social asymmetry (e.g. Conkey 1985; Davidson 1989; Johnson 1982; Moore 1983) and assessing how particular forms of information exchange facilitated habitation in particular environments (e.g. Gamble 1982; Jochim 1983, Lewis 1988; Morwood 1984).

An example of the first type is provided by Johnson (1982), who investigated the relationship between information processing constraints associated with scalar stress and the contexts within which hierarchic social forms are likely to emerge. He identified two forms of hierarchy: simultaneous hierarchy, in which regulatory functions relating to information flow within a society are controlled by a hierarchy which has on-going and continuous existence, and situational hierarchy, whose primary function is the short-term integration of information sources in contexts such as large-scale aggregations. Conkey (1985) extends this to argue that hierarchic formations are reinforced and effectively legitimised by ritual communication, and that material objects play an active role in this process.

The second direction is exemplified by Gamble's (1982) study, in which he investigates the role of information exchange in establishing and maintaining regional alliance networks. He argues that the stylistic similarity of the Upper Palaeolithic female figurines indicates the operation of open social networks during a period of environmental deterioration; he contrasts this to the stylistic heterogeneity of Magdalenian cave art which he argues indicates a closure of social networks. Gamble moves beyond a simple correlation between stylistic similarity and social interaction to consider the conditions that are likely to cause stylistic similarity within a group of artefacts (i.e. similarity-causing interaction among producers of those artefacts).

While Gamble's definition of stylistic homogeneity has been subjected to criticism (see Bednarik 1990; Dobres 1990; Soffer 1987), his study has been highly influential. In particular, the correlation he draws between stylistic homogeneity, poor environments and open social networks has been used to interpret prehistoric art in the European Upper Palaeolithic (Jochim 1983), in Australia (David and Cole 1990; Lewis 1988) and in the Horn of Africa (Brandt and Carder 1987).

Structuralist studies in archaeology have been closely aligned to semiotics, originally the study of linguistic signs (for detailed discussion of this relationship see Conkey 1989a). In linguistic semiotics the relationship between word and concept is encompassed in the notion of a sign, which is composed of the signifier and the signified. De Saussure (1978: 16) interpreted various entities such as the system of writing, symbolic rites, polite formulas and military signals as individual semiological systems. This broad semiotic principle has had much influence on anthropological and archaeological studies and has produced some particularly perceptive analyses of Australian Aboriginal art (e.g. Morphy 1977; Munn 1973; Taylor 1979).

Moreover, the inevitable outcome of the integration of structuralist and semiological interests has been a growing recognition of the diversity of human cultures, together with concern for the processes involved in producing this diversity and the contexts within which diversity can be found. This concern with diversity characterises post-structural archaeology. The nexus between art and ideology in prehistory, and the role of information exchange in human societies, is now being subjected to renewed consideration.

As part of this overall conceptual shift the questions to be addressed by studies of style in prehistory have changed from 'What does it mean?' to 'How is it meaningful? To whom is it meaningful? In what context is it meaningful?' (see Clegg 1985; Conkey 1989b; Davis 1990; Hodder 1987; Morwood 1989; Taçon 1987), or 'How is meaning discovered?' (Davidson and Noble 1989). This involves a reconceptualisation of archaeological interpretation as a contemporary imposition by the archaeologist and of culture as ever-changing and emergent (see Hodder 1987; Leone 1982; Kohl 1981; Wellfare 1990).

I am interested in soliciting semiological, structural or poststructural studies concerning information acquisition and distribution in human societies. I am particularly interested in studies of mythology and oral tradition similar to those of Miac (1986). Thus, this symposium proposes to consider two primary issues. These are:

1) How information is encoded in art.
2) How information is acquired and disseminated within social systems.

I hope to integrate papers on rock art with others from related disciplines which address the functioning of information systems within human societies. I thus invite cross-disciplinary discussion from anthropologists, linguists, philosophers or others concerned with the processes involved in information dissemination in human societies. More specifically, I am interested in soliciting papers on how artefactual material can encode social, ideological and economic information. As part of this, I hope to focus on the influence of context on style; the negotiation of gender relations through material culture; the maintenance and mediation of boundaries; and the manner in which artistic systems incorporate social change.

The Second International Congress on Rock Art will be held under the auspices of the Australian Rock Art Research Association in Cairns, Queensland, Australia, from 30 August to 4 September 1992. Anyone with appropriate expertise in any of the above issues who is prepared to give a paper in a symposium on rock art and information exchange is invited to contact me at the address below.

Acknowledgments

The original idea for a symposium on information exchange was first mooted by Iain Davidson, in discussion with Steve Sutton. The rationale presented here has derived from consultation with Iain and with my supervisors, Mike Morwood and Jane Balme. All three have provided extensive comments on early drafts, some of which have been incorporated into the text. Advice and comment was also given by my external supervisor, Betty Meehan and by fellow students John Appleton, Heather Burke, Cheryl Cooper and, particularly, Christine Lovell-Jones. The rationale as presented is my own responsibility.

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[Co-chair to be announced]

REFERENCES

BAHN, P. G. 1982. Inter-site and inter-regional links during the Upper Palaeolithic: the Pyrenean evidence. *Oxford Journal of Archaeology* 1: 247-68.

BEDNARIK, R. G. 1990. More to Palaeolithic females than meets the eye. *Rock Art Research* 7: 133-7.

BRANDT, R. M. and N. CARDER. 1987. Pastoral rock art in the Horn of Africa: making sense of udder chaos. *World Archaeology* 19: 194-213.

CLEGG, J. 1985. Comment on D. Groenfeldt, 'The interpretation of prehistoric art'. *Rock Art Research* 2: 35-45.

CONKEY, M. W. 1978. Style and information in cultural evolution: towards a predictive model for the Palaeolithic. In C. L. Redman, M. J. Berman, E. V. Curtin, W. T. Laughorne, N. M. Versaggi and J. C. Wanser (eds). *Social Archaeology: beyond subsistence and dating*, pp. 61-85. Academic Press, New York.

CONKEY, M. W. 1980. The identification of prehistoric hunter-gatherer aggregation sites: the case of Altamira. *Current Anthropology* 21: 609-30.

CONKEY, M. W. 1985. Ritual communication, social elaboration and the variable trajectories of Paleolithic material culture. In T. D. Price and J. A. Brown (eds). *Prehistoric hunter-gatherers*, pp. 299-323. Academic Press, Orlando.

CONKEY, M. W. 1989a. The structural analysis of Palaeolithic art. In C. C. Lamberg-Karlovsky (ed.). *Archaeological thought in America*, pp. 135-54. Cambridge University Press, Cambridge.

CONKEY, M. W. 1989b. The use of diversity in stylistic analysis. In R. Leonard and G. Jones (eds). *Quantifying diversity in archaeology*, pp. 118-58. Cambridge University Press, Cambridge.

DAVID, B. and COLE, N. 1990. Rock art and inter-regional interaction in northeastern Australian prehistory. *Antiquity* 64(245): 788-806.

DAVIDSON, I. 1989. Freedom of information: aspects of art and society in western Europe during the last Ice Age. In H. Morphy (ed.). *Animals into art*, pp. 440-56. Unwin Hyman, London.

DAVIDSON, I. and W. NOBLE. 1989. On depiction and language. *Current Anthropology* 30: 337-42.

DAVIS, W. 1990. The deconstruction of intentionality in the archaeology of representation. Paper presented at the 23rd Annual Chacmool Conference, Calgary, Canada.

DOBRES, M. 1990. Considering Paleolithic 'Venus' figurines: a feminist-inspired reanalysis. Paper presented at the 23rd Annual Chacmool Conference, Calgary, Canada.

GAMBLE, C. 1982. Interaction and alliance in Palaeolithic society. *Man* 17: 92-107.

HODDER, I. 1982. *Symbols in action*. Cambridge University Press, Cambridge.

HODDER, I. 1987. The contextual analysis of symbolic meanings. In I. Hodder (ed.). *The archaeology of contextual meanings*, pp. 1-10. Cambridge University Press, Cambridge.

JOCHIM, M. A. 1983. Palaeolithic cave art in ecological perspective. In G. N. Bailey (ed.). *Hunter-gatherer economy in prehistory*, pp. 212-9. Cambridge University Press, Cambridge.

JOHNSON, G. A. 1982. Organisational structure and scalar stress. In C. Renfrew, M. Rowlands and B. A. Seagraves (eds). *Theory and explanation in archaeology, the Southampton conference*, pp. 389-421. Academic Press, New York.

KOHL, P. L. 1981. Materialist approaches to prehistory. *Annual Review of Anthropology*, 10: 89-118.

LEONE, M. 1982. Some opinions about recovering mind. *American Antiquity*, 47: 742-60.

LEWIS, D. 1988. *The rock paintings of Arnhem Land, Australia: social, ecological and material culture change in the post-glacial period*. BAR International Series 415, Oxford.

MIAC, L. D. 1986. Scarcity and survival: the role of oral tradition in mediating subsistence crises. *Journal of Anthropological Archaeology* 5: 39-113.

MOORE, J. A. 1983. The trouble with know-it-alls: Information as a social and ecological resource. In J. A. Moore and A. S. Keene (eds). *Archaeological hammers and theories*, pp. 173-92. Academic Press, New York.

MORPHY, H. 1977. 'Too many meanings': an analysis of the artistic system of the Yolngu of northeast Arnhem Land. Unpubl. Ph.D. thesis, Department of Prehistory and Anthropology, Australian National University, Canberra.

MORWOOD, M. J. 1979. On art and stone: towards a prehistory of western central Queensland. Unpubl. Ph.D. thesis, Department of Prehistory and Anthropology, Australian National University, Canberra.

MORWOOD, M. J. 1984. The prehistory of the central Queensland Highlands. In F. Wendorf and A. Close (eds). *Advances in world archaeology*, pp. 325-80. Academic Press, New York.

MORWOOD, M. J. 1989. The archaeology of Aboriginal art in S.E. Cape York: a research proposal. *Rock Art Research* 6: 71-2.

MUNN, N. 1973. *Walbiri iconography. Graphic representation and cultural symbolism in a central Australian society*. Cornell University Press, Ithaca and London.

ROSENFELD, A. 1977. Profile figures: schematisation of the human figure in the Magdalenian culture of Europe. In P. J. Ucko (ed.). *Form in indigenous art. Schematisation in the art of Aboriginal Australia and prehistoric Europe*, pp. 90-109. Australian Institute of Aboriginal Studies, Canberra.

SAUSSURE, F. de 1978. *Course in general linguistics*. Translated by W. Baskin. Fontana/Collins, Glasgow.

SOFFER, O. 1987. Upper Palaeolithic connubia, refugia, and the archaeological record from eastern Europe. In O. Soffer (ed.). *The Pleistocene Old World: regional perspectives*, pp. 333-48. Plenum Press, New York.

TAÇON, P. 1987. Internal-external: a re-evaluation of the 'x-ray' concept in western Arnhem Land rock art. *Rock Art Research* 4: 36-50.

TAYLOR, L. 1979. Ancestors into art: an analysis of Pitjanjatjara *Kulpidji* designs and crayon drawings. Unpubl. B.A. (hons) thesis, Department of Prehistory and Anthropology, Australian National University, Canberra.

WELLFARE, S. 1990. Inferring gender: an exploratory framework for discerning small scale social relations in fisher-gatherer-hunter archaeology. Unpubl. B.A. (hons) thesis, Department of Archaeology and Palaeoanthropology, University of New England, Armidale, Australia.

WOBST, M. 1977. Stylistic behaviour and information exchange. In C. E. Cleland (ed.). *Papers for the Director: research essays in honour of James B. Griffin*, pp. 317-42. Anthropological Papers, Museum of Anthropology, University of Michigan, No. 61.

Rock art studies as a curriculum for teaching

GIRIRAJ KUMAR and OSAGA ODAK

Rock art is a global phenomenon, and must be studied as such (Bednarik 1990). After well over a century of discoveries and attempts of its serious study, the discipline of rock art and palaeoart studies has matured to the point where it deserves independent status (Odak 1991), and inclusion in academic curricula for teaching, right through all levels of education. Because of its visual quality, rock art is a subject that is easy to grasp, communicate and enjoy. Moreover, its unique characteristics provide further benefits by including it in academic curricula (Kumar 1990).

Throughout the world, palaeoart covers a long span of time in human history. With its vastness, variety and colour, rock art presents visual depictions of life, the environment, the human adaptations, the creative enthusiasm and, above all, the strong will of humans to live with joy. The study of rock art and other forms of palaeoart helps in understanding the complex processes of intellectual and cognitive development of the human species, along with those of socialisation and humanisation, while at the same time providing an understanding of man's attempts to find harmony with nature. In other words, these studies help in discovering ourselves and in developing a spirit of meeting present-day challenges.

Teaching of this new discipline will generate awareness and the capacity of the young generations to better appreciate the human heritage and the need for its protection. This will facilitate conservation and preservation, which are among the crucial tasks to be accomplished at present.

Therefore inclusion of this new discipline in academic curricula is a current need, not only for its appreciation and popularisation, but also for better conservation and preservation. This is a great challenge and we all - the rock art researchers, specialists and connoisseurs - should take decisive steps in this direction. We have to explore the basic needs and requirements, and find practical solutions for their fulfilment.

In this regard, some of the subjects we would like to address in the symposium *Rock art studies as a curriculum for teaching* are:

1) Finding a new name for our discipline, if considered necessary.
2) Teaching of the discipline in academic institutions.
3) Public education: popular books, public lectures with audio-visual aids, exhibitions, rock art study tours, role of the museums, interaction of rock art researchers with public, vocational training programs for amateurs.
4) Publication of books and journals.
5) Preparation of teaching aids.
6) A system for exchange of experiences and teaching aids at national and international level.
7) Role of educational tours to rock art sites and museums.
8) Developing centres for rock art study, research, documentation, conservation and preservation.
9) Creation of employment opportunities for rock art specialists.

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REFERENCES

BEDNARIK, R. G. 1990. Global perspectives of rock art. *Purakala* 1(1): 2-6.
KUMAR, G. 1990. New dimensions in purakala study. *Purakala* 1(1): 7-10.
ODAK, O. 1991. A new name for a new discipline. *Rock Art Research* (this issue).

RAR 8-191

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The rock art of the Sahara

ALFRED MUZZOLINI

Abstracts are solicited for papers to be presented in a symposium centring on Saharan rock art. It is intended to cover the following subjects:

1) *Bases for classification of rock pictures*
The criterion style: use limitations, social functions.
Non-stylistic criteria: technique, patinae etc. How to use these.
The criterion of the physical types depicted.
2) *Debate on the traditional classification and chronology*
The bases for relative chronology: climatic oscillations, faunal markers (domestic or wild), other markers (weapons, chariots etc.).
3) *Special issues*
Tifinar (categories of rock pictures, origin)
Linguistics: new data about settlement of central Sahara, and domestication (dates, species) in relation to rock art physical types and fauna.
4) *Regional studies:* true regional syntheses are required. Reports on specific rock art sites must relate to general problems.
5) *The problem of interpretation*
Debate on Dieterlen-Hampate Ba's theses on rock art links with the Peul.
Empiricist reading: use and limitations.
Shamanism, trance and phosphene theories.
Structural design analysis.
Other approaches.
6) *Today's major problem: vandalism.* How to deal with it, not in the distant future, but in the immediate future, before it is too late. Current measures are totally ineffective.
7) *The state of affairs.* Geography of current research. Why do some regions remain practically blank on the map? Why has research practically ceased in some regions where destruction could be faster than recording?

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[Co-chair to be announced]

RAR 8-192

Spatial considerations in rock art

PAUL FAULSTICH

Rock art is one expression of the ways in which humans have perceived the world. Across wide geographic and cultural boundaries, we have evidence that people's relationship with the land is not simply social and economic, but symbolic as well. While indigenous rock arts have long been considered in relationship to the landscape, they have seldom been considered as *part* of the landscape. This symposium will attempt to integrate diverse approaches to investigating the relationship between rock art and environment.

It could be argued that rock art has not only been influenced by its relationship with the landscape, but that it is actually an aspect of the landscape. Humanistic geographers, for example, might suggest that both contemporary and ancient landscapes have been defined, at least in part, by the artefacts which alter them. On the other hand, many archaeologists would deny that a reciprocal relationship exists between rock art and landscape, proposing instead that a linear relationship exists; e.g. rock art is influenced by the landscape, but not vice versa.

The symposium on *Spatial considerations in rock art* will be an eclectic session addressing broad issues relating to indigenous arts and landscape. From geographic theory to descriptive analysis, this symposium will try to synthesise divergent approaches to comprehending a most fundamental question in rock art studies: what do these arts express about human interaction with the world?

To all peoples, physical geography and art are two classes of symbols which are often brought together in tension or in harmony. This session will consider rock art as primary evidence of the human experience of space and place. It will attempt to answer such questions as: how does rock art augment and define human/nature relationships? How are human aesthetics connected to elements of the environment? How can rock art tell us about a people's interaction with the land? And how does rock art create or alter landscape?

Call for Papers

Papers which address broadly-defined relationships between rock art, spatial sensibilities and meaning will be considered. Proposals for this session are requested from archaeologists, philosophers, artists, geographers, anthropologists and others committed to inquiries of cultural relationships with the land.

The organiser is keen to get a selection of proposals which address such issues as the effect of locality on rock art, environmental foundations of image making, cultural contexts and conventions through which images relate to the environment, spatial concepts of indigenous peoples, motif placement, iconic congruence, rock art site typologies, and interrelationships between sites. A range of interconnecting ideas are solicited, and ideally a dialogue of arguments and counterarguments will make this a most stimulating session.

Dr Paul Faulstich
Los Angeles

Co-chair to be announced. Please submit your abstract, of no more than 200 words, to the AURA Editor.

RAR 8-193

REVIEWS & ABSTRACTS

Images of power: understanding Bushman rock art, by DAVID LEWIS-WILLIAMS and THOMAS DOWSON. 1989. Southern Book Publishers, Johannesburg. 196 pages, 25 colour plates, 5 monochrome plates, 70 drawings, 5 maps, List of books for further reading. Hardback, R49.99. ISBN 1 86812 196 8.

This book is well produced and lavishly illustrated with drawings and colour plates. Also, as could be expected of a teacher of English, it is well (and persuasively) written, so much so that the reader should be alert for non sequiturs, implicit assumptions, and dogmatic statements unbacked by proof or, in some cases, any evidence. References are not given.

Although the explanations of the 'meaning' of the art and the interpretations are claimed to apply to southern African rock art generally, all but a few of the illustrations are taken from one art region, that of the Drakensberg of Natal and the north-east Cape Province.

A map to show the distribution of the paintings and petroglyphs of southern Africa is given. It is regrettable that it is 'after C. Van Riet Lowe, 1941', and not the much better map that author published in 1952, or one of the still more accurate and complete distribution maps produced since then.

There are a number of inaccuracies. The authors state that the southern African rock art represents the longest artistic tradition human beings have produced. Even accepting the single anomalous dating of 26 000 BP from southern Namibia, this is not so. In Australia an antiquity of over 32 000 BP has been established for rock art, and there appears to be a fairly continuous succession of art traditions. And as there is a gap in the southern African datings of painting or engraving stone of some 16 000 years it seems just as likely that the Namibian date marks a local invention which was a flash in the pan rather than the beginning of a tradition. The abbé Breuil's long discredited notion of 'The White Lady of the Brandberg' is resuscitated, only to be given the *coup de grâce* again. But Breuil did not say 'she' was a European, only that she was of Mediterranean stock. And it was not he who identified some Drakensberg paintings as Phoenicians, it was the late Raymond Dart. It is stated that there are fine paintings of ostriches in the Drakensberg; none has been reported. These are not important points but indicate a certain carelessness regarding truth. The thesis of the book is to set forth the authors' claim that the painters were shamans recording what they had seen in trance and generally in a disguised, 'metaphorical' form.

By tenuous reasoning not possible to adduce and discuss in a book review, a number of 'metaphors' are identified by which a painting can be seen to depict trance hallucinations, and not the every-day activities most of them appear to be. Scenes showing death, water creatures, flight and fighting are all metaphors of trance. Regarding the human figures, indicators of trance are apparent bleeding from the nose, kneeling, bending forward, having hands at about head height, standing with arms backwards, holding a fly-whisk, pointing with a finger and having a raised knee. As in any complex group with humans in action one figure is likely to exhibit one of these attitudes, the authors then assert that it is a trance scene. Figure 70 of the book is an example. It is a very spirited and detailed scene of a typical Bushman cattle raid and the reprisal. The Bushmen are driving off the cattle to the left and their mounted pursuers are overtaking from the right. Some of the latter have dismounted to shoot and some shoot from the saddle; even the smoke from their guns is shown. It is generally agreed that the scene is historical but Lewis-Williams has identified a marginal figure as a 'shaman' because it is bleeding from the nose and has two emanations from the head (feathers?). Those features point to shamanism, say the authors, to conform with their general theory that the scene was painted by a shaman after hallucinating the whole scene.

The authors do not concede that any animals were depicted simply because the artist took pleasure in the act of representation; or that any of the artists were not shamans. The animals are all symbols: cattle and horses, and 'perhaps sheep as well', for example, are symbols of potency. Generally they are not depictions of the actual beasts but of shamans who have taken animal form. Not only eland, but crabs and snakes are given as examples.

One painting shows two 'shamans' in the form of lions being shot at by 'shamans' in human form. The authors mention that there are twenty-nine hunting scenes in one valley of the Drakensberg. Are they all to be interpreted as shamans hunting other shamans who have taken the form of animals? In another painting all four human figures depicted with an eland are stated to be shamans.

In places the suppositions of the authors come close to sheer mysticism: a red line fringed with dots may be 'the trajectory of a shaman on out-of-body travel' and 'for the shaman-artists the spirit world lay beyond the wall of the rock shelter'; 'the shamans reached through the wall, extracting the beings and events of the spirit world and then fixed them on the rock for all to see'. This comes, strangely, from a university professor of what is supposed to be a scientific discipline.

As I have said, the purpose of this book is to present carefully selected evidence purporting to prove the thesis of Lewis-Williams - loyally seconded by his pupil Dowson - that the art was executed by 'shamans' emerging from a state of trance in order to depict what they had hallucinated in that altered state of consciousness. There is no scholarly objectivity in this book: it is the work of a clever advocate making a case, not of a truth-seeking judge.

Large bodies of contrary evidence are ignored or sidestepped. The authors make much of the handprints found among the paintings: they are a means of transferring 'potency' in the paint due to the presence of eland blood as the medium, to the rock face whence it can be drawn upon by shamans. Another assertion is that the shaman painters in entering trance experienced 'entoptics' - visual phenomena that include zigzags, chevrons, dots, grids, vortexes and nested U-shapes. This is held to explain the presence of such motifs in the rock art of most South African art regions. It is not explained why, among the tens of thousands of paintings in the Drakensberg/Maluti region, no handprints and no 'entoptics' have been reported. This region has been well explored and studied. If any exist it must be a minute percentage.

Selection is carried so far as to show only part of a scene or group of paintings. In the scene Fig. 21c there is actually a small crouching bowman on the right, facing the eland, and in the scene Fig. 45 there are four human figures marching in from the left. These are certainly relevant to the consideration of what is portrayed but they do not fit the authors' interpretation; they are not shown.

Whoever reads this book and only the 'Suggestions for further reading' given will finish up with exceedingly distorted views on the questions raised. Lewis-Williams and Dowson are as selective in the further reading list as in their presentation of evidence. The list of 19 books omits all works by Rudner, Woodhouse, Lee, Scherz and Willcox (total 10 books), who by and large disagree with, or strongly oppose the view of the authors. A list of 'Some other works cited in this book' is also given. Some of these works were obviously used but they are not cited or mentioned in the text.

A. R. Willcox
Winterton, Natal

RAR8-194

Arte preistorica del Sahara, edited by S. De LUCA and G. d'INZILLO CARRANZA. 1986. De Luca Editore/Arnoldo Mondadori Editore, Rome/Milan. 142 pages, 56 colour plates, numerous monochrome plates and line drawings, bibliography and glossary. Paperback (printed in Italian).

One of the most impressive aspects of this book is the long list given on the preface pages, of the several committees, individual collaborators, photographic studios and various other groups of contributors. But one cannot help wondering whether all of the individuals listed, which include several highly respected scholars, actually consented to their names being featured.

The book consists of three parts. In the first, brief articles by Nicole Petit-Maire, Marina Lupacciolu and Barbara E. Barich deal with the palaeoclimate, the rock art and the Holocene occupation evidence of the Sahara respectively; this is followed by 48 pages of colour plates depicting petroglyphs, paintings and most impressive landscape settings; finally there is a 'Catalogue'.

The three papers provide as reasonable an introduction to the prehistoric art of the Sahara as could be expected in something like 16 000 words (this is roughly the same length as the paper by Muzzolini in a recent issue of *RAR*). The most important detail offered, the absolute chronology of Saharan rock art, is worth a closer look. According to it, the sequence begins with the 'bubaline period', which is separated from the 'pastoral bovidian period' by the period of 'round heads'. The first reference to absolute dates is related to the bovidian period, which seems to be placed between 8000-5000 years BP, and which is followed by the 'horse' and 'camel' periods. Nothing new here. I am intrigued by the

definition of the most recent phase, the *'era volgare'*; can a book featuring such a vacuous value judgment really be considered scientifically relevant? Moreover, the chronological model is presented without discussion of competing models, which in view of the highly controversial status of this chronology of Saharan rock art is not adequate. Some of the most competem specialists such as F. Soleilhavoup and A. Muzzolini are totally ignored in this book, and receive no mention in the bibliography. What I find even more disturbing is that the indigenous rock art researchers of northern Africa, such as Algerians Malika Hachid and Sid Ahmed Kerzabi, or Moroccan Fatima-Zahra Alaoui are simply ignored in such a book. Hachid in particular should be acknowledged for her innovative work. Thus I must regard the book as not only scientifically deficient, but as simply another exercise of European rock art enthusiasts in an area in which they excel: cultural imperialism.

The colour photographs of petroglyphs are of uniformly high standard, and mercifully free of any evidence of physical enhancemem. In many of them skilful use has been made of oblique lighting but scales have been omitted throughout. Some chalking has occurred but it is restricted to a few of the monochrome plates, i.e. on pp. 116, 117 and probably on p. 108. The same concern for conservation is not reflected, however, in the plates of rock paintings. Plates 43, 45, 47 and 55 depict paintings which have been clearly wetted for colour contrast, and no doubt other panels were recorded with the same unethical method. It cannot be stressed enough that this selfish and vandalistic practice results in the gradual destruction of rock paintings, through various processes.

Several other shortcomings ought to be mentioned here, and some of them should have been self-evident to the publishers. For instance the way space is utilised in this book suggests a lack of attention on their part. This is particularly evident in the section 'Catalogue'. It begins with two-page descriptions of the Fezzan, Tassili, Acacus, Ennedi, Uweinat and Adrar des Iforas regions, each one accompanied by the very same map. These maps thus occur on every second page, each one taking up half a page. If this is intended to save the reader having to turn the page to refer to the map it would have made sense only if the two pages dealing with each region were on two opposite pages. But they are not. To compensate for this liberal use of space, seven drawings of excavation sections and site plans in the subsequent part have been reduced with such a vengeance that they are entirely unreadable, being the size of postage stamps. Not even with a magnifying glass can one decipher these crucial drawings or their legends, without which the archaeological 'data' referred to are of little value. Such data are sparse enough as they are, for instance Barich presents many pictures of decorated pottery shards, but not one of a reconstructed vessel. The decoration patterns are not very diagnostic, they occur in many other regions. And she raises the question of early domestication, but without providing hard and fast evidence.

The purpose of the pictorial *catalogo* remains obscure to me - unless it is intended to copy Leroi-Gourhan's 1971 tome. Apart from the unreadable drawings and a lot of empty spaces, it consists of monochrome plates, usually six per page. Because they have been reduced to about 50x50 mm, details are often impossible to see in them, in fact some of these miniatures amount to little more than square patches of vague grey, others are reduced duplicates of panels featured elsewhere in the book. The publishers have thus wasted a considerable amount of space while at the same time scrimping on space in the presentation of crucial archaeological evidence.

I wonder whether it was appropriate to print this book about the Sahara, which has been denuded of most vegetation, on what appears to be 200 g/m^2 paper - bearing in mind the number of trees that had to be sacrificed to produce the book. But perhaps the publishers' motto was 'Never mind the quality, *feel* the width!'

Robert G. Bednarik RAR 8-195

(Reprinted from *Sahara*, with permission)

Les roches gravées nord catalanes, by JEAN ABELANET. 1990. Prehistoria de Catalunya nord 1, C.R.E.C., C.E.P.C. No. 5, et revue *Terra Nostra*, Perpignan-Prades. 209 pages, 173 figures, 6 maps, 37 photographs, 150 Francs.

Trois éditeurs, rien de moins, se partagèrent la tâche de publier cet ouvrage: *Le Centre de Recherches et d'Etudes Catalanes* de l'Université de Perpignan, *Le Centre d'Etudes Préhistoriques Catalanes* et la revue *Terra Nostra* de Prades. Mais c'est au responsable de cette dernière, Raymond Gual, que l'on en doit l'initiative et la réalisation matérielle, dans le premier volume d'une série intitulée *Prehistoria de Catalunya* nord.

Cette parution vient interrompre une longue attente puisque la rédaction du manuscrit remonte à 1977, date de sa présentation à l'Université de Montpellier sous la forme d'une thèse de 3ème cycle. Le texte original, dont l'auteur nous avait dévoilé quelques aperçus et livré les conclusions abrégées dans son livre *Signes sans paroles. Cent siècles d'art rupestre en Europe occidentale* (Hachette 1986), est ici complété d'une troisième partie consacrée aux principales découvertes survenues durant les années 80.

Les deux autres parties, qui constituent l'essentiel du mémoire, concernent respectivement et à part égale 'l'art rupestre d'ambiance dolménique' tout d'abord, 'l'art schématique linéaire' ensuite. Chaque volet de ce dyptique comporte un corpus établi par région et une étude archéologique détaillée, dotée d'un appareil descriptif, chronologique et interprétatif fourni et bien argumenté.

Réduite à de sommaires cupules, bassins, déversoirs, rigoles et plus rarement à des signes schématiques élémemaires (cruciformes divers dans la plupart des cas et exceptionnellement des spirales, quadrilatères, motifs occulés ...), la première forme donna lieu, par le passé, à une littérature aussi abondante qu'imaginative qui jeta sur elle un durable discrédit.

Il aura donc fallu toute l'indépendance d'esprit, le non-conformisme et aussi la délicieuse candeur qui le caractérisent pour que Jean Abélanet ose s'attaquer à ces documents jusqu'alors dépréciés par des *'théories fantaisistes'* qui contribuèrent *'à détourner les archéologues de ce type de manifestations rupestres et à en retarder l'étude'*.

Ne négligeam aucun des aspects susceptibles d'éclairer le comment et le pourquoi de ces vestiges, dont l'apparition coïncide avec celle des sépultures mégalithiques, l'auteur insiste, à juste titre, sur l'importance de la nature pétrographique du support. Ainsi la géologie intervient-elle de façon déterminante dans le choix des motifs, le granite étant impropre à recevoir les figures que l'on peut aisément obtenir sur du schiste.

De même, la morphologie des cupules, éléments de base de cet 'art rupestre dolménique' diffère suivant la roche considérée. L'exécutant utilisait un simple galet de quartz dans le cas du granite, un pic lorsqu'il opérait sur schiste, comme cela ressort de l'expérimentation.

Après avoir fait justice des théories astronomique et topographique et opéré une utile distinction entre les cupules 'utilitaires', celles qui nous occupent ici, et les cupules imégrées à une construction figurative, observables sur certaines stèles anthropomorphes provençales, l'auteur nous propose sa propre interprétation dans une convaicante démonstration. En bref, bassins et cupules seraient des réceptables à offrandes et libations, destinées à des rites funéraires.

L'art schématique linéaire, dont l'esthétique est plus immédiatement perceptible, forme la matière de la seconde partie qui débute par un utile répertoire des signes et motifs recensés. Celui-ci gagnerait toutefois a être illustré de figures extraites du corpus.

A l'exception de la *Cova Bastera*, dont certains pilliers stalagmitiques révélèrent des gravures linéaires non étudiées dans l'ouvrage, les oeuvres appartiennent à l'univers du schiste, qu'il s'agisse du Capcir et de la Cerdagne pour les zones de haute altitude, du Conflent et des Aspres pour les territoires de moyenne et basse altitudes.

L'ensemble fameux, aujourd'hui mutilé, de la *Peyra Escrita* à Formiguère, occupe bien évidemment une place de choix au sein de cet ouvrage. Mais l'abondance et la diversité des représentations rupestres de ce site exceptionnel ne doit pas pour autant faire oublier des ensembles plus modestes et notamment ces saisissantes chasses au cerf inscrites au flanc de quelques rochers des Aspres.

Restait à définir, dater, interpréter cet art schématique linéaire qui nous met *'en présence d'un code élaboré, traditionnel, commun non seulement aux populations locales des Pyrénées, mais largement répandu dans le monde de la Méditerranée occidentale'* et non du *'produit de l'imagination et de la main de bergers illettrés'*.

Jean Abélanet, fort d'une expérience acquise au cours de plusieurs missions accomplies en milieu alpin, dans la *Vallée des Merveilles*, s'y applique avec succès et nous livre là encore le fruit d'une réflexion longuement murie au contact des documents eux-mêmes. Le recours aux textes médiévaux et le décryptage des décors de quelques pièces archéologiques datées l'incitent en outre à situer, avec vraisemblance, la période d'exécution de ces chasses à courre 'sacrées' et *'ex-voto'* divers, depuis *'les derniers siècles avant notre ère jusqu'à l'implantation du christianisme dans nos campagnes (IV au VIè siècles?)'*.

La seconde partie de l'ouvrage s'achève sur un inventaire des principaux symboles comenus dans ce système iconographique et sur leur évemuelle signification par référence au monde symbolique, en usage dans l'antiquité méditerranéenne.

Il convient de souligner la clarté de l'exposé, rédigé dans un style simple et élégant, qui traduit parfaitement la familiarité et l'aisance de l'auteur à l'égard de son sujet. La qualité de l'illustration se hisse au niveau du texte. Regrettons toutefois la double numérotation, l'une réservée aux figures au trait, la seconde aux photographies, les cartes échappant curieusement à l'une et à l'autre, et l'absence d'un index des noms de lieux. Le lecteur découvrira en outre, et avec surprise, que, dans cet ouvrage rédigé en Français, la langue catalane prévaut quand il s'agit de mentionner le nom d'une commune ou d'une contrée.

Du point de vue de la méthode des relevés, on pourra reprocher à l'auteur l'emploi du noir de fumée et plus encore de la craie blanche, étant bien entendu que les dégradations dues aux agents d'érosion, aux actes de vandalisme et aux tentatives de prélèvements sont autrement dommageables à ce patrimoine qui, tout autant que l'art pariétal paléolithique, mérite une réelle prise en compte et d'efficaces mesures de protection.

Souhaitons enfin que les différents territoires des massifs montagneux de France bénéficient d'un effort identique au niveau de la prospection, de l'analyse et de la synthèse.

Dr Dominique Sacchi RAR8-196
Carcassonne, France

Saraakallio, by PEKKA KIVIKÄS. 1990. Atena Kustannus Oy, Jyväskylä, Finland. 128 pages of text, 16 pages of colour photographs and drawings, numerous figures and monochrome photographs in text, bibliography. Hardback (printed in Finnish).

About fifty postglacial rock art sites have so far been discovered in southern and central Finland. Saraakallio (also known by the nearest place name, Laukaa) is one of them, situated not far from the town of Jyväskylä, where the author resides. As an art teacher, his eye is experienced in perceiving colour shades, and his hand in documenting what he sees. Saraakallio is one of the most significant rock art sites in Finland, being second only to the famous Astuvansalmi rock painting site. An area of 40 x 6 m of a vertical rock wall is covered with images, which are divided into 50 groups. Their content is similar to that of the other Finnish sites, notably straddled-legged stick figures, horned beasts, boats with human silhouettes, and hand prints. However, there are peculiarities, which the author tries to define. His task is difficult since most of the paintings are on the brink of disappearing, where only the intuition of an experienced observer can help.

Kivikäs describes the site systematically, dividing it into more than twenty sections. Almost all of these were subdivided and designated. Everything was photographed and also drawn up or copied in colour. This work turned out to be heavily weather dependent and complicated.

The age of most of the Finnish sites can be roughly estimated since they are situated on rocky lake shores. The postglacial oscillations of the water level are well recorded, which provides clues to when the painting activities were feasible. In general Finnish rock art is said to be some five millennia old. Apparently such datings refer to the radiocarbon scale. A dendrochronological correction will possibly add one more millennium to them. As a rule, this correction is overlooked.

The author compares the paintings with rock art from other sites and recalls folkloric themes which may be represented. The reviewer is inclined to think that the writer has been successful at least as far as the sequential images are concerned. Those from Saraakallio show striking similarities with data from elsewhere presented in the book.

The small Finnish community of rock art enthusiasts is to be congratulated for having produced already two high-quality monographs on the local prehistoric rock art (the first book having been that by Eero Autio about Karelian petroglyphs, published in 1981).

Heino Eelsalu RAR8-197
Tartu, Estonia

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RECENT ROCK ART JOURNALS

La Pintura. Quarterly newsletter of the American Rock Art Research Association. Edited by FRANK G. BOCK. In addition to numerous notices and announcements, recent issues contain the following scientific papers:

Volume 15 (1988/89), Numbers 1-4:
HEDGES, K.: At the Top End Down Under: a report on the First AURA Congress, Darwin, Northern Territory, Australia.
LEE, G.: Petroglyphs of Lana'i, Hawai'i.
LEE, G.: The Lavabeds, California, Rock Art Project.
BEDNARIK, R. G.: Rock art vs. natural markings: a reply to Loendorf.
BOCK, A. J.: Six ARARA members attend Getty workshop on rock art preservation.
CROTTY, H.: Working conference on looting and vandalism, sponsored by the Society for American Archaeology.
RAFTER, J.: Spiderweb petroglyphs.
WARNER, J.: The unobvious symbolism at Coppermine Springs, Arizona.

Volume 16 (1989/90), Numbers 1-4:
CROTTY, J.: 1989 A.S.N.M. rock art recording field school.
FAULKNER, C. H.: Eight more underground caves in southeastern United States reveal pictograph rock art.
RITTER, E.: Conservation and preservation committee report.
FREEMAN, P.: The psychology of aggression as it relates to rock art vandalism.
LEE, G.: The Puako Petroglyph Project, Hawaii.

Volume 17 (1990/91), Numbers 1-4:
LEE, G.: Conservation and protection committee report.
DORN, R. I.: Rock varnish dating of rock art: state of the art perspective.
CROTTY, J.: Report of the 1990 New Mexico rock art recording field school.
BOCK, A. J.: Rock art recording on Arizona Strip, Petrified Forest.
BEDNARIK, R. G.: On mammoth figurines.
BARABAS, M.: Basic care: photographs and paper.
LEE, G. and A. J. BOCK: Leave only footprints ... but please, NOT on the rocks!
BEDNARIK, R. G.: Dating eons with cations.
LEE, G.: Conservation and protection committee report.

Enquiries to the Secretary, ARARA, P.O. Box 65, San Miguel, CA 93451, U.S.A.

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The Rock Art Quarterly. Journal of the Rock Art Association of Canada/Association Art Rupestre du Canada. Edited by Maurice Lanteigne. The first volume contains the following scientific papers:

Volume 1, Numbers 1+2 (1990):
FREEMAN, G. R., P. J. FREEMAN and R. G. FORBIS: Sacred rocks of Alberta.
BEDNARIK, R. G.: About professional rock art vandals.
STEINBRING, J. and M. LANTEIGNE: Directions in rock art research.

Volume 1, Numbers 3+4 (1990):
BEDNARIK, R. G.: Global developments in prehistoric art studies.
BEHM, J. A.: Petroforms in Wisconsin: implications for the agricultural belt of the Midwest.
GOODS, B. M.: Pigment analysis of the Rice River pictograph, Manitoba.
PAGER, S.-A.: Rock art research in South Africa.
PARKMAN, E. B.: If rocks could talk, what we might learn by listening: a discussion from California's North Coast Ranges.

Enquiries to the Secretary, RAAC, Department of Anthropology, University of Winnipeg, 515 Portage Avenue, Winnipeg R3B 2E9, Canada.

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SIARB Boletin. Number 4, June 1990. Annual journal of the Sociedad de Investigación del Arte Rupestre de Bolivia, principal rock art periodical of Latin America. Edited by MATTHIAS STRECKER. Number 4 has 90 pages, and includes English summaries. It contains the following major contributions:

BEDNARIK, R. G.: Sobre la práctica de tizar los petroglifos.
FALDIN ARANCIBIA, J. D.: Las pictografías de Kalakala, Depto. de Oruro, Bolivia.
HOSTNIG, R.: Una nueva localidad de arte rupestre en Apurímac - Perú: Ulamayoce.
GALVEZ MORA, C. A., R. BECERRA URTEAGA and S. CASTILLO REYES: El petroglifo de Alto de la Pichona, Valle de Chicama, Perú.
ONETTO, M.: Secuencia estilística del arte rupestre en el Valle de Piedra Parada, Provincia de Chubut, Argentina.
SEDA, P.: Estudio de cronología en el arte rupestre de Minas Gerais, Brasil: el Sitio Boqueirao Soberbo.

The journal also contains book revies and numerous announcements, including international news.

Enquiries to the Secretary, SIARB, Casilla 3091, La Paz, Bolivia.

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The journals of other IFRAO-affiliated organisations will be reviewed in the next issue of *RAR*, including *Survey* (CeSMAP), *Purakala* (RASI), *Jahrbuch der GE.FE.RI.*, *Pictogram* (SARARA), as well as other rock art journals.

Forthcoming book reviews:
L'Image des animaux dans l'art préhistorique, by Henri Delporte.
The rock paintings of the upper Brandberg, by Harald Pager.
Signs from the ancestors, by M. Jane Young.
The great art of the early Australians, by Jan Jelínek.
The rock paintings of Arnhem Land, by Darrell Lewis; and others.

ORIENTATION

Cairns '92

THE SECOND AURA CONGRESS
Cairns, 30 August - 4 September 1992
SECOND ANNOUNCEMENT

ACADEMIC PROGRAM

As indicated earlier, the Second AURA Congress will consist of about ten subject-based symposia. Work is currently proceeding on the following symposia:

Rock art studies: the post-stylistic era. Chaired by Dr Michel Lorblanchet (France) and Dr Paul Bahn (U.K).
Rock art and information exchange. Chaired by Claire Smith (Australia) and co-chair.
Rock art studies as a curriculum for teaching. Chaired by Dr Giriraj Kumar (India) and Professor Osaga Odak (Kenya).
The rock art of the Sahara. Chaired by Alfred Muzzolini (France) and co-chair.
Spatial considerations in rock art. Chaired by Dr Paul Faulstich (U.S.A.) and co-chair.
Dating of rock art. Chaired by Alan Watchman (Australia) and Professor Jack Steinbring (Canada).
Rock art of north Queensland. Chaired by Dr Mike Morwood (Australia).
General session. Chairpersons to be appointed.

(Proposals are still being considered for about two more symposia, and more suggestions are invited.)

The rationales for the first five symposia listed appear in the *Brief Reports* section of this issue, and are being published in numerous other journals. The rationales for the remaining symposia will appear in the next issue of *RAR*.

A total of 180 scientific papers will be accepted for presentation, and another 20 as 'stand-by papers'. Preference will be given to papers presented in English, although French, German, Russian and Spanish papers may be accepted, and would be followed by English summaries. Most papers will be published within two years of the Congress.

The plenary session will be held at the Cairns Civic Centre and will be open to the public and the media. There will be a public performance of the Tjapukai Aboriginal Dance Company (considered to be the best in Australia). The plenary session is expected to be opened by the Prime Minister of Australia and there will be key note addresses by distinguished Aboriginal luminaries, including the Congress Patrons, Lindsay Roughsey (Burrud) and George Musgrave. The academic key note lecture will be delivered by AURA Vice-President, Professor Jack Steinbring.

The 1992 Meeting of IFRAO is to comprise two parts: an open, plenary session, and a business session attended only by the official delegates of the Federation. The Second General Meeting of AURA will take place on Friday, 4 September, at the congress venue, and can be attended only by AURA members.

FIELD TRIPS

Here is the North Queensland Program as designed by Stephen Trezise, the General Tours Co-ordinator of the Congress. The Laura district boasts one of the major rock art concentrations of the world. Queensland is estimated to possess in excess of 50 000 rock art sites. The congress field trips in the Laura area are conducted under the auspices and with the co-operation of the Ang-Gnarra Aboriginal Corporation, which represents the indigenous custodians and owners of the art sites and will provide trained guides for the congress tours.

Pre-congress field trips
Jowalbinna Bush Camp (S. Trezise):
25-29 August: 5-day 4WD safari, $A750 all inclusive.
26-29 August: 4-day 4WD safari, $A600 all inclusive.
28-29 August: 2-day fly/fly, $A595 all inclusive.
29 August: 1-day fly: twin engine $A275, single engine $A210.
These tours include major rock art sites at the Quinkan Reserves and optional day tour to the wetlands of Lakefield National Park. Participants supply own sleeping bag.

Deighton River Bush Camp (S. Trezise):
24-28 August: 5-day wilderness rock art trek, drive/fly, $A625 all inclusive.
Bush walk to rock art sites of the Deighton River region, for the fitter participants. Supply own sleeping bag.

Overview of north Queensland rock art (organised and led by Bruno David):
24-28 August: 5-day self drive (4WD only), approximately $A200 per person.
This self-drive (or shared 4WD rental vehicle) tour takes in Davies Creek, Chillagoe, Mitchell, Palmer and Laura. Participants provide camping equipment and food.

Self-drive tour to Laura:
Car rental companies permit only 4WD vehicles to travel to Laura due to road conditions, although Laura is accessible by conventional vehicles. The driving time from Cairns to Laura is about 5 hours. Laura is a very small town with limited services. Camp grounds and guide services are available.

Bus tour to Laura:
26-29 August: 4 days, $A200, guide fee extra ($6-15 per day), camp ground fee included, provide own tent, bedding, food.

Post-congress field trips
Jowalbinna Bush Camp (S. Trezise):
5-9 September: 5-day 4WD safari, $A750 all inclusive.
5-11 September: 7-day 4WD safari, $A950 all inclusive.
5-6 September: 2-day fly/fly, $A595 all inclusive.
5 Sept.: 1-day fly, twin engine $A275, single engine $A210.
These tours include major rock art sites of the Quinkan Reserves and optional day trip to wetlands of Lakefield National Park. The 7-day safari also visits Deighton River Bush Camp. Participants supply own sleeping bags.

Deighton River Bush Camp (S. Trezise):
5-8 September: 4-day wilderness rock art trek, $A500 all inclusive.
For the fitter participant. Supply own sleeping bag.

Bus tour to Laura:
5-8 September: 4 days, $A200, guide fee extra ($6-15 per day), camp ground fee included, provide own tent, bedding, food.

4WD safari to Laura:
5-9 September: 5 days, $A550 all inclusive, guide fee extra ($6-15 per day), supply own sleeping bag.

Boat trip to Flinders and Stanley Islands (Dr Nicky Horsfall):
Post-contact rock art, 3-5 days, details to be advised.

Rock art in Townsville area (Elizabeth Hatte):
Details to be advised.

Expeditions led by Percy Trezise:
Details to be advised.

In addition to these pre- and post-congress field trips, Trezise Bush Guide Service will be operating Jowalhinna Bush Camp and Deighton Bush Camp as usual. The current exchange rate is about $A1.00 = US$0.77.

Field trips outside north Queensland
These are yet to be planned. One firm proposal is currently available:
Grand Tour 1992 (Dr Hugh Cairns and Howard P. McNickle):
This is to be modelled on the Kimberley-Pilhara tour of 1988, and will cover northern Australia, including the Pilbara. Some of the sites seen in 1988 will be revisited, for instance it is intended to spend more time at the extensive Spear Hill rock art complex. Logistics will be by private or hired, mostly large, 4WD vehicles, expedition style, duration 1-2 months.

REGISTRATION AND ACCOMMODATION

Registration fees will be identical to those of the First AURA Congress: $A80 for members registering early ($A40 for students), $A100 for late registrations (students $A50). Registration forms will be in the November 1991 issue of *RAR*.

Full details of accommodation options will also be provided then, including the discounted rates we have been able to obtain. Accommodation costs range from $A10 (backpacker's hotel, a few minutes' walk from congress centre) to several hundred dollars per night. We will provide details of between 20 and 30 hotels which have offered special rates to congress participants.

EXHIBITION

The opening of the AURA Exhibition, originally planned to coincide with the Congress, is now scheduled for 1993, at the Australian Museum, Sydney. A small part of it, however, is expected to be assembled at the Regional Art Gallery in Cairns. Additional exhibits (recordings of rock art) will be invited from congress participants.

As in Darwin, films and videos of rock art and related subjects will be shown in Cairns.

FURTHER INFORMATION

The *venue* of the Congress will be one of the three 5-Star hotels still being considered. A decision is to be made soon, and will be based on the bidders' facilities and on costs, including the discounted accommodation rates for delegates.

The *pre-congress publications* will include all abstracts of the papers listed for presentation at the Second AURA Congress.

Ansett Airlines has been appointed as the *official domestic airline of the congress* (their offer includes free travel passes and an extensive package of negotiated hotel rates, discounted air fares and tours, co-ordinated by Coles Myer Ansett Travel).

For conditions of *subsidisation* of congress participants, please refer to page 152, November 1990 issue of *RAR*.

R. G. Bednarik (Congress Co-chair)

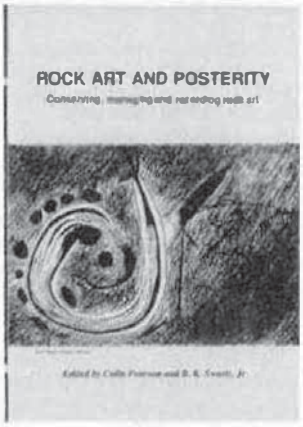
RAR 8-198

DARWIN CONGRESS PROCEEDINGS

The first volume of the Darwin congress proceedings, *Rock art and posterity*, has just been released and will be followed by others over the coming months. The first volume covers Symposium M ('Conservation and site management', edited by Professor Colin Pearson) and Symposium E ('Recording and standardisation in rock art studies', edited by Professor B. K. Swartz, Jr). It is profusely illustrated (62 plates and figures, plus many maps and tables) and set in quality Linotronic print on art paper.

The list price is $A26.00, but the volume is available to members of any of the 17 organisations affiliated with IFRAO (practically all the major rock art organisations in the world) at *50% discount* ($A13.00, or US$10.00, plus packing in sturdy cardboard boxes, $A1.50, and surface postage of $6.20 within Australia, $A9.40 to anywhere in the world, *only one copy per member*). This price represents the actual printing cost (allowing for review and complimentary copies), plus packing and postage. Printing costs were underwritten by the Australian Institute of Aboriginal and Torres Strait Islander Studies and by the Australian Commission for Unesco. **All money recovered from sales will be paid into the AURA Publications Fund**, which will meet the costs of printing future quality work in palaeoart studies. Thus by purchasing this volume you are contributing to the establishment of a perpetual publishing base for rock art studies.

To order at the special rate, please state that you are a member of a rock art organisation, and send a bank draft or postal money order for **$A20.70 within Australia**, or **$A23.90 (US$18.40) anywhere else** in the world, or simply provide your Visa or MasterCard number and expiry date, to:
AURA, P.O. Box 216, Caulfield South, Vic. 3162, Australia



BIBLIOGRAPHICAL DETAILS
Rock art and posterity: Conserving, managing and recording rock art. 1991. Edited by Colin Pearson and B. K. Swartz, Jr. Proceedings of Symposia M and E, First AURA Congress, Darwin 1988. Occasional AURA Publication No. 4, Australian Rock Art Research Association, Melbourne. 160 pages, 40 plates, 22 line drawings, 21 maps, 19 tables, on 100g/m² matt art paper, set on Linotronic 330 - 2500 dpi, laminated paperback cover, full A4 format (297x210 mm). ISBN 0 646 03751 X. RRP $A26.00 (50% discount applies to orders from affiliated specialists and to multiple orders, min. 5 copies).

Comprises contributions by Robert G. Bednarik, Tony Blanks, Roberto Bosio, Steve Brown, J. Brunet, Elizabeth Caldicott, Mauro Cinquetti, J. Clarke, Paul Faulstich, J. Flood, Tommaso Guiot, Ivan P. Haskovec, P. Haydock, William D. Hyder, D. Lambert, Colin Pearson, N. North, Peter Randolph, Piero Ricchiardi, Eric W. Ritter, Dario Seglie, Nicholas P. Stanley Price, Hilary Sullivan, B. K. Swartz, Jr., Andrew Thorn, Gajendra S. Tyagi, P. Vidal, J. Vouvé, Joseph Wallam, Grahame L. Walsh, John K. Zamcanella.

The proceedings of Darwin Symposium G, ***Rock art and prehistory***, have just been released by Oxbow Books, Oxford. We have requested a special discount for members. At this stage, only the bibliographical details are available:

Rock art and prehistory, edited by Paul Bahn and Andrée Rosenfeld. Fifteen papers presented in Symposium G, First AURA Congress. A4 format, 144 pages, with figures and plates. ISBN 0 946897 29 8. Paperback, £15.00, US$30.00.

Order from: Oxbow Books, Park End Place, Oxford OX1 1HN, United Kingdom.

Or: Oxbow Books, P.O. Box 5605, Bloomington, IN 47407, U.S.A.

AURA EXHIBITION

There have been some changes in this major project. The departure of Robert Edwards from the Museum of Victoria has resulted in the proposal to open the exhibition at the Australian Museum (Sydney). That museum has responded positively to the opportunity of developing the concept, and a meeting of the steering committee (Robert Bednarik, Ross Clendinning, Robert Edwards, Dr Paul Taçon) will be held in Sydney on 14 June 1991.

About 20 firm draft proposals have been received, for exhibits representing the main rock art regions of the world, and including some truly spectacular material (especially in the form of portable palaeoart). The project involves significant logistic challenges and is tentatively planned to open in 1993. After travelling to major Australian cities the exhibition will be made available to other countries, under the auspices of IFRAO.

MEMBERSHIP FEES

Annual membership fees and subscriptions are now due for 1991. They remain the same as in previous years. However, the following changes apply in order to streamline procedures, particularly for payments from abroad:

1) We have facilities to accept payment by Visa or MasterCard: simply provide card number and expiry date. Since other overseas payments must be in the form of bank drafts or international postal money orders, many subscribers will find this considerably more convenient, and less costly. (Personal cheques from abroad can attract bank charges of up to $40.00, payable by AURA!)

2) Another measure to counter the annual inconvenience to subscribers (especially those abroad) is the new option of obtaining **Life Membership** with AURA. The cost equals ten annual fees at current rates: $A150.00 (or US$116.00) for subscription of *RAR*; $A200.00 (or US$154.00) for full membership (which includes journal subscription); and $A280.00 (or US$216.00) for full membership with air mail delivery of periodicals. Life members are not affected by any future price increases, their subscriptions are paid for life.

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CHACMOOL 1990: A post-impressionist view

MAURICE P. LANTEIGNE

The denial of metaphysical Truth (the inability to deconstruct intentionality, as suggested by Davis 1990) goes a long way towards developing an acute awareness of the resistivity of prehistoric ideologies for factual analysis, in conceiving the transparency of language as a rational witness of an irrational process. Constant repudiations of traditional significance (and signifiers), however conversant with semantic realities, not only erode past correspondences of methodological knowledge now held in stasis, but also become Robespierre-like guillotines constitutive in and of themselves with the essence of a 'meta-science'.

Consumed by the textual conversion of the past into the present (Shanks and Tilley 1989) and the present into the past (Hodder 1989), such self-mutilating castigations as currently being performed between critical theorists, eco-feminists, post-processualists, post-structuralists, post-modernists etc., while inspirational in re-defining relations of relevance between the past and present, are nothing more than mirrors of the participants in the colloquy, not of the subject-matter in hand.

The now fashionable denunciation of intellectual elitism, questioning of creative enterprises, diacritical pontification of conventional social sciences, and socio-political fragmentation of textual strategies beyond the pragmatic as vaunted by many literary technicians were evident (although not always with intellectual clarity, nor intentionality) at the 23rd Annual Chacmool Conference held in Canada on 9-11 November 1990. In an attempt to set up challenges to the theoretical foundations of archaeology, the University of Calgary's conference opened as a basis for discussing the concept of ideology, in all of its varied manifestations: as the ideology of cities, of death, of art etc. Many scholars snapped at the abstraction like salmon at a fly, with dog-eared tails flaying about in a concentric swirl of foam as the barbs, intrinsic in the philosophy of ideology, hooked deeper into their discourse.

Some naively believed that ideology, as embodied in comparative sets of past relations, constituted ultimate meaning transmittable by ultimate signifiers. Others considered the transmission of variability as commensurate with adaptive social evolution. While still others conceived analytical procedures more suited for the natural sciences, quite incompatible with socio-ideological phenomena. Many claimed to be 'post-*dernier-cri* (Renfrew 1989: 34) and, thus, now beyond the boundaries of past criticisms. But all failed to assuage the various dissatisfactions - as voiced by Shanks and Tilley (1987a, 1987b, 1989). The arrogance of some was exemplified by their solemn approach to criticisms, in others with their flippant unabashed candour, and in others - the humour with which they bantered and baited their antagonists. In short, the conference reflected not so much a concern for the ideologies of the past, but of ideologies *about* the past.

Perhaps a bloody revolution as that being proposed by Shanks and Tilley (op. cit.) is necessary in order to eradicate the lesions festering in the social science body - archaeology. There was no revolution, however privately wished for by many, at Chacmool 1990; nor even a hint that one was imminent. What was evident was the *conspicuousness of an absence*. Many spoke at great length about ideologies, with ideologies, for ideologies; mouthing words others wanted to hear, waited patiently in the darkness, wistfully listened while many slept dreaming of the encroaching silence surely to follow. But what of they of whom we spoke?

The University of Calgary's Archaeology Department is perhaps one of the more progressive in Canada, and its international reputation was clearly evident at the Chacmool conference; with more than 120 scholarly papers and 200 participants, representing some 16 countries, involved in four full-day and 11 half-day sessions. Of these, 37% were on rock art; 84% (38/45) of which occurred in two full-day and two half-day sessions. The international renown of some added a great deal of credence to the conference, and included such notables as: H. Mueller-Beck, N. Rolland, C. Meiklejohn, J. Nordbladh, A. Kehoe, G. Sauvet, M. Conkey, D. Lewis-Williams, W. Davis, C. Chippindale, P. Russell, P. Bahn, J. Steinbring and F. Bock, among others.

Many could write about the niceties of the conference, discuss in calm, rational monotones the epistemological delights

and shortcomings of individuals, methodologies, interpretations and general trends. There are several broad, well-lit avenues of thought which have been forged for us and we, at one time or other, have gazed in wonderment at their intellectual straightness and purity. Not many speeding along the freeway, however, would hazard to stop and rifle through the detritus and brambles rooted beneath these pillared constructs, for fear of being run over by self-intent intellectuals bringing up the rear. The city fathers, in their infinite wisdom, had foreseen such idiosyncratic behaviour, and installed concrete barriers and wire fences. Fortunately, there is a sufficient number of freelancers who, with wire-cutters in hand, would explore the forbidden paths upon which such foundations are set, to risk reputations and accolades in search of, dare I say, Truth.

Despite the political and social hiccups which continue to erupt beneath the cultural patchwork quilt of Canada (e.g. Native protests at Oka and Kanesatake), Canada's intellectual elite still fabricates invisible barriers against those of whom, about whom, we speak; those whose past and present sets of relations form the very *raison d'être* of a social discipline ultimately devoted to understanding why ideologies, the perceptual reasons for being, play such major roles in determining how we interact with others. At Chacmool, these 'others' were absent; these 'others' were not invited; these 'others' continue to be denied reasonable access to this elite doctrinaire socialisation process.

For whom do we write? To whom? And why not these 'others'? It cannot be because we fear Truth, for that is our goal - is it not? It cannot be because we fear the 'fringe effect' - for many of us are exceedingly eccentric in the first place. It cannot be because we fear our elitist position to be threatened - for many of us, such security has never been a motivational factor. Can it be because we ourselves are an inseparable by-product of our own ideologies, of social reality, a political set which denies the participation of an 'otherness'? Such unsettling questions are probably the most existential challenges facing the discipline today, an amoral ignorance of which can only continue to contribute to the current malaise and social irrelevance of our work. Yes, social irrelevance. At least it is so perceived by these 'others' who have been literally 'studied to death'!

In self-defence, some would view the foregoing as the diatribes of a disillusioned poet; others - the sabre-rattling of a 'Johnny-come-lately' clamouring for attention; while still others would be prepared to acknowledge the concerns, but require ready-made 'solutions' provisionally attached prior to any consideration of the questions. The more doubtful, however, would demand to see the faces, to thrust fingers into the spiked hands and slashed abdomens, to witness the blood-sweat dripping from thorn-lacerated foreheads of these 'others'. The metaphor is not outrageous. Out of Canada's First Nations we create our Christs, time and time again, until we are totally consumed by the ravages of our guilt, a MacBethian guilt we can never wash from our hands. Like true Wagnerians, we subsume this guilt under the pseudonym - Academia, place it under glass and curate it for our eternal pleasure.

Some would suggest that politics does not belong in science (e.g. Trigger 1990: 381); perhaps, but social relevance surely must. Despite our smugness, Canada has failed to succinctly address the conscientiousness of our 'science' with the same concern as has been done for animal experimentation. Many readers would not connect the two; how can ethical issues embodied in medicine be compared in the same breath with archaeological research? To borrow from a Dickens parable: I speak not of the Past nor of the Present, but of that which is Yet To Be.

There is a private understanding among the more astute of an impending mitosis, but not a standard mitosis with a 'sharing' of genetic characteristics. Rather a mutation, a separation of humanistic morality from clinical empiricism (of church and state - if you will). True, this maturity is essential if scientific advances are to be made; but even in the making of the most perfect of democracies, as that advocated by Shanks and Tilley (1990), from such reformations the socially disadvantaged shoulder the greatest burden.

Few among us would deny the altruistic value of our work. Few among us would deny our noble intentions. Few among us would deny that our hearts are in the right place, when we say that we strive to protect indigenous and prehistoric art heritage for the benefit of all concerned. For those of us who work in geographic regions of prehistoric art created by a people long since vanished into time, our task is relatively straightforward: to protect, preserve and manage in a responsible and effective manner; such that our children may understand, appreciate and respect the art, the artists, and our efforts. For those of us who work in regions of the world where the original peoples of these arts still strive to survive, to eke out an existence from an unforgiving landscape, and to forestall the onslaught of a culture whose power is ultimately derived not from finance, politics, military or science, but from ideas; our task is doubly difficult. We must not only protect the art heritage of these peoples from the destructive capabilities of our own cultural ignorance and selfishness, but these things we must do in a manner sensitive to the dignity and respect of the peoples themselves.

It has been stated elsewhere that rock art researchers are more responsive than archaeologists, to the issues and concerns of the indigenous peoples we study, for we do not 'ransack cultural heritages and destroy' (Bednarik 1990: 105) in the pursuit of our work. As we are well aware, in today's world destruction can come about in many shapes and sizes; revealing not only the fragility of this planet, but also our own physical and spiritual limitations. Throughout human history, the most comprehensive forms of cultural destruction have always been initiated through misaligned beliefs.

It may seem incredulous to think that our study of, and munificent concern for, indigenous arts could possibly be perceived to be insulting or debasing to anyone. But it is; and to peoples who have been 'studied to death' the reasons are relatively straightforward. Among many Native cultures in Canada art is still seen as the physical documentation of their laws, history, teachings and religion. Its lessons are granted only to the chosen few, individuals who have entered into a sacred trust and demonstrated a lifetime's commitment to upholding and preserving a people's propriety and value system. It is not for the prying eyes of the unlearned, the unpurified, the unknowledgeable, the tourist or the camera. The 'scientist's' interpretation is often uncomplimentary, oversimplified, contradictory to, and blasphemous of, indigenous teachings. Blatant misuse of these teachings sometimes results in a loss of cultural credibility, manifested by open ridicule, both within and outside the culture under investigation. To a people who have lost their rights to land, to freedom of movement, to language, to laws, to history; to be threatened with the loss of human dignity can be the most debilitating of all.

Just as the state of environmental health may be measured by specific categories of organisms, so too the health barometer of a nation is measured by its indigenous peoples. Among some world nations there is an active amelioration in public policies for addressing indigenous political, social and religious concerns. In Canada, however, such developments are piecemeal and born of bureaucratic necessity, not of the heart. We delude ourselves to suggest that our discipline's rapport with indigenous peoples is inherently good. My own personal experiences indicate otherwise. If our work is to have any lasting and positive value, then it must be socially-relevant to today's world and to today's problems. Although Canada's intellectual elite applaud themselves on the altruistic value of their work, have we really examined its social pertinence? Have we properly addressed the concerns of the Native peoples we study? Have we striven to include them, as intellectual equals, in all aspects of the decision-making process as to conservation, research ethics, education, site management etc. Have we really honoured our social commitments? Have we even attempted to define what these social obligations are?

Such is what was avoided at Chacmool 1990 - a sensitive, understanding ideology based on social relevance!

While the humanistic facet of my literary regimen exhorts that I should now close this vitriolic precis of Chacmool 1990 so as to maximise its effect, the epistemological training which I have subjected myself to in the pursuit of a cogent research strategy advises that I should at least acknowledge the existence of another perspective of the conference. Certainly none of the conference participants 'intentionally' ascribed to eschewing indigenous scholars from the event, as intimated by the foregoing. Nor would they not support efforts designed to help alleviate the negative aspects of our discipline currently de-humanising Canada's Native peoples. The quandary is not founded upon artifice, but upon neglect; our willingness to acknowledge, enfeebled by our perceived ineffectiveness to resolve, the problem.

My own dissatisfaction with Chacmool 1990 is a precipitate of a spiritual growth tempered by a cumulative effect of five years of casual socialising with Native peoples in northern Canada. My participation as an organiser and chair of a rock art session at the Canadian Archaeological Association conference in Whitehorse, Yukon in May 1990 has helped to accentuate the failure of these, what I now recognise to be, very limited personal interactions. What fascinated me most about the Whitehorse conference was the degree to which Native peoples participated in its organisation and scholarly presentations. In retrospect, this was not surprising given that Yukon College has the highest Native student percentage ratio of all post-secondary educational institutions in Canada. Native scholars - such as Johnny Epoo on the role of Native-designed institutes for protecting their cultural heritage, Louise Profeit-LeBlanc's admonition for stricter research ethics and sensitivity to Native religious concerns and Elsie Netro's innovative approach for teaching archaeologically-derived cultural information to Native peoples - garnered the greatest attention and praise from Whitehorse conference participants.

The past twenty years of my rather avocational interest in archaeology has been harangued by lingering doubts about its efficacy as a 'humanistic' science, and its seemingly lethargic concern for Native peoples in the present tense. I left Whitehorse with renewed confidence, convinced that finally my compatriots were becoming intensely concerned with the social relevance of their discipline and willing to initiate major progressive changes. My arrival at Chacmool, just six months later, was flushed with the expectation that critical aspects of Native ideology recounted by Native scholars, as at Whitehorse, would be forthcoming. Not only were Native scholars not in representation as conference participants, neither were they as members of the audience. What was more astonishing was that, of the almost 200 international scientists, no-one seemed to notice (let alone, even mind) their 'absence', with the sole exception of E. Breck Parkman who attempted to fill the void by opening and closing his presentation in a unique and provocative Native oratorial tradition. Although his gesture was but a small attempt to inject some compassion into an otherwise clinical forum, the echoes of the 'Three-Clap Ceremony' which terminated his presentation reverberate to this day throughout the empty mirrored-halls of MacEwan Centre.

REFERENCES

BEDNARIK, R. G. 1990. Global developments in prehistoric art studies. *Rock Art Quarterly* 1(3-4): 101-6.

DAVIS, W. 1990. The deconstruction of intentionality in the archaeology of representation. Paper presented at the 23rd Annual Chacmool Conference, Calgary.

HODDER, I. 1989. Comments on archaeology into the 1990s. *Norwegian Archaeological Review* 22(1): 15-8.

RENFREW, C. 1989. Comments on archaeology into the 1990s. *Norwegian Archaeological Review* 22(1): 33-41.

SHANKS, M. and C. TILLEY 1987a. *Re-constructing archaeology*. Cambridge University Press, Cambridge.

SHANKS, M. and C. TILLEY 1987b. *Social theory and archaeology*. Cambridge University Press, Cambridge.

SHANKS, M. and C. TILLEY 1989. Archaeology into the 1990s. *Norwegian Archaeological Review* 22(1): 1-54.

TRIGGER, B. G. 1990. *A history of archaeological thought*, reprint. Cambridge University Press, Cambridge.

RAR 8-199

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Notices

THE 1991 AURA MEETING was held at Halls Gap, Victoria, 23-24 March 1991. A report will appear in the next *AURA Newsletter*.

PROPOSALS FOR CAIRNS are still welcomed, for symposia as well as for field trips outside of north Queensland.

The INFORMATION EXCHANGE NETWORK of AURA has grown significantly in recent years and now spans dozens of agencies and countries. A list of the organisations AURA has formal arrangements with will be provided in one of the next issues.

AURA EDITOR Robert G. Bednarik has been appointed by Chairman Dr Jean Clottes as National Co-ordinator of the Comité International ICOMOS pour l'Art Rupestre.

SAHARAN ROCK ART is the focus of a new association formed in Milano (Italy), the Association des Amis de l'Art Rupestre Saharien. It has elected a provisional executive headed by Alfred Muzzolini, the editor of the magnificent journal *Sahara*. The A.A.A.R.S. has held a second meeting in St-Martin-d'Ardèche (France) on 10-11 May 1991, the results of which were not available at the time of going to press.

SCANDINAVIAN ROCK ART was the subject of a conference from 21-26 May 1991, at the University of Gothenburg, Sweden. Entitled 'Evaluation of rock art research: the case of the Nordic countries', it was organised by Jarl Nordbladh, Ulf Bertilsson and Knut Helskog.

PIEDRA PINTADA is a small mail order book business specialising in titles (mostly U.S. publishers, both in print and out of print titles) on the subject of rock art. Readers wishing to be placed on the mailing list for catalogues should write to AURA member Bob Edberg, Piedra Pintada Books, P.O. Box 1376, Claremont, CA 91711, U.S.A.

All BACK ISSUES of *RAR* prior to 1988 are now out of print. The rapid expansion of AURA in 1987/88 has depleted our stocks of the issues predating the increase of circulation.

FORTHCOMING EVENTS:

IIIrd International Rock Art Symposium in Bolivia, 25-28 June 1991, Santa Cruz, Bolivia. Contact Matthias Strecker, SIARB Secretary, Casilla 3091, La Paz, Bolivia.

'Rock art - the way ahead'. SARARA conference and IFRAO Meeting 1991, 25-31 August 1991, Cathedral Peak Hotel Conference Centre, Natal, South Africa. Contact S.-A. Pager, SARARA President, P.O. Box 81292, Parkhurst 2120, South Africa.

Chalcolithic and Bronze Age menhir statues (seminar), 20 August - 8 September 1991, Capo di Ponte, Italy. Contact Professor E. Anati, CCSP, Capo di Ponte (Bs), 25044 Italy.

Prehistoric and tribal art: Old World and New World, convergences and divergences, 20-25 September 1991, also at Capo di Ponte.

International rock art conference in China, 5-10 October 1991, Yinchuan, China. Contact Professor Chen Zhao Fu, RARAC President, Central Institute for Nationalities, 100081 Beijing, China.

Second AURA Congress and IFRAO Meeting 1992, 30 August - 4 September 1992, Cairns, Australia. Contact *RAR* Editor.

IFRAO Report No. 6

IFRAO KEYWORDS
International standardisation of keywords for rock art
DARIO SEGLIE

The use of keywords (4-5 words that frame and synthesise a paper) is already widely accepted and frequently adopted by most of the scientific periodicals dealing specifically with rock art as a general subject (e.g. *Purakala, RAR, Survey*).

The use of keywords can take place at different levels:

(a) To assist a reader who manually skims through a publication or library file.
(b) To enable computerised research on bibliographical files contained in data banks using a specific computer program.

The major application obviously resides in point (b). With a computer system it is possible to formulate specific and exhaustive enquiries. The possibility of nominating between one and five keywords would enable a researcher to obtain a complete list, as far as the data bank is complete, of the accessible relevant literature at the library or bibliographic documentation centre concerned.

The keywords are therefore of basic utility in bibliographic research. To optimise their potential it is necessary for all the principal rock art organisations to use the same common language, i.e. it is vital to have only one standard keyword list for rock art research.

IFRAO, which unites all the major institutions for rock art in the world, has the opportunity of introducing a project of standardisation and of computerised management, through the standardisation of keywords in future bibliographies and the progressive re-classification of the existing literature.

To obtain a standard list of keywords it is first of all necessary to have the co-operation of every relevant organisation (such as institutes, research centres, museums, societies etc.) and member of IFRAO, as well as the contributions of individual researchers in the field of rock art studies. Each institution or researcher should complete and suggest a list of keywords being used, or believed to be useful.

It is proposed that the CeSMAP (Centro Studi e Museo d'Arte Preistorica, Pinerolo, Italy) would carry out the standardisation project by consolidating all the suggested lists and compiling a unified draft list of *IFRAO Keywords for Rock Art*. This list should then be checked by those who have provided the initial suggestions.

Once all further suggestions of integration or modification have been considered, the first final standard list of *IFRAO Keywords for Rock Art* would be compiled, with a specific software for its computer-aided management.

Considering the global scope of this project, the correspondence, information exchange and submission of keywords should all be in one single language, English.

Suggestions for appropriate 'Keywords for Rock Art' are invited in accordance with the standard form shown below.

To: IFRAO Keywords Project
Centro Studi e Museo d'Arte Preistorica (CeSMAP)
Viale Giolitti, 1
10064 Pinerolo
Italy

From: (Name, position held, institute, address, country, Telephone No., Fax No.)

LIST
Suggestions for IFRAO Keywords (alphabetical order not necessary)

Keyword	Definition (brief explanation of the Keyword)	Similar words, synonyms included in the keyword
Petroglyph	Anthropic mark on rock produced by deductive process	Engraving, carving, pecking, pounding etc.

Comment by IFRAO Convener

The potential significance of this initiative by Professor Seglie of CeSMAP goes far beyond the utility of a bibliographical tool. Although ours is still a very young discipline, there are already tens of thousands of publications in rock art studies. But it is still feasible to standardise existing publications effectively, and all future publications could be added to this data bank. Once the program is complete and available world-wide, it would provide comprehensive access to the world's entire specialist literature in our discipline. Not only could future reference lists be comprehensive, unintended plagiarism and neglect of previous work could be eliminated.

There are many potential applications of the proposed program. At present, it is a lengthy and very time-consuming process to compile even a reasonably complete bibliography on a specific topic, and even after weeks of work one can never be certain how many relevant publications one has still missed. The IFRAO Keywords program would provide comprehensive bibliographical lists within seconds. In most cases one would only need to type in three keywords to obtain quite concise lists, and by entering a fourth keyword, the list would be reduced to just a few entries. One could then explore other possible directions, by selecting alternative keywords. Thus the number of potential applications is limited only by the user's willingness to explore them.

There are basically two types of rock art publications: those that are region-specific, and those that are not. I suggest that the keywords of the former should always include one geographic designation, and that those that deal with a specific site include the name of that site. Keywords should be either single words, or combinations of two words not exceeding 20-25 characters. Each keyword in a set should be distinctive, there is no point in including two similar terms: if one keyword is 'engraving' it would be pointless to use 'petroglyph' as another.

An obvious effect of the proposed program is its standardising influence on the discipline's literature in general. It would be readily available internationally, which means that all literature would be equally accessible, and this would safeguard against the development of specialist enclaves in different regions which are isolated by their lack of awareness of what is happening elsewhere in the world. Thus the proposal is a significant contribution to consistency within the global research community, and as such deserves the unqualified support of all IFRAO members.

RAR 8-200

THE IFRAO STANDARD SCALE
R. G. BEDNARIK

I have been concerned for many years that so much rock art photography is conducted throughout the world which is of limited research potential because there is no indication of size on the photographs. It is so simple to include a standard scale, such as that recommended by Taylor et al. (1979: 306), yet so many photographs are being taken without scales.

However, the need for colour scales on colour photographs

may be far more fundamental. Not only are chroma, value and hue inevitably distorted by lighting, film type, optics and other factors, the fading of photographic film and prints is inevitable: the dyes of all films are unstable.

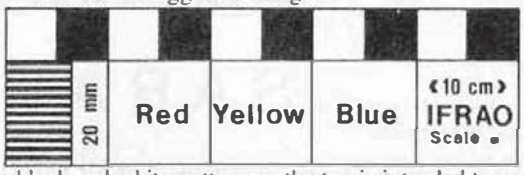
Recent developments in computer technology make it imperative that a colour standard be included in all future photographs of rock art. Colour enhancement methods are available already (Rip 1983, 1989) and it can be only a matter of time before programs of computerised colour correction of photographic material become widely available. But for this technology to be useful for future generations of researchers there must be an appropriate colour standard included on photographs, of known colour definitions (of the three basic colours, standard red, yellow and blue) which a scanner would use as reference points, and reconstitute the true colours of the image or rock surface at the time the photograph was taken.

Not only would this effectively compensate for the shift in colour in conventional photography, it seems only a matter of time before the conversion of colour images to digital format is generally used for permanent and immutable recording and storage. The facility of reconstituting, in future centuries, the past appearance of rock art would be of enormous value to our discipline.

Firstly, this would permit the monitoring, over long time spans, of colour changes in paints and patinae, for which an effective method has so far remained elusive. Secondly, it will facilitate the reconstitution of true colour at the time a photograph was taken, quite irrespective of whether colour was in fact faithfully recorded in the photograph. Thirdly, once the colour definitions have been digitised, they can be manipulated on computer in various ways: they can be compared, cross-checked, nearby other art can be searched for similar values etc. Fourthly, such digitised colour information would be an ideal basis for colour enhancement techniques to identify obscured images. Fifthly, it is obvious that digitised data is ideal for storage, in any form whatsoever.

In short, the anticipated technology would revolutionise rock art studies and conservation strategy. All that is needed at the present time is the placement of a colour standard on each photograph. After 50 years, a photograph will be faded and largely useless, but if it bears a colour standard it may render the art's appearance 'recoverable', with the technology that will no doubt be available then. And the rock art researchers of the year 2041 will praise our foresight in anticipating their capabilities, and in preparing the record for a technology we did not yet possess.

During my IFRAO tour of India last year I suggested that IFRAO might create and distribute a standard colour scale for this purpose. Indian researchers supported the idea enthusiastically. I propose that this project be proceeded with, and offer here for discussion a suggested design:



The black and white pattern on the top is intended to assist in focusing a camera, while serving as a scale of size, and the small pattern on the left is intended for close-up photography. I invite constructive comments on how to improve this design, and on how to finance the production of 5000 or 10 000 such cards. It is intended to distribute them through the IFRAO network (e.g. they can be posted out with journals). The cost of producing and distributing this *IFRAO Standard Scale* would be minimal in comparison to its very considerable long-term benefits. Comments are invited from readers with extensive experience in photography, colour physics or computer technology.

REFERENCES

RIP, M. R. 1983. Digital recording and image processing of rock art by computer. *South African Archaeological Bulletin* 38: 77-9.

RIP, M. R. 1989. Colour space transformations for the enhancement of rock art images by computer. *Rock Art Research* 6: 12-16.

TAYLOR, J. M., W. BOKMAN and I. N. M. WAINWRIGHT 1979. Rock art conservation: some realities and practical considerations. In D. Lundy (ed.), *CRARA '77. Papers from the Fourth Biennial Conference of the Canadian Rock Art Research Associates*, pp. 293-323. Heritage Record No. 8. The British Columbia Provincial Museum, Victoria.

RAR 8-201

CODE OF ETHICS FOR SAMPLE REMOVAL

ROBERT G. BEDNARIK

While archaeology has to obtain its stratigraphical information through the systematic destruction of archaeological deposits, rock art research prides itself in being a non-destructive discipline. Apart from certain recording practices (Bednarik 1990a, b) which have now been abandoned in most parts of the world, this is generally true, but there are some exceptions.

It is clear from the recent specialist literature that there has been a burgeoning of rock art dating techniques and projects during the 1980s. Methods of direct rock art dating involve the removal of samples, however tiny they may be. As long as they are used only on rare occasions, the extremely small quantities of accretionary deposit or paint sacrificed as samples can reasonably be disregarded. For instance, at the time of writing, only nine AMS-dated pigment samples have been published world-wide (e.g. Lorblanchet et al. 1990; McDonald et al. 1990). But as this new technology will become very popular among researchers, methods will proliferate, and further analytical techniques are also used or being contemplated, such as extender analysis (Clottes et al. 1990), animal proteins (Li 1991), and most recently, the spectacular work of Watchman (1991, publications in progress) has signalled an entirely new phase in analytical rock art studies.

In view of these very recent developments it is essential that an internationally acceptable code of ethics regulating sample removal be created which can provide guidance to the relevant agencies in the various countries. While the flurry of research activity in the field of direct rock art dating is to be welcomed, it should be regulated by appropriate standards that, without stifling research needlessly, prevent unrestrained proliferation of sampling. Such a code must consider many aspects, including the views and rights of indigenous owners of art (where they exist), cultural heritage protection standards, specialist opinions, research directions and priorities, realistic conservation targets, and others. Non-interfering analytical methods of rock art dating (Bednarik 1991) should obviously be favoured where they exist, and will predictably be exempt from many restrictions that may be imposed on other methods.

To this end I propose that preliminary discussions should take place during the forthcoming IFRAO Meeting in South Africa, with the aim of acquainting Federation members with the issues and requesting appropriate feedback. Relevant specific proposals, from any rock art researcher in the world, should be sent to the current IFRAO Chairperson, the President of SARARA (Shirley-Ann Pager, P.O. Box 81292, Parkhurst 2120, South Africa), before the end of July 1991.

The recommendations and suggestions resulting from the SARARA conference should be debated by the community of researchers for one year, and at the Second AURA Congress in Cairns (August/September 1992), a symposium and workshop should be held with the aim of formulating an appropriate code of ethics.

REFERENCES

BEDNARIK, R. G. 1990a. Rock art researchers as rock art vandals. *Pictogram* 3(1): 4-6.

BEDNARIK, R. G. 1990b. About professional rock art vandals. *Rock Art Quarterly* 1(1+2): 11-16.

BEDNARIK, R. G. 1991. A new method to date petroglyphs (in press).

CLOTTES, J., M. MENU and P. WALTER 1990. New light on the Niaux paintings. *Rock Art Research* 7: 21-6.

LI FUSHUN 1991. Rock art at Huashan, Guangxi Province, China. *Rock Art Research* 8: 29-32 (this issue).

LORBLANCHET, M., M. LABEAU, J. L. VERNET, P. FITTE, H. VALLADAS, H. CACHIER and M. ARNOLD 1990. Palaeolithic pigments in the Quercy, France. *Rock Art Research* 7: 4-20.

McDONALD, J., K. OFFICER, T. JULL, D. DONAHUE, J. HEAD and B. FORD 1990. Investigating ^{14}C AMS: dating prehistoric rock art in the Sydney Sandstone Basin, Australia. *Rock Art Research* 7: 83-92.

WATCHMAN, A. 1991. Recent analytical work on Australian rock paintings. Paper presented at the Annual AURA Meeting 1991, 23 March, Halls Gap, Australia.

RAR 8-202

DESTRUCTION OF ROCK ART SITES
IFRAO emergency plan

ROBERT G. BEDNARIK

The IFRAO action to avert the threatened destruction of the Cuevas de Borbón, Dominican Republic (*RAR* 7: 154-5) appears to have been a success. Rock art researcher and speleologist Domingo Abréu Collado, Santo Domingo, reports:

> On 7 March we will be in the area of Borbón to start to develop a plan against contamination and damage caused by mining companies. That plan has been suggested by the Dirección National de Foresta, whose director, Pedro de Jesus Candelier, has been informed about the situation of the caves and their importance for our natural and cultural heritage.
>
> Your letter, and the letters of persons and organisations you have prompted to write, have contributed to stop the destruction of Borbón Caves. We can consider that in this case, the community of rock art associations in the whole world has won another battle in the defence of threatened rock art sites.

I thank the many IFRAO organisations and individuals who contributed to this result. It shows the value of a speedy international response to such a threat, and as this is the third such case within a couple of years I perceive a need to establish a system of dealing effectively with similar threats in the future. This is to be discussed at the next IFRAO Meeting, and suggestions should be submitted to Chairperson Shirley-Ann Pager, SARARA President, P.O. Box 81292, Parkhurst 2120, South Africa.

RAR 8-203

ROCK ART RESEARCH ASSOCIATION OF CHINA FORMED

CHEN ZHAO FU

A meeting was held at Beijing on 12 March 1991, to form a society for promoting the cause of China's rock art studies. The potential of China's rock art as an important source material for the study of Chinese prehistory has been well recognised recently. To co-ordinate the studies and preserve the precious cultural heritage, the need for an organisational structure had been felt for some time, and I had advised the founding meeting of IFRAO in Darwin (3 September 1988) of the preparations to establish a Chinese rock art research association.

The Rock Art Research Association of China (RARAC) has now been founded, and was immediately affiliated with IFRAO as its seventeenth member. It has a committee of nine, comprising Professor Chen Zhao Fu (President), Professor Li Fushun (Vice-President), Li Yu Chang (Secretary General), and Permanent Council Members Zhang Bo, Yang Geng sheng, Deng Lin, Jiang Zhen ming, Lou Yu Dong and Lu Hua Tang.

RARAC will hold an international rock art conference at Yinchuan, from 5 to 10 October 1991. The main objective of the conference will be to establish a world-wide awareness of the importance of China's rock art within global prehistoric cultures.

The contact address of RARAC is:

Rock Art Research Association of China
Prof. Chen Zhao Fu (President)
Central Institute for Nationalities
100081 Beijing
China

RAR 8-204

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Notices

The sixteenth member of IFRAO is the Società Cooperativa Archeologica Le Orme dell'Uomo. This was decided by ballot (Questionnaire No. 3), the result of which was 11 'for', none 'against', 3 abstentions. The Società's aims are the research, publication, promotion, preservation and conservation of rupestrian imagery, focusing on Val Camonica, north Italy. It operates from Cerveno, Brescia. Its committee includes Ludwig Jaffe, Angelo Fossati and Mila Simoes de Abreu. The contact address is:

Società Cooperativa Archeologica Le Orme dell'Uomo
Ludwig Jaffe (President)
Piazzale Donatori di Sangue, 1
25040 Cerveno (Brescia)
Italy

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Official IFRAO letterheads have now been issued to all member organisations. In line with our policy, IFRAO does not possess a 'head office' or a 'president', executive power is equally shared by all 17 offices of IFRAO, and all should therefore be entitled to use the official letterhead.

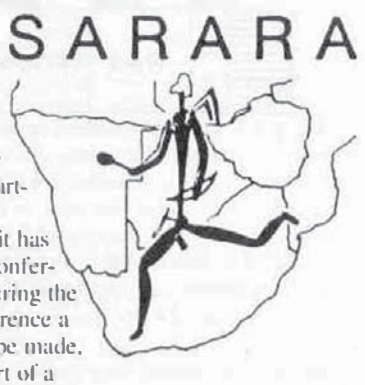
*

ROCK ART - THE WAY AHEAD. The International conference on rock art conservation, recording and study to be held in Natal, South Africa (25-31 August 1991) will include one more field trip in addition to those announced earlier. This will be a safari to meet Kalahari San and learn about their way of life. This post-conference field trip will be from 5 to 10 September 1991.

Participants will see various aspects of traditional San culture: the building of huts, manufacture of rope, setting of traps, use of arrow poison, the gathering of food, the extraction of water from tubers. San artists, too, can be met, and traditional songs and dances will be seen. Safari participants will sleep in tents or under the stars on camp stretchers around the camp fire.

The cost of the safari is R2 190.00, including economy class air fare Johannesburg/Gaborone/Johannesburg, all equipment, vehicles and meals.

For all details regarding the SARARA conference write to: Shirley-Ann Pager, SARARA President, P.O. Box 81292, Parkhurst 2120, South Africa.



It is to be noted that SARARA is against all forms of discrimination, whether racial, political, religious or sexual, and unequivocally condemns the practice of apartheid. Not only is this set out in SARARA's constitution, it has been reaffirmed in various conference announcements, and during the opening session of the conference a statement to that effect will be made, followed by a call for support of a resolution to condemn apartheid!

NOTES FOR CONTRIBUTORS

Manuscripts of major research papers should preferably be from 4000 to 8000 words. Longer articles will be considered on the basis of merit. Submissions should comprise the original together with one copy, typed in double-space, with a wide margin on one side of each page. Underline words to be italicised and identify each page by number and author's surname. The content of the paper should be outlined by three to five keywords (e.g. 'Petroglyphs - patination - ethnography - Pilbara') placed above the title. The manuscript must include an abstract of 50 to 100 words, summarising the article.

Spelling and punctuation in this journal follow the *Style manual for authors, editors and printers of Australian government publications* and the *Macquarie dictionary*; where the two disagree the former has precedence. Footnotes should not be used. The bibliography and references in the text should follow the style indicated in this issue.

If line drawings are included they must be larger than the intended published size (preferably by a factor of 1.5 to 2) and line thicknessesstippling, lettering sizes etc. must be selected accordingly. Photographs should be black and white gloss prints of high contrast. Photographs of rock art which were obtained by physical enhancement or other interference will be categorically rejected. In regions where traditional indigenous rock art custodians exist, their approval must be obtained before submission of any illustrations of rock art, and where copyright applies the author must obtain the appropriate consent. Captions (on a separate sheet) are required for all illustrative material, together with an indication in the text as to where they, and any tables and schedules, are to be placed.

Announcements intended for a specific issue of this journal ought to be available at least two months before the month of intended publication. Galley proofs are issued of all articles and must be returned promptly after correction by the author(s). Each author or group of authors receive thirty free copies of their article, additional reprints are available at cost.

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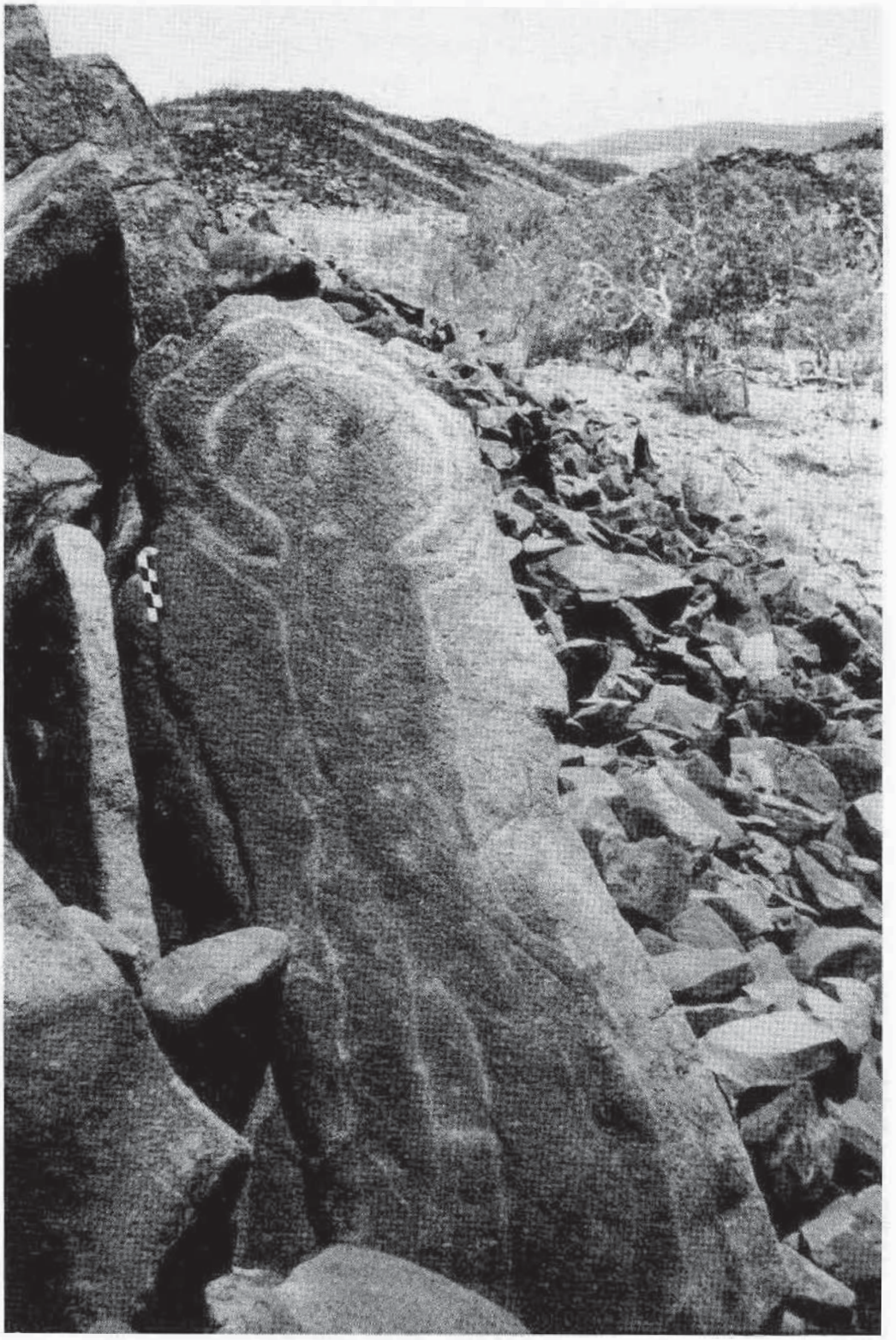

Anthropomorphous petroglyph with *Wandjina*-like 'halo', Dampier Archipelago, Western Australia. Photograph taken at time of re-discovery, by Europeans, in April 1968, by R. G. Bednarik. The scale is therefore still in inches!