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Photograph by Grahame L. Walsh

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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 overseas; and to generally promote awareness and appreciation of Australia's prehistoric cultural heritage.

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FRONT COVER:
The Wandjina Nyandugadali, Kalingi-Odin site, west Kimberley (Western Australia).
See article on page 151. Photograph by Grahame L. Walsh,
Takarakka Rock Art Research Centre.

KEYWORDS: Epistemology - Interpretive/non-interpretive hypotheses - Testability

!SCIENCE, HYPOTHESIS TESTING AND PREHISTORIC PICTURES

DANIEL TANGRI

Abstract. A number of scholars have argued that archaeology could be made a scientific discipline if hypotheses were rigorously tested. Hypotheses concerning prehistoric pictures are commonly seen as either non-interpretive or interpretive. It might be argued that interpretive hypotheses are not testable, given the methodology for testing that is currently available. Possible solutions to this problem are assessed here, and it may be that the methodology of 'refutation' is the best solution.

Introduction

In this paper aspects of the relevance of prehistoric pictures will be discussed. Relevance will be assessed through a discussion of current concepts of !science[1] in archaeology. In particular, relevance will be assessed by reference to a major conundrum in present-day archaeology—whether or not hypotheses about prehistoric pictures are testable. Those who profess that hypotheses should be testable usually condemn 'untestable' hypotheses as unscientific and irrelevant. This paper will discuss various forms of hypothesis testing and their relations to hypotheses about prehistoric pictures, in order to ascertain whether or not some hypotheses about those pictures are 'relevant' in the scientific sense.

It would be apt, therefore, to draw a distinction between *interpretive* and *non-interpretive* hypotheses. A number of authors have argued that there are no solid scientific grounds for testing interpretations of prehistoric pictures, and that such endeavours contribute little to our knowledge (e.g. Halverson 1987: 70; Sieveking 1979: 209). If this were so, interpretations could not be considered !scientific, and might be considered irrelevant.

The argument presented in this paper is that current formulations of acceptable procedures for hypothesis testing (e.g. M. H. Salmon 1982; Wylie 1985) do more to aggravate than to solve this problem. In particular it will be argued that such procedures have conflated research into a debate about the veracity of propositions—that is, whether an hypothesis is true or false. If one believes that truth and falsity are paramount one may well be forced to accept that no interpretation of prehistoric pictures is !scientific, as those interpretations cannot be proven true or false (see below). To save the major focus of studies of prehistoric pictures from the fate of irrelevance I shall consider possible alternatives to the current paradigms. I shall argue that one such alternative, the theoretical relativism of the Post-Processuals, presents more problems than it counters, and may still not guarantee that archaeological research is relevant. However, I shall also argue that a new methodology might preserve the !scientific relevance of some research, and provide a better focus than the methodologies currently accepted.

MS received 30 November 1988

!Science and Archaeology

Claims that archaeology is a !scientific discipline are not new. Physical anthropologists such as Bendyshe (1865) and Hunt (1866) considered that their work was !scientific, in the sense that it was based on observed facts and not on palpable flights of fantasy. Carpenter (1933: 127) professed that Classical archaeology could be made a more rigidly !scientific discipline

> with the perfection of procedure in excavation, with the steady emphasis on complete exactness in architectural inference, and the steady accumulation of objective criteria for dating and attributing vase paintings and sculpture.

The development of methods and aims in Classical archaeology can be seen to be analogous to the achievement of these general aims (Niemeyer 1978; Schiering 1969). This Classicist notion of !science, however, is not the same as notions of !science that are current today. Watson, Redman and LeBlanc (1971) proposed that archaeological !science should be related to the interpretation of data. Part of Carpenter's scheme, the need for objective attribution, is also connected with interpretation. However, his idea of !science differs from Watson et al.'s in one fundamental respect: he tried to interpret his data as little as possible. Even attributions of pots to individual artists could be seen to involve a minimum of interpretation as pots were often signed by painters, and styles could be matched with their signatures (see Beazley 1944 for a discussion of this methodology). !Science to Carpenter was primarily procedural

and non-interpretive. His case is useful, however, as it epitomises the dichotomy that I would like to draw. This dichotomy resides between two views of how prehistoric pictures should or could be studied. One view stresses the methodological security of non-interpretive !science, much like Carpenter did. The other view follows Watson et al. and concerns itself with interpretations, and the testing of those interpretations.

These two positions reflect two conditions of the data itself. Hypotheses about data can be divided into two classes—*interpretive* and *non-interpretive*. Hypotheses are a general class into which interpretations can be subsumed. However, interpretations are unlike other hypotheses because they involve the imposition of ideas onto data, in order to ascertain the *meaning* and *function* of that data. Other hypotheses do not involve such considerations. Consequently they are non-interpretive.

An example of an interpretation is an hypothesis about the meaning of a picture, and its social or economic role in a society. Here the aim of the hypothesis is to find out a picture's function. By contrast, non-interpretive hypotheses do not address the question of the function of a picture. An example of non-interpretive hypothesis might be: how old is the art in this cave? This hypothesis is sometimes readily testable. If, for instance, one has art from the wall of a cave that has fallen to the ground and been sealed within a deposit, as at Uan Muhuggiag in the Acacus Mountains of southwest Libya (Mori 1965), the date of the art is usually relatively easy to estimate as it will be some time before the fragment was buried.

The crucial question is usually whether or not interpretations are testable. The major manifestos of !scientific procedure in archaeology today are mainly concerned with the interpretation of data. This is explicit in the work of New Archaeologists (see Binford 1967, 1968; Fritz and Plog 1970; Hill 1968; Watson et al. 1971). For example Binford (1968: 16-8) saw sciences as deductive disciplines that tested hypotheses about data, and chose the hypotheses that were most likely to be true.

Today there are many concepts of !scientific archaeology available in the literature. Binford has even changed his mind; he now believes that archaeological data are contemporary phenomena, and that archaeologists study patterns in the past from theory-dependent frames of reference (1987: 394-5). Crucially, the task of the archaeologist is to interpret those patterns through analysis of the loci of responsibility for the production of that data. Thus the objective observation of the contexts in which data are produced allows !scientific interpretation. It is noticeable that this new concept is not clear about the role of interpretation; it is still presumed to be the primary aim of !scientific archaeologists but, rather than being deductivists, they are now presumed to be able to ascertain which interpretations are relevant by the examination of contexts. How exactly interpretations are deemed relevant is still not clear.

This is one modern concept of archaeological !science. Different concepts exist (see Dunnell 1982; Hodder 1986; Murray and Walker 1988; Rowlands 1984; M. H. Salmon 1982; Wylie 1985). They are often contradictory but they all focus on the interpretation of data, and the testing of those interpretations as the definitive aspects of !science in archaeology. In the remainder of this section I shall examine more thoroughly these concepts of !scientific interpretation. My focus will be on the testing of interpretations, to assess whether or not interpretations are testable and relevant aspects of !scientific research. As noted above, Binford was not clear about how exactly interpretations are to be tested. No such lack of clarity is to be found in some of the other recent works on !scientific methodology (M. H. Salmon 1975, 1976, 1982; Smith 1977; Wylie 1982, 1985, 1988). Here I shall briefly outline their arguments.

Salmon, Smith and Wylie all agree that archaeologists should adopt an inductive mode of reasoning, in which statistical probabilities are calculated for hypotheses in order to assess their relative plausibilities in the archaeological situations under study. This inductive method has been gleaned from the works of philosophers of science, notably Copi (1972) and W. C. Salmon (1963, 1967). The basic procedure recommended by these authors is to use multiple working hypotheses, and then assess the prior probabilities of each of these hypotheses. Prior probability is 'the degree of plausibility of [an hypothesis] prior to the testing situation being considered' (M. H. Salmon 1982: 42). Once prior probabilities have been assigned the most probable hypothesis is deemed the most plausible explanation of a particular situation.

In order to objectively quantify the probabilities for hypotheses, statistics are used. Blair (1973), M. H. Salmon (1982), W. C. Salmon (1967) and Orton (1980) all advocate the use of Bayesian statistics, whereby the hypothesis with the highest prior probability is confirmed. Other types of statistics may be used (Clark 1982: 231-2).

Smith summarises several ways in which archaeologists may establish prior probabilities (1977: 607-15). He argues that the closeness of fit of an hypothesis to an archaeological situation should be the primary criterion; the closer the hypothesis to the archaeological situation, the more plausible the hypothesis (see also Wylie 1985: 97-8). It is crucial to note that, although these authors discuss *hypotheses* as a class, they are actually discussing *interpretations*. All their examples are of interpretations.

In order to assess the closeness of fit one should look at several classes of data. These classes can be reduced to Copi's seven criteria for evaluating hypotheses (Copi 1972: 358-62):

(1) The number of situations which share the attributes in question.
(2) The dissimilarity of the situations which share the attributes.
(3) The number of shared attributes.
(4) The number of inferred attributes.
(5) The significance of the shared attributes.
(6) The specifity of the inferred attributes.
(7) The number of points of difference between situations.

These criteria speak for themselves; essentially one looks for similarities and differences between the hypothesis and the situation, and looks for the

closest fit between an hypothesis and an archaeological situation. This close fit should be strengthened by the occurrence of the most probable hypothesis in the greatest number of analogous situations. It is this latter requirement that gives away the aim of the authors. They are really talking about interpretations, because they are looking for associations between data and behaviour that occur in modern societies. The most common associations (or even universals) may then be deemed likely interpretations of similar data.

This leads us to another crucial aspect of the inductive method. All hypotheses are to be tested using observations of events in the modern or ethnographic world. The usual procedure is to select the ethnographic analogues and choose the one that is closest to the archaeological situation, and occurs frequently in the ethnographic record (Smith 1977: 609). For example an hypothesis relating shamanistic religious practices to pictures in caves (Lewis-Williams 1980, 1981, 1983, 1984, 1987) will receive support from modern ethnographies and, in part, from historical records of 'contact' societies. If the shamanistic hypothesis meets the requirements of fit and closeness to the archaeological situation it will be confirmed, and receive a high prior probability.

This inductive method is not without its problems. There are two reasons for this.

The first problem is that the procedure does not guarantee that an hypothesis will be true, or even likely. This is because the procedure is hampered by its inductive nature. Chalmers (1982: 14) has argued that 'inductive arguments are not logically valid arguments'. This is because one can never confirm an hypothesis enough to guarantee that it is not false. For example, one can test the interpretation of cave pictures as !shamanistic in the modern world, but in the ancient world one cannot. If one resorts to numbers and contexts of confirmations, in the belief that the more instances one has of the validity of an interpretation in the modern world the more likely that interpretation is to be true for ancient pictures, one has merely sidestepped the problem. Even if one has six thousand !shamanistic pictures in relevant contexts this does not mean that the archaeological case must follow suit: 'No matter how many examples of a regularity one has, there can be no logical affirmation that other cases will tend to conform' (Fletcher 1985: 392). The use of confirmation leads one into the 'fallacy of affirming the consequent' (Gould, in Gould and Watson 1982: 373).

This fallacy results from the necessity to draw confirming instances from the modern or ethnographic world. If one admits that the past was different from the present, then one must agree that the degree of association between behaviour and data (or shamanism and paintings) may not have been the same in the modern and ancient worlds. Any number of ancient pictures may have been shamanistic, but the ancient examples need not have been the same as the modern ones. Consequently the !scientific testing procedures of Salmon and Smith are no guarantee that an interpretation will be realistic or truthful.

The second problem with confirmation relates to its sociological impact on archaeology. M. H. Salmon notes that one important criterion for testing hypotheses lies in the *source* of those hypotheses. If an important archaeologist says that a picture was shamanistic, then the opinion of that archaeologist carries more weight than the opinion of a lesser archaeologist. Salmon (1982: 42) defends this position by arguing that experts may more often be right than amateurs, which may be true, but may also be false. If one remembers the first problem with the inductive method, however, this criterion is the only one left for testing interpretations. This may be the case anyway, for as Plog (1982: 26) noted: 'truth was measured in direct proportion to the consensus of professionals . . . [a good explanation] is whatever the archaeologists who matter accept as one.' While this may be a sociological reality, it hardly guarantees that one's interpretations are plausible, truthful, !scientific or relevant, and such a criterion is invalid. Prevalent theory and opinion may well be popular, but they need not be correct. For instance, during the reign of diffusionism an argument that the European megaliths are older than the pyramids of Giza would have been assigned a low prior probability —because it clashed with accepted theories. However, ^{14}C dating has since shown that this assumption is correct (Renfrew 1976: 94-101).

Thus the scientific methods for guaranteeing the veracity of interpretations do not meet the demands made of them. This might well be problematic, as one cannot guarantee that those interpretations are plausible or relevant as a result. I shall return to this problem later; it will be necessary first to examine the degree to which interpretive and non-interpretive hypotheses are and have been used in studies of prehistoric pictures.

!Science and prehistoric Pictures

There are two major forms of use of prehistoric pictures in archaeology. The first is non-interpretive (see Clegg 1986: 55; 1987a: 236; and some of the work of Marshack, especially 1970, 1977). The second use of prehistoric pictures is interpretive. Both uses have been intertwined within archaeology since the authenticity of prehistoric pictures was accepted; however, interpretations have been under consistent assault and have been modified extensively as a result.

Pictures can provide archaeologists with much information that does not require the interpretation of data. What sort of information? Generally, pictures may be studied in terms of their spatial and temporal distribution without imposing interpretation on the data. For instance, McMah (1965: 53) attempted to define styles of pictures in Australia. As Clegg (1986: 57) notes, her work demonstrated that pictures could be used 'for the elucidation of prehistory' without recourse to interpretation. This elucidation is of fundamental archaeological matters such as dating, distribution and regional variation.

Such studies, however, are not always seen to be the most relevant to archaeology. Many believe that prehistoric pictures may be most relevant to archaeology because of the information they might provide about human behaviour. However, in order

to find out about human behaviour many authors have relied on interpretations of prehistoric pictures. They have done so because they believe that the meaning or function of pictures within a society relates to human behaviour, and that those meanings and functions must be detected to allow human behaviour to be studied.

There are several *classes* of interpretive studies, all of which would now be assigned various degrees of relevance, based on the apparent security of the interpretations in terms of their basis on data and !scientific methodology. Perhaps the least popular class is also the oldest. This is the direct imposition of ethnographic analogies onto pictures in order to understand their meaning. This procedure is at least as old as 1864, when Lartet and Christy argued that prehistoric pictures had a purely aesthetic function (Ucko and Rosenfeld 1967). Similarly, Breuil's (1952) contention that Upper Palaeolithic pictures were representations of hunting and fertility magic was an interpretation that was based directly on the ethnography of Australian Aborigines, southern African San and Siberian nomads. These interpretations were soon criticised. Verworn (1908) pointed out that modern societies may not be suitable as analogues for ancient ones, as they come from different contexts and have different histories. Two French prehistorians in particular were extremely sceptical. Laming observed that

> the deficiencies and uncertainties of comparative ethnographic methods are very apparent. They seek to make comparisons between archaeological data and heterogeneous communities with nothing in common save their classification as 'primitive peoples'. There is much indiscriminate quoting of facts appertaining to communities which often differ greatly one from another and whose social, economic or religious structure may be very different from those of prehistoric communities of which, in any case, practically nothing is known (Laming 1959: 167).

Leroi-Gourhan also criticised the use of ethnographic analogy. He stated:

> *Du comparatisme ethnographique il ne pouvait pas sortir autre chose, car on ne pouvait pas emprunter aux Australiens ou aux Bochimans leur métaphysique, la simple honnêteté et le bon sens auraient suffi pour interdire d'appliquer une image trop précise aux documents préhistoriques, on ne pouvait emprunter que des bribes de faits matériels, sans lien ni entre eux, ni avec le fonds intellectuel* (Leroi-Gourhan 1964: 145).

This problem with the comparative method leads to a problem with the !scientific nature of the method. Interpretive analogies are untestable (see Gould 1980: 29-47; Morwood 1975: 112; Verworn 1908; Wylie 1985: 65). Wylie notes that this is because one cannot prove the truth or falsity of analogies. One can only test interpretive analogies using the confirmatory procedure that I have already criticised as *not actually testing interpretations*. Thus the first class of interpretation epitomised by the carefree use of analogy cannot be labelled !scientific on the standards delineated above. Consequently this class of interpretation might not be relevant to a !scientific archaeology.

The solution adopted by Laming and Leroi-Gourhan was to place greater emphasis on the contexts of prehistoric pictures. According to Leroi-Gourhan (1965a: 31-2) the best way to study prehistoric pictures was to exclude analogy and deal with the data on its own terms. Thus he (1965b) and Laming (1959) studied the positions and associations of pictures in French Upper Palaeolithic caves. However, these authors still used analogy and interpretation. For example Leroi-Gourhan (1964: 29) interpreted the deer as a universal symbol of virility on the basis of 'cross-cultural studies'. In general his post-Freudian sexual interpretations of his data went beyond the terms of the data and imposed interpretations on them from an ethnographic timescale. Thus his and Laming's work was as untestable in its interpretation as Breuil's, though at least the study of association was clear, testable, non-interpretive work (see below for the criteria for testing). This work, then, could be seen to be irrelevant to archaeological !science.

Two major streams emerged in the interpretation of prehistoric pictures during the 1970s. On the one hand, microscopic studies of pieces of *art mobilier* by Marshack yielded interpretations concerning Upper Palaeolithic symbolic and cognitive behaviour (Marshack 1972a, b, 1975, 1979, 1984, 1985a, b). These interpretations are as untestable as those of Laming and Leroi-Gourhan.

The second stream emphasised overall distribution patterns of pictures, usually on a regional or inter-regional basis. Conkey (1980), for example, noted that certain pieces of *art mobilier* were distributed throughout northern Spain, but only at Altamira were *all* of the types of bone artefacts and motifs found. Consequently Conkey interpreted this distribution as an example of hunter-gatherer aggregation.

In the same vein, Bahn (1982) and Davidson (1986) conducted analyses of distributions of *art mobilier* in Europe. Both toyed with interpretations about !social territories and !intensification. However, their analyses rely on untestable assumptions in the interpretation of their distribution patterns, and thus might be labelled un-!scientific.

The problem of interpretation also applies to culture-historical studies. For instance, Dennell (1983) used Mesolithic depictions of sea animals to support an hypothesis of Mesolithic seafaring economic behaviour. This raises the problem of the validity of inferences concerning representative art (see Davis 1986b: 51; Goodman 1971: 34-43 for discussions). Dennell's interpretation relies on the belief that pictures *do* represent sea creatures, and that the pictures are in fact depictions. These assumptions are intuitively satisfactory but are, as Clegg (1987b: 31) implies, untestable. Different interpretations as to what is being represented may be made; a porpoise to one person might look like an alligator to another. Consequently some researchers have admitted that inferences about representative art are merely assignations of names which may not be correct (Clegg 1983: 92). However, !scientists should be disturbed by the untestable nature of these interpretations, and should avoid iconographic studies of representative art.

Archaeological data are often studied in a manner that incorporates both interpretive and non-interpretive aspects. McMah (1965), for instance, carried out studies of patterns and then included untestable economic, environmental and cultural interpretations. Morwood (1984), Lourandos (1985)

and Conkey (1985) all follow this procedure too, interpreting changes in picture styles over time as evidence for social and religious intensification. Both interpretive and non-interpretive hypotheses are included in these studies.

A final, recent trend in studies of prehistoric pictures has been the search for universals of human behaviour that may correlate with pictures. The most recent example of this is Lewis-Williams and Dowson's paper on entoptic phenomena (1988). These authors do indisputably report universals; but as Bahn (1988) points out, these universals are of minor import, and Lewis-Williams and Dowson are still forced to rely on correlations between modern behaviour and pictures, and the assumption that similar ancient pictures reflect similar ancient behaviour. Consequently this sort of study might also be deemed untestable, as it has not broken away from interpretation.

Thus there are two major approaches to prehistoric pictures. The first is non-interpretive, the second is interpretive. If interpretations should indeed be assessed from the standpoint of the inductive method outlined above, interpretations might be seen to be untestable and un-!scientific, and therefore irrelevant. As noted above, interpretations are not tested by inductive methods. What of non-interpretive hypotheses? Statements about time and space seem readily testable, since they rely solely on the presence or absence of data. Hence if inductive methods are indeed the major methods for hypothesis testing, non-interpretive hypotheses might be seen to be testable, !scientific and relevant, whereas interpretations might be seen to be the contrary.

This would not mean that *all* theorising is a worthless procedure, just because it is presumed to be un-!scientific. Certain questions may be approached from both interpretive and non-interpretive standpoints. An example might help. Recently there has been a great deal of discussion of the origins of pictures (see Anati 1981, 1986; Bednarik 1984, 1986; Collins 1986: 270-81; Collins and Onians 1978; Conkey 1983; Davidson in press; Davis 1986a; Delluc and Delluc 1978; Freeman 1978; Hahn 1972; Halverson 1987: 66; Leroi-Gourhan 1982; Marshack 1985a; Preziosi 1982; Rosenfeld 1981; Wendt 1974, 1976). This discussion encompasses advocates of several different theories, including adherents of the development of human cognitive abilities (Bednarik 1986; Marshack 1985b); on the origins of representative art (Davis 1986a; Halverson 1987: 66) and nonrepresentative art (Bednarik 1986); and on entoptic phenomena (Lewis-Williams and Dowson 1988). The problem of identifying representative art is central to Davis's and Halverson's theories. Conkey (1983: 211-2) notes that the earliest Aurignacian pictures could be interpreted as !vulvae (Breuil 1952: 22) or !animals (Hahn 1972). With regard to the !vulvae Bahn (1986: 99) has argued that there is little evidence that many of the pictures so identified were indeed representations of vulvae. This shows that the problem of interpreting pictures as representative may lead to conflicting interpretations, which would be considered untestable if the inductive method were the only available methodology. Davis's and Halverson's theories could thus be irrelevant, as it is crucial to both to be able to identify *when* and *why* a picture was representational.

This problem, though, does not apply to theories that do not rely on the recognition of representative art. Bednarik's ideas about human psychology are not put at risk, as his theory does not require that pictures be representational. Hence an archaeological question can still be approached with non-interpretive hypotheses.

New Avenues of Research

In the preceding discussion it was shown that interpretations of prehistoric pictures cannot be tested with the inductive method, and hence might be considered !scientifically irrelevant. In this section I shall argue that such a gloomy picture is not necessary. Rather, new avenues could be considered.

One such avenue would be to change our current requirements of !scientific testing. Archaeology might be envisaged as a humanistic, relativistic discipline in which !science is unimportant. All hypotheses would be seen to be little more than assumptions, which are all equally valid. This avenue is of course the path trodden by Post-Processual archaeologists (Hodder 1982, 1984, 1985, 1986; Miller and Tilley 1984; Shanks and Tilley 1987a, b; Tilley 1981, 1982). Hodder has argued that the past is not observable in real, objective terms:

> to say anything about the past, and about past ideas, involves moving beyond the data to interpret them, and there can be no testing of these interpretations because the data are formulated within and are part of the same argument as the theories. Speculation and the subjective are thus part of the 'scientific' process (Hodder 1984: 28).

To Hodder, archaeology should be a 'cultural and social product' (ibid.). It reflects the attitudes of its adherents, and no hypotheses are true or testable. This has led Hodder to argue that archaeology can only be justified by allowing different social groups to use archaeology in their own ways. Each group would create its own, idiosyncratic archaeology (Hodder 1984: 30-1, 1986: 161-4). Thus interpretation would be entirely subjective, merely the reconstruction of social prejudices, norms, ideals and values. In the words of Feyerabend (1975: 196): 'the only rule is—"anything goes" '.

This line has since been adopted by Shanks and Tilley, who argue that archaeologists should use their data to subvert prevalent politicians; archaeology would become a component of contemporary class struggles (1987b: 201-9). However, Hodder has since retracted his proposition somewhat. He acknowledges that this 'anything goes' position is not entirely satisfactory. Obviously if all hypotheses about the past are untestable, all hypotheses are as good as each other. Hence the views of Kossina on prehistory would be as sensible and 'realistic' as those of Binford. This sort of solipsism is undesirable, says Hodder. Instead theories can be seen to progress towards ever better and more realistic accommodations of the data. This is an extremely odd retreat, for the belief that recent theories are more realistic than old ones is simply untenable. If prevalent interpretations are as good as each other one can never know which interpretations are most realistic. They are simply preferred

today because they are not at odds with our paradigms. They are not more realistic as a result. Today racist theories such as Kossina's are scorned as unrealistic; yet last century many scientists scorned nonracist theories as unrealistic (Hunt 1866). Therefore Hodder's retreat into realism is not a success.

The relativistic anarchism of the Post-Processuals might be a welcome path for some archaeologists. However, it is simply unnecessary. This is because hypotheses can indeed be tested (regardless of whether they are interpretive or not). The method for testing hypotheses, however, is not the methodology of inductive confirmation; rather, it is the methodology of refutation. Refutation provides a second path for archaeologists to follow.

Many archaeologists, while claiming that archaeology should be scientific, do not appreciate how some of the disciplines they model themselves on operate, and what sort of propositions are testable in those disciplines. Proponents of 'hard' science may use inherently subjective procedures. Sociological studies (Harvey 1979; Tweney, Doherty and Mynatt 1982) show that hard scientists may use confirmation and plausibility arguments. Actually philosophers of !science have long argued that theories in the hard sciences may be chosen not because of sophisticated testing procedures, but because they agree with the pre-existent paradigmatic biases of the scientists (Berger and Luckmann 1967; Feyerabend 1975; Kuhn 1962). A possible solution to the interpretation of prehistoric pictures, then, would be to argue that, as hard sciences are subjective, archaeology is !scientific as well, by virtue of being no worse.

However, this ignores a fundamental difference between archaeology and hard sciences. While the practitioners of hard sciences may use subjective procedures, they also ensure that their hypotheses are testable. The test, in this case, is not a probabilistic specification of the degree of confirmation required by proposition; rather it is a specification of the precise circumstances in which an hypothesis may be refuted (Lakatos 1970: 96). Regardless of whether or not scientists try to confirm their hypotheses, if the data indicate that an hypothesis is refuted, scientists may consider that the hypothesis needs to be modified, reassessed or abandoned. Refutation as such is a strikingly different procedure from confirmation. Confirmationists try to fit hypotheses to the data, and accept hypotheses which may not be supported by all the evidence. If the evidence in favour of an hypothesis is greater than the evidence against it, confirmationists will accept the hypothesis. However, refutationists consider that any disconfirming data is usually enough to warrant modification or rejection of hypotheses (Popper 1968).

This leads us to the archaeological applicability of refutation. A number of archaeologists have criticised refutation on the grounds that, if hypotheses are theory laden, a theory can never be refuted as the refutation of a theory is always conceived of as part of a theory. Therefore a theory need not be *false* if it is refuted (Hodder 1986: 155; Lewis-Williams and Dowson 1988: 234-5; Shanks and Tilley 1987a: 42).

However, this leads to abandoning refutation for the wrong reasons. Scientists such as Whewell (1847: 40-1) and Huxley (1886) believed that theories could never be proven true or false. However, if a theory specified the conditions under which it could be refuted, than that theory could be rejected. It might never be known whether the theory was true or false, but it could be said that the data did not fit the theory at a certain time, and that the data refuted the theory as a result. Hence refutation can be used to test hypotheses, and subject them to the scrutiny of data.

This counters many precepts of confirmation as it has been accepted by archaeologists. Chief amongst these is the belief that whether an hypothesis is true or false is simply irrelevant. All that is relevant is that data can be used to diminish the validity of hypotheses. Refutation, however, demands a more rigorous approach than has been acceptable hitherto. As Murray and Walker (1988) point out, the only way in which refutation will be applicable to archaeology is in its strictest sense: any disconfirming data must be accepted, and hypotheses modified or abandoned accordingly. If one defines exactly the analogues one uses and what sort of data one expects to find if the analogue is valid, and one finds data that contradict the analogue, the analogue must be rejected. Confirmationists tend to argue that a little disconfirming evidence may be ignored if the supportive evidence is greater. However, such arguments, based on the plausibility criteria discussed above, are untenable, for the reasons also discussed above. If hypotheses are to be tested the only possible way seems to be by strict refutation.

To relate this back to the focus of the discussion, the dichotomy between interpretive and non-interpretive hypotheses can now be broken down. Whereas before, interpretations could not be tested by confirmation, refutation is not so incapable, for interpretations may be analogues. If they specify exactly the data that should occur if they are valid, and contradictory data are found, interpretations may be refuted. Hence refutation removes the encumbrance that has plagued students of prehistoric pictures for so long—the approach to meaning and function. Hypotheses about such things may never be known to be true or false, but they will always be refutable. If they are refutable they may be deemed !scientific, and therefore relevant.

Conclusion

This paper attempts to do a number of things. Firstly, it tries to tackle the familiar dichotomy facing those who study prehistoric pictures, between interpretation and non-interpretive study. It then relates those two aspects to familiar concepts of !science in archaeology, and shows that, while non-interpretive hypotheses may be tested, the inductive method does not enable archaeologists to test interpretations. However, the road out of this is not through abandoning hypothesis testing, as the Post-Processuals claim. The solution is to adopt refutation, and the different approach to archaeological data that it entails.

Note

1 'Science' is somewhat of a confusing term in archaeology, having been ascribed a plethora of definitions, none of which necessarily correspond to what philosophers call 'science' (see Feyerabend 1975) or sociologists call 'science' (see Harvey 1979). Therefore I write the word '!science', to make it clear that I am describing the totality of archaeological definitions of science, rather than the !real or !other definitions of science. Exclamation marks will be used throughout to emphasise that certain things are *names* and *not* facts; this usage has been established in prehistoric art studies (see Clegg 1983).

Acknowledgments

I would like to thank John Clegg and Andrée Rosenfeld for commenting on earlier drafts. John Clegg in particular attacked this essay in its various manifestations throughout. Apart from that which I have referenced the essay constitutes my ideas.

*

COMMENTS

By PAUL FAULSTICH

Tangri offers a fine review of the main streams of thought concerning the validity of hypothesis testing. I will not comment on the review portions of his essay, but will limit myself to other fundamental aspects of his argument.

Firstly, I question Tangri's very notion of what a hypothesis *is*. He states that 'an example of non-interpretive hypothesis might be: how old is the art in this cave?' (p. 84). This, however, strikes me as a question, not a hypothesis. According to the two dictionaries I consulted a hypothesis is an explanation, assertion, proposition, premise or assumption, but it is not a question. With Tangri's example of the antiquity of cave art, his 'hypothesis' ('how old is the art in this cave?') is really only a question, and his 'test' (estimating age through associative dating) is really a hypothesis. If a hypothesis requires a premise, and a test requires data, then Tangri has his terminology jumbled. Until we have a hypothesis we have nothing to test.

My second contention is over the idea of 'interpretive' and 'non-interpretive' hypotheses. I assert, as others have done before me, that *all* hypotheses are interpretive, involving the imposition of ideas onto data. (Tangri addresses this point but only devotes minimal attention to it.) Interpretations are unavoidable in theoretical archaeological work, and I suggest that our central question should not be whether interpretations are testable, but whether they are honest and lucid. After all, a hypothesis test does not so much confirm as convince. Conclusions based on 'testable hypotheses' are really nothing other than our own constructions of data, and we must ask ourselves 'what is the validity of a test when that test is immersed in the same inductive processes as the hypothesis?' (Here is where Tangri's concept of refutation can play its most significant role.)

This is not to say that 'anything goes', as Tangri suggests of the Post-Processuals. Rather, I simply maintain that positivist approaches are not any less subjective than interpretive approaches: they both entail interpretations of interpretations. Contrary to Tangri's reading of early 'post-processual' archaeology, not all hypotheses are 'as good as each other'. Certainly some are more convincing than others; some more rigorously investigated; some less refutable; some more articulate about past human constructions.

As Tangri asserts, a Darwinian approach to the evolution of theories is not acceptable; theories do not necessarily progress toward more realistic accommodations of data. I fully accept Tangri's notion that preferred interpretations are such—not necessarily because they are closer to some ultimate truth—but because they are not at odds with the dominant paradigm. But this is an inescapable concomitant of being cultural animals, and the dominant paradigm has influences that encompass not only interpretive approaches, but so-called testable (and refutable) approaches as well. Prevalent questions, hypotheses and tests have developed, and are all located within, the framework of societal values and norms.

Refutation, as Tangri contends, can be a valid alternative (or addition) to tests based solely on confirmation. It seems unnecessarily limiting, however, that *any* anomalies or disconfirming data should warrant modification or rejection of the hypothesis, as Tangri suggests. We must take careful account of the nature and quality of the opposing data, and re-evaluate our position from there. Anomalies, as long as they are not crippling, can add dynamism and vitality to our discipline and help us to avoid the dogmatic search for truth. I applaud Tangri's implied notion that it is not so much a search for universal 'truth' that we should strive for, but a search for relevance. Certainly, conflicting interpretations do not signal the demise of the archaeological endeavour. I, for one, would find much less stimulation from a discipline in which there was one single answer to our complex questions.

I would like to transgress for a moment to address a question I have concerning Tangri's use of exclamation marks (as established by Clegg 1983) to indicate that things are *names* and not *facts*. Although this application roughly parallels the phenomenological technique of bracketing assumptions, I do not exactly see the relevance of Tangri's specific application. For example he uses the exclamatory prefix for '!science', '!social territories' and '!animals' (among others), but he does not use it for terms such as 'archaeology', 'meaning', 'true' or 'false'. This strikes me as selective relativism —a relativism that is laden with subjectivity. Perhaps Tangri could further elaborate his use of this prefatory tool.

Tangri's penetrating and deconstructive look at hypothesis testing certainly refutes many of the main currents of thought concerning validation. But again I question Tangri's semantics: he states that 'hypotheses . . . may never be known to be true or false, but they may always be refutable'. Consulting my American Heritage Dictionary once again I see that 'refute' means 'to prove . . . to be false or erroneous'. Just what does Tangri mean

by 'refutable' if not 'to prove to be false'? Refutation, of course, is immersed in cultural subjectivity.

Archaeology's task is not to tame the human spirit, but rather to explore its unique constructions. In this light archaeology, as a discipline, must be true to its own goals and limitations, and above all it must be true to the nature of the human animal. Human constructions are not necessarily 'scientific' and therefore are not necessarily best investigated through scientific methods. From 'the science of man', let us rethink archaeology as 'the exploration of humanness'. Let us have the courage to allow ours to be an integrated discipline. Let us not be afraid to make mistakes. Let us be open to highly interpretive approaches. Let us accommodate our own humanness. And let us move on into yet unventured territory.

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By JARL NORDBLADH

When the term 'scientific' is used in archaeological or social/humanistic disciplines, something special is always sought for replacing an undesirable research situation. As a social phenomenon this is extremely interesting, but as a rigorous attitude it ends in failure. Tangri's paper demonstrates this in a clear way, arguing its way from one culde-sac to another. It presupposes a lot of phenomena which to me are not at all clear. I see no possibility of finding a totally clear division between so-called 'interpretive' and 'non-interpretive' hypotheses. All texts are interpretive and all data are produced through such procedures. Art as a concept is of course such a product, as well as the—to me very strange—division between meaning and function. Neutrality, even if claimed, is impossible. Whatever we do has scope and direction.

The article seems to be based on a conviction that there is one meaning to every picture, a sort of one-to-one relation between form and content. This view limits what can be done subsequently. To me our task is to find meanings from pictures, not *the* meaning of them. Prehistoric materials are simply materials for which the relevant social structures and processes are not accessible, as are the concepts making them distinguishable. We fill this vacuum with our concepts, and the materials become meaningful for us. We must realise that we are investigating phenomena from our own perspective, to gain an understanding and appreciation of them here and now; we are not investigating them in order to take the places of other peoples. The problems we are exploring are problems formulated by us, in our social context, and they can hardly be maintained as universals. The history of research provides itself a little insight into this topic.

A scientific and behaviouristic approach is just as context-bound as any other approach, but it is shallow and offers no scope for fundamental questions regarding content. It starts from the conviction that our own society is the standard against which others, called 'ethnographic', are compared. Our Western model is thus made the norm.

There should only be one ethnography, which includes our own society. The world situation cannot be allowed to legitimate present conditions. It is, in this view, practically impossible, for instance, for a Bushman to visit and correctly interpret Palaeolithic art sites in France. His views and experiences would in this context only be exotic, inadequate or of limited value. However, our own ideas and experiences are no better, safer or more interesting in themselves, than those of others: they are simply different, but definitely not 'normal'. The big questions of values, world views, society and ethics cannot be solved by scientific means, they have to be resolved by society, and some might be accessible via our own special discipline.

As for the question of hypothesis testing it is all right to conduct such an exercise, but it is not *the* solution to the investigation of prehistoric pictures. Refutation or confirmation of a hypothesis are context-bound and time-bound, and will not prevent other hypotheses from appearing.

In a way Tangri's article ends where it begins: meaning or interpretation are bound to isolated hypotheses and as long as they can be individually confirmed or refuted, 'safe' knowledge is obtained. But quite different hypotheses can exist at the same time and be justified with sound arguments. In the long run testing will endlessly produce more hypotheses, bringing the scientific scope from cosmos to chaos, where the parts are more or less equally reasonable, but incompatible. Or is it, as Robert M. Pirsig says in his book *Zen and the Art of Motorcycle Maintenance: an Inquiry into Values* (New York 1975), that we tend to stop at the step where the first hypothesis is confirmed, denying the fact that the number of possible hypotheses is without end?

Our patience and energy are not.

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By KINGSLEY PALMER

A hypothesis developed by induction can be tested. In its own context, and given the limits of inductivism, it can be put to the test. If we observe water to boil at 100°C we can put the proposition that all water boils at 100°C under all conditions of the test. If we carried out the experiment enough times we could *predict* that it would probably 'always' be so—if we were naive inductivists. The problem for science is that predictability is not certainty: we can never know that water will always boil at 100°C. What can be tested can verify a probability but not a certainty.

As a superior (so some would argue) process, deductive reasoning and falsifiability (or refutation) provide a better way out. Statements about the world are not to be taken as 'true', but as working models which, as heuristic devices, may help us towards a better understanding of the way things are. By comparing developed principles from these theory-like statements, sets of facts are derived and matched with the observed world. If the statements do not hold up they are modified or abandoned.

This much then is really covering a good deal of old ground and Tangri is perhaps saying that falsification as a process of scientific inquiry is a better way to go than a couple of others. This is hardly a novel idea for science since Popper introduced the idea of falsifiability in 1934 (Popper 1934, 1976: 87), though it may be for archaeological studies.

There is a problem with the concepts surrounding the distinction between 'interpretive' and 'non-interpretive' activity upon which the paper is predicated. I am unclear as to the fate of 'non-interpretive' archaeology in this debate. Non-interpretive studies appear to be dismissed because 'to be most relevant' they have to be interpreted. But since the epistemology of 'interpretation' is nowhere set out this dismissal is unconvincing. Moreover, non-interpretive archaeology is claimed to be testable (because it is deductive, not inductive?) and therefore more 'scientific', but apparently pays the price for its science by being less relevant.

Tangri makes a false distinction between 'interpretive' and 'non-interpretive' hypotheses about art. He asserts that the interpretation is itself a hypothesis, but not like others, 'because they involve the imposition of ideas onto data'. But a hypothesis is a statement about the presumed state of things: that which it seeks to explain, the data it seeks to comprehend, is 'interpreted' to a greater or lesser extent. But all data are interpreted to some extent. Interpretation of art is an activity, just as inductivism is a process. You cannot 'assess' the interpretation by inductivism as Tangri states. Inductivism is a process from which possible explanations are derived. Linking interpretation with the process of induction as a serial activity conflates two parallel acts. The interpretation is itself an inductive act: observe the art and induce possible meaning or explanation. This results in a hopeless conflation of the manner in which data are assembled and the hypotheses which are subject to scrutiny as well as being based on a fundamental misunderstanding of the ways we obtain data and present them for consideration in the first place.

Assessment of all archaeological phenomenon is always interpretive to a greater or lesser degree. Thus the real issue is not how do concepts of the philosophy of science fit into an artificial distinction made for archaeology but whether the processes of so-called science have any real place in the debate as to how archaeologists tackle prehistoric 'pictures'. This in turn begs a number of questions about the study of prehistoric art and the application of science. At the heart of the matter lies the fact that art carries with it a meaning that lies beyond the immediately obvious and apparent. Meaning derived from association with symbolic forms is both variable and labile. An artefact is an object to be measured and weighed. It can be comprehended by an assemblage of 'facts'. A picture of an anthropomorphous form has weight and dimension but it also has (we must suppose) 'a meaning' (and probably a range of meanings that may change according to time and place) beyond the meaning of its physical substance.

Enumeration or description, photographing and weighing are themselves acts of interpretation because they involve acts that are culturally bounded and potentially theory laden. This is particularly the case for prehistoric art, where the focus of our recording involves choice over one thing rather than another. What we define as the art is a product, at least to some extent, of our interests and preoccupations. Moreover, our ability to record is constrained by our senses, the tools available to us and so on. It is a culturally bound activity by which we begin the process whereby we translate an object into a form that we can recognise and comprehend. This is also an act of interpretation. Incidentally, understanding the reality of this state of affairs has led critics of falsificationism to question the worth of statements that are inevitably theory dependent and are used in the falsification process. Tangri appears to accept the nature of 'data' as a given entity without criticism. While 'facts' are slippery customers, they may become less so by recognising and admitting the physical and intellectual context of their coming into being. Their usefulness to scientific inquiry is then much enhanced.

To whatever stage of interpretation our recording of art is taken there is a fundamental problem with equating it with scientific method. As I conceive Tangri's preferred method, utilising falsification we should proceed as follows:

(1) Identification of a relevant theory-laden statement (e.g. water boils at 100°C);
(2) set of principles qualifying the above;
(3) prediction that in a specific state a billy will boil at 100°C;
(4) we do an experiment and show this not to be the case given certain conditions: proposition 1 is modified etc.

Refutation, like any testing activity, requires an act of testing. Ideas about pictures are not only impossible to test, but they lack theory-laden statements (Tangri calls these 'hypotheses' at the beginning, but later they become 'theories') of sufficient weight to be probed and, most importantly, lack the facilities to test by refutation or any other means. Suppose we overcome the first problem by stating that all rock art in Australia (or of a certain *genre*) was associated with species renewal. Our procedure must go as follows:

(1) Rock art of this *genre* was drawn in association with species renewal;
(2) this art depicts rituals of species renewal to stimulate the natural world;
(3) that rock art with the same characteristics, but at another location, was for species renewal;
(4) refutation: the statement cannot be refuted.

Proposition No. 4 is true because the people who could provide the information we require are not here to inform us. Even if the artist was available there is no certainty that he or she could provide a single set of 'meanings'. Art does not operate in such a limited fashion.

Consequently the process of falsification is unavailable in this case. Unless we resort to ethnographic parallels (and I agree with much that Tangri says about their limitation) or we can develop some other information that categorically refutes or modifies the proposition, the process of refutation is not available to us.

I have argued elsewhere (Palmer 1988) that those who study prehistoric art must recognise the limitations of their study. Inductivism with a large serving of probabilism is the usual path we must tread. This does not mean that refutation should be abandoned. If, for example, there was clear evidence that a particular *genre* of rock art was not what we had thought it to be, we should use the evidence to modify the first proposition. However, understanding prehistoric art is generally not a matter for science, any more than understanding the art of our own culture is a matter for science. Art has meant many things to many people and will continue to do so. Because prehistoric art is tied up with what has become increasingly a scientific enterprise (archaeology) there has been an assumption that understanding it could also acquire some of the accoutrements of scientific study. While I am yet to be convinced that archaeology can deal with the same philosophical concepts as science, I also recognise that much so-called science deals with these rather awkwardly. However, Tangri's assertion that his discovery of refutation will make our study of prehistoric art scientific does not hold up. We must be content to follow less stern epistemologies. We cannot hope to deal in falsifiable theory-laden statements. However, exact recording and constrained but self-conscious interpretation and probability, which is entertained with an open mind, can provide insights which may advance our understanding of prehistoric art. In studying prehistoric art only one thing is certain: nothing is certain! However, some things are more likely than others, provided we have based our suppositions on careful homework.

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By B. K. SWARTZ, Jr

In writing on a subject like this it is important, I believe, for the commenter to identify his personal theoretical position. The statement that follows was made by the eminent American anthropologist, A. L. Kroeber, in 1935:

> I suggest as the distinctive feature of the historical approach, in any field, not dealing with time sequences—though that almost inevitably crops out where historical impulses are genuine and strong—but an endeavor at descriptive integration. By 'descriptive' I mean that the phenomena are preserved intact as phenomena, so far as that is possible; in distinction from the approach of the nonhistorical sciences, which set out to decompose phenomena in order to determine processes as such. History of course does not ignore process, but it does refuse to set it as its first objective. Process in history is a nexus among phenomena treated as phenomena, not a thing to be sought out and extracted from phenomena. Historical activity is essentially a procedure of integrating phenomena as such; scientific activity, whatever its ultimate resynthesis, is essentially a procedure of analysis, of dissolving phenomena in order to convert them into process formulations.
>
> These two approaches are applicable to all fields of knowledge, but with varying degree of fruitfulness. It is in the nature of things—I do not pretend to explain why—that in the inorganic realm the processual approach to science has yielded the most results, but, as we pass successively into the realms of the organic, psychic, and socio-cultural-'historical', this approach encounters more and more difficulties and its harvest diminishes. It is customary to say that the phenomena are more 'complex' on the organic and suborganic levels. I incline to doubt this and to believe rather that the difficulties lie in their being epiphenomenal—from the point of view of the analytic, processual science approach (Kroeber 1952: 63).

Subsequent developments in anthropology and archaeology have not yet altered my thinking on Kroeber's observations (Swartz 1967, 1977).

I reject the assertion of Tangri, and apparently many other scholars, that effective research in the social sciences (including cultural interpretation of petroglyphs) must be accomplished by hypothesis testing (whether by confirmation or refutation). Effective petroglyph research is conducted by formulating interpretive generalisations that best explain phenomena from intimate familiarity of rich databases. One should keep Occam's razor cutting.

At the beginning of his article Tangri mentions 'those who profess that hypotheses should be testable usually condemn "untestable" hypotheses as unscientific and irrelevant'. Following from this one could maintain that Darwin's hypothesis of natural selection is irrelevant since there is no way to prove (test) that evolution took place!

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REPLY

By DANIEL TANGRI

These Comments are both constructive and useful. It is clear, however, that their authors and I disagree on some fairly fundamental matters.

The first is the dichotomy between interpretive and non-interpretive hypotheses. Faulstich, Nordbladh, Palmer and Swartz assert that the dichotomy is false and that all hypotheses are interpretations. I remain unconvinced and would advise the authors to read Clegg (1986) for a weighty exposition of the dichotomy. He notes that there are ways of using pictures without interpreting them, notably through studies of such things as the distribution of pictures in time and space. It is difficult

to see, for example, how the simple statement that the art of Tassili n'Ajjer is at least 4000 years old is interpretive. The distinction is important as it underpins several recent theories, which is why I tried to write an article on hypothesis testing in relation to interpretive and non-interpretive hypotheses.

In support of their arguments the commenters either utilise a weak critical theory (Nordbladh) or refuse to contemplate discussion of the epistemology of our discipline (Swartz). I thoroughly agree with Nordbladh that many of our interpretations are reconstructions of our own values. However, to argue that this is the only possibility is to advocate, along with Shanks and Tilley (1986), a nihilistic solipsism. Critical appreciation of our own flaws has not yet shown us how to prevent the repercussions of our values. Our values are often used to support political positions, and without hypothesis testing there is little that one can do to show that such use may be misuse. For example one of the major comments on archaeological research purporting to show that Great Zimbabwe had been built by black, rather than white, people was that the archaeological hypotheses were not more probable than the fairy tales dreamt up by right-wing Rhodesian settlers (Tangri in press). Critical theory and, as I tried to show, confirmation do not allow us to refute hypotheses that may be open to political misuse; my hope was that refutation would.

Faulstich suggests that the Post-Processuals did not argue that all hypotheses are equal. Our reading of their work must be at odds, as I take Hodder (1984) and Shanks and Tilley (1986) to be advocating an 'anything goes' position. Such a position is both politically volatile and naive, which is probably why Hodder (1986) seems to have had second thoughts, as I noted.

Faulstich also questions my concept of refutation. I must note that the American Heritage Dictionary is hardly a compendium of archaeological jargon, and that I do not believe that 'refute' means 'to prove to be false'. I envisage refutation working rather as Faulstich suggests—as a sort of guideline to the re-evaluation of hypotheses and the generation of research questions. I am grateful to Faulstich for correcting my semantic slip-ups but would stress that I defined refutation rather differently.

Swartz and Nordbladh, and to some degree Faulstich and Palmer, question the relevance of hypothesis testing. For political reasons it would appear to be wise. However, their compromise approaches (Palmer's 'less stern epistemology' and Swartz's 'interpretive generalisations') do not appear to be any advance on confirmation. *Contra* Palmer, I do not see refutation as being inextricably linked to deduction, but believe both inductive and deductive situations may be evaluated with refutation. Palmer appears to be advocating the sort of realism I was trying to avoid; his example cannot be refuted if one wants to ascertain the veracity of the statement that art is associated with species renewal. However, one can still refute the statement whithout a recourse to realism. One could look for motifs commonly associated with species renewal in recent pictures. If all those motifs were found in a prehistoric example one could fairly state that the hypothesis had not been refuted, though it would not be true either. However, if some motifs were not found one should be led to re-evaluate the hypothesis. Refutation as I advocate it is very stern, it does not allow any meanings to be real or likely; but at least it allows hypotheses to be refuted, thus negating their utility in political discussions. It should be noted that this is not similar to Popper's 'falsification'. Popper's central concern was using falsification as a guide to eventual truth, which was not my concern and was why I chose the term 'refutation'.

Finally, Swartz is correct in arguing that some might describe Darwinism as irrelevant if it cannot be tested. However, scientists concerned about this have striven to demonstrate that such principles as natural selection can actually be tested.

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Résumé. Plusieurs savants ont raisonné que l'archéologie puisse être une discipline scientifique si des hypothèses étaient rigoureusement eprouvées. Les hypothèses qui concernent aux images préhistoriques sont comprisés en général comme ou non-interpretives ou interpretives. On peut raisonner que les hypothèses interpretives ne soient pas capables d'être eprouvées, au moyen de la méthodologie actuelle pour les eprouver. Des solutions possibles de ce problème sont évaluées; la méthodologie de la 'refutation' peut être la meilleure solution.

Zusammenfassung. Einige Gelehrte haben erörtert, dass Archäologie zu einer wissenschaftlichen Disziplin gemacht werden könnte, wenn Hypothesen rigoros geprüft würden. Hypothesen die sich mit vorgeschichtlicher Kunst befassen, werden im allgemeinen entweder als interpretierend oder nicht-interpretierend angesehen. Es könnte erörtert werden, dass interpretierende Hypothesen mit der derzeit verfügbaren Methodik nicht zu prüfen sind. Mögliche Lösungen zu diesem Problem werden hier besprochen, und es scheint, dass die Methodenlehre der 'Widerlegung' die beste Lösung ist.

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KEYWORDS: Pleistocene - Hand stencils - Limestone cave - Tasmania

JUDDS CAVERN: A SUBTERRANEAN ABORIGINAL PAINTING SITE, SOUTHERN TASMANIA

RICHARD COSGROVE and RHYS JONES

Abstract. A group of newly discovered hand stencil motifs contained within a large limestone cave in south-west Tasmania is described. Beginning with a brief discussion of Tasmanian Aboriginal art and its antiquity, a detailed account of fifteen red-ochred hands and their placement within the cave is made. Chemical analysis of small samples of pigment revealed the presence of mammal blood. Using geomorphic evidence of calcium carbonate formation over portions of the art, a minimum age of 11 000 years BP is suggested for their execution.

Introduction

Aboriginal rock art sites in Tasmania are rare. Until recently, only eleven sites were known, most consisting of petroglyphs confined to a few areas on the west and north-west coasts (Meston 1931, 1932; Sims 1977: 429; Stockton 1977a; Murray 1980; Cosgrove 1983; Walsh 1988). The petroglyphs fall within what is sometimes called the Panaramitee type of Aboriginal rock art, and have broad stylistic similarities with areas on the mainland such as in South Australia (Maynard 1979; Clegg 1983; Nobbs 1984). These consist of pecked, pounded and/or abraded geometric motifs such as circles together with footprints of birds and other animals. A recent intensive, systematic survey of over 140 km of Tasmania's west coast located an additional two petroglyph sites (Cosgrove 1983). In terms of overall Australian Aboriginal rock art, the Tasmanian sites appear to display no great variation in motif, although some differences exist between sites on the basis of style and technique (Sims 1977: 438).

On the mainland some examples of such art have been shown to have a Pleistocene antiquity. Recently, the newly developed cation-ratio dating method on 'rock varnish' covering such decorated dolomite rock surfaces at Karolta in the north-east of South Australia has yielded values ranging from c. 1500 years BP back to 31 000 years BP (Dorn, Nobbs and Cahill 1988; Nobbs and Dorn 1988).

In Tasmania, the age of this art type is unknown. Excavations by one of us (RJ, with Harry Lourandos) in 1969 at the Mount Cameron West art site revealed a soil horizon overlying carved rocks with a carbon 14 date of c. 1000 years BP and broken fragments of carvings buried by shell midden dated to about 1300 years BP. It is believed the petroglyphs at this site may have been made between 2000 and 1000 years ago (Jones 1983: 89-90).

Paintings on rock surfaces in Tasmania are rare and until recently, only three examples were known to exist, two from the Derwent River valley and one from the Tasman Peninsula in south-eastern Tasmania (de Teliga and Bryden 1958: 191; Stockton 1977b; Gaughwin 1985). The first two, Meg's Mit and de Teliga Site, consist of hand stencils of red and yellow ochre made on the walls of sandstone rockshelters. The latter site was inundated by the Meadowbank dam on the Derwent River in 1958 and only a brief description was made of them before their destruction. The surviving Meg's Mit sandstone rockshelter site is situated a few hundred metres downstream on the west bank of the Derwent and is a few metres higher than the lake level (Figure 1). On the back wall are three faint red ochre hand stencils and some lines drawn in red ochre.

Because of the scarcity of painted rock art, the origin of these stencils became a matter of debate. It was suggested by Stockton that they may have been executed by mainland Aborigines brought into Tasmania in the 1820's to help in capturing the surviving remnants of the local tribal Aboriginal population (Stockton 1977b). An interesting European analogue of such scepticism for the antiquity and independent origin of cave art is found in the French archaeological writings of the 1860's. Cave paintings in Altamira, Chabot, La Mouthe and at Pair-non-Pair were initially attributed to the work of Gallo-Roman forgers or pre-Roman

MS received 19 April 1989

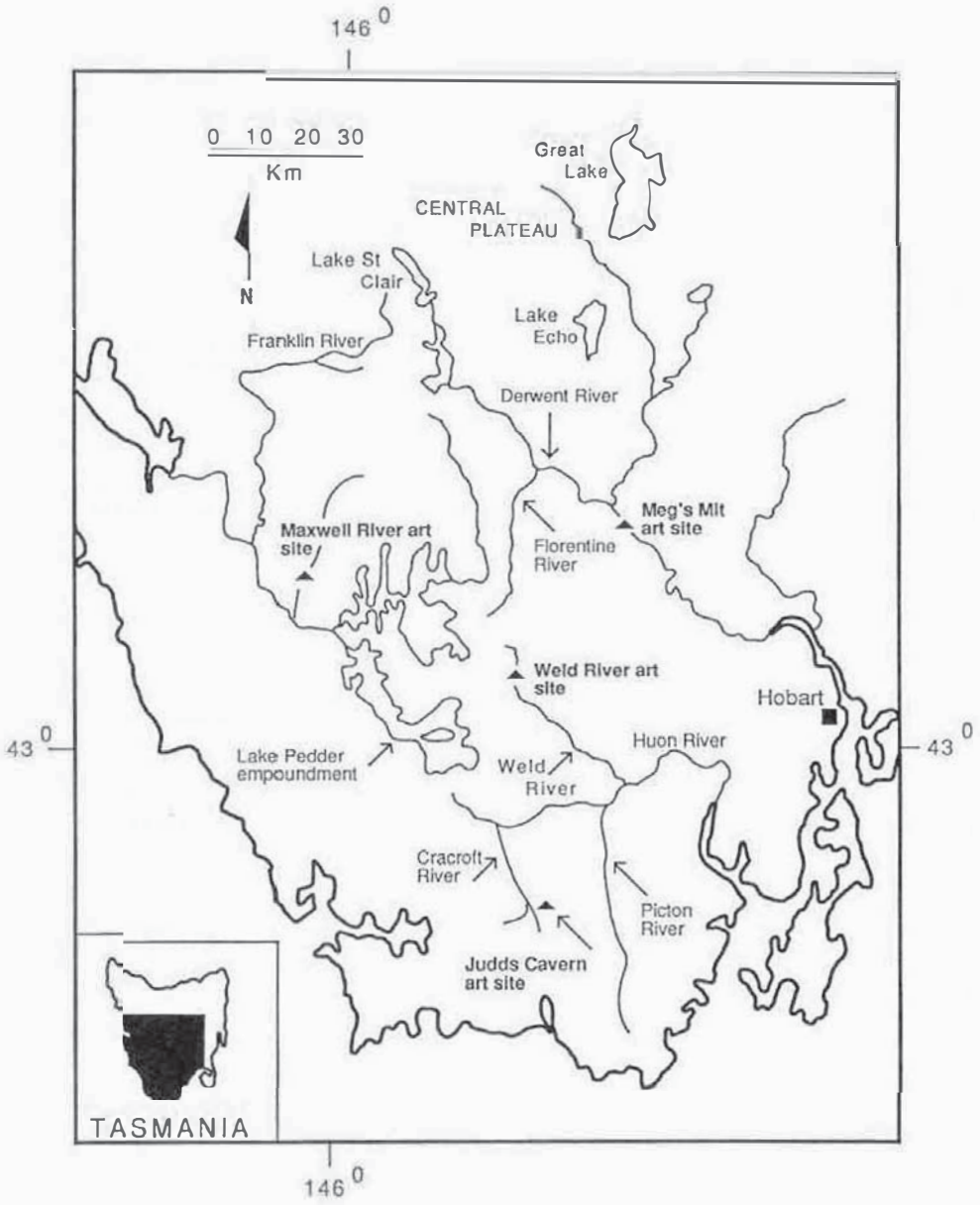


Figure 1.
The present distribution of hand stencil art sites in south-west Tasmania.

Ancient Gauls (Daniel 1975). It was known, however, that the Tasmanian Aborigines at contact did draw on bark on the insides of their huts, a practice recorded in the central highlands on a number of occasions by Robinson (Plomley 1966: 514, 542, 563, 571), Calder (1849) and Ross (1830: 113), and in eastern Tasmania by Browne (C.S.O. 1/323/7578).

A small excavation at Meg's Mit in 1978 directed by one of us (RJ) showed occupation deposit extending downwards to a depth of 0.7 m with a carbon 14 date of at least 750 years BP. The stencils are also extremely weathered and faint, having all the hallmarks of considerable antiquity. We believe them to be fully prehistoric and authentically made by Tasmanian Aborigines.

The third site, Rogers 2, situated on the western side of Tasman Peninsula, consists of three closely associated red ochre hand stencils painted on a sloping rear wall of a sandstone rockshelter. They remain undated (Gaughwin 1985: 51).

Three recent discoveries in south-western Tasmania have transformed our thinking on the origins and antiquity of Tasmanian rock art. The first was made in January 1986 at Ballawinne Cave in the Maxwell River valley dolomite karst region where, 20 m from daylight, in a completely dark chamber, a series of sixteen red-ochred hand stencils were found (Plates 1 and 2, see p. 101; Harris, Ranson and Brown 1988: 94-5). The second, and the subject of this paper, was made during September 1987 in the Cracroft River valley at the large limestone cave called Judds Cavern, 85 km to the south-east (Jones and Cosgrove 1987). The discovery was made as part of investigations as to the archaeological heritage values of the Southern Forests region to be presented before the Helsham Inquiry (Jones, Cosgrove et al. 1988: 13-20). Our field survey resulted in the discovery of at least fifteen hand stencils located on walls 30-35 m from the entrance. The third was made in May 1988 in the upper Weld River valley within the Southern Forests, at a so-far unnamed cave site, TASI no. 3614 (Allen, Cosgrove and Brown 1988). Here, three red hand stencils were found: two 10 m from the entrance and one on a low ceiling 40-50 m from the entrance in total darkness.

All three sites are located in inhospitable wil-

derness country and appear to be associated with the Pleistocene occupation of the region now dated to between 30 500 and 11 000 years ago (Cosgrove 1989).

Judds Cavern

Judds Cavern is one of the largest river caves in Australia with over 1.7 km of explored passages. Its European discovery was by the explorer Henry Judd (1896). For many decades its location was lost, but during the last 15 years it has been the subject of several speleological and geomorphological studies (Goede 1974, 1977; Gillieson and Taylor 1986). Judds Creek flows out of it and joins the South Cracroft River, which 2 km to the west eventually joins the Huon River.

Some 2 or 3 km east of the cave is the divide separating the Cracroft and Picton River valleys, with the headwaters of Farmhouse Creek flowing down into the latter. The cavern is at the foot of a distinctive limestone hill situated on the eastern slopes of the Cracroft valley. 10 km to the west of the cave area are Mt Hopetoun and Federation Peak (1224 m, first climbed by Europeans as recently as 1949), and the Arthur Range lies 15 km to the north-west.

The mountain slope behind the cave and the alluvial fans in front are clothed in rainforest and wet sclerophyll forest. The Cracroft plain is covered with button grass and stunted tea-tree. These are situated 2 km west of the cave and extend along the floor of the valley.

The mouth of the cave forms a tunnel, 30 m wide and 13 m high (Figs 2a, 2b). This extends deeply into the mountain side for the distance of several hundred metres. About 30 m from the entrance on the western side is a large alcove, its floor 8 m above the river level (Plate 3, see p. 102). This alcove is formed by the intersection of two walls of the bedded limestone, which dip down to the east. One is 18 m long, forming the south wall parallel to the angle of dip, and the other is 10 m long, forming the east wall and consisting of a series of lintels parallel to the strike. The entire alcove floor is covered in thick flowstone (reprecipitated calcium carbonate) with major speleothem concretions of more than a metre thick joining roof to floor. This large area is the location of most of the recorded art and is at the very limits of daylight penetration.

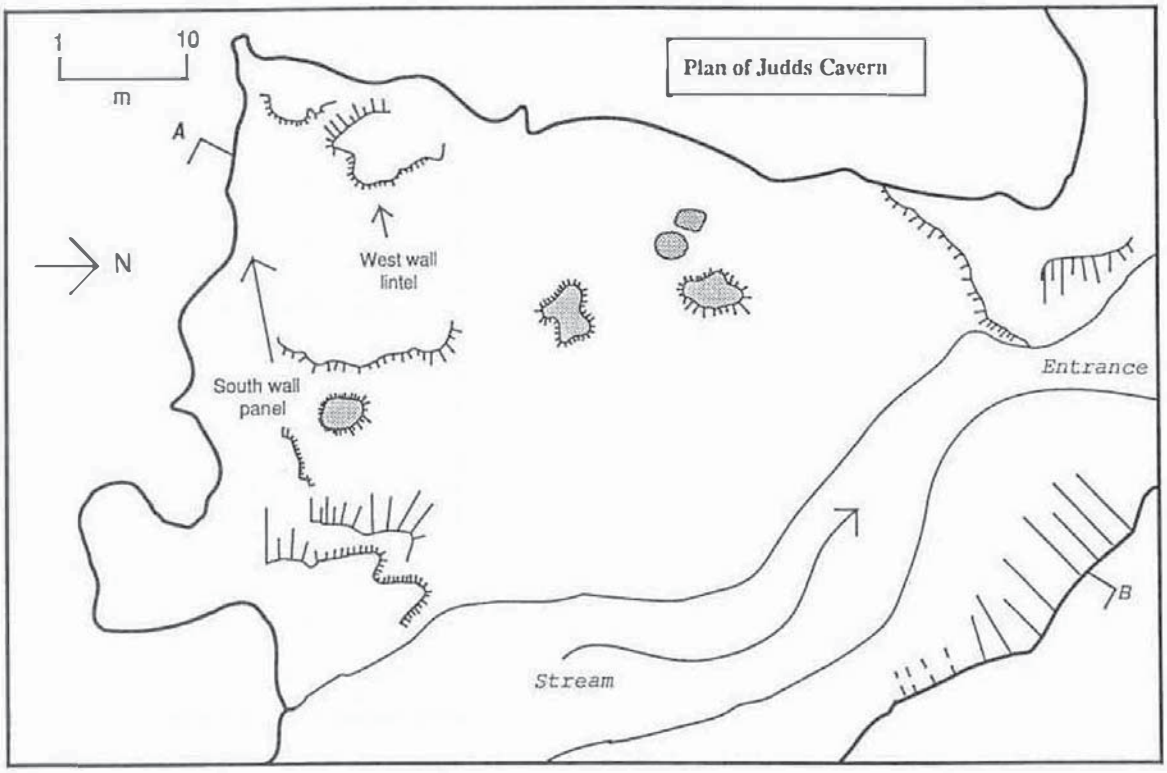
The Art Panels

(1) The Lintel: Several panels can be described. The first is located on a lintel on the western wall of the alcove (Plate 4). Here, on an area measuring 1.2 m by 0.7 m and free of speleothem deposit are five hand stencils. Four are left hands and one is right, with dimensions commensurate with adult hands (Table 1). The lintel face upon which the stencils have been made forms a sloping ceiling between 2.0 and 2.4 m above the present floor. From this fact and the orientation of two of the left hands which were angled away from the body it follows that at least two people were involved in the making of these stencils. To execute the pattern the artist would need to have been elevated off the floor somewhat and may have stood or squatted on another person's shoulders. In the case of the highest-located stencil, a clear splash pattern could be seen extending upwards beyond the stencil shadow of the fingers. One hand had a crooked little finger (arrowed, Plate 4), bent sideways like a scimitar towards the other fingers, which may have been a congenital deformity or, more probably, the result of some injury.

(2) The South Wall: The second panel is located on the south wall. This wall is bedded and slopes downwards towards the east (left hand side of the panel) with raised oblique parallel strips of harder rock standing out from the intervening concave areas. Two sets of three hand stencils are located

Hand Number	Length of middle finger to wrist	Length of 4th finger to thumb	Width of palm
west wall 1	19cm	17cm	12cm
west wall 2	17cm	not available	not available
west wall 3	19cm	18cm	10cm
west wall 4	middle finger = 12cm	not available	not available
west wall 5	16cm	not available	11cm
south wall 1	single finger only = 8cm	not available	not available
south wall 2	18cm	12cm	10cm
south wall 3	middle finger = 6cm(child)	not available	not available
south wall 4	middle finger = 14cm	not available	not available
south wall	circle	Diameter = 5cm	Pigment thickness = 3-4 mm

Table 1. Dimensions of Judds Cavern hand stencils.



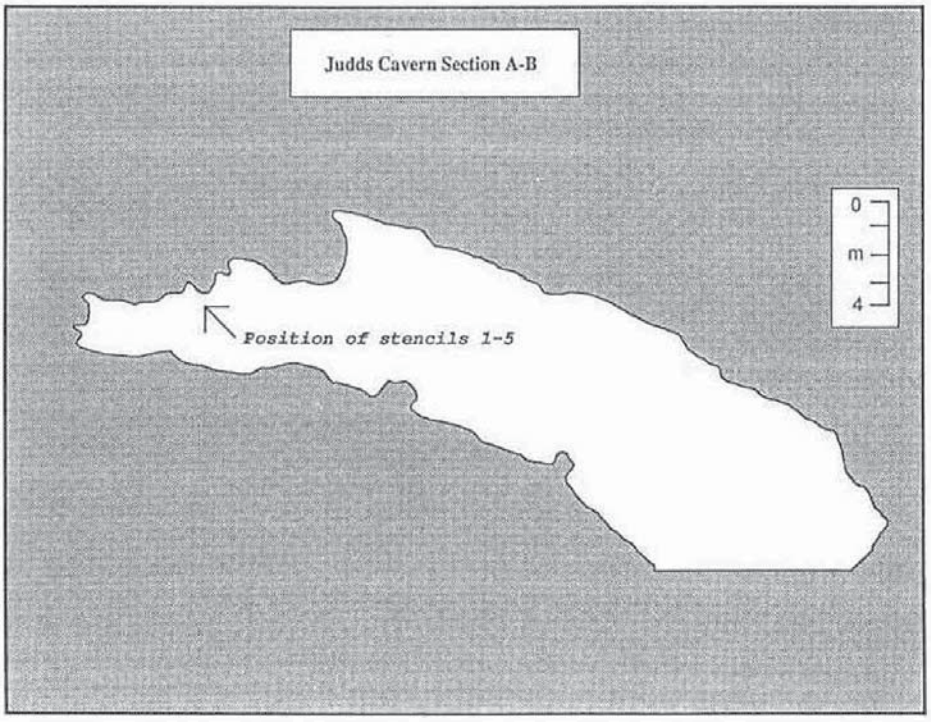


Figure 2a *(above) and* ***Figure 2b*** *(right). Judds Cavern in plan and section, redrawn from L.P.W.S. photogrammetric survey.*

in the adjacent, narrow, sloping, concave areas ('channels') slightly wider than the width of a human hand, giving the impression of definite compositions. Most of the stencils are orientated sideways, parallel to the slope, with the fingers up. At least two of them are of right hands. In that orientation, being away from the body, their execution would have necessitated another person to blow the pigment against them. One hand was that of a child, and one stencil is that of a single finger. These motifs are positioned 1-1.5 m above the sloping flowstone floor. Two other faint stencils are located 3 m above the present floor where the wall curves over to meet the roof. To put them in such a remote position some type of scaffold may have been needed. The red hand stencils were placed directly on limestone bedrock where the white secondary calcite deposit is now absent. Lower down on the left hand side of the upper of the two stencil-decorated 'channels' is a motif of a roughly drawn circle. This has a diameter of 50 mm with a width of pigment of 3-4 mm. It appears to have been drawn onto the rock with some kind of a pigment crayon.

Elsewhere on this wall are extensive areas of deep-red pigment smeared directly onto the lime-

stone. One area measures at least 1 by 1.5 m, and other parts have been eroded but originally extended over an area of 4 by 2 m. This pigment occurs in most places where the secondary calcium carbonate crust has eroded away and close inspection of the margins of these areas shows a microstratigraphy of carbonate overlying the pigment. There are other splashes of pigment on the speleothem-covered roof of the cave, 4 m above the floor. The presently visible decoration of the south panel extends over a horizontal distance of 8 m, and 3 m high. It is possible that other areas of decorated wall are at present covered by speleothem growth. Some stalactites have grown down in front of the hand stencils proving that they postdate the blowing of the motifs onto the wall behind them (small arrow, Plate 3).

(3) Other decorated areas: There are a few other, isolated hand stencils—very faint—on the roof of the cave in areas where it has not been covered by speleothem deposit. There are also several extensive areas with red pigment painted or smeared onto the wall. One of these, in a restricted west wall area of the alcove, measures several metres across. Other painted areas are situated on the cave walls and solution crevices on both sides of the main stream tunnel extending deep into the cave, 50 m from the entrance, at the point where total darkness prevails.

Antiquity of the Art

It must be stressed that much of the interior walls of the cave and the deposits within it are covered in thick calcite flowstone and stalactites —so it is highly likely that the art presently visible is only a tiny fraction of what exists under the carbonate deposit. Tiny samples (<1 g) of pigment and crystallised carbonate skin were taken by us in September 1987 from the rock surface at several locations. Preliminary analysis by Tom Loy, Department of Prehistory, Research School of Pacific Studies, Australian National University, indicates that the pigment on both the main south panel and also a smear on the portal of the small alcove to the east of the main panel gave positive responses to anti-mammal immunoglobulin G (IgG) tests (Jones, Cosgrove et al. 1988: 19). This shows that mammal blood is present within the pigment on these areas. Further tests are being conducted using monoclonal antibody reactions to determine whether or not the blood was human. Finally, having such organic compounds within the pigments gives the potential opportunity of dating them directly using the accelerator mass spectrometer method.

For two reasons we believe this art to date from the late Pleistocene period. Firstly, no cave site in the wilderness valleys of south-west Tasmania has been found with stratified deposit younger than 11 000-12 000 years old (Blain, Fullager et al. 1983; Jones, Ranson, Allen and Kiernan 1983; Jones and Allen 1984; Jones and Cosgrove 1987; Allen, Cosgrove and Brown 1988; Cosgrove 1989). It is highly likely that this art in Judds Cavern, as in Ballawinne Cave on the Maxwell River and at TASI 3614 on the Weld River, was made by the same peoples who left numerous traces of their occupation in the late Pleistocene sites of the region. Secondly, the art on the walls of Judds Cavern is covered by reprecipitated carbonate deposit, some of it extending continuously to thick flowstone and large stalagmites. The art clearly predates the last major phase of the speleothem formation which entombs the interior of the cave to the present day. Systematic radiometric dating of this chemical depositional event in caves elsewhere—in south and west Tasmania, using uranium-thorium and other methods—indicate that this took place during the more humid, terminal Pleistocene - early postglacial period 12 000 to 8 000 years ago (Goede and Harmon 1983; Kevin Kiernan pers. comm.). There is no evidence anywhere in the cave of human occupation or depositional events later than this speleothem-forming phase. It is highly probable that the art in Judds Cavern was painted before 11 000 years ago.

Discussion

Within Australia, these south-western Tasmanian cave art sites are an important artistic phenomenon—because of their antiquity and the fact that they are painted deep within subterranean passages. On the mainland, sites that can be compared to them are Koonalda and other deep caves on the Nullarbor Plain, but here the Pleistocene art within the underground passages consists of scratched or abraded grooves and finger markings, and is not painted (Wright 1971; Edwards 1971: 364). Similar artistic phenomena have been recorded in limestone caves in Western Australia, at Mount Gambier and in Victoria (Bednarik 1986). There are also examples of hand stencils in some caves on the Nullarbor, e.g. Wombat Cave, 25 km east of the South Australian - Western Australian border, where there is a series of hand stencils on the roof, c. 12 m from the entrance and at the limits of light penetration. Other examples of hand stencils in limestone caves are known along the mainland's south coast (R. Bednarik, pers. comm.) but their age is unknown.

Near Judds Cavern are other caves, such as Matchlight Cavern in which important fossil faunal remains have been found (Goede 1978; Goede and Harmon 1983). On our brief 1987 survey we did not carry out a systematic investigation of these sites, though one of us (RC) visited Matchlight in 1985 and found long bones of the red-necked wallaby (*Macropus rufogriseus*) at the cave entrance. Given the confirmed archaeological status of Judds Cavern, further work is highly desirable.

Postscript

As noted in the *Orientation* column of *Rock Art Research* (1988, 5: 171), the art at Judds Cavern has been a key element in the nomination of the Cracroft valley of the Southern Forests for nomination for World Heritage listing. This had been explicitly excluded by the majority Helsham Commission findings. However, extensive public protests and the professional support of archaeological and rock art organisations (Chippindale 1988) has happily led to a reversal of this decision by the Commonwealth Government. The nomination goes

Plate 1. *Subterranean dolomite chamber at Ballawinne Cave, Maxwell River, which has hand stencils. Fifteen red-ochred hands have been placed along the walls and ceiling. Arrow indicates location of the stencil shown in Plate 2.*

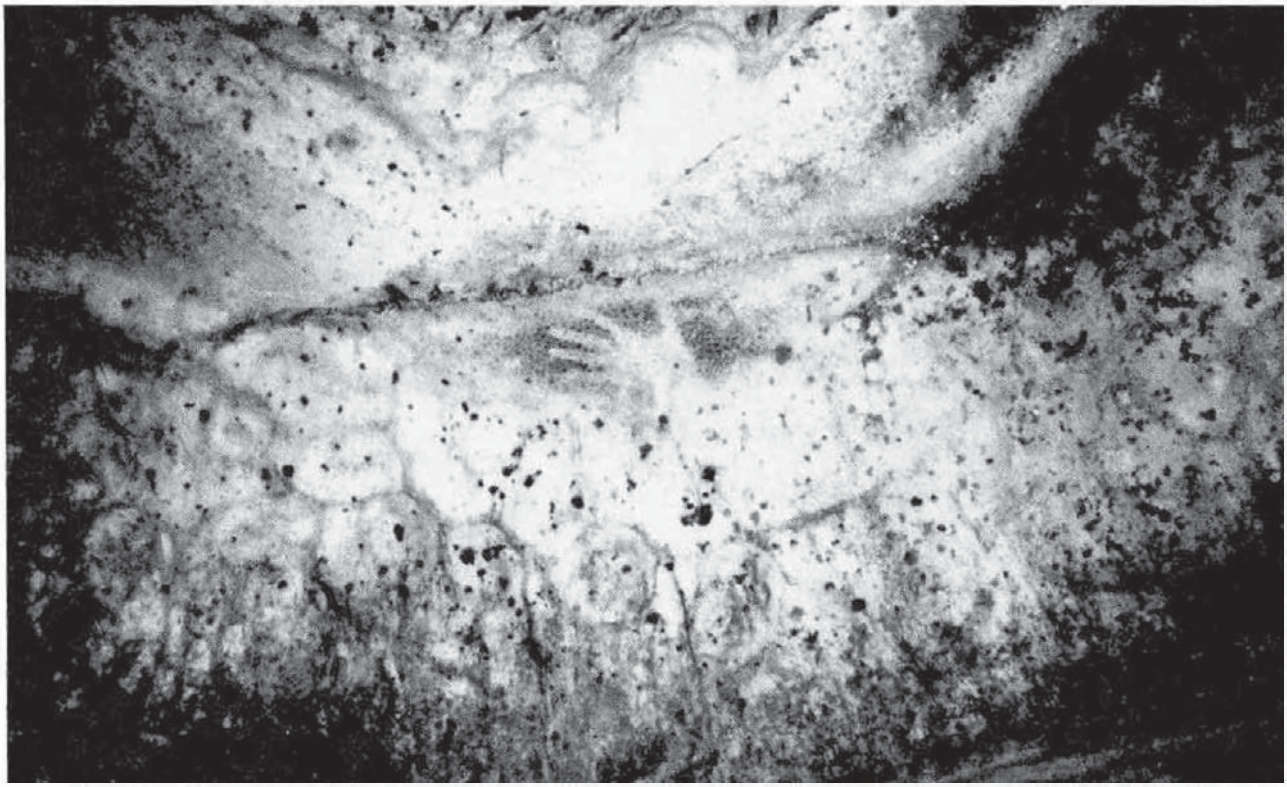


Plate 2. *Ballawinne Cave, Maxwell River. This stencil, placed at the narrow entrance to the chamber, is the most distinctive of the group of fifteen.*

Plate 3. *The large decorated alcove, situated on the western side of Judds Cavern, contains most of the stencils. The large arrow denotes the ochred lintel area, while the smaller arrow points to straws which have grown in front of the stencilled hands since their execution.*

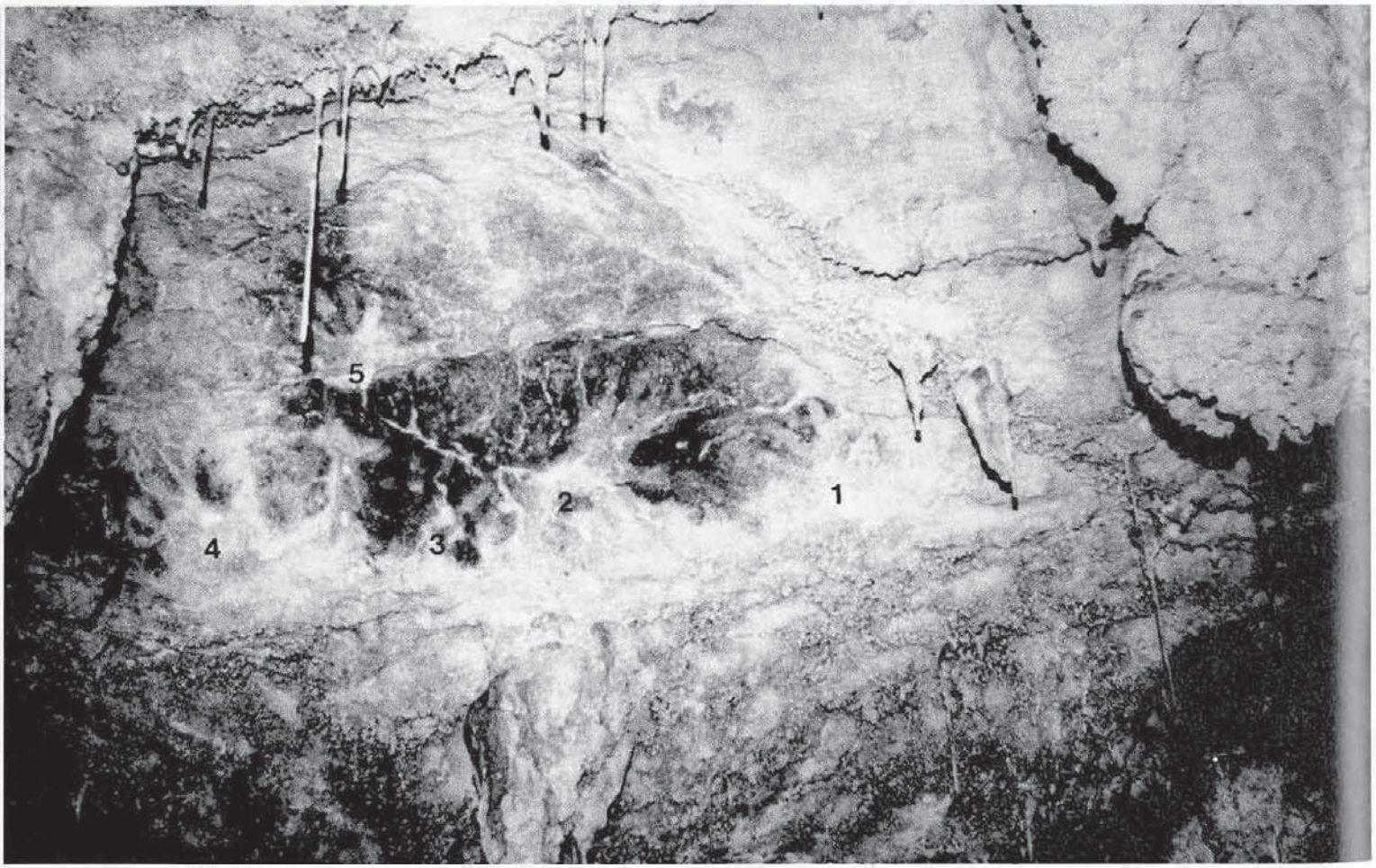


Plate 4. *Judds Cavern, details of the lintel area. The dark zone surrounding the hands is bright red ochre pigment. A red splash pattern extends from the ends of the fingers of stencil 5 towards the ceiling.*

to the World Heritage Council at the end of 1989 for ratification.

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Résumé. Une groupe de motifs de mains, peintes au pochoir et nouvellement découverte à l'interieur d'une grande caverne calcaire au sud-ouest Tasmanie est décrite. En commençant par une courte discussion de l'art Aborigène de Tasmanie et son antiquité, il y a une description detaillée de quinze mains en ochre rouge, et on a établi leur emplacement à l'interieur de la caverne. L'analyse chimique des échantillons de pigment a révélé la présence de sang de mammifère. Par moyen d'evidence géomorphique de la présence de calcium carbonate sur quelques examples de l'art, une date de >11 000 années BP se suggère pour leur origines.

Zusammenfassung. Eine Gruppe neuentdeckter Handnegative in einer ausgedehnten Kalksteinhöhle in südwest-Tasmanien wird beschrieben. Nach einer kurzen Diskussion tasmanischer Aboriginalkunst und ihres Alters wird ein detaillierter Bericht der fünfzehn Ocker Negative und ihrer Lage innerhalb der Höhle vorgelegt. Chemische Analyse kleiner Pigment-Proben ergab die Anwesenheit von Säugetierblut. Unter Verwendung geomorphischer Beweise von Kalziumkarbonat-Ablagerung über Teile der Felskunst wird für deren Ausführung ein Mindestalter von 11 000 Jahren vorgeschlagen.

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THE POST-GRADUATE DIPLOMA COURSE IN ROCK ART CONSERVATION

ALAN WATCHMAN

As reported in a previous RAR announcement (5: 66), the Getty Conservation Institute and the Canberra College of Advanced Education are jointly funding a one-year post-graduate diploma course in the conservation of rock art at the CCAE this year. The fourteen students attending the course are:

Mary Blyth, Northern Territory
Ross Brown, Tasmania
Paul Finn, Victoria
Bruce Ford, Australian Capital Territory
Nicholas Hall, Australian Capital Territory
Philip Haydock, Western Australia
Katherine Hogue, U.S.A.
Jane Kessy, Tanzania
Johannes Loubser, South Africa
Bilinda Nandadeva, Sri Lanka
Sharon Odekirk, U.S.A.
Antoinette Padgett, U.S.A.
Katharine Sale, New South Wales
Nick Tupara, New Zealand

The course began in February with the Summer School of Park Management, a field trip to the Grampians National Park and an intensive set of lectures and seminars on the management of rock art sites. The students each prepared a plan of management for the rock art sites visited in the Grampians.

In the first semester Rock Weathering, Archaeology and Anthropology of Art, Rock Art Recording and Conservation Procedures have been the units taught. Thirty-three guest lecturers with specific knowledge and skills have contributed significantly to conveying information to the students. For example, Jacques Brunet from the Laboratoire de Recherche des Monuments Historiques in France gave a series of six lectures on various aspects of rock art conservation in the caves of France.

Field trips to western New South Wales, Namadgi National Park, Morton National Park and the Gosford area have all added greatly to the teaching and understanding of the many conservation problems of Australian rock art. A field trip to Carnarvon Gorge, Chillagoe and Laura in July has also broadened the knowledge and experience of the students, and tested their abilities to comprehend the deterioration problems at art sites.

In semester two the units being studied include Research Planning, Analytical Techniques, Rock Art Recording and Conservation Procedures. Management plans for three sites in the Namadgi National Park and in Morton National Park will test the students on being able to prepare practical suggestions to manage art sites, in their liaison skills in dealing with Aboriginal people, rangers and administrative staff, and in the design of monitoring and visitor survey programs.

After students attend the AURA Annual Meeting in Sydney, the course ends on 17 November 1989, and hopefully all of the graduates from the course will be able to find employment. Another course is not intended to be run again at the CCAE for several years though preliminary discussions are being held to establish a similar course for masonry and building stone conservation. It would be encouraging to find out how many people who could not attend the year-long course would be interested in several short courses, over three or four years, which would ultimately lead to a diploma in rock art conservation. If readers are interested in such a course over a longer period they should contact

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KEYWORDS: Upper Palaeolithic - Female figurines - Arm positions - Pregnancy - Europe

LA GESTUELLE DU MEMBRE SUPÉRIEUR DANS LES FIGURATIONS FÉMININES SCULPTÉES PALÉOLITHIQUES

JEAN-PIERRE DUHARD

Abstract. The female sculpted figures of the Upper Palaeolithic are described as being far from rigid. Animation of the bodies and some movement are apparent, especially in the 'directed' attitude of the upper limbs. A review of the known figures has shown that over three quarters of them have their upper limbs depicted, which in seventy per cent of examples have apparently 'directed' attitudes. The most common is an emphasis on the abdomen, often coinciding with a protruding belly; it may have been the conventional manner of depicting pregnancy, even when the abdomen was flat.

Un mot nouveau pour une idée qui ne l'est pas

En transformant l'adjectif 'gestuel' (qui concerne les gestes, c'est-à-dire les mouvements du corps et principalement de la main, du bras et de la tête) en nom féminin 'la gestuelle', pour désigner la position de la main ou le mouvement du membre supérieur en direction d'une partie du corps, nous avons créé un néologisme. Cela aurait pu être exprimé en d'autres termes: 'le port du membre supérieur' (Pales 1976), qui est la manière de tenir ce membre, ou 'arm positions' (Ucko et Rosenfeld 1972), ou l'un de leurs synonymes: attitude, posture, maintien. Mais il nous a semblé qu' aucun de ces termes ne traduisait à la fois la position et l'intention, éventuelle, sous-jacente.

Car il nous parait que cette gestuelle n'est ni quelconque, ni indifférente: au même titre que l' adiposité du sujet, la forme et le volume des seins, de l'abdomen, des fesses ou des hanches, la présence ou non d'une fente vulvaire, tout ce que nous avons défini comme l'identité physiologique des figurations féminines paléolithiques (Duhard in prep. a), elle doit être prise en compte pour une meilleure compréhension de ces images sculptées de la femme.

Cartailhac et Breuil (1904, 1906) furent les premiers à attirer l'attention sur

> le geste des bras tendus en avant, ou bien levés en l'air . . . si souvent réitéré qu'il nous est difficile de croire que les dessinateurs ne leur aient donné cette attitude que parce qu'ils ne savaient pas où placer les bras . . . Les exemples sont . . . assez nombreux pour nous laisser, soupçonner que cette attitude réponde à une cause uniforme.

MS received 7 June 1988

Il s'agissait pour eux d'un geste de prière, d'invocation, d'où le nom 'd'orants' qu'ils leur donnèrent.

G.-H. Luquet (1934) avait également pressenti que la position des bras pouvait avoir un sens:

> Les bras, quand ils ne sont pas omis, pendent naturellement le long du corps. Dans les rares cas où ils sont ramenés sur la poitrine et le ventre, ce geste aurait tout au plus pour rôle, dans les représentations féminines, d'attirer l'attention sur les attributs distinctifs de leur sexe.

Comme souvent, bras est employé par Luquet dans son sens commun de membre supérieur, alors que le sens anatomique est plus restrictif et correspond au seul segment huméral—que prolongent l'avant-bras puis la main.

Un essai pour défendre l'idée d'animation dans la sculpture

L'impression première et très approximative que l'on a de ces figurations féminines est celle d'une expression corporelle relativement figée et peu variée dans la sculpture, alors qu'elle serait plus riche et dynamique dans la gravure.

> L'ivoire, le bois de cervidé, certaines pierres imposaient à la statuaire leurs limites naturelles de forme et de volume, auxquels n'étaient pas astreints les graveurs ou les sculpteurs de bas-reliefs sur pierre. Le volume réduit des premiers supports nommés fut ainsi préjudiciable dans bien des cas à l'expression de la réalité anatomique et certainement à la dynamique des personnages (Pales 1976).

Nous ne partageons pas ce sentiment du Dr Pales et montrerons que plus des trois-quarts des figurations ont une gestuelle et que sept fois sur 10 celle-ci est dirigée.

Cette animation de la sculpture, que l'on pourrait définir comme un 'réalisme kinésique' (du grec

kinêma: mouvement), n'est d'ailleurs pas limitée au membre supérieur: la tête peut être fléchie (dans la ronde-bosse), en extension ou en rotation latérale (dans le bas-relief); les membres inférieurs en sont pas toujours en extension ou en adduction, ils peuvent séparés au niveau des cuisses ou des jambes ou fléchis; nous avons même observé des mouvements plus complexes, comme la position ischiatique (ou attitude hanchée), qui se caractérise, en appui du corps sur un seul membre, par une bascule du bassin du côté opposé et une saillie de la hanche du même côté: c'est le cas du 'Losange' de Grimaldi, de la statuette en os de Péchialet et du 'Torse' de Brassempouy, que nous avons étudiées au Musée des Antiquités Nationales de Saint-Germain-en-Laye (Duhard 1989a).

Une étude par la force des choses limitée

Un grand nombre de figurations sculptées—que nous estimons à une centaine—ne peut être retenu en raison de leur état d'ébauche, leur caractère schématique ou des mutilations subies ayant altéré ou fait disparaître une partie plus ou moins importante de leur corps.

Dans le premier groupe nous incluons: les figurines n° 3 et 4 d'Avdeevo, n° V, XII, XIII et XIV de Dolní-Věstonice, n° 4, 14, 15, 20 et 27 de Malta, II et III de Willendorf, n° 4, 5, 6 et 8 de Gagarino, ainsi que la 'Fillette' et 'L'ébauche de poupée' de Brassempouy, les statuettes de Gönnersdorf, Mezine, Mezerice, Oelknitz, Nebra, Pavlov, Pekarna, Petersfels, Předmost, Rytirska, ainsi que celles du Lac Trasimène, de Trou Magrite, Vogelherd et Weinberg.

On trouve dans le second groupe: toutes les têtes isolées (Brassempouy, Dolní Věstonice, Kostenki etc.) et toutes les statuettes où l'on ne peut dire si les membres supérieur était ou non représenté ('La Poire' et 'L'ébauche' de Brassempouy, la Figurine VI de Dolní-Věstonice, la n° 2 de Buret' et 'l'Homme', supposé, de Laussel). La 'Figurine à la pélerine' de Brassempouy n'a pas été retenue, bien qu'apparaisse sur le fragment de torse le membre supérieur droit fléchi, non plus que le fragment n° 27 de Kostenki à gestuelle abdominale, où manque la moitié supérieure du corps.

Les figurations de sexe féminin incertain (figure de droite du bas-relief de Terme-Pialat, figurine anthropomorphe de Laugerie-Basse) ou de sexe masculin ('Figurine à la ceinture' de Brassempouy, Duhard 1987b) ont naturellement été écartées. Ce sont finalement 75 figurations que nous avons retenues et étudiées, 27 en ayant accés à l'oeuvre originale et 3 à leur moulage et 45 d'après les photographies ou relevés de la littérature (notamment la publication d'Abramova sur les statuettes russes).

Les différentes positions du membre supérieur

P. J. Ucko et A. Rosenfeld les ont brièvement décrites lors du Symposium de Santander (1972)

> *On figurines, the arms are most commonly shown extended downwards close to the body, followed by arms meeting, or nearly so, on the stomach. ... The rare positions on figurines include: arms over the top of breast; arms to the chin and possibly arms outstretched. ... More elaborate arm positions occur on a few parietal examples only and include ... one outstretched and the other down or on the stomach, or one on the head, and the other akimbo ...*

L. Pales et M. T. de Saint-Péreuse, étudiant les Humains gravés de La Marche (1976), distinguent différents ports du membre supérieur: (1) en rectitude, avec quatre variétés: tendu à l'horizontal, tendu en bas et en avant, tombant le long du corps et oblique vers le haut; (2) en flexion, avec deux variétés: bras tombant—avant-bras horizontal ou oblique—et bras horizontal, avant-bras vertical.

Ceux dont l'approche de la question est restée superficielle ont émis des avis non confirmés par l'étude des faits. Il en va ainsi de Luquet, déjà cité, ou de Ducros:

> ... l'attitude [de la Vénus de Lespugue se retrouve] ... dans d'autres statuettes du Paléolithique européen, jusqu'en U.R.S.S. ... Presque toutes ... ont la même disposition de bras collés au torse et d'avant-bras reposant sur les seins (Ducros 1983).

En réalité il n'y en a aucune en Russie.

Nous faisons pour notre part une distinction entre les figurations dépourvues de gestuelle du membre supérieur et celles qui en présente, considérant que l'absence de membre supérieur doit être prise en considération, aussi bien que sa présence. Quand elle existe, la gestuelle peut être simple ou complexe. Elle est simple si les deux membres ont la même position, latérale ou antérieure: dans la gestuelle latérale les membres sont appliqués le long du corps, en extension ou en flexion légère du coude; ils peuvent être complets, avec leurs trois segments, mais sont assez souvent réduits à deux, voire un seul segment; dans un cas particulier d'une scéne à deux personnages opposés, l'un semble se tenir les chevilles. Dans la gestuelle antérieure les membres, avec la main figurée ou non, sont soit fléchis (attitude en 'orants' de Breuil), soit posés sur un organe (sein) ou une région (abdomen); dans un cas particulier les mains sont en position pré-crurales (Duhard 1987a). La gestuelle est complexe quand les deux membres n'ont pas la même position: l'un en flexion latérale et l'autre sur l'abdomen ou le long du corps, ou encore l'un au contact de la tête et l'autre sur la hanche.

Analyse des figurations retenues

Sur les 75 figurations sculptées retenues, 63 sont en ronde-bosse et 12 en bas-relief. Deux sont de découverte récente: la statuette du Courbet (1986) et la troisième figure debout de la Magdeleine-des-Albis (1987).

Les figurations sans gestuelle (17):

Ce sont celles où font défaut les membres supérieurs, sans que leur absence soit imputable à une fracture ancienne; en d'autres termes, ils n'ont jamais existé; 11 sont en ronde-bosse et 6 en bas-relief.

Sur les 11 ronde-bosses, 5 peuvent être considérées comme gravides: (1) le 'Torse' de Brassempouy, de morphologie gynoïde, aux hanches larges, à la poitrine opulente, à l'abdomen ovoïde et saillant de paucigeste; (2) la 'Femme au goitre' de Grimaldi, à seins ptosés, abdomen volumineux et bas et vulve béante de multigeste parturiente (Duhard 1988c); (3) la 'Dame' de Monpazier, également multigeste parturiente (Duhard 1987c), dont la saillie abdominale associée a l'hyperlordose évoque un bassin antéversé; (4) la 'Vénus' de Tursac, figurine où deux régions relativement réalistes, abdomen et segment

pelvi-pédieux, évoquent une femme gravide près du terme, dont la stéatopygie postérieure résulterait en partie de la position accroupie; (5) la statuette de Ptrkovice, au corps svelte, sans ptose mammaire, et au ventre légèrement saillant et anguleux de jeune primigeste au début de sa grossesse.

Les 6 autres figurines ne nous semblent pas représenter des femmes gravides: (1) la statuette du Mas d'Azil ne présente ni gros ventre, ni adiposité excessive, mais l'allongement des seins avec migration du mamelon au pôle inférieur se voit seulement chez la multipare; (2) la 'Venere di Chiozza', à morphologie de multipare obèse: seins gros et pendants, abdomen plicaturé et ptosé, cuisses épaissies; (3) la statuette d'Elissevichi au ventre plat, au buste étroit et long, ressemblant à une jeune femme nulligeste atteinte d'adipose inférieure, (4) la statuette de Péchialet, dépourvue de caractéres sexuels secondaires, mais portant sur sa face dorsale des reliefs qui pourraient être mammaires, si l'on accepte, l'idée d'une distraction de l'artiste; (5) la 'Venusstatuetten von Mähren' aux seins volumineux reposant sur un abdomen dont la paroi relachée creuse un profond pli hypogastrique, manifeste un excès d'adiposité par des dépôts graisseux autour de la taille et sur le pubis et présente l'aspect d'une multipare grasse; (6) la 'Vénus' de Vibraye (ou 'Vénus impudique' de Laugerie-Basse) aux cuisses minces, aux hanches et au thorax étroit, à la fente vulvaire verticale évoque une fillette pré-pubère.

Sur les 6 bas-reliefs sans gestuelle, 4 nous paraissent gravides: (1) la petite figure de l'abri Pataud a un buste étroit, une taille très fine, des hanches minces, lui donnant un aspect svelte de femme jeune dont l'abdomen rond en bouclier évoque une grossesse avancée et l'allongement des seins un passé de nourrice; (2) la deuxième figure d'Angles-sur-l'Anglin a la morphologie d'une femme svelte, où l'abdomen fortement proéminent et non ptosé nous semble caractériser un état de grossesse chez une primigeste (Duhard, in prep. b); (3) la troisième figure debout de la Magdeleine, haute de 48 cm, a l'aspect d'une femme longiligne mince dont l'abdomen légèrement saillant et arrondi a le volume de celui d'une grossesse débutante; (4) la figure de gauche de Terme-Pialat montre de profil un sein extrêmement ptosé et un abdomen dont le volume a été soigneusement mis en relief en dégageant le contour antérieur par un champ-levé et en surcreusant la région de la hanche et de la taille.

Les deux autres figures de la frise aux femmes de l'abri Bourdois d'Angles-sur-l'Anglin n'ont pas de caractères gravides: la femme n° 1 a des hanches discrètement empatées et, sous un ventre plat, un repli hypogastrique que pourrait expliquer le relâchement d'une paroi abdominale un peu grasse chez une femme-pare; la femme n° 3, sans doute en raison du relief naturel de la paroi à cet endroit, a le ventre creux et ne peut donc être enceinte; nous avons écarté la femme n° 4, à qui font défaut l'abdomen et la partie supérieure de corps.

Ainsi, sur 17 figurations sans gestuelle, 9 sont gravides (5 en ronde-bosse, et 4 en bas-relief) et 8 ne le sont pas (6 ronde-bosses et 2 bas-reliefs).

Les figurations à gestuelle indifférente (18):

Les figurines où les membres supérieurs tombent le long du corps et celles où, simplement esquissés, ils ne se terminent pas de façon non équivoque sur les seins ou l'abdomen, appartiennent au groupe des figurations à gestuelle indifférente et non-dirigée.

Dans ce groupe 6 nous semblent être gravides: (1) le 'Polichinelle', bien mal baptisé par Luquet, n'est ni masculin, ni bossu; la béance vulvaire de cette femme stéatopyge a gros ventre est compatible avec une parturition; l'asymétrie du corps et l'étroitesse du bassin pourraient accompagner un rétrécissement pelvien (Duhard 1988b); (2) le 'Losange', contrairement à ce qui a pu être écrit (Duhard 1989a), a des membres supérieurs esquissés, rejoignant les ilions et présente une obésité généralisée à prédominance pelvienne et une poitrine fortement ptosée de multipare; chez cette femme aussi l'association d'un abdomen rond et fortement saillant et d'une vulve béante suggèrent une expulsion foetale imminente; (3) la statuette en stéatite jaune présente également des membres supérieurs se terminant au contact de la crête iliaque; l'abdomen, avec sa saillie médiane ovoïde, fait penser à un état gravide chez une multigeste obèse gynoïde à seins hypertrophiés et ptosés; (4) la 'femme au cou perforé', comme les trois précédentes, provient de Grimaldi; les membres supérieurs sans segments individualisés, sont collés le long du corps et se terminent à hauteur de la taille par une encoche, semblable à celle que l'on observe chez le 'Polichinelle'; elle a la singularité de présenter deux faces, l'une avec des seins plats, un ventre saillant et une fente vulvaire, l'autre sans vulve avec un ventre plat et des seins gravés; la face 'gravide' a du être aménagée la première et l'hypothèse d'une parturition nous semble légitime; (5) la statuette n° 5 de Kostenki présente *'[a] protruding abdomen ... [and] ... arms outlined only above the elbows'. 'It had been intentionally smashed by a strong blow in the region of the breast'* (Abramova 1967); cette figuration massive pourrait être celle d'une obèse; (6) la figurine n° 3 de Gagarino est décrite par Abramova avec *'... sagging, low-placed breasts, elongated torso, swollen abdomen ... The arms, tightly pressed to the body, can be pressed only down to the elbows'*; son aspect est celui d'une multigeste svelte longiligne *('it is extremely elongated in length ...')*.

Pour 11 d'entre elles la grossesse semble pouvoir être exclue. Trois de ces figurines pourraient être des multipares: (1) Avdeevo n° 1: *'[has] low-placed, pendulous breasts [and] a round, relatively flat abdomen ... The arms are outlined by a light line and are designated only down to the elbows'* (Abramova 1967); (2) la 'Venus I' de Dolni-Vestonice est une obèse gynoïde à seins et abdomen ptosés; les épaules sont bien traduites, mais les membres supérieurs simplement ébauchés; (3) Gagarino n° 1 est la répresentation d'une femme à obésité gynoïde dont le pelvis est entouré d'une véritable bouée de graisse où se confondent et se rejoignent les dépôts fessiers, iliaques et abdominaux; les seins énormes et ptosés de façon considérable participent à l'inflation adipeuse; l'abdomen, tombant vers l'avant nous semble celui d'une obèse non en-

	35 à gestuelle non dirigée		40 à gestuelle dirigée				
	17 absente	18 indifférente	4 mammaire	28 gestuelle abdominale	2 d'expulsion	6 flexion orientée	
Ronde bosses	Torse (*) Femme au gaitre(*) Monpazier (*) Tursac (*) Mas d'Azil (*) Péchialet (*) Laugerie-Basse (*) Petrkovice Eliseevichi Moravany Chiozza di Savd.	Polichinelle (*) Losange (*) Stat. stéat.J.(*) Fem.cou perforé Kostenki n°5 Avdeevo n°1 D.Věstonice 1 Gagarino n°1,3,7 Buret' n° 1 et 4 Malta n°3, 7, 10, 11, 13 et 16	Lespugue (*) Manche de Poignard(*) Willendorf(°) Savignano	Fig.non décrite(*) Malta n°1,2 Parabita grande Malta n°5,6 Parabita petite Malta n°8 Kostienki n°1, 2 Malta n°9 Kostienki n°3, 4 Malta n°12 Kostienki n°6 Malta n°19 Courbet (*) Malta n°23 Angles 63 (*) Malta n°24 Avdeevo n°2 Malta n°25 Buret' n°2,3 Malta n°28 Buret' n°5 Malta n°29	Hermaphrodite(*)	Sireuil (*) Gagarino n°2	63
Bas reliefs	Abri Fataud(°) Angles n°1(*) Angles n°2(*) Angles n°3(*) La Magdeleine 3(*) Terme-Pialat(*)	0	0	Femme à la corne (*)	Personnages opposés (*)	Fem.à la tête quadrillée(*) Vénus Berlin (°) La Magdeleine 1(* La Magdeleine 2(*	12
	17	18	4	28	2	6	

Table 1. *Tableau des 75 figurations féminines étudiées, 35 à gestuelle non dirigée et 40 à gestuelle dirigée. (*) Figurations examinées personnellement (27) (°) Moulages (3)*

ceinte; *'The . . . thick arms, tightly pressed to the body, show up only down to the elbows, and from that point on they cannot be traced . . .'* (Abramova 1967): il ne nous semble donc pas légitime de supposer qu'ils rejoignent les seins (dessus ou dessous) et ne l'avons pas retenu dans les gestuelles mammaires.

Six autres figurines appartiennent au groupe de Malta. D'après la description d'Abramova et les photographies et dessins qu'elle donne, toutes ont les membres supérieurs *'closely pressed to the body'* ou *'slightly outlined along the sides'* ou séparés du corps par une *'distinct line'*. Le ventre est plat, les seins parfois absents, la vulve non figurée; 3 pourraient être vêtues (3, 13 et 16) et les 3 autres nues (7, 10 et 11).

Les deux autres sont de Buret': la n° 1 serait également vêtue; les membres supérieurs pendent le long du corps; seins et abdomen sont plats; il est bien évident que pour ces figures humaines vêtues la féminité est discutable; la n° 4 a les fesses protubérantes mais le ventre plat; *'the arms roundly bent at the elbows . . . can be seen along its sides'*.

Il n'y a pas non plus de gestuelle dirigée chez la dernière figurine en provenance de Gagarino, que nous numérotons 7 (elle ne figure pas dans Abramova mais est décrite par Delporte (1979) sous le n° 4): les mains atteignent la face latérale du bassin; la mutilation de l'abdomen empêche de se prononcer sur un éventuel état gravide. Les seins ptosés, la stéatopygie étalée, les dépôts trochantériens orientent vers une multiparité. De même que les statuettes n° 2 et 3 de cette station, elle a les jambes légèrement écartées, et le membre inférieur droit un peu fléchi, ce qui prouve que la statuaire n'est pas nécessairement figée.

Les figurations à gestuelle mammaire (4):

Deux des quatre figurations de ce groupe sont très connues et leur image, très fréquemment reproduite a pu laisser croire que toutes les 'Vénus' paléolithiques leur ressemblait (Ducros 1983), ce qui est loin d'être le cas. La 'Vénus' de Lespugue et la 'Vénus I' de Willendorf, la première longiligne, l'autre bréviligne, ont l'aspect de femmes obèses à stéatopygie étalée et hypertrophie mammaire ptosée. La mutilation accidentelle, lors de sa découverte, de la face antérieure de la statuette de Lespuque ne permet pas de savoir si elle portait un ventre gravide mais, recouvert pas les seins, il ne pouvait avoir de grandes dimensions; ce qui persiste du sommet du triangle pubo-génital ne permet pas de penser que la vulve était indiquée; seul est conservé l'avant-bras droit, reposant sur le sein; un reliquat de relief à gauche laisse supposer que la gestuelle est bilatérale; à signaler un interstice separant le bras du corps des deux côtés, seul exemple connu dans la ronde-bosse. La statuette de Willendorf, mieux conservée porte un abdomen étalé en bouée (comme Gagarino n° 1 et n° 2) et une fente vulvaire est nettement indiquée; les avant-bras reposent sur la partie supérieure des seins et les doigts de la main sont esquissés.

P. Graziosi a décrit en 1923 *'una statuetta steatopigica'* italienne, *'la Venere del Savignano'*: *'Le braccia appena accenate e prive di mani si ripiegano verso il petto quasi a premere i seni . . .'*. Vaufrey (1926) précise: 'La position des bras n'a pas été remarquée par les auteurs italiens. Bien que le relief en soit aujourd'hui très atténué, on voit suffisamment qu'ils reposaient sur les seins'. Elle se présente par ailleurs comme une femme adipeuse à poitrine opulente, taille épaisse, bourrelets iliaques, stéatopygie postérieure et abdomen saillant et rond de paucigeste.

C'est lors du second examen du 'Manche de poignard' de Brassempouy que notre attention a été attirée par un trait gravé rectiligne, oblique en bas et en dedans à partir des épaules et barrant le tiers supéro-externe de chaque sein. En recourant à un faible grossissement de la binoculaire, nous avons eu la surprise de constater l'existence indubitable de membres supérieurs très courts, jusque là passés inapperçus, formant un léger relief à contour postérieur bi-curviligne, soulignant le bras et l'avant-bras. Large à sa racine, le membre

	35 à gestuelle non dirigée		10 à gestuelle dirigée				
	17 absente	18 indifférente	4 mammaire	28 gestuelle abdominale	2 d'expulsion	6 flexion orientée	
Gravides	Torse (*) Femme au goitre (*) Monpazier (*) Tursac (*) Pirlevice Pataud (°) Magdeleine 3 (*) Angles n°2 (*) Terme-Pialat (*)	Polichinelle (*) Losange (*) Stat. Stéat. J. (*) Stat. cou perforé Kostenki n°5 Gagarino n°3	Savignano	Parabita grande Kostenki n°1 Kostenki n°3 Kostenki n°4 Kostenki n°6 Buret' n°3 Malta n° 2 Malta n°12 Malta n°28 Femme à la corne (*) Angles 63 (*) Courbet (*)	Hermaphrodite (*) Personnages opposés (*)	Sireuil (*)	31
Non gravides	Mas d'Azil (*) Péchialet (*) Laugerie-Basse (*) Chiozza di S. Elisseevichi Monruany Angles n°1 (*) Angles n°3 (*)	Avdeevo n°1 D. Vestonice 1 Gagarino n°1 Malta n°3 et 7 Malta 10 Malta 11 Malta 13 et 16 Buret' 1 et 4	Willendorf (°)	Parabita petite Buret' n°2 Buret' n°5 Malta n° 1 Malta n°5 Malta n°6 Malta n°8 Malta n°9 Malta n°19 Malta n°23 Malta n°24 Malta n°25 Malta n°29 Avdeevo n°2	0	Gagarino n°2 Femme à la tête quadrill. (*) Vénus berlin (*) Magdeleine 1 (*) Magdeleine 3 (*)	39
Incertaines	0	Gagarino n°7	Lespugue (*) Manche Poign. (*)	Kostenki n°2 Figurine non décrite (*)	0	0	5
	17	18	4	28	2	6	

Table 2. *Tableaux ventilant les figurations suivant la gestuelle et la gravidité.*
(*) Figurations examinées personnellement; (°) Moulages.

s'amincit à mi-longueur et se termine en pointe émoussée; il est mieux visible à gauche qu'à droite. Cette figurine a subi une importante (et peut-être abusive) restauration, notamment de la quasi totalité de l'abdomen rendant hasardeuse une appréciation sur sa forme et son volume originels. L'importante hypertrophie et ptose des seins et la non moins conséquente stéatopygie étalée avec stéatocoxie associée, sont en faveur d'une morphologie de multipare à obèsité gynoïde.

Les figurations à gestuelle abdominale (28):

Dans ce groupe important, une seule est en bas-relief: la femme à la corne de Laussel, dont nous avons récemment repris l'étude (Duhard in prep. c). Elle présente un abdomen saillant non ptosé, à profil convexe, en bouclier, et contour circulaire qui ne laissent guère de place au doute concernant son état gravide; la répartition gynoïde de ses masses grasses et la ptose des seins témoignent de grossesses antérieures; la complexité de l'histoire qu'elle exprime nous parait remarquable, celle d'une femme-pare, enceinte, la main gauche reposant sur le ventre et la main droite portant une corne incisée de 13 marques, où nous avons lu un calendrier de fécondité (Duhard 1988a).

Chez 2 d'entre elles la détérioration de la face ventrale ne permet pas de préciser si des mains terminaient les avant-bras, ni quels étaient la forme et le volume de l'abdomen. Ce sont: (1) la 'Figurine non décrite' de Grimaldi, qui n'est pas non plus dépourvue de membres supérieurs: le bras tombe le long du corps, légèrement oblique en avant, et se poursuit par un avant-bras demi-fléchi dirigé vers l'abdomen; les seins coniques, pointant en avant, ont un volume et une forme de pré-lactation; les cuisses devaient être fléchies à angle droit sur le bassin, ce qui rendrait compte de la forte saillie du massif fessier; (2) la statuette n° 2 de Kostenki a du présenter des maternités antérieures, comme l'attestent la poitrine volumineuse et ptosée et la répartition gynoïde des graisses, avec les dépôts iliaques et trochantériens.

Alors que la statuette 57 d'Angles-sur-l'Anglin ressemble à un phallus (comme l'avait vu Breuil, mais sans calembour possible), la 63 nous parait bien être une figuration féminine et nous souscrivons tout à fait à la description de Saint-Mathurin (1978). Ce pourrait être une femme en position agenouillée, sans figuration nette des seins (mais la face ventrale est quelque peu détériorée), avec un abdomen ample et saillant, bien séparé de la région pubo-génitale; plus nettement à gauche qu'à droite, l'avant-bras fléchi se dirige vers l'abdomen.

Chez 10 autres statuettes la probabilité d'une grossesse nous parait très grande: (1) la grande statuette de Parabita, aux seins de jeune femme, a les avant-bras qui se rejoignent sur la partie inférieure du ventre et, malgré des hanches et des cuisses un peu épaisses, présente une morphologie de paucigeste; (2) la figurine n° 1 de Kostenki *'[Has] pear-shaped, loose-hanging breasts and a protruding abdomen ... The arms are reposing on the abdomen'* (Abramova 1967). Son aspect est celui d'une multigeste; (3) la figurine n° 3 de Kostenki ressemble beaucoup à la n° 1, et malgré l'amputation très partielle des seins et de l'abdomen, nous la considérerons aussi comme une multigeste; (4) la figurine n° 4, à poitrine volumineuse et ptosée, forte stéatocoxie, mais stéatotrochantérie plus modérée, a les mains qui se rejoignent sur la partie médiane et inférieure de l'abdomen; (5) chez la figurine n° 6 *'the abdomen is enlarged and the arms clearly shown in relief are reposing on it'* (Abramova 1967); (6) Buret' n° 3: *'the abdomen is small but protruding ... The arms are closely pressed to the sides of the body ending in curves on the abdomen'* (Abramova 1967); (7) Malta n° 2: *'There are ... rather small loose-hanging breasts which are portrayed in faint relief, a somewhat small but swollen abdomen ... the hands repose on the abdomen'*

(Abramova 1967); (8) la figurine n° 12 de Malta, assez grossière, a un abdomen protubérant vers lequel se dirigent les membres supérieurs; (9) figurine n° 28 de Malta: *'It is quite evident that it was not finished, but on it are indicated clearly and in relief the voluminous abdomen and the small breasts which are triangular in shape'* (Abramova 1967); les membres supérieurs, ébauchés, atteignent l'abdomen; (10) la statuette du Courbet a été brièvement publiée (Ladier 1987): 'poitrine volumineuse tombant su un ventre saillant, fessier très proéminent'; après examen, il nous apparait que l'abdomen saille en avant, sans sillon hypogastrique ni ptose et peut être considéré comme gravide; il n'existe pas de seins, mais des membres supérieurs jointifs sur la ligne médiane; la femme figurée présente une obésité inférieure avex stéatopygie postérieure.

Chez les 14 autres, il ne semble pas y avoir de gros ventre de grossesse accompagnant la gestuelle abdominale: (1) la petite statuette de Parabita a le ventre plat, mais des mains extrêmement agrandies se superposant sur la partie inférieure du ventre; (2) la figurine n° 2 de Buret', bien que partiellement amputée, montre un ventre plat et une gestuelle abdominale: *'The thin arms are reposing on the abdomen'* (Abramova 1967); (3) chez la figurine n° 5 de Buret', au ventre plat, *'The arms, tightly pressed to the sides and somewhat turned towards the abdomen, are lightly outlined'* (Abramova 1967); (4) Malta n° 1: *'the abdomen is round but not bulging . . . The arms . . . are shown resting on the abdomen'* (Abramova 1967); (5) Malta n° 5: la gestuelle abdominale est nette, mais le ventre plat; (6) Malta n° 6: les seins, soulignés par des traits profondément gravés, sont pendants, les membres supérieurs se terminent sur un ventre plat; (7) Malta n° 8: les seins sont en léger relief, mais le ventre plat; les fesses hautes donnent un aspect longiligne a cette figurine; (8) Malta n° 9: seins ptosés, fesses et ventre plats, hanches larges et gestuelle abdominale résument la morphologie de cette statuette; (9) Malta n° 19: c'est un peu le modèle réduite de la précédente, mais avec des seins en léger relief et non ptosés; (10) Malta n° 23: les fesses sont hautes, les hanches larges, les seins triangulaires, le ventre plat; (11) Malta n° 24: *'the arms are resting on the abdomen, and the rounded breasts are shown in relief'* (Abramova 1967); les fesses et le ventre sont plats, les hanches plutôt étroites; (12) Malta n° 29: cette figurine, pratiquement dépourvue de caractères sexuels secondaires féminins, présente néanmoins une gestuelle abdominale et nous l'avons gardé dans notre sélection de figurations féminines, considérant qu'aucune statuette de Malta n'est explicitement masculine (ce qui n'est pas le cas, par exemple, à Brassempouy); (13) Malta n° 25 est une réplique en miniature de la n° 29 et les mêmes considérations peuvent être faites; (14) la figurine n° 2 d'Avdeevo a l'aspect un peu massif de la statuette n° 5 de Kostenki; *'The shoulders are almost as wide as the thighs, and the arms, pressed close to the body, come together on the protruding lower part of the abdomen'* (Abramova 1967).

Il faut noter que 12 des figurines de ce dernier groupe appartiennent aux stations de Malta et Buret', très proches du point de vue style et culture. Elles ont particulièrement en commun: la fréquence de la gestuelle abdominale et la discrétion des reliefs antéro-postérieurs: les fesses sont peu saillantes, les seins généralement plats et simplement gravés et le ventre plat ou peu protubérant—la figurine n° 28 apparaissant comme une exception, mais il semble qu'elle ne soit pas achevée. On peut penser que l'absence de relief, notamment abdominal, résulte du peu d'épaisseur de la matière, à moins que la gestuelle abdominale ne supplée l'absence de gros ventre.

Les figurations à gestuelle d'expulsion (2):

Le nom 'd'Hermaphrodite' donné par Luquet en 1934 à une figurine de Grimaldi n'est pas plus heureux que celui de 'Polichinelle'. Nous avons eu l'occasion de donner notre interprétation, démontrant qu'il ne s'agit ni d'un hermaphrodite, ni d'un sujet masculin: nous estimons que cette statuette représente une femme primigeste svelte en cours de parturition, aidant de ses mains l'extraction du foetus (Duhard 1987a).

Les 'Personnages opposés' de Laussel, sculptés en bas-relief ont été diversement interprétés: scène de coit (Lalanne 1911; Giédion 1965), scène d'accouchement (Lalanne 1946); Luquet hésitait entre les deux hypothèses (1934) et Breuil trouvait les deux plausibles, alors que Leroi-Gourhan (1965) n'en retenait aucune. Après examen de la pièce, nous pensons qu'il pourrait s'agir d'une scène d'accouchement ou la femme, agenouillée, aurait les mains cramponnées aux chevilles pour aider aux efforts expulsifs.

Les figurations avec flexion orientée du membre supérieur (6):

La statuette de Sireuil porte des seins petits, coniques et saillants de jeune femme dont la proéminence abdominale serait compatible avec un état gravide. Les membres supérieurs, demi-fléchis, ont une direction oblique en bas et en avant et émergent du plan frontal au-dessous des seins. Breuil imaginait la figurine en décubitus ventral (1930) et Delporte (1979) la voit agenouillée: nous la croyons en décubitus dorsal, dos cambré, membres inférieurs fléchis et membres supérieurs 'bloqués' dans une posture de parturiente au moment des efforts expulsifs. C'est la seule de ce groupe paraissant enceinte.

La figurine n° 2 de Gagarino est morphologiquement très proche de la n° 1 (multipare à obèsité gynoïde), mais se singularise par une étonnante gestuelle: *'The arms have been treated in a totally unusual manner . . . They are closely pressed to the body down as far as the elbows, then turn upwards and rise to the level of the chin'* (Abramova 1967). Nous serions enclins à y voir la posture d'une mère tenant un enfant pour l'embrasser ou le bercer.

La 'Vénus' à la tête quadrillée de Laussel se présente à nous avec des seins volumineux et ptosés, un ventre plat, un buste large, des bras épais évoquant, malgré des hanches non visibles, une obésité généralisée de multipare. La 'Vénus' de Berlin présenterait plutôt une obésité de type gynoïde, avec un abdomen également plat; le volume et la ptose des seins indiquent un passé de nourrice.

Les deux figures couchées sculptées en bas-re-

lief dans la grotte de La Magdelaine ont une morphologie de femme-pare (seins triangulaires). Celle de droite a le dos de la main gauche reposant sur le flanc, alors que la main droite supporte le menton. Celle de gauche a la main droite appuyée sur la hanche, alors que la gauche rejoint la partie supéro-lateral de la tête. Ici, la gestuelle des membres supérieures doit être intégrée a celle du corps en général, qui parait allongé, en genou demi-fléchi, dans une posture d'abandon (voluptueux?) ou de repos (Duhard 1989b).

Essai de synthése

Nous avons repris, sous forme de tableaux les différentes gestuelles du membre supérieur en répartissant les figurations suivant le type de sculpture (fig. 1) et suivant l'état ou non de gravidité (fig. 2), cela permet de mieux saisir les observations que cette étude permet de dégager:

(1) Dans la ronde-bosse, ou les difficultés techniques sont les plus grandes, les figurations à gestuelle dirigée sont plus nombreuses que celles à gestuelle non dirigée (34 contre 29, soit 54%), alors que la répartition est identique dans le bas-relief. Cette constatation, contre-disant le Dr Pales (1976), nous pousse à penser que la gestuelle donnée par l'artiste aux figurines malgré, nous le répétons, une plus grande contrainte technique, devait avoir un sens.

(2) Dans le bas-relief il n'y a pas de gestuelle indifférente: ou bien elle est absente, ou bien elle est dirigée.

(3) Sculptures en ronde-bosse et en bas-relief confondues, les figurations a gestuelle dirigée apparaissent les plus fréquentes (40 contre 35, soit 53.33%).

(4) De tous les types, c'est la gestuelle abdominale qui revient avec la plus grande fréquence: près du tiers de l'ensemble des figurations et de la moitié de celles présentant une gestuelle et plus des deux-tiers des figurations à gestuelle dirigée.

(5) Les figurations non gravides sont plus nombreuses quand la gestuelle est indifférente (11 sur 18, soit 61%) que lorsqu'elle est abdominale (14 sur 28, soit 50%), ce qui peut faire évoquer une relation entre la gravidité et la gestuelle abdominale, d'autant que cette gestuelle est un plus fréquente chez la femme gravide (38, 7%) que chez la femme non gravide (35, 9%).

La gestuelle est un moyen d'expression du corps, un langage muet, spontané ou conventionnel, mais qui revêt un sens et l'on ne peut reconnaître le même à des positions aussi variées des membres supérieurs. De même qu'il nous semble que dans le corps dénudé de la figuration l'artiste a représenté des détails permettant de reconnaître l'identite physiologique du sujet, il nous parait que la gestuelle porte une signification et fait partie de l'histoire physiologique ou symbolique de l'oeuvre.

La position des avant-bras ou des mains reposant sur les seins des 'Vénus' de Lespugue et de Willendorf n'est pas naturelle, aucune femme n'a spontanément cette posture. Nous croyons que l'artiste a sciemment disposé les mains sur les seins, moins pour attirer l'attention sur eux (ils sont suffisamment volumineux pour être vus) que pour souligner une fonction essentielle de la femme, celle de nourrice, une fonction vitale pour l'enfant en un temps où sa survie immédiate et médiate en dépendait, aucun autre substitut n'existant au lait maternel les premiers mois de la vie. Tel est, pensons-nous le sens de cette gestuelle mammaire, qui peut ou non être associée a une gros ventre de grossesse ('Vénus' de Savignano).

Il est fréquent pendant la grossesse que la femme pose les mains sur son ventre pour percevoir les mouvements de l'enfant, apprécier l'arrondi de ce volume nouveau, soulager une tension, une pesanteur, des tiraillements, calmer des mouvements excessifs. Cette gestuelle est par contre rare en dehors de la grossesse, la femme portant plus spontanément les mains à hauteur de la région pubienne que de l'abdomen. Une des raisons à cela est anatomique et tient à la longueur des membres supérieurs: lorsqu'ils pendent naturellement le long du corps, leur extrémité atteint, voire dépasse, le milieu de la cuisse; si l'on rapproche les mains de la ligne médiane, vers l'avant, elles viennent couvrir la région pubo-génitale. Pour porter les mains au ventre ou à la poitrine, il faut à la fois une abduction des bras et une flexion des avant-bras. Cette abduction n'est pas figurée dans la sculpture paléolithique excepté dans un cas: la statuette de Lespugue où existe un petit espace vide entre le bras et le thorax. Et la technique de sculpture en ronde-bosse ne suffit pas l'expliquer, puisque la 'Femme a la corne', dont la main gauche repose sur l'abdomen, a le bras étroitement collé au corps. L'abduction-flexion du membre est le mouvement naturel pour ramener la main sur le ventre ou les seins, mais on peut aboutir au même résultat par un mouvement contraint: en gardant les bras collés au corps, a condition de déplacer les coudes vers l'arrière. Mais on ne retrouve aucune de ces deux positions, la naturelle et la contrainte, en examinant ces figurations: les bras sont serrés contre le corps, dans le plan frontal médian, ou un peu en avant, ce qui signifie, sans qu'il soit nécessaire de les mesurer, que les membres supérieurs sont plus courts qu'ils ne le devraient. Ce raccourcissement, fréquemment observé aussi sur le membre inférieur, et le non-respect habituel des proportions (notamment entre la tête et le corps) ne nous étonne pas: c'est moins un portrait anatomique qu'un portrait physiologique que nous a laissé l'artiste. Quand il montre des seins hypertrophiés et ptosés, des hanches élargies, des fesses étalées ou saillantes, ce sont les caractères morphologiques liès à la parité qu'il modèle. Et, s'il a souvent négligé de représenter le membre supérieur (22.6% des cas) ou l'a simplement esquissé et laissé en position indifférente (24% des cas), il a bien pris soin, quand il le jugeait utile, de positionner la main pour préciser l'histoire physiologique, en soulignant ce sur quoi devait être attirée l'attention, et le plus souvent l'abdomen, comme nous l'avons constaté.

Conclusion sur la signification de la gestuelle abdominale

Nous avons abordé cette étude sans a priori, souhaitant simplement vérifier les allégations de nos prédecesseurs. Nos résultats, si l'on peut accorder quelque valeur aux chiffres, nous font découvrir des constatations nouvelles en révélant la préémi-

nence de la gestuelle abdominale. Chiffres et pourcentages doivent cependant être acceptés avec réserve car non seulement nous ne connaissons pas toutes les oeuvres qui ont été produites par les paléolithiques (tous les sites n'on pas été découverts, toutes les découvertes ne nous sont pas connues et une partie de la production a pu disparaître du fait d'un support périssable ou d'une destruction volontaire) et, de plus, un nombre important de celles dont nous disposons sont altérées ou fragmentaires et dans notre sélection, comme dans notre interprétation entre une part d'arbitraire.

Ces réserves faites, nous pensons que la gestuelle abdominale, plus fréquemment rencontrée dans les figurations avec gestuelle et gravides (12/22, soit 54.54%) que dans les figurations a gestuelle mais non gravides (14/31, soit 45.16%) doit être considérée comme un signe de grossesse et donc un substitut au gros ventre quand celui-ci fait défaut. Il est significatif à nos yeux que 12 des 14 femmes non gravides à gestuelle abdominale appartiennent au groupe de Malta-Buret' remarquable par la discrétion des reliefs et la fréquence de la gestuelle abdominale, intéressant 70% des figurines. La comparaison des deux statuettes de Parabita renforce notre conviction: les deux ont une gestuelle abdominale, mais seule la grande, la mieux façonnée, porte un gros ventre manifestement gravide; la petite, plus sommaire, a les fesses et le ventre plat et les hanches étroites et, sans la présence des seins, on pourrait douter de sa féminité: pourtant, malgré le caractère schématique de cette oeuvre, l'artiste a pris soin de représenter l'essentiel: les seins (pour montrer qu'il s'agit d'une femme) et deux énormes mains en position abdominale (pour montrer qu'elle est enceinte). Cette petite figurine résume en quelque sorte la plus grande, ainsi que notre démonstration.

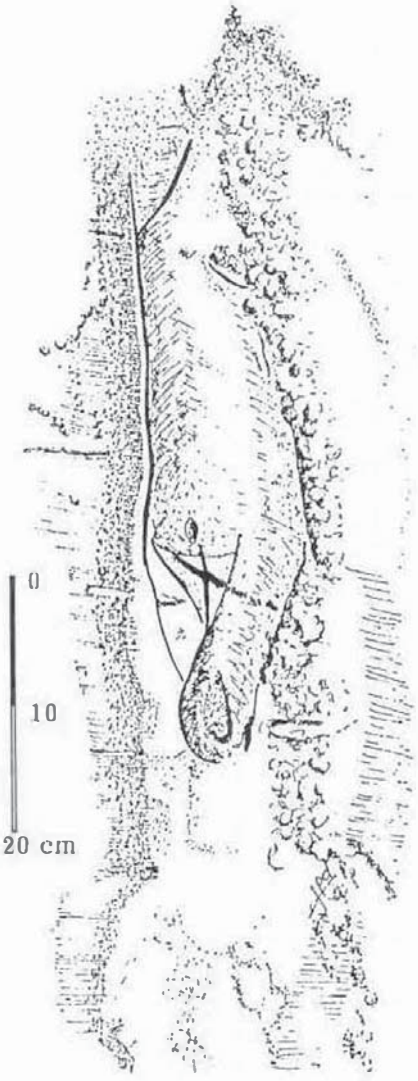
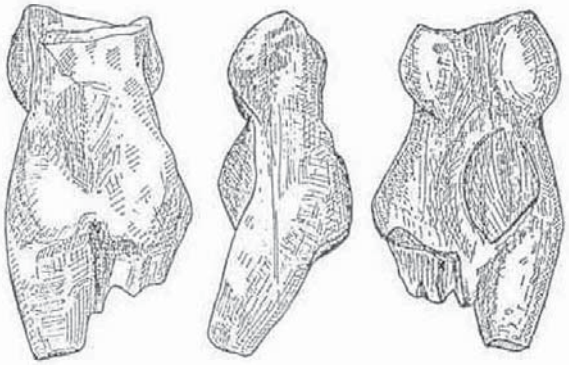
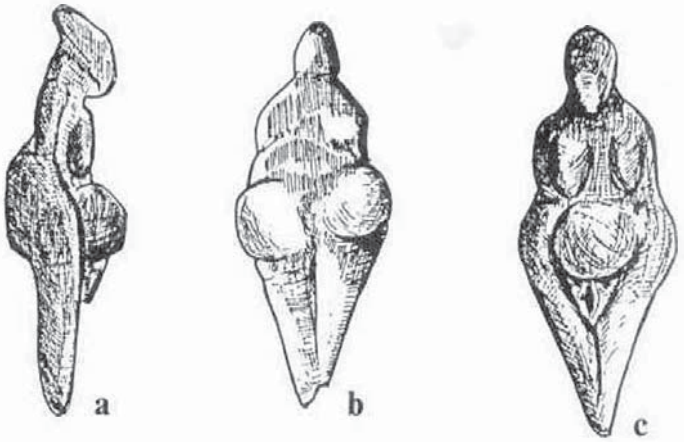


Figure 1.
Figure féminine debout de La Magdelaine (inédite), sans gestuelle.

Figure 2. →
Le 'Torse' de Brassempouy, sans gestuelle, avec position sur la hanche droite expliquant son plus grand débord (détail inédit).





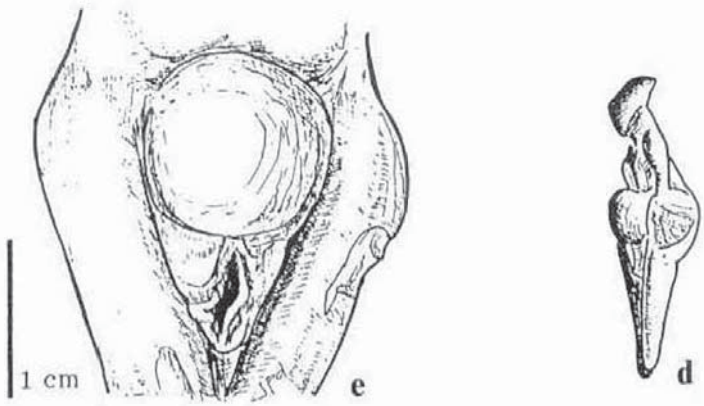


Figure 3. →
Le 'Losange' de Grimaldi, avec gestuelle latérale et position sur la hanche droite.

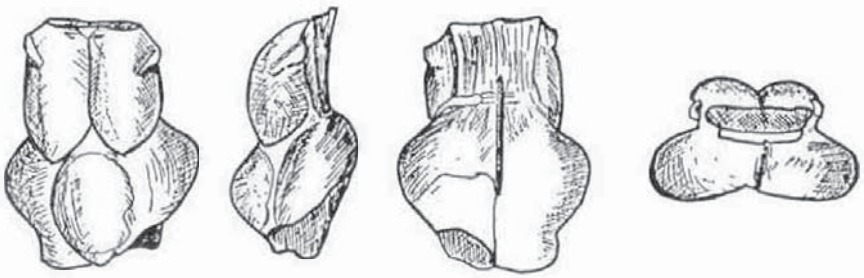


Figure 4.
Le 'Manche de poignard' de Brassempouy avec membres supérieurs très réduits et gestuelle mammaire (détail inédit).

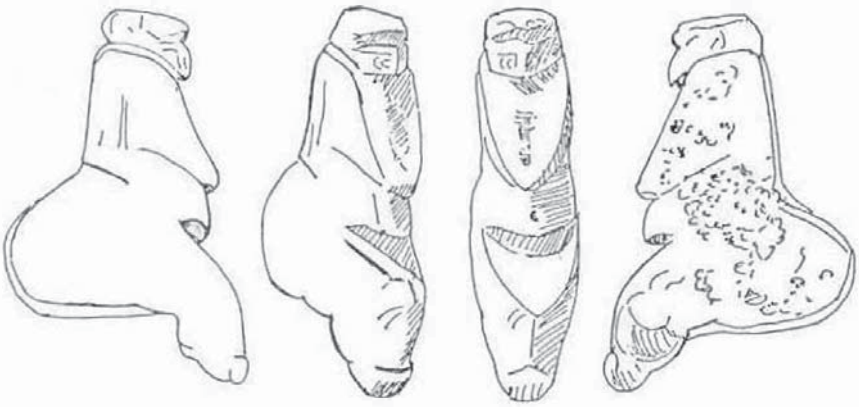


Figure 5.
La 'Dame' du Courbet avec gestuelle abdominale (interprétation inédite).

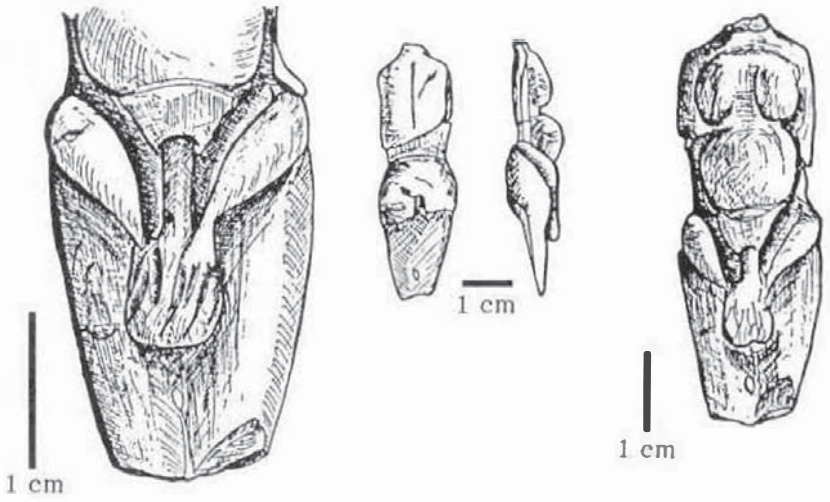


Figure 6.
'L'Hermaphrodite' de Grimaldi avec gestuelle d'aide manuelle à l'expulsion (interprétation personnelle).

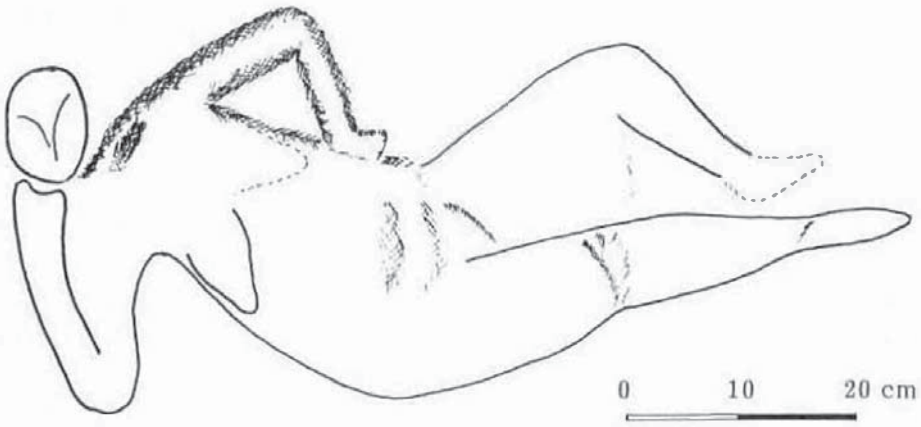


Figure 7.
Une figure couchée de La Magdelaine (paroi droite), avec gestuelle complexe.

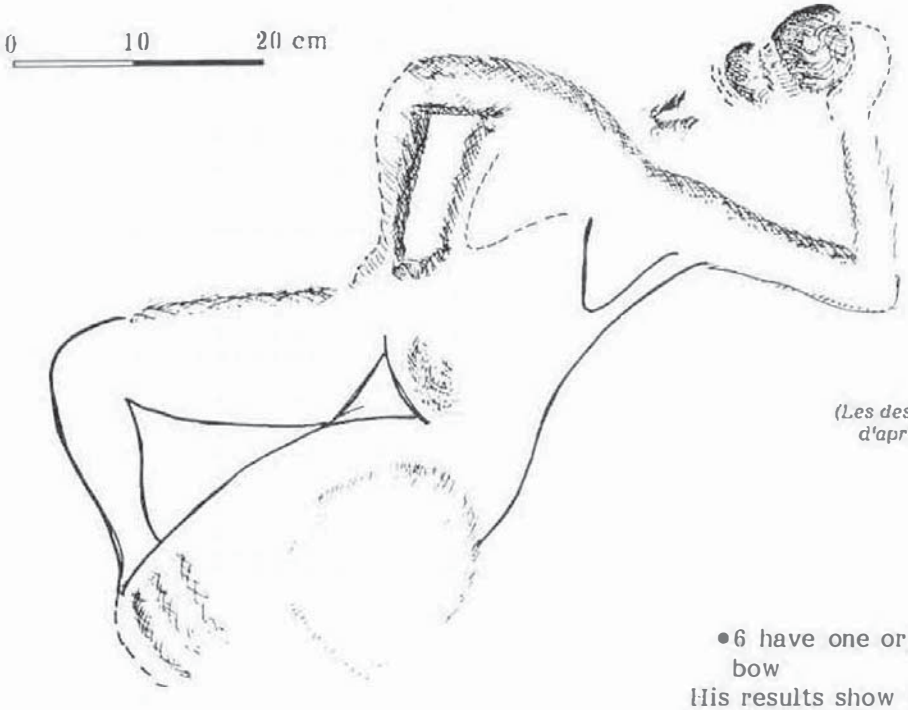


Figure 8.
La deuxième figure couchée de La Magdelaine (paroi gauche), avec gestuelle complexe.

(Les dessins sont de l'auteur et faits d'après les oeuvres originales.)

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SUMMARY AND COMMENT

By PAUL G. BAHN

Dr Duhard's paper focuses on the presence and position of the upper limb in Upper Palaeolithic female carvings. In the past it has generally been assumed that the problems of working with such a limited volume led to a lack of dynamism in the figures, but Duhard's study reveals that over 75% of them do have the upper limb depicted, and over 70% of those have the arms 'directed' to a specific point, thus giving the carvings some animation. Similarly, poorly informed authors, basing themselves on the most famous figurines (those of Lespugue and Willendorf I) have often claimed that almost all the carvings have their forearms resting on the breasts.

Duhard has used a series of 75 carvings (omitting those where, through damage or incompleteness, the presence or position of arms cannot be determined), and has divided them into three groups: those without arms depicted, those with 'simple arms' (i.e. both arms in the same position, either hanging at the sides or brought round to the front) and those with 'complex arms' (where the arms take different positions). His sample comprises 63 figures carved in the round, and 12 bas-reliefs.

- 17 of these have no upper limbs depicted (11 figurines, 6 bas-reliefs)
- 18 have arms hanging at their sides
- Only 4 have arms directed to the breasts
- 28 have arms directed to the abdomen
- 2 seem to have arms involved in giving birth
- 6 have one or both arms bent upward at the elbow

His results show that, of the three-dimensional figures, 54% (34 of 63) have 'directed' arms, and he suggests that this has some meaning. In bas-reliefs, arms are either absent or directed. No less than 40 figures (53.33% of the whole sample) have their arms directed somewhere, as opposed to being absent or just hanging. Of the areas to which the arms are directed, the abdomen is the most common, comprising almost a third of all figures, a half of those with arms, and over two thirds of those with directed arms.

He stresses that the placing of forearms on top of the breasts is unnatural, and seems to have been done not to draw attention to the mammary glands (which are only too conspicuous on the few figurines in question) but to underline their role in nourishing infants. Similarly, the placing of the hands on the abdomen is common only during pregnancy —normally, the female's hands hang lower, and a more normal pose would be to have them clasped at pubic level. In short, Duhard believes that there is a link between pregnancy and this abdominal gesture: it is a means of bodily expression, a part of the figurines' physiology and symbolism.

In these figures, the upper limbs, like the lower, are often shortened, and there are other signs of disrespect for realistic proportions (especially between head size and body). He therefore sees them not as anatomical portraits but as physiological portraits, that depict morphological features linked to pregnancy. The arms are used to draw attention to these features in many cases.

Dr Duhard is a professional gynecologist, and his expertise shows clearly not only in this paper but also in the others he has published in recent years as well as in his doctoral thesis on the Upper Palaeolithic female depictions (Duhard 1989a). It is always useful and welcome to have a particular expertise brought to bear on a problem in prehistory; and in prehistoric art particularly a detailed knowledge of anatomy can help to shed fresh light on the identification or ethology of animal and human figures, as well as to dismiss some of the more

fanciful and inaccurate pronouncements of subjective and ill-informed researchers. This was one of the many major contributions made by the late Dr Léon Pales to the field. As a medical man and expert on animal anatomy he brought badly needed and rigorous standards to the study of Palaeolithic depictions both of humans and of animals, and squashed definitively an accumulated mass of sloppy descriptions, suppositions and interpretations (see summary in Bahn and Vertut 1988, Chapter 6).

In view of Duhard's professional qualifications, his assessments of age, obesity and possible state of pregnancy in these female depictions are certainly far more reliable than the somewhat arbitrary estimates made from published illustrations by Rice (1981); her study was tentative and interesting (Bahn and Vertut 1988: 138), but has already been made redundant not only by Duhard but also by Gvozdover (1989: 92) who found that Rice's criteria do not correspond with reality and cannot be determined on the original objects.

It is also noteworthy that Gvozdover's study, of the female figurines of the Kostenki culture, shows a similar focus of attention on the abdomen and breasts; and as for my own area of interest, the vulva (Bahn 1986), she stresses the lack of depicted genitalia (Gvozdover 1989: 43, 52). Duhard too, in his thesis (1989), supports the lack of importance and the infrequency of depiction of the genital organs, and hence the lack of preoccupation with sexuality, as witnessed by the extreme rarity of copulation scenes (Bahn and Vertut 1988: 161-5). I was particularly interested to read in the present article that his examination of the original damaged Lespugue figurine suggests that the vulva was not necessarily depicted, whereas in Guthrie's paper (1984: 66) that figurine was reconstructed with the vulva clearly marked, in order to fit his view that Palaeolithic images of females were all erotica for macho hunters (see Bahn 1985: 58).

One point on which I differ with Duhard is his view of the Laussel 'playing card' figure as a possible birth scene. Readers of *RAR* may recall that this same image is seen by Heinz Hunger (in Bahn 1986: 109-10) as most likely two figures in genital union (albeit with an absence of explicitly depicted genitals), whereas a gynecologist now sees it as a birth: each to his own 'preoccupancy'! Personally I concur with Leroi-Gourhan that no interpretation of this enigmatic image can be in any way reliable, since it has also been seen as a full Venus figure and as a person standing waist-deep in water (see Bahn 1986: 119; Bahn and Vertut 1988: 163)!

Being neither a medic nor a nudist, my own firsthand knowledge of the female form comes, I am happy to say, from a fairly narrow band of the spectrum of ages and possible physical types, and all, as far as I am aware, mercifully free from pregnancy. I therefore approach the topic of this paper only as a gynephile, with an occasional tendency to gyneolatry. I cannot argue with Duhard's expert assessments of the figurines' morphology and his deduction of their symbolic significance; and indeed I think it highly probable that some, among so many images of females, will have a link with the reproductive function—hence the emphasis on the abdomen and, to a lesser extent, on the breasts.

What I am less comfortable with, however, is the somewhat subjective selection of which parts of the figures to 'take at face value' and which not. Duhard himself stresses that these are not anatomically realistic portraits, and points to the shortened arms and legs, the lack of proportion between head and body; but he also treats the breasts, abdomen, hips and buttocks as being relatively realistic, a fairly accurate guide to the subject's physiological history, how many children she has had etc. One is therefore compelled to wonder why we should take these features as 'gospel', but not the proportions of the head and limbs.

This reminds me of the tradition of seeing any round-bellied horse in Palaeolithic art as being pregnant, even though professional veterinarians cannot tell from a profile view whether a horse is pregnant or not (Bahn and Vertut 1988: 161). The spotted horses of Pech-Merle have often been claimed to be pregnant by people who took their huge bellies at face value, but ignored their short legs and tiny heads. All these features are stylistic conventions, and perhaps the same is equally true of the female figurines. These are not Palaeolithic photographs but images by artists with a message to convey. What were the messages?

It is likely, as Duhard points out in his thesis, that the female figures are naked not for the sake of aesthetics or erotica but as a means of depicting morphological characteristics; and it is interesting to have available an expert's appraisal of those characteristics as depicted. But we need to bear in mind that the notion that each figure was meant to represent a female of a precise age and physical state is a theory, a possibility, not an accepted fact. For that reason I share the view, expressed by Pales in a letter to Duhard and reprinted in his thesis (1989: 8), that prehistoric artists may have been concerned with different meanings, and may have had a very different vision of contour and volume. On the other hand, we today can only see their work through our own society's eyes, and hence despite the questionable validity of excessively detailed analysis of clearly stylised images it is nevertheless useful to have the pronouncements of an expert about the interesting places these prehistoric ladies have been made to place their arms and hands.

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REPLY

By JEAN-PIERRE DUHARD

Plutôt qu'une résponse à mon estimé collégue le Dr Paul G. Bahn, les lignes suivantes sont une tentative pour expliquer la notion nouvelle de 'réalisme physiologique', que je propose.

Si nous examinons d'un point de vue anatomique les figures paléolithiques, aussi bien animales qu'humaines, nous observons qu'aucune ne reproduit fidélement—comme le ferait un portrait photographique—le modèle supposé. La tête est trop petite ou trop grosse, les membres raccourcis, des segments du corps disproportionnés, des détails omis—comme les traits du visage ou les doigts.

Cependant, malgré toutes ces imperfections graphiques, nous parvenons à identifier un humain et préciser son sexe féminin et pouvons même dire si ce sujet est gravide ou non, a nourri ou pas. En bref nous parvenons à établir son identité physiologique. C'est grâce au fait que l'artiste a pris soin de détailler les parties du corps les plus représentatives de la femme (Leroi-Gourhan 1965) et les plus influencées par sa fonction de reproduction. Et il l'a fait avec suffisamment de réalisme pour que nous reconnaissions, comme chez le vivant: une stéatopygie postérieure ou étalée (Duhard 1988d), un ventre gravide ou obèse, des seins de jeune fille ou de nourrice. Concernant ces régions féminines, les ressemblances étroites entre les figurations féminines paléolithiques et les femmes que nous côtoyons quotidiennement dans notre exercice professionnel supposent que le corps féminin est resté inchangé au fil des millénaires et impliquent à la fois une bonne observation des modèles et une reproduction fidèle des formes—qui n'ont pas pu être inventées.

On peut s'étonner de la différence de traitement des seins et de la région pelvi-abdominale, bien rendus, et des parties distales, souvent négligées, et y trouver un argument contre notre théorie du réalisme physiologique. La raison de ce contraste est que ces dernières parties ne sont pas nécessaires à l'identification du sujet et ne participent pas à son histoire physiologique. Il nous semble significatif que les têtes isolées soient pourvues des traits—permettant un diagnostic d'humain—voire présentent une mimique—rire et sourire notamment, comme à La Marche (Duhard 1989b). Il nous semble intéressant aussi de souligner que les mains isolées sont incomparablement mieux détaillées que les mains incorporées et que les pieds sont remarquablement figurés quand ils sont un des rares moyens d'identifier un humain (chez les 'sorciers' ou humains 'composites', Duhard 1989c).

Chez l'animal nous observons pratiquement l'inverse: l'important paraît être non pas son sexe ou son état gravide éventuel, mais plutôt son espèce, que permettent de caractériser les sabots ou les griffes, les cornes ou les bois, les bosses ou les fanons, la fourrure ou le pelage. Avec une même ligne de dos, suivant que l'on ajoute tel détail ou tel autre, on fera naître un cheval, un bison ou un boeuf (Leroi-Gourhan 1965).

N'en doutons pas, il existe une différence de nature entre l'art animalier, abondant, et l'art humain, faiblement représenté, comme existe une signification différente entre les représentations de figures entières et de parties du corps.

Nous n'avons jamais essayé d'imaginer, pas davantage dans cet article sur la gestuelle du membre supérieur que dans notre thèse, ce que pensaient les paléolithiques. Nous nous sommes bornés à constater: (1) que nous reconnaissions des humains de sexe féminin; (2) que ces figures féminines présentaient avec nos contemporaines d'étroites ressemblances que l'on ne peut expliquer par le hasard; (3) que le corps chez la femme est le reflet de ses fonctions, et particulièrement de celle de reproduction; (4) que l'on trouvait dans les figures paléolithiques la même diversité que chez le vivant, ce que le Dr Pales avait également constaté:

> En dépit de l'analogie de nombreuses représentations, les formes figurées ne sont pas stéréotypées; les artistes ont exprimé la diversité qui devait nécessairement exister chez leurs contemporaines, ne serait-ce qu'en fonction de l'âge, comme il en est aujourd'hui (Pales 1976).

Et cette citation nous servira de conclusion.

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Résumé. Les figurations féminines sculptées du Paléolithique supérieur sont loin d'être figées. Il existe une animation des corps, expression d'un réalisme kinésique et se traduisant, notamment, dans la gestuelle du membre supérieur. Une revue des figurations connues nous a montré que plus de trois sur quatre avaient une gestuelle du membre supérieur et que sept sur dix, elle était dirigée. La plus fréquente est la gestuelle abdominale, souvent associée à un ventre gravide; et qui pourrait être un moyen conventionnel de représenter la grossesse, même lorsque le ventre est plat.

Zusammenfassung. Die weiblichen plastischen Figuren des oberen Paläolithikums sind bei weitem nicht starr. Es besteht eine Lebhaftigkeit der Körper und Ausdruck einer realistischen Bewegung, die sich besonders in der absichtlichen Haltung der Arme offenbart. Eine Durchsicht der bekannten Figuren hat gezeigt, dass mehr als drei Viertel davon ihre Arme dargestellt haben, die in siebzig Prozent der Beispiele anscheinend absichtliche Haltungen aufweisen. Die häufigste ist die Betonung des Unterleibes, oft verbunden mit einem gewichtigen Bauch; dies mag die übliche Weise gewesen sein, Schwangerschaft darzustellen, auch wenn der Bauch selbst flach ist.

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THE GALGENBERG FIGURINE FROM KREMS, AUSTRIA

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Abstract. The discovery of a female figurine near Krems, Austria, is reported. Carved from serpentine or schist and coming from a dated occupation deposit, this find is among the earliest sculptures known. The figure is briefly described, its early Upper Palaeolithic cultural context is considered and other representatives of this early tradition of sculpted art are discussed in order to establish their common characteristics. The new find provides further evidence that an 'advanced' art tradition existed in central Europe well before the Gravettian, and before the appearance of iconic art in western Europe.

Introduction

On 23 September 1988, two fragments of green stone (serpentine or schist, perhaps chlorite-schist) were located at the base of an 'Aurignacian' occupation layer near Krems, Austria. Bearing engraved marks, the pieces were clearly fashioned by human hand. A thorough search produced a further five large and many small fragments of the same, obviously extraneous stone on the following day. When fitted together, the large fragments formed a complete, 72 mm long and 7 mm thick, flattish figurine depicting a woman in an animated pose, weighing 10.8 g. Six charcoal samples from the same occupation horizon yielded radiocarbon dates of about 30 000 years BP, suggesting that the carving is perhaps 5000 years older than the 'Venus of Willendorf', found 80 years earlier, almost exactly to the day, and just 20 km away. The new find is among the oldest sculptures known in the world. In contrast to some of the other art finds from the early Upper Palaeolithic (such as the first of the Willendorf figures) this sculpture was recovered under immaculate conditions of stratigraphic documentation, by the excavation director herself, from apparently undisturbed deposits.

The 'Venus of the Galgenberg', as it has already come to be known, provides an important link in reconstructing the circumstances surrounding the beginnings of Upper Palaeolithic art. Perhaps more significantly, the artistic sophistication of the new Austrian find raises once again the subject of pre-Upper Palaeolithic art production. I shall briefly describe the Galgenberg figurine and its context, and then discuss its significance.

Geographical and Archaeological Setting

In spite of Austria's central location in Europe and the proximity of the important concentrations of Palaeolithic sites in neighbouring countries, the Palaeolithic period is only poorly represented in Austria. Much of the country is mountainous and would have been inhospitable during long periods of the Pleistocene (see Schmid 1963 for snow limits during cryocratic peaks). Nevertheless, interglacials provided optimal climatic and bioenvironmental conditions (Šegota 1967), and the almost complete lack of Lower and Middle Palaeolithic remains in Austria is conspicuous. It must be at least partly attributable to the destruction of occupation deposits by various agents (for instance inundation, no doubt often caused by solifluction, has destroyed many sites in valleys).

The Galgenberg is located 3.2 km due north of Krems, Lower Austria, a picturesque small town on the northern shore of the Danube (Fig. 1). The hill offers sweeping views over the surrounding country, rising to 374 m above sea level, i.e. about 180 m above the river. Immediately upstream of Krems the Danube has had to cut its way through the southern fringes of the metamorphic rock Bohemian massif, thus separating the Dunkelsteiner Wald from the Waldviertel. The 35 km long, narrow valley from Krems to Melk is the Wachau (Fig. 2), which forms a natural gateway that may well have influenced the movements of migratory herds in the Pleistocene. Several castles or ruins on the valley's steep hills suggest its strategic role in the more recent past, and include Dürnstein, where Richard the Lionheart was held to ransom in A.D. 1192-93. Geomorphologically the Wachau is characterised by remnants of loess deposits nestled among the steep slopes that are now covered by terraced vineyards. The loesses of the Wachau and of the surrounding part of Lower Austria have provided the geochronological basis of the European Würm glaciation (Brandtner 1950, 1954, 1956; Fink 1954,

1956, 1961; Zeuner 1954; Woldstedt 1956; Felgenhauer, Fink and De Vries 1959; H. Gross 1960a). This region also produced the only readily definable cluster of Palaeolithic sites found in Austria. They are usually attributed to just two lithic industries, the Aurignacian and the Gravettian (East Gravettian, Klima's 'Pavlovian'), but it has been argued that these few dozen sites have been lumped together primarily because they occur in Würmian loess and share a 'cold' fauna, often dominated by mammoth, horse and/or reindeer (e.g. Prüfer 1958).

Most of the Palaeolithic occupation evidence of the Krems region is traditionally thought to derive from Würm II loesses, some (e.g. the lower four levels of Willendorf II, or Senftenberg) frequently ascribed to the Göttweig interstadial. However, many aspects of the published reports and of the remaining evidence, in collections and in the field, prompt one's scepticism. The chronological placement of almost every Austrian Palaeolithic site remains controversial, and most typological designations are open to debate. The situation is not helped by the tendencies of local archaeologists on the one hand to emulate the western European sequence, and on the other to perpetuate Penck's error of confusing Göttweig deposits with those of the last interglacial (and hence Rissian with Y.L. I loesses; see Soergel 1919; Lais 1941; Brandtner 1950; Zeuner 1954). Pittioni's (1938, 1954) Palaeolithic chronology of Austria, which is erroneous in almost every detail (Zotz 1956; Prüfer 1958; Narr 1966: 451; Bednarik in prep.), continues to serve as a standard reference (e.g. Neugebauer and Simperl 1979), although the placement of the alpine cave stations (Olschewian) into the Eem (e.g. Ehrenberg 1958) has been refuted time and again (for example by Bayer 1928; J. C. Gross 1929; Soergel 1940; Zotz 1944: 21, 1951; H. Gross 1959; Schmid 1957: 54, 1963). Yet this outdated model continues to be followed with the 'sectarian zealotry' H. Gross (1960b: 379) has already noted.

It is also evident that most of the Lower Austrian loess sites have been incorrectly identified (Pittioni 1954: 94). For instance some of the Wachau sites earlier thought to be of the Würm II cannot possibly predate the Paudorf oscillation, while at least one (Senftenberg) is Göttweig, if not final Würm I (Felgenhauer et al. 1959). Aggsbach cannot be from a Y.L. II, at 22 450±100 or 25 600±100 (depending on whether we accept Schmid's [1963] or Movius's [1960] date of the same sample, GRO 1327). The Paudorf *Bodenbildung* most certainly underlies the Gravettian of Stillfried (cf. Franz 1925; and Bayer's 1927 reprimand), for which we have a series of radiocarbon dates centring on ±26 000 to 27 000 years BP (Felgenhauer 1980). It is clear that some or many of the open air loess sites belong to Paudorf or Y.L. III (cf. Bayer's 1927 and 1928 comments; and Felgenhauer 1951). Prüfer (1958) noted that a younger, overlying loess is often lacking at the Lower Austrian sites, and it has even been suggested that some of these loess sites are Magdalenian (e.g. Gobelsburg, by Obermaier 1908).

Magdalenoid industries do occur in a few cave sites of the region, notably the Gudenus Cave (Hacker 1884) and others near the confluence of

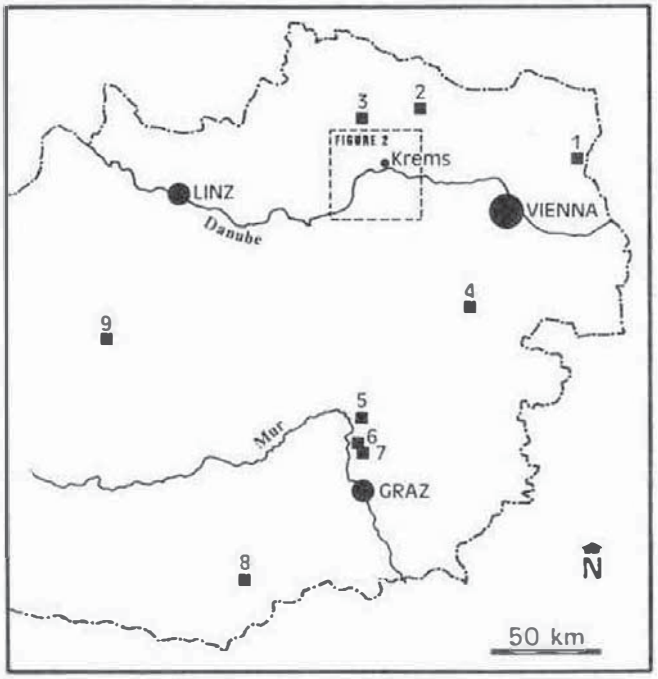

Figure 1.
Map of eastern Austria, showing locations of Palaeolithic sites mentioned in the text. They are:

1 - Stillfried
2 - Teufelslucken
3 - Frauenlucken
4 - Promenadensteig Cave
5 - Drachen Cave
6 - Repolust Cave
7 - Badl Cave
8 - Griffener Cave
9 - Salzofen Cave

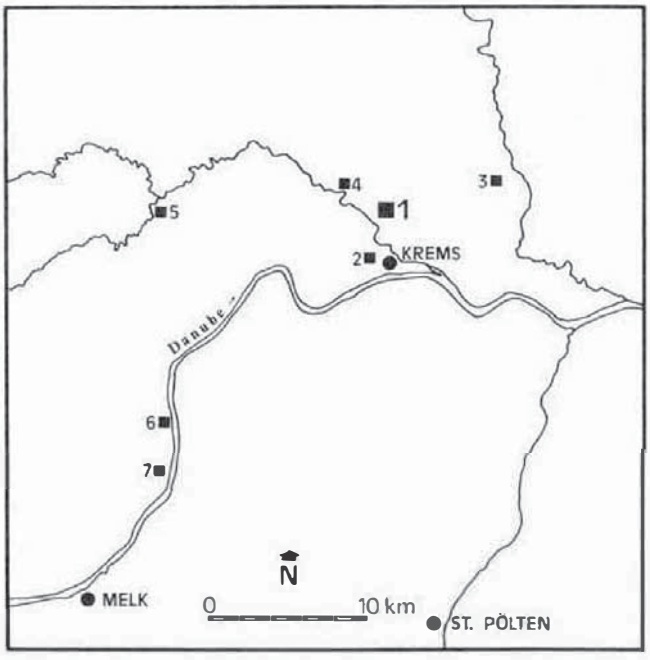

Figure 2.
Map of Wachau and Krems region, Lower Austria. Palaeolithic sites mentioned in the text are:

1 - GALGENBERG
2 - Krems-Hundsteig
3 - Gobelsburg
4 - Senftenberg
5 - Gudenus Cave
6 - Willendorf
7 - Aggsbach

the Grosse and Kleine Krems rivers; the Frauenlucken (Wichmann and Bayer 1924); and 80 km to the south-east, in the Promenadensteig Cave (Bednarik 1970). At the other end of the chronological spectrum, the Gudenus Cave—within two hours' walk of the Wachau valley—also contains Austria's only indisputable pre-Upper Palaeolithic strata. This small cave in the Kleine Krems valley yielded Mousterian levels (Breuil and Obermaier 1908; Bayer 1924), and well below them an acheuloid handaxe assemblage (Bednarik in prep.).

Only one of the loess stations near Krems has been generally accepted as typologically comparable to a stage of the French sequence: there is an Aurignacian level at Krems-Hundsteig, but the site was destroyed before it could be studied (Pittioni 1954: 67) and may have comprised more than one occupation phase (perhaps this is why Pittioni describes the site as Aurignacian, but lists it in his table [p. 121] as Gravettian!?). During the Göttweig, the Upper Palaeolithic industries of the region from southern Germany to Hungary differed significantly from the typologically better-understood sequence west of the Rhine and Alps. It may therefore be sensible to clarify the 'cultural' affiliations that the loess sites may have with the roughly contemporary or slightly earlier central European cave site industries of the Szeletian (Kadic 1916) and Olschewian (Bayer 1929), and those of Swabia, southern Germany.

The first Palaeolithic finds reported from the Galgenberg at Krems were made in the spring and summer of 1941. Emil Weinfurter collected remains of Pleistocene mammals, including mammoth and reindeer, recorded ample charcoal in the Würmian loess, and collected numerous silica flakes, mostly *débitage*. Among the implement types he records a cone scraper and other forms associated with the early Upper Palaeolithic (Weinfurter 1950).

Since the construction of the Krems water supply facilities on the Galgenberg, i.e. since 1985, the Österreichisches Bundesdenkmalamt has been excavating in a nearby vineyard owned by a cloister, Kremsmünster. The project is directed by Dr Christine Neugebauer-Maresch. It resulted in the recovery of the female figurine just before the end of last year's field season.

The Galgenberg Figurine

In contrast to most of the Gravettian statuettes, which are sculpted in the round, the Galgenberg figurine is flat, and of fairly uniform thickness. This may have been influenced by the original shape of the stone it was fashioned from: the mineral has been alternatively described as serpentine and schist, both of which occur frequently in tabular or shaly form. Neither has been recorded before in Palaeolithic art, although it is to be noted that the 'Venus of Savignano' has been alternatively described as steatite (by Graziosi and Aloisi) and serpentine (by Antonielli and Millosevich) (Bahn, pers. comm.). Serpentine and steatite are petrographically and chemically similar.

Besides the pieces forming the figurine itself, a number of small fragments of the same green stone were found in close vicinity. They are not part of the sculpture, and may well represent parings or carving residue from manufacture. While it is of course possible that they originate from the production of another object, it seems more likely that the figurine was found in the location of its manufacture; perhaps it was discarded after it fractured during production.

The Galgenberg figurine bears several cut marks, especially dorsally, which need to be subjected to detailed 'internal analysis' (Marshack 1972, 1985; cf. D'Errico 1988). Carbonate encrustation, tool and erosion marks cover its surface, producing a mottled effect. At least some of the carbonate precipitate has been removed by the researchers; it could have been subjected to radiocarbon, uranium-series and/or oxygen isotope analysis, thus providing valuable data for other projects. Technologically the production of the sculpture is significantly more advanced than that of any Gravettian (and thus more recent) figurine. The soft stone can be readily fashioned with flint tools but in this case the object is rather brittle and delicate. While the limestone of the larger and considerably more robust Willendorf I figurine (Szombathy 1910) may have been a demanding medium (Eppel 1950), a greater technological capability is manifested in the Galgenberg figurine. The several salient parts (left arm and breast, head) could all easily fracture at their base, and to carve or bore the two openings (between torso and right arm, and between legs) involved a very delicate production process.

The stone's physical properties would not permit the fashioning of a free-standing limb, especially an arm. To overcome this limitation the artist utilised two different conventions still being used by contemporary sculptors: the right arm and the legs are structurally supported (and thus braced) at both ends, while the left arm is shortened to half the anatomical length by being depicted in a folded-back position. This alone shows that the artist was well versed in the techniques of producing human figures with 'free' limbs, an art that was apparently not mastered by the Gravettian artists. Such advanced skills demand an accumulated store of artisan's know-how and cannot be explained as anything but the product of a lengthy tradition in which people had experimented for thousands, and probably tens of thousands, of years (perhaps with perishable media?).

The extraordinary skill of the Galgenberg artist is also shown by his or her ability to maintain a definite and vivid visualisation of the intended form throughout manufacture, despite the various technological challenges involved in producing the figurine. This is evident from the internally coherent attitude of the figure: the posture of all body parts is correctly balanced with the whole. The body's weight is depicted as being supported mostly on the left leg; the right leg is angled and resting on a slightly higher support than the left. This facilitates the casual placement of the right hand on the upper thigh. The upper torso is therefore turned to the left, a position also demanded by the steeply raised left arm. This attitude brings the left breast almost into profile, showing it to be large and consistent with that of a young woman. The second breast is in low relief, due to the stone's flatness. Facial detail is lacking, and while the wide upper

part of the head appears to be so shaped intentionally, we cannot know whether it represents a coiffure or is merely incidental. The vulva is depicted naturalistically, and the figurine lacks any suggestion of obesity, steatopygia or emphasis of female characteristics. With the exception of the limbs, which are rendered only as thin as the artist dared to, the figure is of anatomically correct dimensions and features an apparently young woman standing on a pedestal-like support.

The torso, head and left arm survived in a single piece, while the two legs and the support were found in three separate sections. The right arm had suffered the most damage; it was recovered broken in three. The various fragments come from the same occupation horizon as a series of white-patinated lithic implements, including burins and broad scrapers. Silica nodules and cobbles occur in the highest part of the Galgenberg, which consists of Tertiary gravels that may have provided some of the raw material. The pelvic bone of a woolly rhinoceros (*Coelodonta antiquitatis*), a typical Würm species (Kurtén 1968: 144), was also recovered. Six radiocarbon samples from the occupation stratum have provided dates clustered around a mean of slightly more than 30 000 years BP. This places the site at the onset of the second Würm stadial. Charcoal sample GRO 16135 was collected from the immediate vicinity and the same layer as the figurine's fragments, and produced a date of 31 790±280 years BP.

Discussion

The full significance of the Galgenberg find does not, however, rest upon the figurine itself, but on how it fits into what is known about very early art, and how it can affect our concepts about cultural evolution around the beginning of the Upper Palaeolithic and during the preceding period. Rather than being another 'Venus figurine', this find adds considerable weight to the hypothesis that the sculpted art preceding the Gravettian figurines, which are characterised by varying degrees of stylisation, was one of sophisticated realism. What the few 'Aurignacian' sculptures of central Europe have in common seems adequate to differentiate between them and the more recent Gravettian figures, which occur from western Europe to the U.S.S.R. The latter, while maintaining much of the earlier naturalism, nevertheless are typically static and stylised, although such treatment takes various forms.

Perhaps the best-known series of Upper Palaeolithic sculptures is that of the so-called 'Venuses': made of ivory, bone, steatite, burnt clay or limestone, they may be characteristically corpulent (e.g. Willendorf I, Gagarino, Balzi Rossi-Grimaldi; cf. bas-reliefs of Laussel, probably late Gravettian) or somewhat steatopygous (e.g. Savignano, Dolní Věstonice, Sireuil, Tursac, Lespugue), or they may be slender (Mal'ta, Buret, Laugerie-Haute, two of the Brassempouy figures). Some of the statuettes included in this series are only vaguely anthropomorphous (e.g. Willendorf II, which offers little more detail than the Vogelherd anthropomorph), and to be consistent with the tendency to apply the term 'Venus' we would have to name any

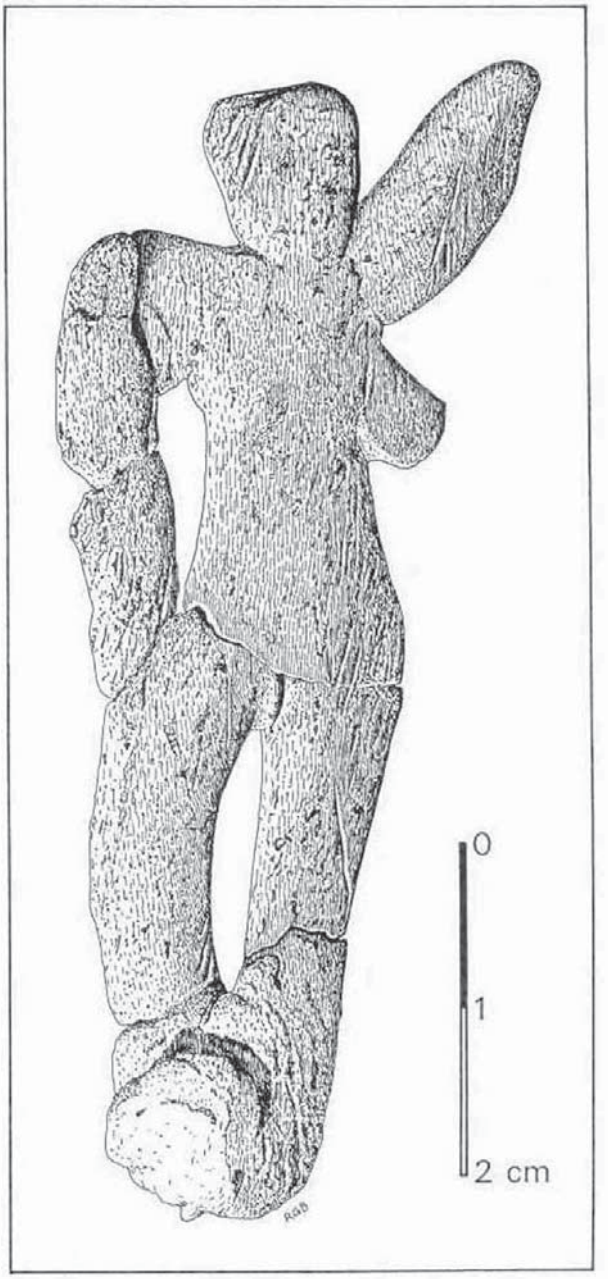

Figure 3.
The Galgenberg figurine from Krems, Austria.

apparently female sculpture so, irrespective of its provenance within the world.

The more 'typical' 'Venus' statuettes appear in the Gravettian (late Gravettian or Gravetto-Solutrean in western Europe), and the tradition seems to continue in the figurines of some Magdalenian sites (e.g. Pekarna Cave, Petersfels) and the later Upper Palaeolithic sites of the U.S.S.R. The Galgenberg-Krems figurine has also been designated a 'Venus', while clearly predating the series by a considerable margin and sharing none of its stylistic traits. Boring techniques were not employed in the manufacture of 'Venus' sculptures, and their compact overall form may well account for some of their stylistic aspects: the usually stunted arms (in about a quarter of all specimens these are altogether missing; Duhard 1989) may have less to do with stylistic conventions and more with technolo-

gical aspects or conventions of production. This is suggested by several of the bas-relief figures on rock which are shown holding their arms in very 'natural' poses, clearly extending away from the body: the four Laussel specimens (Lalanne 1912; Leroi-Gourhan 1971; Duhard 1988; Huyge 1988) and the two 'reclining Venuses' of La Magdelaine (Breuil 1954; Duhard 1989: Figs 7 and 8).

Another fundamental difference between the Krems find and the 'Venus' statuettes is that the latter are generally symmetrical, the exceptions being minor variations, such as the position of an arm or hand. The Galgenberg image is totally asymmetrical; not one body part is in a bilaterally corresponding position relative to the mediansagittal plane—which is itself slightly distorted because the upper torso is turned to the left. Such a proficient method of sculpting reappears in the late Magdalenian, but even then it does not quite match the simple harmony exemplified in this tiny figurine—which is, after all, at least twice as old.

However, the most fundamental difference between the Galgenberg discovery and 'Venus' figurines is that the latter are without exception static figures, lacking even the slightest hint of motion. Their inertia provides a stark contrast to the animation and vivacity of the Galgenberg specimen.

The 'Aurignacian' figurines from the Vogelherd Cave (near Stetten, Swabian Alb, south-western Germany) come from Layers 4 and 5 of that site (Riek 1934), the lithics of which differ from those of the French Aurignacian (de Sonneville-Bordes 1965). The lack of other sculptures from the Göttweig encouraged Müller-Beck (1957a, b, 1965) to question Riek's dating and to attribute the site to the Stillfried (Paudorf). The figures are no longer unique, however: the therianthropic figure from the Stadel im Hohlen Stein, north of Ulm (Hahn 1971; Marshack in press) is also 'Aurignacian'. It is of mammoth ivory, 281 mm long, and depicts a naturalistically proportioned human with a lion's head. This is one of the most sophisticated images known from any Palaeolithic period, in expressiveness, level of craftsmanship and the cosmological constructs it implies; yet it is also one of the oldest Palaeolithic images known. There are five parallel notches on its upper left arm, and Marshack (in press) has noted that a similar pattern occurs on the tiny anthropomorphous relief carving from the Geissenklösterle near Blaubeuren, 20 km west of Ulm. This roughly rectangular ivory plaque measures only 38 mm. One side bears a crudely fashioned, but realistically proportioned, human figure with raised arms, while the other has been decorated with over 50 marks arranged in four distinct rows. Twelve more notches have been cut into one longitudinal side of this object, which has also been attributed to the Aurignacian.

Vogelherd, Hohlenstein-Stadel and Geissenklösterle are all located in the Swabian Alb, within a hundred kilometres of each other, and they appear to be roughly contemporary. The sculptures are associated with lithic assemblages of a very early Upper Palaeolithic typology looking like early Aurignacian, and they share certain traits with the Galgenberg find. The subjects are depicted in distinctive and highly expressive attitudes. For instance the Hohlenstein therianthrope, while simply standing upright, seems to have something menacing about its stance; perhaps it is the slightly forward-flexed torso, the posture of the lion head, or the backward-bent, anticipatory attitude of the arm. The latter is modelled separated from the body and may well represent the foreleg of a lion rather than an arm—the ambiguity seems almost intentional. However, legs, feet (which are almost never detailed in Gravettian figures) and torso, especially the shoulders, are unmistakably human. The legs are fully separated—another feature that is not found in Gravettian or other 'Venus' figurines. Conversely, the posture of the head, the location of the lion's ears in relation to the back of the head, and the lack of space between the head and the lion's face all indicate that the figure does not depict a human with an animal mask, but a true therianthrope.

The expressive potency of this figure's pose is reflected in some of the finds from the Vogelherd Cave: the horse figure, again very tiny (Marshack 1976, 1985), is particularly sophisticated, as Marshack observes. It communicates much more than mere form; it captures elegant movement and liveliness, especially the graceful way a horse moves its head. The Vogelherd feline (Marshack 1985, in press) 'was carved with the same skill and sophistication as the horse, catching the characteristic species posture of watchful attention, with the head low and far forward' (Marshack 1985: 96).

With the Krems-Galgenberg statuette we can add another anthropomorphous image to this series, sharing its characteristics. This art object belongs to a highly evolved tradition of producing such works, a tradition contemporary with the early Aurignacian of western Europe, such as the Aurignacian I of La Quina (30 760±490 and 31 170±350 BP; Movius 1960), which is apparently devoid of iconic art. The earliest figurative motifs there appear in the next millennia in the form of a very few incomplete and very crude animal figures, together with motifs considered to depict vulvae (Delluc and Delluc 1978; but cf. Bahn 1986 on the 'vulvae'): at La Ferrassie, Abri Cellier, Abri Blanchard and Abri du Renne. (It is to be noted, however, that Breuil, who visited Piette's dig in 1897, maintained that the Brassempouy figurines came from the early Aurignacian, perhaps even from the Châtelperronian; cf. Bahn and Vertut 1988.) These rudimentary beginnings were preceded by a sophisticated central European tradition from which, also, only a few objects have so far come to light. It follows that the long-standing status of western Europe as the 'cradle of art' can no longer be upheld—especially in view of recent evidence from several countries (Bednarik 1988). As I noted above, the early central European figurines can only be explained as the product of a long antecedent art tradition. Cognitive or art-historical speculation has, I must emphasise, no bearing on this postulate; I refer to purely technological aspects. We have no reason whatsoever to assume that nonutilitarian technologies could have evolved faster than utilitarian ones did during the Middle Palaeolithic. In their speculations about art origins, cultural archaeologists need to distinguish between the 'cogni-

tive' aspects of early 'art evolution', and the technological ones: while one may reasonably argue for a comparatively sudden development, a kind of quantum jump, in the former, that would be unacceptable for the latter.

Conclusions

It was probably the animated pose of the Galgenberg figurine that prompted the Austrian archaeologists to name their find 'Fanny, the dancing Venus of the Galgenberg', under which name it has been introduced to the local mass media (Melchart 1988; the name Fanny derives from an Austrian ballerina of the 19th century, Fanny Elssler). To interpret the figure's attitude as 'dancing' is obviously subjective and lacks any supporting evidence. No human depictions of the Upper Palaeolithic can be shown to be dancing, yet apparently dancing anthropomorphs (depicted in frequently repeated, distinctively dynamic postures) have been reported from all continents. Moreover, contemporary Western observers tend to interpret the attitudes of prehistoric (and ethnographic) human figures quite subjectively, as 'praying', 'adoring', 'flying', 'worshipping' etc., when in fact we usually lack any knowledge of the cultural or semantic content of the relevant iconographies, or of the artistic or communicative processes governing their depictive processes. By applying contemporary standards, gestural intent or body language we could—more convincingly, I dare say—attribute to the Galgenberg figure a deliberate and provocative pose (Kurtén 1986). Certainly, if a well-endowed young female were depicted in an identical pose in contemporary imagery, some of the semantic connotations could be said to be unequivocal, but it does not follow that a similar interpretation can be postulated for a carving that is apparently thirty millennia old (cf. Bahn 1986).

The elated Austrian archaeologists have also claimed that their Galgenberg sculpture is the oldest female figurine in the world. This, too, needs to be qualified, and placed in a global perspective. According to Goren-Inbar (1986), the scoria pebble from Berekhat Ram bears artificial grooves around the neck and arms (Goren-Inbar intends to submit a detailed description of this and a second object to RAR). It comes from a levalloid Acheulian horizon that was sealed under a basalt flow about 233 000 years ago. I have myself (Bednarik 1988) called for independent authentication of the claimed modification traces on this figurine (they have also been questioned by Davidson, at the First AURA Congress in Darwin), but on reflection this is not the crucial aspect of the Israeli find. The object does have the shape of a woman, and whether this is its natural form or has been emphasised by modification is not the central issue from the cognitive epistemologist's point of view—for whom the change from proto-sculpture (Gallus 1977) to sculpture is far from fundamental. For him the capacity to recognise iconicity (Davis 1986) is more important than the ability to emphasise, and eventually create, iconicity. The mere stratigraphical provenance of the Berekhat Ram object suggests that those responsible for its deposition were aware of its iconic properties (assuming that the pebble is indeed extraneous), and those questioning its relevance will need to address this aspect rather than the subject of artificiality.

What are the circumstances that precipitated the profound cultural and technological developments in central Europe during the Göttweig interstadial, and where are the precursors of the figurines from the Swabian Alb and the Kremser Galgenberg? It seems that a most sophisticated tradition of producing sculpted art was well established as the interstadial drew to a close. If the Willendorf II radiocarbon dates (De Vries 1958) were reliable, the lowest of the nine occupation levels might be roughly contemporaneous with the Galgenberg horizon. They are not, judging by the inversions (the Willendorf samples were collected in 1908). Level 4 (Felgenhauer 1959) is thought to be of Middle Aurignacian age (Kromer 1950: 76) and is followed by five Gravettian layers (the 'Venus' is reputed to belong to the uppermost of them). Distinctive central European industries such as the Šipkian, Szeletian and Olschewian provide typological links between the mousteroid and the Upper Palaeolithic occupations, and occur from the early Würm glacial through to the end of Göttweig. Some of these sites have been described as Proto-Aurignacian, some as Proto-Solutrean; they provide ample proof that there is no clear typological division between the Middle and Upper Palaeolithic in central Europe. Rather, mousteroid stone toolmaking techniques survive (e.g. in Salzofen, Repolust and Griffener Caves, Teufelslucken) in these early blade and bone artefact industries (cf. Drachen, Badl, Potočka, Špehovka, Lovke, Mladeč and Istállóskö Caves). The Olschewian is often compared to the Aurignacian II, and is of about the same antiquity as Galgenberg (e.g. at Istállóskö Cave), or some millennia earlier. Perhaps it is among these 'cultures' that we should expect to find the contemporaries or predecessors of the Galgenberg artist.

One final comment: in rock art we often distinguish between dynamic and static art. The former tends to be regarded as artistically the more sophisticated, yet oddly it is often seen among the earliest forms of a regional sequence, as shown by superimposition patterns (e.g. the 'Dynamic Figures' of Arnhem Land, Australia - Chaloupka 1984; the green dynamic paintings of Mirzapur, India - Wakankar 1983). If we were to classify Upper Palaeolithic sculptures by the same criterion, those of the Gravettian would be static, those of the preceding central European period described here, dynamic. While this does not indicate a universal trend, it does confirm that art development does not conform to the biological concept of evolution: it evolves, it develops, but not necessarily in a direction of what we might view as increasing sophistication. Art works communicate world views and it would be self-contradictory to pronounce any art as more developed than any other (leaving aside purely technological aspects) because all world views are anthropocentric by definition and cannot be expected to correctly define reality (Bednarik 1985). Therefore it would be more correct to say that contemporary art works, for instance, are an aggravation of anthropocentricity, than to say that

they are more developed than the art of the Neanderthals.

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Résumé. Cet article conceme la découverte d'une figurine féminine près de Krems en Autriche. Cet objet, taillé en serpentine ou schiste, provient d'une couche d'habitation datée, et est parmi les plus vieilles sculptures connues. L'auteur présente une brève description de la figure, considère son contexte culturel dans la partie ancienne du Paléolithique Supérieur, et discute d'autres objets qui représentent cette ancienne tradition d'art sculpté afin d'établir leurs communs caractères. Cette nouvelle trouvaille fournit une preuve supplémentaire qu'une tradition artistique 'avancée' a existé en Europe centrale bien avant le Gravettien, et avant la parution d'un art iconique en Europe occidentale.

Zusammenfassung. Die Entdeckung einer weiblichen Statuette nahe Krems, Österreich, wird berichtet. Dieser aus Serpentin oder Schiefer hergestellte und aus einer datierten Siedlungsablagerung stammende Fund ist eine der ältesten Plastiken der Welt. Der Verfasser legt eine kurze Beschreibung der Figur vor, erörtert ihre früh-jungpaläolithischen kulturellen Zusammenhänge, und bespricht andere Vertreter dieser frühen Erscheinungsform plastischer Kunst, um ihre gemeinsamen Merkmale festzulegen. Der neue Fund bringt weitere Beweise, dass eine fortgeschrittene Kunstform in Mitteleuropa bereits lange vor dem Gravettien existierte, und vor dem Erscheinen bildlicher Kunst in Westeuropa.

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KEYWORDS: Rock art protection - Funding program - Project analyses - Australia

THE AUSTRALIAN INSTITUTE OF ABORIGINAL STUDIES' ROCK ART PROTECTION PROGRAM: DISCUSSION OF THE FIRST TWO YEARS' PROJECTS

GRAEME K. WARD

Abstract. This paper complements the initial presentation in *Rock Art Research* by Ward and Sullivan (Vol. 6, No. 1, 1989), in which were given the background to the development of the conservation program and details of the initial projects funded. It provides an outline of the results of the first year's funding, lists the successful applications of the second year's funding, discusses the results of both years' projects and provides a brief analysis of the differences between the two years' proposals, both those successful and unsuccessful.

INTRODUCTION

A discussion of the background to and development of the Institute's Rock Art Protection Program was provided by Ward and Sullivan in the previous issue of *Rock Art Research* (6: 54-62). In the initial year (Financial Year 87) the Australian Institute of Aboriginal Studies (AIAS) received thirty applications from various sources, the total value of which was approximately $480 000. A meeting at the Institute early in December 1986 decided funding for a total of twelve projects to expend the $150 000 available. There was one grant with a national scope funded at $26.2k and eleven regional projects whose average value was $11.7k.

The Minister for Aboriginal Affairs confirmed, late in September 1987, that a further year's funding for the Rock Art Protection Program was to be provided at the same level as the first year. The Institute called for proposals, and its advertisement of the project again stressed that the program had three main aims: (1) the physical preservation of endangered sites, including those threatened both by the natural elements or animals and by interference by humans; (2) the detailed recording of sites, especially those which could not be preserved; (3) research into the Aboriginal cultural significance of sites.

Thirty-one applications were received, their total value was again far in excess of $400 000. A meeting to decide disbursement of the RAPP funds was held at the Institute in Canberra during October 1987; it comprised the following Council Members, representatives of organisations and others: Dr A. Chase and Ms S. Sullivan (AIAS Council); Mr R. G. Bednarik (AURA); Dr J. Flood (AHC); three NASAC representatives: Mr R. W. Ellis (NT Aboriginal Sacred Sites Protection Authority), Ms C. Gartside (NSW NPWS) and Mr M. McIntyre (VAS); Professor D. J. Mulvaney (Joint Academies); Mr M. Robinson (WA Museum - invited State representative); Mr G. Walsh (Qld NPWS); Dr K. Palmer and Dr G. Ward (AIAS staff).

MS received 12 March 1989

APPLICATIONS AND CONSERVATION PRIORITIES

The 31 applications were received for funding of conservation projects from a variety of sources throughout Australia; further proposals advanced at the meeting as protection priorities brought the total to 34 project proposals. Two projects were considered to be of national scope. The total value of all proposals was in excess of $450k. FY88 (along with FY87) proposals are characterised in Appendices A and B.

Sources of Applications

The range of sources could be divided into two major categories, those from individual researchers, usually based at tertiary institutes, and those from institutions, departmental, statutory or Aboriginal, with some form of responsibility, statutory or assumed, for the protection of cultural heritage.

Of the first category, two applications derived from a College of Advanced Education, four from

Table 1.
Summary of individual and institutional sources of FY88 applications (n = 31).

Individuals			Institutions		
CAE	Unvrsty	Unattached	StateAuth.	OtherState	LandCouncil
2	4	2	13	7	3
8			23		

staff and students of three different Australian universities, and the remaining two from unattached individuals, one of whom proposed to be based at a university department should the application be successful. Of the second category, thirteen applications derived from four state and territory agencies directly responsible for the management of sites, three were submitted by other governmental agencies concerned with land management, another four applications were made by a state museum, and the remaining three were submitted by an Aboriginal land council (Table 1). Enquiries were made by other Aboriginal agencies but these did not result in applications being received at AIAS.

Applications were received from sources in all states and territories and for work in all states and territories (sometimes for more than one geographical area); these data are summarised in Table 2 (below). As the table shows, sources in the Northern Territory and New South Wales predominated.

The foci of the various proposals were evenly divided between locations in the north of Australia, where a large number of sites is known to exist, and the south of the country; a large proportion of the latter (nearly one third) were for work in New South Wales.

Proposal Criteria

The 31 applications could be characterised in terms of the reference each made to the three main criteria for protection listed above. As Table 3 shows, they were roughly evenly divided between the first two criteria, with only one application focusing upon the Aboriginal significance of its subject matter.

SUCCESSFUL APPLICANTS

During the meeting, six preservation and management and/or survey and documentation projects were withdrawn from consideration by representatives of the applicants. None of the further three proposals referred to the meeting as conservation priorities from other AIAS sources or promoted during the meeting as being matters of urgency were funded. In the case of two institutions seeking funding for a total of five projects all applications were rejected and two 'seeding grants' were offered for projects to be redefined in consultation with a steering committee formed from among those at the meeting.

The three main aims of the Rock Art Protection Program, (1) the physical preservation and management of endangered sites including those threatened by the natural elements, animals or interference from humans; (2) survey and documentation of new and major sites; (3) research into the Aboriginal cultural significance of sites, were taken into account in assessing grant applications. A total of twelve projects were funded for a little less than $150k (some money being retained for the purpose of publicising the program); all administrative costs were covered by AIAS. In some cases the amount of funding requested had been cut to allow the total available to be further spread. The successful applications can be summarised as follows:

(1) Canberra College of Advanced Education (Dr C. Pearson, Cultural Heritage Science Division); Graduate Diploma in the Conservation of Rock Art (Planning Meeting) (National; Criteria met: 1, 2, 3); amount of grant offered: $4882.
(2) Queensland National Parks and Wildlife Service (Mr E. Power, Central Region); Kenniff Cave Site Protection (Qld; 1); $14 921.
(3) Northern Territory Museum of Arts and Sciences (Mr G. Chaloupka/Mr B. Harney); Protection and Conservation of Wardaman Rock Art Sites (NT; 1); $10 000 plus c. $4000 carried over from previous year.

Table 2.
Summary of geographical sources and foci of FY88 applications.

	WA	NT	SA	Qld	NSW	ACT	Vic	Tas	A*	Totals
Source	4	6	1	2	11	4	1	2	n.a.	31
Focus	5	7	1	3	10	1	1	2	2*	32
* Australia-wide or three or more States/Territories										

Table 3.
Summary by protection criteria of FY88 applications.

Criterion	#	%
1 Physical preservation & management	31	52
2 Survey and documentation	28	47
3 Aboriginal significance	1	2
Totals	60	101

Table 4.
Summary by protection criteria of successful FY88 applications.

Criterion	Project												
	1	2	3	4	5	6	7	8	9	10	11	12	Ttl
1 Physical presrvtn & mngmnt	x	x	x	x	x	x	x	x	x	x	x	x	12
2 Survey & documentation	x	-	-	x	x	x	x	-	x	-	-	x	7
3 Aboriginal significance	x	-	-	-	-	x	x	-	-	-	-	-	3

(4) New South Wales National Parks and Wildlife Service (Ms B. Conyers, Central Region); Red Hands Cave, Blue Mountains National Park - Interpretation (NSW; 1, 2); $5800.

(5) New South Wales National Parks and Wildlife Service (Ms B. Conyers, Central Region); Garigal Site Walk, West Head, Ku-ring-gai Chase National Park - Interpretation (NSW; 1, 2); $13 000.

(6) Northern Land Council (Dr F. McKeown/Mr M. Pickering); Management Project (NT; 1, 2, 3); $14 000.

(7) Conservation Council of the Northern Territory (Mr J. Fletcher/Mr H. Pierce); Management and Protection Survey, Gregory National Park (NT; 1, 2, 3); $14 000.

(8) Tasmania Department of Lands, Parks and Wildlife (Mr T. Blanks); Mt Cameron West Petroglyph Protection (Tas; 1); $12 500.

(9) Western Australian Museum, Department of Materials Conservation (Dr I. MacLeod/Mr P. Haydock); McKay Caves Complex, Mt Magnet - Environment Management and Conservation (WA; 1, 2); $11 150.

(10) Mr A. Watchman: Occurrence and Composition of Naturally Formed Silica Skins - their Effect in Preserving Rock Art; (Ntl; 1); $26 305.

(11) Western Australian Museum, Department of Materials Conservation (Dr I. MacLeod/Mr P. Haydock); Walga Rock, Cue - Environment Management and Conservation (WA; 1); $6750.

(12) Victoria Archaeological Survey (Ms R. Buchan); Grampians Rock Art - Interpretation (Vic; 1, 2); $15 000.

('Criteria met' refers to the three criteria detailed above.)

SUMMARY OF THE AIMS OF THE SECOND YEARS' APPLICATIONS

The two national projects had very different foci. Pearson's application was to support a meeting of experts from around Australia to decide upon a curriculum and organisation of a postgraduate degree course in rock art conservation being planned for the Canberra College of Advanced Education. That by Watchman sought funds for a year-long study of the formation and potential usefulness of silica 'skins' in physically protecting painted rock faces; the researcher proposed to collect samples of silica skins from a variety of rock types and climatic locations; to make a detailed geological analysis of the silica skins; to identify the factors affecting their development and stability; to make recommendations on their use and effectiveness; and to replicate naturally occurring silica skins by applying siliceous solutions to rock faces. It was anticipated that a major factor in the preservation of paintings would be better understood and perhaps able to be used artificially in their conservation.

The Queensland National Parks and Wildlife Service proposed to construct a boardwalk within Kenniff Cave, providing an elevated viewing platform and protecting the archaeological deposits there from disturbance by trampling and the painted imagery from being further obscured by dust. This application followed the success of those visitor control measures built in recent years at other rock shelter sites within Queensland national parks which have been subject to visitation pressures.

Other purely protective projects were that for which funds were sought by the Northern Territory Museum for Mr Bill Harney, a fencing contractor and member of the Wardaman community, who wanted to fence traditional sites in the Wardaman area; the continuation of the dune stabilisation project at Mt Cameron West engraving site being carried out by the Tasmanian Department of Lands, Parks and Wildlife; and the project designed by the Department of Materials Conservation, Western Australian Museum, to manage the environment of a site at Walga Rock. The Western Australian project deserves further note:

At Walga Rock, near Cue, the environs of the site had changed from being vegetated to largely barren since the time of European arrival in the area and this, as pointed out by Haydock and Rodda in an earlier study of the site, had adversely affected the preservation of the paintings there by leaving them open to the effects of dust, driving rain and microclimatic changes. The project proposed, in consultation with the Murchison-Gascoyne Aboriginal community and botanists, to transplant appropriate seedlings into the environs of the shelter with the objectives of physically protecting the decorated surfaces and re-establishing a favourable microenvironment.

Most of the remaining projects all had some combination of physical protection and interpretative aspects. Both New South Wales National Parks and Wildlife Service projects funded, those at Red Hands Cave (Blue Mountains National Park) and at Garigal (West Head, Ku-ring-gai Chase National Park) near Sydney, were designed to provide information to the public and/or to control public access to areas of rock art with the intention of protecting the art surfaces.

The Red Hands Cave is the only painted site open to the public in the eastern Blue Mountains; an old protective metal grid was being replaced

and the Service proposed to provide information in the form of signage and brochures to visitors to the site with the aims of promoting public awareness of Aboriginal culture, and of the significance of such sites to Aborigines, of the vulnerability and protection requirements of painting and petroglyph sites, and of promoting public awareness of the role of the Service in site management.

West Head, in the Ku-ring-gai Chase National Park near Sydney, contains many painted shelters and engraved sandstone areas. The New South Wales National Parks and Wildlife Service sought to provide an educational resource for visitors including school students to the Park by developing a walk linking a stencil site with an engraving and a shelter containing an archaeological deposit, as well as carrying out conservation work at the painted site, upgrading paths and providing signs. The aims of the development were to provide information on Aboriginal culture, a demand recently intensified as a result of the introduction of Aboriginal studies in secondary schools; to present a more holistic picture of Aboriginal culture rather than one focusing on individual sites; to promote awareness of the range of sites in the region and their protection needs; and to encourage awareness of Aboriginal heritage, and of the contemporary importance of these sites to the Aboriginal community.

The McKay Caves complex near Mt Magnet on the highway to Cue was subject to adverse visitor impacts including some vandalism; access was uncontrolled. It was proposed by the Western Australian Museum's Department of Materials Conservation to develop a system of visitor management that would separate recreational use from viewing of the paintings, and that was non-intrusive upon the environmental and cultural context of the sites, thus encouraging their conservation without adversely affecting their presentation. Site use would be researched; areas revegetated as necessary; and information prepared for site-specific signage with the aim of raising visitor awareness of the environmental significance of the area and to provide information designed to foster cultural appreciation of the paintings there.

Similarly, the Grampians interpretation proposal developed by the Victoria Archaeological Survey emphasised the information needs of visitors to the sites with the aim of enhancing visitor understanding and hence, it was argued, site protection. The project built upon the visitor appreciation survey undertaken the previous year which had clarified the needs of tourists at the sites. It was proposed to develop a self-guided interpretation program for visitors to accessible sites in the Grampians; to improve the way in which the sites were promoted and interpreted to the public (a workshop would be conducted targeting local tourist officers and ranger staff); and to remove graffiti from sites.

The two remaining projects encompassed all three criteria. Proposals from the Bureau of the Northern Land Council sought funding for three similar projects, each designed to find and to record in detail sites in an area of Arnhem Land subject to development pressure, to analyse the archaeological significance of the paintings, and to produce recommendations for their management and physical protection. The grant offered to the BNLC was to obtain assistance in developing a conservation/management program for the areas concerned. As finally approved, it was used to fund a consultant to find, document, and assess the Aboriginal significance of a sample of sites; to use the data obtained to formulate models for site location and type to assist the BNLC in planning and conducting further surveys and in developing practical strategies for site conservation; to establish guidelines to assist the BNLC in assessing sites; to make recommendations for the protection of sites, taking into account the views of traditional custodians; and to carry out any necessary practical measures possible at the time.

The Conservation Commission of the Northern Territory requested funding for two projects to explore for painting sites in two national parks. It was offered a grant for a consultancy to develop a management plan and conservation strategies for the resources in areas under its control. Its project, as approved, dealt with a relatively poorly known area about to become one of the Northern Territory's major parks, the proposed Gregory National Park; the aims were to record, document, protect and develop for visitor access (taking into account Aboriginal priorities) a Wardaman site in the Victoria River district; to conduct a workshop on site recording and protection for Commission staff; to produce policy and procedures for protection of paintings and petroglyphs and their presentation to the public.

PROJECTS NOT FUNDED

It is again instructive to consider the types and scopes of projects which were unsuccessful (cf. Ward and Sullivan, **RAR** 6[1]). Nineteen of the 31 initial applications were not funded; as in the previous year, most were acceptable proposals in terms of their statement of aims and methods. Of those unsuccessful projects considered, four were graded B (acceptable but of lower priority than those graded A), and of those remaining thirteen which were graded C, one was rejected as being outside the guidelines, and five were decided to be of lowest priority in the current circumstances (and individual applicants were recommended to re-apply for other AIAS grants); five others were rejected so that 'seeding grants' could be provided to the two institutions applying; and another was rejected pending clarification of the application of previous funds at the site. Of those four graded B, one was given priority should other offers of funding be rejected; the others were acceptable but described as being of lesser priority (often compared with similar projects in the same region).

Taking a wider view, it was clear that the members shared a generally perceived appreciation that priority should continue to be given to projects, firstly, whose major aim was direct protection and, secondly, where interpretation work would contribute toward appreciation and thus protection. This view was further reflected in the

	FY87		FY88	
	#	$k	#	$k
NUMBER OF PROPOSALS REC'D	30	-	34	-
Approx. Total Value	-	480	-	450
Approx. Average Value	-	16.0	-	12.4
or National focus ($\bar{X}$)	2	23.1	4	17.5
& Regional focus ($\bar{X}$)	28	15.5	30	12.7
NUMBER OF PROJECTS FUNDED	12	-	12	-
Total Value	-	150.0	-	148.8
Approx. Average Value	-	12.5	-	12.4
or National focus ($\bar{X}$)	1	21.2	2	15.6
& Regional focus ($\bar{X}$)	11	11.7	10	11.8

Table 5.
Basic statistics of FY87 and FY88 proposals.

large proportion of the funding that went to a major project to evaluate the significance and potential value of a natural process claimed to physically seal many painted rock faces.

It is important to note here that, of those unfunded projects with a strong field research emphasis, that is, of survey and mapping of little-known areas, four were resubmitted to AIAS as recommended by the meeting, and three were offered funding from subsequent grant allocations during 1988.

Summary of FY88 Grants Funded

Received: 31 applications and three further proposals; combined value c. $450k.

Funded: Twelve projects, total value $148.8k; average grant $12.4k; or two national grants totalling $31.2k, and ten regional projects, average value c. $11.7k.

Recommendation

Further discussion before the close of the meeting elicited agreement that any future consideration of disbursement of grant funds in this program, consideration should be given to survey and recording projects as a priority for funding.

Further Funding

Following announcement of the FY89 Budget, the Minister for Aboriginal Affairs confirmed that a further year's funding was to be provided for the Institute's Rock Art Protection Program.

COMPARISON OF THE TWO YEARS' APPLICATIONS OF THE ROCK ART PROTECTION PROGRAM

Basic statistics of the two years' proposals are summarised in Table 5.

	FY87				FY88			
	Received		Funded		Received		Funded	
SOURCE	#	%	#	%	#	%	#	%
INDIVIDUAL								
CAE/University	6	20.0	1	8.3	7	20.6	2	16.6
Unattached	8	26.7	1	8.3	1	2.9	0	0.0
Totals	14	46.7	2	16.6	8	23.5	2	16.6
INSTITUTIONAL								
StateAuthority	12	40.0	8	66.7	17	50.0	7	58.3
OtherState	1	3.3	1	8.3	3	8.8	2	16.6
LandCouncil	1	3.3	0	0.0	3	8.8	1	8.3
OtherAblOrgnztn	2	6.7	1	8.3	0	0.0	0	0.0
Totals	16	53.3	10	83.3	23	67.7	10	83.3
OTHER								
(AIAS meeting)	0	0.0	0	0.0	3	8.8	0	0.0
GRAND TOTALS	30	100.0	12	99.9	34	100.0	12	99.9

Table 6.
Comparison of sources of FY87 and FY88 proposals.

	FY87				FY88			
	Received		Funded		Received		Funded	
GEOGRAPHICAL SOURCE	#	%	#	%	#	%	#	%
Western Australia	4	13.3	1	8.3	4	11.8	2	16.7
Northern Territory	3	10.0	3	25.0	7	20.6	3	25.0
South Australia	3	10.0	1	8.3	1	2.9	0	0.0
Queensland	5	16.7	1	8.3	2	5.9	1	8.3
New South Wales	7	23.3	2	16.7	10	29.4	2	16.7
Capital Territory	3	10.0	1	8.3	4	11.8	1	8.3
Victoria	4	13.3	2	16.7	1	2.9	1	8.3
Tasmania	1	3.3	1	8.3	2	5.9	1	8.3
Other	0	0.0	0	0.0	3	8.8	1	8.3
TOTALS	30	99.9	12	99.9	34	100.0	12	99.9

Table 7.
Comparison of geographical sources of FY87 and FY88 proposals.

Notable in these figures is the reduction, in the second year, across several categories, of the average costing of individual proposals. The average monetary value of proposals considered in FY88 was only three-quarters that of those applications received in FY87. The average value of the projects funded in each year (except for the national projects) was almost the same.

Comparison of Proposals by Source

It is useful to compare the sources of the various conservation proposals considered by AIAS during each of the two years of the program so far (Table 6). In the initial year, approximately equal numbers of applications were received from individuals, on one hand, and land and heritage management agencies on the other, whereas in the second year three times as many proposals derived from institutional sources. While the level of interest from individuals attached to tertiary institutes was about the same as in the previous year, a substantial reduction was recorded in applications from 'unattached individuals'. Notable is the comparable level of interest expressed by land councils and other Aboriginal organisations.

Applications continued to be received from across Australia albeit in diminished numbers from most states and territories (Table 7). The notable exceptions were the Northern Territory (seven applications) and New South Wales, from where ten applications were received (nine of these derived from the NSW NPWS).

Comparison of Proposals by Geographic Focus

In terms of the focus of the current proposals there was again a wide spread, with concentration on the northern Australian areas (as reflected in the large numbers for work in northern Western Australia, the Northern Territory and Queensland). There was also a significant increase in the number, resulting in a large proportion of all applications (10/35: nearly 30%), focused on New South Wales, probably reflecting the perceived visitor pressures on sites in the relatively densely settled areas along the south-eastern seabord (Table 8).

	FY87				FY88			
	Received		Funded		Received		Funded	
GEOGRAPHICAL FOCUS	#	%	#	%	#	%	#	%
Western Australia	3	10.0	1	8.3	6	17.1	2	16.7
Northern Territory	5	16.7	4	33.3	7	20.0	3	25.0
South Australia	4	13.3	2	16.7	1	2.9	0	0.0
Queensland	6	20.0	1	8.3	4	11.4	1	8.3
New South Wales	7	23.3	1	8.3	10	28.6	2	16.7
Capital Territory	1	3.3	0	0.0	0	0.0	0	0.0
Victoria	1	3.3	1	8.3	1	2.9	1	8.3
Tasmania	1	3.3	1	8.3	2	5.7	1	8.3
Australia-wide *	2	6.7	1	8.3	4	11.4	2	16.7
Totals	30	99.9	12	99.8	35	100.0	12	100.0
* Australia-wide or three or more States/Territories								

Table 8.
Comparison of geographical foci of FY87 and FY88 proposals.

1 PHYSICAL PRESERVATION & MANAGEMENT
1a Research & development of physical conservation techniques
1b Physical protection & management of threats (i) from natural sources, native and domestic animals: installation of drip-lines, vegetation removal, removal of nests, stock fencing; (ii) from human sources: installation of grids, barriers, provision of walkways, upgrading of paths, etc.
1c Assessment of threats to conservation and development of site management plans prior to implementation of protective management measures (*cf.* 2c)
1d Protection & management of site through provision of educational information in the form of signage, brochures, guided walks and the training of staff, development of public awareness (*cf.* 2d)
1e Monitoring of visitor impact
1f Retouching/repainting/'renovation' of images
2 SURVEY & DOCUMENTATION OF NEW & MAJOR SITES
2a Search for unrecorded sites in little-known areas
2b Detailed recording and mapping of known sites or site complexes, assessment of resources
2c Production of recommendations for management (*cf.* 1c)
2d Production of detailed documentation about sites and/or site contents (*cf.* 1d)
2e Analysis of motif or other data, relationship to environmental factors, or other research-oriented aspects, modelling of research results, etc.
3 RESEARCH INTO ABORIGINAL CULTURAL SIGNIFICANCE OF SITES

Table 9.
Rock Art Protection Program conservation criteria.

Comparison of Proposals by Subject Focus

With the increase in the variety of subject foci displayed by the second-year conservation proposals, it was found impractical to characterise proposals in terms of the four categories used in the description of the initial year's applications (Ward and Sullivan 1989: 57-8). Instead, subdivisions were made of the three criteria originally enunciated in the main aims of the Program as adopted by the Rock Art Working Group. So developed, the RAPP Conservation Criteria can be structured as presented in Table 9.

Using the criteria developed above, the proportions of FY88 proposals considered and projects funded, as divided into the various criteria subcategories of Table 9, can be compared with those of the previous year (Table 10).

Considering only the totals for each major criterion (protection/management, survey/documentation, Aboriginal significance), the pattern in the two sets of proposals appears to reflect remarkable similarity of each of the two years' proposals; the third category shows a significant decrease in number and proportion of all proposals for funding (from approximately 10 to 2%), and the first category shows an increase (from approximately 43 to 53%). If, however, one considers the subcategories within each major criterion, some interesting differences are apparent.

Firstly, there was more than double the number of proposals to develop protection/management (from visitation) of a site (Category 1b[ii]); similarly, there were more than double the number of proposals for protection through education of visitors (Category 1d). On the other hand, there were significant reductions in the number of protection assessment studies (1c) and visitor impact studies (1e) proposed. May one conclude that authorities/land managers are tending to decide that the basic work has been done and that the knowledge can now be applied? An echo of this is sounded in the drop in the number of 'research and development' (1a) applications received.

Generally, there appears to have been an increase in the proportion of multifaceted proposals.

Secondly, despite the few applications in the search (2a) category funded the previous year, there was an almost two-fold increase in proposals involving this focus; there were slight drops in the numbers of proposals which included the preparation of detailed documentation (2d) and development of analyses (2e).

Thirdly, only one application could be seen to have as a focus the investigation of Aboriginal cultural significance (Criterion 3); the drop is more profound if one considers that the proposal included here is one which involves teaching about the appreciation of and investigation of significance, rather than one involving field or analytical research.

	FY87				FY88			
	Received		Funded		Received		Funded	
CRITERION	#	%	#	%	#	%	#	%
1 PROTECTION MANAGEMENT					1	1.6	1	4.4
1a (R & D)	3	4.2	1	4.6	1	1.6	1	4.4
1b(i) (natural)	7	9.7	6	27.3	7	11.5	5	21.7
1b(ii) (visitation)	4	5.5	3	13.6	9	14.8	2	8.7
1c (assessment)	8	11.1	3	13.6	5	8.2	1	4.4
1d (education)	5	6.9	3	13.6	11	18.0	3	13.0
1e (visitor impact)	3	4.2	1	4.6	0	0.0	0	0.0
1f (retouch)	1	1.4	0	0.0	0	0.0	0	0.0
Totals	31	43.1	17	77.3	32	52.5	13	56.5
2 SURVEY / DOCUMENTATION					1	1.6	1	4.4
2a (search)	5	6.9	0	0.0	7	11.5	2	8.7
2b (mapping)	10	13.9	0	0.0	8	13.1	2	8.7
2c (recommendation)	8	11.1	1	4.6	5	8.2	2	8.7
2d (documentation)	5	6.9	1	4.6	3	4.9	0	0.0
2e (analysis)	6	8.3	1	4.6	4	6.6	0	0.0
Totals	34	47.2	3	13.6	28	45.9	7	30.4
3 ABORIGINAL SIGNIFICANCE								
Totals	7	9.7	2	9.1	1	1.6	3*	13.0
TOTALS	72	100.0	22	100.0	61	00.0	23	99.9
* This figure affected by re-definition of two projects								

Table 10.
Comparison of the first two years' proposals in terms of various criteria (all subject foci mentioned).

This analysis may be misleading, since all foci mentioned in an application have been included in Table 10. If only the major thrusts of each proposal are used as the basis for the analysis (Table 11, see p. 134), could different conclusions be possible?

As Table 11 shows, the overall patterns of the two years' proposals again are similar, with a slight trend toward overall de-emphasis of Criterion 1 proposals in favour of Criterion 2. Possibly significant differences are seen in (a) the reversal of proposals emphasising categories 1b(i) and 1b(ii) and, similarly, those emphasising categories 2a and 2b. However, without considerably more analysis—in order to place appropriate emphasis upon each of the stated aims of each proposal—it is not clear what allowance should be made in the current Table for artefacts arising from the mode of presentation.

Attention now can be focused upon those projects which were funded as presented in Tables 10 and 11.

The first significant difference between the results for the two years can be seen (Table 10) in the significantly greater proportion of funding of Criteria 2 projects in FY88 compared with FY87 (33 cf. 14%) and a commensurate reduction in Criteria 1 projects funded (62 cf. 77%). It is clear, however, that although there were still, in FY88, considerably more—nearly twice as many—Criteria 1 than Criteria 2 projects funded, the proportion (13:7) had diminished greatly compared with the previous years (17:3).

Looking more closely at Table 10, it appears that the reductions in Criteria 1 funded projects were fairly evenly spread across the various subcategories, but that the increases in Criteria 2 were concentrated in 2a (search), 2b (mapping) and 2c (recommendations), with reductions in 2d (documentation) and 2e (analysis).

Table 11, presenting the primary aims of the funded projects, emphasises these conclusions, showing that while Criterion 1 almost totally outweighed Criterion 2 (92 to 8%) in the first year, there was a more even distribution of funding (75 to 25%) in the second. Again, however, it must be stressed that the method of analysis is relatively superficial.

There was a slight increase in the proportion of projects designed to research or document the Aboriginal cultural significance of sites and site complexes (as indicated in Table 10) but, as Table 11 shows, this aim was a minor one in projects whose major focus was course development or the

	FY87				FY88			
	Received		Funded		Received		Funded	
CRITERION	#	%	#	%	#	%	#	%
1 PROTECTION MANAGEMENT					1	2.9	1	8.3
1a (R & D)	2	6.7	0	0.0	1	2.9	1	8.3
1b(i) (natural)	6	20.0	6	50.0	3	8.8	3	25.0
1b(ii) (visitation)	1	3.3	1	8.3	6	17.7	1	8.3
1c (assessment)	3	10.0	0	0.0	3	8.8	1	8.3
1d (education)	3	10.0	3	25.0	4	11.8	2	16.7
1e (visitor impact)	2	6.7	1	8.3	0	0.0	0	0.0
1f (retouch)	0	0.0	0	0.0	0	0.0	0	0.0
Totals	17	56.7	11	91.7	18	52.9	9	75.0
2 SURVEY DOCUMENTATION					1	2.9	1	8.3
2a (search)	4	13.3	0	0.0	7	20.6	0	0.0
2b (mapping)	5	16.7	0	0.0	3	8.8	0	0.0
2c (recommendation)	0	0.0	0	0.0	1	2.9	2*	16.7
2d (documentation)	2	6.7	1	8.3	2	5.9	0	0.0
2e (analysis)	1	3.3	0	0.0	1	2.9	0	0.0
Totals	12	40.0	1	8.3	15	44.1	3	25.0
3 ABORIGINAL SIGNIFICANCE								
Totals	1	3.3	0	0.0	1	2.9	0	0.0
TOTALS	30	100.0	12	100.0	34	99.9	12	100.0
* This figure affected by re-definition of two projects								

Table 11. *Comparison of the first two years' proposals in terms of various criteria (main subject foci only).*

preparation of recommendations for protection.

Distribution by Geographical Focus

At various times, concern has been expressed at the geographical (or, more strictly, state or territory) distribution of grants, and it is useful to make a comparison here of the spread of projects funded by national and state/territory categories (Table 12). The money value of projects, however, is probably more revealing of the distribution of resources (this is also detailed in Table 12).

A major proportion of FY88 funding (more than a fifth) was directed to the two national projects and this represents a significant increase on the previous year.

National projects aside, comparison of the state/territory distribution of funds shows that the north of Australia again was favoured both in terms of the bulk of the successful proposals and monetary value of funding, with an even greater share of the available funds than in the previous year; more than half of these went to the Northern Territory. Less funds were divided among the southern states in FY88. Comparison of Table 12 with Table 8 shows that, generally, the allocation of funds reflected the geographical foci of conservation proposals.

It might be observed that a major 'loser' in FY88 compared with the previous year was South Australia, with a small porportional reduction being experienced by Queensland. Major gains absolutely and proportionally were made by the national category (there were two projects in FY88 as opposed to one in FY87) and by New South Wales while all other states except Tasmania gained some increment both absolutely and proportionally.

It might be more meaningful to some to measure the absolute amounts distributed to each state in terms of that state's population or in terms of the relative distribution of taxation revenue. A more useful comparison might be made in terms of the number of painting and engraving sites in each state/territory, but these figures are not available in even approximate numbers. It could be agreed, however, that the three northern Australian states, Western Australia, Northern Territory and Queensland, have the greatest proportion of sites, followed by New South Wales, South Australia, Victoria, with Tasmania and the Australian Capital Territory well under-represented. Even in this light it could be argued that the Northern Territory continues to obtain a disproportionately large amount of funding, and Western Australia, Queensland and the Australian Capital Territory obtain

STATE / TERRITORY	FY87			FY88		
	#	$k	%	#	$k	%
National	1	21.2	14.1	2	31.2	21.1
Western Australia	1	16.2	10.8	2	18.0	12.1
Northern Territory	4	37.1	24.7	3	38.0*	25.6
South Australia	2	21.4	14.3	0	0.0	0.0
Queensland	1	18.2	12.1	1	14.9	10.0
New South Wales	1	10.2	6.8	2	18.8	12.7
Capital Territory	0	0.0	0.0	0	0.0	0.0
Victoria	1	12.5	8.3	1	15.0	10.1
Tasmania	1	13.2	8.8	1	12.5	8.4
TOTALS	12	150.0	99.9	12	148.4*	100.0
* Excludes $4k carried over from FY87						

Table 12. *Comparison of distribution of funded projects by national importance and state/territory location in terms of number of projects, monetary value and proportion of funds disbursed.*

disproportionately small amounts. It might be noted, however, that the RAPP funds are not the only monies available for this purpose in each area, other potential sources being state agencies, the National Estates Grants and the Australian Heritage Grants programs.

CONCLUSION

In the second year of the Rock Art Protection Program, funding was divided among twelve of the 34 proposals considered by AIAS during October 1987. The monetary value of all proposals considered exceeded $450k; the funds available for expenditure totalled $150k.

Proposals were received from individuals working at universities and a CAE, those not attached to any tertiary institution, and from various organisations; most applications derived from state agencies with heritage management and/or land use responsibilities and Aboriginal land councils. Proposals came from all states and territories, with the Northern Territory and New South Wales being best represented. The focus of proposals for protection work was evenly divided between locations in the north where a greater proportion of sites are known and the south of Australia but with a large proportion of the latter focused upon New South Wales, perhaps reflecting perception of threats due to visitor pressure on a limited number of sites.

The proposals considered varied greatly in scope and aims. Among them were two of national scope, a postgraduate course curriculum development exercise and one concerned with basic research into the potential of natural silica skins to protect paintings. The regionally oriented projects funded were fairly evenly divided between those focused upon physical preservation and management works and those concerned with survey and/or documentation of sites; proposals to study the Aboriginal significance of imagery ran a poor third.

The twelve funded projects included two 'seeding grants' designed to meet a wide range of criteria and achieve multiple objectives. The two proposals of national interest were funded; one was widely but indirectly concerned with all protection criteria while the second met only the first criterion. The ten successful regional applications varied in aim and objective but all sought to address physical preservation and management matters, and another six were also concerned with survey and documentation of sites. A summary of the aims of all funded projects is provided.

Of the proposals not funded, the majority were considered worthwhile as projects but were not given as high a priority as the successful ones generally because of their aims. (Four were subsequently resubmitted to AIAS for consideration for funding from other sources, three successfully so.) The meeting's priorities were clearly with proposals concerned with immediate and direct conservation measures and those seeking to control visitation rather than projects seeking to survey for 'new' sites or to record in detail known subject material. On the other hand, the meeting made a recommendation that future consideration should be given to other priority criteria of the program.

In comparing the various proposals and the distribution of the first two years' funding, these points might be made: there was a slight increase in the number of proposals considered and a small proportional decrease in the overall monetary value of proposals; there was a commensurate reduction in average cost of the FY88 proposals compared with those of the initial year. There was an increasing tendency for applications to come from institutional sources rather than from individuals, with a marked reduction in requests from 'unattached' workers. There was, again, a wide spread of applications from across all states and territories but the distribution was biased by disproportionately large numbers of applications from two regions. The focus (as opposed to the source) of proposals saw an increase in the tendency to emphasise the north of Australia (WA, NT, Qld) compared with other regions, with 55% of proposals directed there.

In terms of the various RAPP criteria, there was an overall similarity of pattern in the two years' proposals with the exception of a reduction in the number of applications for research into the Aboriginal significance of petroglyphs and paintings. When the proposals are considered in terms of the subcategories of the extended conservation criteria, it becomes noticeable that there were marked changes in emphasis in the second years' proposals. There appeared, for example, to be a new tendency (within Criterion 1 proposals) toward application of methods to particular sites rather than the stress on assessment studies of the previous year. On the other hand, there was an increase in the proportion of proposals with multiple functions. Within Criterion 2, there was a marked increase in the number of applications in the 'search' category.

When the successful proposals are considered it is seen that while the proportion of Criterion 2 projects funded increased markedly over that of the initial year, there were still nearly twice as many successful Criterion 1 proposals; there was a slight increase in the proportion of projects investigating Aboriginal significance of sites, even if this was not the major concern of these studies.

Comparison of the territorial distribution of funding in the first two years of the RAPP shows that, the national projects aside, the northern Australian states and territory again were favoured both in terms of the bulk of the successful proposals and monetary value of funding, with an even greater share of the available funds than in the previous year; more than half of these went to the Northern Territory.

A summary of the results of the various applications of the RAPP funding will be offered elsewhere. Appendix C provides a list of the various reports resulting from the initial two years of the RAPP; these may be consulted at the AIAS Library in Canberra.

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REFERENCE

WARD, G. K. and S. SULLIVAN 1989. The Australian Institute of Aboriginal Studies Rock Art Protection Program. Rock Art Research 6: 54-62.

APPENDIX A:
Outline of FY87 conservation proposals

RAPP #	STATUS	SOURCE	FOCUS	CRITERIA	SOUGHT	GRADE	GRANT
FY87 01	Uni	ACT	NT	2d 2e 2c 3	5.000	A	3.500
FY87 02	SA	NT	NT	1b(i)	17.600	A	11.200
FY87 03	SA	NT	NT	1b(i)	16.300	A	11.200
FY87 04	SA	NT	NT	1b(i)	15.000	A	11.200
FY87 05	OA	WA	WA	1d 3	22.000	A	16.200
FY87 06	Ind	Vic	NSW	2b 2c 2d 3	20.000	B	0.000
FY87 07	Ind	Vic	SA	1b(i) 1b(ii)	18.300	A	4.200
FY87 08	Ind	ACT	A*	1a	22.500	B	0.000
FY87 09	OA	WA	WA	2b 2d 1e 3	22.500	B	0.000
FY87 10	OS	Qld	Qld	1b(ii) 1c	20.700	A	18.200
FY87 11	Ind	SA	SA	2b	5.600	B	0.000
FY87 12	Ind	Qld	Qld	2a 2b 2e	18.500	C	0.000
FY87 13	Uni	Vic	Qld	2b 2e 2c	10.000	C	0.000
FY87 14	Uni	ACT	NSW	2a 2b 2c	10.700	C	0.000
FY87 15	SA	SA	SA	1b(i) 1b(ii) 1c	20.000	A	17.200
FY87 16	Ind	NSW	NSW	2a 2e	19.800	C	0.000
FY87 17	Ind	WA	NT	2a 2c	8.300	C	0.000
FY87 18	Uni	SA	SA	1e 2b 2c 2d 2e	19.000	B	0.000
FY87 19	Uni	NSW	NSW	2e 2c	19.500	C	0.000
FY87 20	SA	NSW	NSW	1a	3.500	C	0.000
FY87 21	SA	NSW	NSW	1d 1a	23.600	A	21.200
FY87 22	SA	NSW	NSW	1d	20.000	A	10.200
FY87 23	LC	WA	WA	3 1d 1f 1c 1b(i	12.800	B	0.000
FY87 24	SA	NSW	NSW	1c 1b(ii) 1d 2b	14.800	B	0.000
FY87 25	SA	Vic	Vic	1e 1c	14.840	C	12.500
FY87 26	SA	Tas	Tas	1b(i)	19.220	A	13.200
FY87 27	Ind	Qld	ACT	2d	2.700	C	0.000
FY87 28	SA	Qld	Qld	2b 2c 1c 3	26.300	C	0.000
FY87 29	SA	Qld	Qld	1c 2a	10.800	C	0.000
FY87 30	Uni	NSW	Qld	1c 2b 3	18.900	C	0.000
*** Total ***					478.760		150.000

APPENDIX B:
Outline of FY88 conservation proposals

RAPP #	STATUS	SOURCE	FOCUS	CRITERIA	SOUGHT	GRADE	GRANT
FY88 01	Uni	ACT	A*	1c 1d	12.351	B	0.000
FY88 02	OS	NT	NT	2c	20.000	C	0.000
FY88 03	Uni	ACT	A*	1 2 3	5.370	A	4.882
FY88 04	Uni	ACT	WA NT	2a	4.200	C	0.000
FY88 05	OS	Qld	Qld	1b(ii)	14.920	A	14.921
FY88 06	SA	NT	NT	1b(i)	34.738	A	10.000
FY88 07	SA	NSW	NSW	1d	5.800	A	5.800
FY88 08	SA	NSW	NSW	1b(ii) 1d	18.800	C	0.000
FY88 09	SA	NSW	NSW	2b 1c	5.928	C	0.000
FY88 10	SA	NSW	NSW	1d	13.800	A	13.000
FY88 11	SA	NSW	NSW	1b(ii) 1d	12.800	B	0.000
FY88 12	SA	NSW	NSW	1b(ii)	18.600	C	0.000
FY88 13	SA	NSW	NSW	1b(ii) 1d	4.800	C	0.000
FY88 14	SA	NSW	NSW	1d	7.700	C	0.000
FY88 15	SA	NSW	NSW	1b(ii) 1d	3.600	C	0.000
FY88 16	LC	NT	NT	2a 2b 2e 2c	13.420	C	0.000
FY88 17	LC	NT	NT	2a 2b 2c 2e	13.420	C	0.000
FY88 18	LC	NT	NT	2a 2b 2e 2c	13.440	C	0.000
FY88 19	OS	NT	NT	2a	28.839	C	0.000
FY88 20	SA	Tas	Tas	2b 2d	8.158	C	0.000
FY88 21	SA	Tas	Tas	1b(i)	12.500	A	12.500
FY88 22	SA	WA	WA	1c	19.650	A	11.250
FY88 23	Uni	ACT	A*	1a	27.260	A	26.305
FY88 24	Ind	SA	SA	2b	7.996	C	0.000
FY88 25	Uni	ACT	NSW	2d	4.472	C	0.000
FY88 26	Uni	Qld	Qld	2a 2b 2c	2.647	C	0.000
FY88 27	SA	WA	WA	1b(i)	19.660	A	6.770
FY88 28	SA	WA	WA	1c 1b(i) 1b(ii)	6.050	C	0.000
FY88 29	SA	WA	WA	1c 1d	12.325	B	0.000
FY88 30	SA	Vic	Vic	1d 1b(ii)	19.990	A	15.000
FY88 31	Uni	NSW	Qld	2e	15.434	C	0.000
FY88 32	A*	M*	WA	2a 2b	15.000	B	0.000
FY88 33	A*	M*	Qld	1b(i) 1b(ii) 1d	10.000	C	0.000
FY88 34	A*	M*	A*	2d	25.000	C	0.000
FY88 35	LC	NT/ meeting	NT	2c 2a 2b 3 1b(i)	0.000	A	14.000
FY88 36	OS	NT/ meeting	NT	2c 2a 2b 3 1b(i)	0.000	A	14.000
*** Total ***					458.668		148.428

APPENDIX C:
List of reports resulting from the first and second years' grants, AIAS Rock Art Protection Program

LEGEND
Expressions in square brackets:
R/*nn* numbers an AIAS Report.
IR/*nn* numbers an Interim Report.
Ms and pMs represent Manuscripts and Pamphlet Manuscripts held by AIAS Library; Ms* or pMs* indicates that, while the volume is held by the Library, it awaits expressions of options from the depositor and is not yet available to readers. B indicates a monograph number.
Photographs are held by name of photographer or depositor; N*nnnn* indicates a negative (black and white or colour), while colour transparencies are given individual numbers (currently in the series 9*nnnn*).

RAPP GRANTS FY87

(1) **The Shape of the Dreaming: Report on the cultural significance of Victoria River rock art** by Darrell Lewis and Deborah Rose 1987 (processed, 70 pp., includes 18 figures, 13 plates, references). [Ms2566]
NB: Report published by Aboriginal Studies Press in AIAS Report Series, 1988, as The Shape of the Dreaming. Report on the cultural significance of Victoria River rock art by D. Lewis and D. Rose. 79 pp., includes 18 figures, 12 plates, references (ISBN 0 85575 187 8). [B L673.29/S1]

(2) **Report on acquittal of 1986 Grants, Rock Art Protection Programme** by George Chaloupka 1987 (TS, 2 pp., plus one b&w plate and 13 colour plates). [Ms*]
Copies of colour plates lodged in AIAS Photographic Archives. [Chaloupka.G1.CP: N4436]

(3) **Awareness Program: Aboriginal rock art sites and others and the need to protect them and the natural environment (course co-ordinators Ken Colbung and Jan Rodda), draft final report** by J. Rodda 1987 (TS, ii + 18 pp., plus appendices [photocopies of course materials]). [pMs4658]

(4) **The Paroong Cave Preservation Project** by Robert G. Bednarik. Australian Rock Art Research Association, Melbourne 1988 (processed, ii + 63 pp., includes seven figures, 35 b&w plates, references and three appendices). [B B412.51/P1]

(5) **Final Report on the Hook Island - Nara Inlet Art Site Protection Project - A joint project of the Australian Institute of Aboriginal Studies and the Queensland National Parks and Wildlife Service 1988** (processed, iii + 9 pp., includes three figures, references, plus 28 colour plates, four appendices including 12 b&w plates and perspective drawings). [Ms2604]

(6) **Arkaroo Rock Painting Sites Conservation - Final report 1989** (processed, 14 pp., includes one map, eight plates, plus three appendices: Excavation by N. Draper, radiocarbon determinations; Report on dust damage by D. Lambert; Costing. [pMs4657]

(7) **Conserving Australian Rock Art: A manual for site managers** 1988. (TS, 119 pp., includes six figures, two tables, 31 colour and three b&w plates, references and three appendices). [Ms2562]
NB: Published by Aboriginal Studies Press in AIAS Report Series, November 1989, as **Conserving Australian Rock Art: a manual for site managers** by D. Lambert, edited by G. K. Ward (X + 112 pp., includes five figures, three tables, 31 colour and four b&w plates, references, three appendices, and index) (ISBN 0 05575 210 6).

(8) **Market Research Study. Guided tours of Aboriginal sites at West Head [report to] National Parks and Wildlife Service** by Cameron McNamara P/L 1987 (processed, 33 pp., includes two tables, recommendations, references, plus three appendices). [Ms2620/Ms2619]

(9) **Visitor Survey of Aboriginal Art Sites within and adjacent to Grampians National Park, September 1987, Final Report** by Fay Gale and Jacquie Gillen 1987 (processed, 82 pp. [lacks pp. 3 to 20 recommendations], includes one map, 21 tables, plus 12 b&w plates and five appendices). [Ms2524]

(10) **Petroglyph Protection: Mt Cameron West Aboriginal Site - Results of conservation programme undertaken in 1987** by S. Brown 1987 (TS, 3 pp., plus 8 pp. appendix on long term project proposal). [pMs*]

RAPP GRANTS FY88

(1) **Canberra College of Advanced Education. Report of the Australian Advisory Committee on the Proposed CCAE/GCI Graduate Diploma in Conservation of Rock Art** 1988 (TS, ii + 17 pp., includes two appendices: Australian Advisory Committee membership; Outlines of eight courses). [Ms4655]

(2) **Kenniff Cave Site Protection** by R. E. Power, Division of Conservation, Parks and Wildlife, Department of Environment and Conservation 1989 (TS, vii + 28 pp., includes three figures, six b&w plates, references, four appendices: Interpretive signage text; Cabe lighting specifications Kenniff Cave; Mt Moffatt National Park; Financial statement. [pMs4621]

(3) **Interpretation Work, Red Hands Cave, Blue Mountains National Park. Report to the Australian Institute of Aboriginal Studies,** January 1989 by B. Conyers 1989 (TS, 3 pp., plus 11 colour plates and brochure). [pMs4626]

(4) **Garigal Heritage Walk West Head: Report to the Australian Institute of Aboriginal Studies** by B. Conyers 1989 (TS, 4 pp., plus one figure, four plates and brochure). [pMs4627]

(5) **Survey and Assessment of Rock Art Sites in the Kudjumarndi and Kukalak Areas, Western Arnhem Land. A report to the Northern Land Council,** June 1988 by R. G. Gunn [main volume] 1988 (TS, 106 pp. includes references, eight tables, 26 figures, 52 colour plates plus nine appendices) with [supplementary volume] **Restricted Information on the Artwork at Sites KP7 & KP8, Kudjumarndi, Arnhem Land. A report to the Northern Land Council, June 1988** by R. G. Gunn (TS, 6 pp., includes three figures, references). [Ms2689]
Collection of fourteen colour negatives (NLC.2BW: N4627 to N4640] and 68 colour transparencies [Gunn.R2.CS: 97601 to 97668] lodged in AIAS Photographic Archives.

(6) **Recording and Assessment of Rock Art Sites at Lungai (Victoria River Crossing area), Gregory National Park. A report to the Conservation Commission of the Northern Territory and the Australian Institute of Aboriginal Studies** by R. G. Gunn 1989 (TS, 68 pp., includes references, 8 tables, 38 figures, plus 38 colour plates, nine appendices [pp. 69-106] including 12 figures). [R89/105]
Collection of thirteen colour negatives [Gunn.R1.BW: N4708 to N4720] and 85 colour transparencies [Gunn.R1.CS: 97516 to 97600] lodged in AIAS Photographic Archives.

(7) **Rock Art Protection Project. Petroglyph Protection: Mt Cameron West Aboriginal Site - Results of Conservation Programme undertaken in 1988. A report to the Australian Institute of Aboriginal Studies** by S. Brown 1989 (TS, 4 pp.), plus **Mount Cameron West. Notes for the Proposed Management Plan on Archaeological Aspects,** 7 pp., plus **Mt Cameron West Rock Art Protection Project: An assessment of the Conservation Programme undertaken to June 1988** by T. Blanks and S. Brown 1988 (TS, 18 pp., includes references). [pMs4629]
A collection of ten colour transparencies [Brown.S1.CS: 97360 to 97369] lodged in AIAS Photographic Archive.

(8) **Environmental Management and Site Conservation Programme: The Granites Complex, Mt Magnet** by P. Haydock 1988 (processed, vi + 27 pp., includes management recommendations, one figure, ten plates, bibliography, plus two maps). [pMs4641]

(9) **Silica Skins: Their Composition, Formation and Role in Conserving Aboriginal Rock Art. Final Report presented to Australian Institute of Aboriginal Studies, May 1989** by Alan L. Watchman (processed, i + 76 pp., includes recommendations, references, four appendices incl. figures and plates). [Ms2673]

(10) **Environmental Management and Site Conservation Programme: Walga Rock Revegetation Project** by P. Haydock 1988 (processed, vi + 11 pp., includes one figure, ten plates, bibliography and two appendices: 'Management proposal' by P. Haydock and J. Rodda (3 pp.); 'A report on the potential for revegetation of an area adjacent to painted rock surfaces: Walga Rock, Cue, Western Australia' by G. Cockerton, Australian Revegetation Corporation, Perth, 1987 (12 pp.). [pMs3393]

(11) **Grampians Rock Art - Interpretation.** Interim report received from D. Ranson, Victoria Archaeological Survey, July 1989. [IR89/75]

Résumé. Le texte original dans Rock Art Research *par Ward et Sullivan (vol. 6, no. 1) consiste du contour pour le développement du programme de préservation des details des projects étant récipient des bourses originales. Ce texte est complémentaire. C'est l'aperçu des résultats des bourses de la première année les candidats elus des résultats pour les deux ans, et une analyse bréve des différences entre les propositions de chaque année, celles réussies et celles non réussies.*

Zusammenfassung. Dieser Artikel komplementiert den früheren Beitrag von Ward und Sullivan in Rock Art Research *(Band 6, Nummer 1, 1989), in dem der Hintergrund zur Entwicklung des Schutzprogrammes sowie Einzelheiten der ersten unterstützten Projekte gegeben wurden. Er umreisst die Resultate dieser Förderung im ersten Jahr, beschreibt die erfolgreichen Gesuche für Kostenzuschüsse im zweiten Jahr, erörtert die Resultate der Projekte beider Jahre, und bietet eine kurze Analyse der Unterschiede zwischen den Anträgen beider Jahre, der erfolgreichen wie der erfolglosen.*

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 COMMENTS

Bilzingsleben: to be or not to be Homo erectus
By **PHILLIP J. HABGOOD**

As the title of the paper by Mania and Mania (1988) indicates, they are confident that the skeletal material from Bilzingsleben is of *Homo erectus*. In fact this assumption is not questioned in any of the published Comments on the paper. For example Bednarik (p. 96) states 'The Bilzingsleben site has yielded many hominid remains belonging to a *Homo erectus* ...'. Also, Davidson, who questioned the validity of many of the assumptions made by Mania and Mania, did not question the attribution of the hominid remains to *Homo erectus*. However, that attribution is still open to debate.

I wish to comment on this subject for two reasons. First, if the difficulties in the attribution of the material are not highlighted, readers of the paper will assume that the Bilzingsleben hominid remains are definitely of *Homo erectus* and this could become entrenched in the literature. Secondly, most of the commenters on the paper (i.e. Bednarik, Davidson, Davis, Halverson) found it difficult to accept Mania and Mania's assertion that *Homo erectus* had the faculty for abstract thinking. However, one could ask the question that if the Bilzingsleben hominid remains had been attributed to *Homo sapiens*, would the commenters have been more willing to accept this and other assumptions made by Mania and Mania?

Also, Davidson requested in his Comment more information on the dating of the site which I will also provide.

Before examining these subjects I would, however, like to observe that I was surprised that the commenters unanimously accepted that the markings on the bones were not only made by hominids, but were also deliberate engravings and not the result of some utilitarian work process. This was especially surprising considering that no detailed taphonomic analysis of other bone material from the site was presented and that microscopic studies of the bones have not been completed as yet. Mania and Mania also mention their disagreement with the identification by Behm-Blancke of markings, including a possible representation of an animal, on other bones from Bilzingsleben. The identification of hominid-produced marks on bones from Bilzingsleben does not appear to be clear-cut. Mania and Mania state (p. 93) that the lines 'cannot be explained as incidental or use wear'. I would have been interested in reading about how they came to this conclusion, and look forward to the more exhaustive and systematic analysis of the markings that is to appear in the near future. I must admit that the markings on Artefacts 1 and 2 form regular patterns. My reluctance to accept the identifications as willingly as the other commenters without detailed discussion of them is probably due, in a large part, to the fact that my field of study is still—and always will be—haunted by the ghost of Piltdown Man.

Attribution to Homo erectus

A debate over the presence of *Homo erectus* in Europe has raged for years (Jelinek 1978, 1980; Stringer 1981, 1984; Wolpoff 1980a, b). The hominid material from Bilzingsleben has been attributed to *Homo erectus* (Mania and Vlček 1981, 1987; Vlček 1978), but are these remains really of *Homo erectus*? The fragmentary nature of the material makes this a difficult question to answer. The most significant of the remains are two occipital fragments that join to form a large portion of the occipital bone which is very thick and strongly flexed, and a glabellar segment of the frontal bone that reveals a very prominent supraorbital torus that is continuous across the glabella.

The occipital bone is said to show greatest similarity to the *Homo erectus* specimen Sinanthropus 3 from the Chou-k'ou-tien Lower Cave in China (Mania and Vlček 1981, 1987; Vlček 1978). The identified similarities are the sharp angle and thickness of the occipital, the form of the occipital torus and the position of the inion coinciding with the opisthocranion. However, Sinanthropus 3 was identified by Weidenreich (1943) as belonging to an 8 to 9-year-old child, rendering morphological development and/or change possible, if not probable. The occipital morphology of Sinanthropus 3 also differs in certain aspects from that of other Sinanthropus remains (Weidenreich 1943). Thus, how representative of *Homo erectus* is the occipital morphology of Sinanthropus 3? One could also question many of the parallels identified by Vlček (1978; Mania and Vlček 1981, 1987; see Habgood in prep.).

Also, little similarity was found between the Bilzingsleben occipital and other Asian *Homo erectus* material such as that from Ngandong. Differences between the Bilzingsleben occipital and Asian *Homo erectus* material were regarded by Vlček (1978; Mania and Vlček 1981, 1987) as being the result of geographic variation. This is hardly a satisfactory explanation for the differences between the Bilzingsleben occipital and the bulk of the Asian *Homo erectus* sample.

Initially Vlček (1978; Mania and Vlček 1981) was unable to compare the Bilzingsleben occipital with that portion of Petralona, the most complete of the Middle Pleistocene hominids from Europe. In a later paper (Mania and Vlček 1987) few comparisons were made with Petralona. This comparison has been done more recently by Stringer (1980, 1981) who suggested that the occipital torus of Bilzingsleben was like a smaller version of that on Petralona. The thickness and angle of the occipital are also similar, but the opisthocranion of Petralona is higher than that of Bilzingsleben. Wolpoff (1980a) has also suggested that the Bilzingsleben occipital is similar to Petralona as well as Vértesszöllös 2. Vlček (1978; Mania and Vlček 1981, 1987) also found similarities with Vértesszöllös 2. Smith (1982) suggests that the Fontéchevade 5 calotte would be a good fit for the Bilzingsleben occipital. None of these hominids are *Homo erectus*. Petralona and Vértesszöllös 2 are best described as early robust archaic *Homo sapiens*, while Fontéchevade 5 is an early Neanderthal (Habgood in prep.; Stringer 1980, 1981, 1984).

The robust glabellar fragment is said to be most similar to the *Homo erectus* crania of Olduvai H9 and Sangiran 17, and quite different to the European late Middle Pleistocene Arago 21, Steinheim and Ehringsdorf 9 hominids (Mania and Vlček 1981, 1987; Vlček 1978). Stringer (1980, 1981), however, argues that the glabellar morphology of Bilzingsleben is more similar to that of Arago 21 than had been suggested by Vlček (1978; Mania and Vlček 1981, 1987), and that it also resembles Petralona in mediosagittal section. Some of the differences between the Bilzingsleben glabellar fragment and Arago 21 are probably caused by postmortem deformation of Arago 21 which has resulted in a flattened and more vertically oriented frontal squama. Arago 21, however, has a frontonasal suture that is more horizontal than that of Bilzingsleben, which has a frontonasal suture that forms an inverted 'V'. In this feature Bilzingsleben is similar to Petralona. However, Bilzingsleben has a continuous torus whereas both Petralona and Arago 21 have tori consisting of two distinct arches.

There are, therefore, more similarities between the Bilzingsleben occipital and glabellar fragments and similar portions on late Middle Pleistocene hominids from Europe than Vlček (1978; Mania and Vlček 1981, 1987) indicated. These similarities are especially evident on the Petralona cranium which, as mentioned earlier, was not initially compared with the Bilzingsleben remains.

Mania and Vlček (1981, 1987), Vlček (1978) and Day (1986) regard Bilzingsleben as a *Homo erectus*, whereas Stringer (1980, 1981, 1984) and Wolpoff (1980a, b) think it is an archaic *Homo sapiens*. Wolpoff suggests that European Middle Pleistocene males like Petralona and Bilzingsleben were very robust and resembled *Homo erectus* crania more than the females such as Steinheim and Swanscombe. Stringer (1980, see also 1984) also suggests that if only the glabellar portion of the frontal and the occipital squama of Petralona were preserved, it would be classified as *Homo erectus*. The remainder of the cranium, however, demonstrates that it is of an archaic *Homo sapiens*.

If the Bilzingsleben material is late Middle Pleistocene in age it could be younger than Vértesszöllös 2. This would present a marked morphological dichotomy in that the younger material from Bilzingsleben would present the more archaic morphology. If the Bilzingsleben site is as young as the dates obtained by Harmon et al. (1980) suggest, the hominid remains could be younger than those from Steinheim and Swanscombe, and so the morphological dichotomy would be even greater. It would also necessitate a scenario that has *Homo erectus* and archaic *Homo sapiens* occupying eastern Europe during the late Middle Pleistocene. Also, where are all the other examples of European *Homo erectus*? I believe that a much more parsimonious scenario is that during the late Middle Pleistocene, Europe was occupied by robust archaic *Homo sapiens* that gradually developed morphological features foreshadowing the Late Pleistocene Neanderthals (Habgood in prep.).

There are definitely differences between the Bilzingsleben material and other European archaic *Homo sapiens*, and similarities between it and *Homo erectus* material, but as Stringer (1981: 9) states, 'there is certainly equal justification for grouping the Bilzingsleben fossils with European middle Pleistocene specimens'.

The Bilzingsleben remains are too fragmentary to allow an 'exact' taxonomic attribution of them, if such a thing exists. Having examined most of the relevant original skeletal material from the Old World, and casts of other material including the glabella and occipital fragments from Bilzingsleben, I concur with the authors mentioned above who feel that the Bilzingsleben hominid(s?) is best regarded as a very robust male member of a morphologically variable archaic *Homo sapiens* population of the latter half of the Middle Pleistocene from Europe.

Dating

There is much debate over the dating of the Bilzingsleben site (see discussion in Cook et al. 1982). Mania and Mania (1988: 91) contend that the occupation horizon is of the Holstein Complex, with an age of about 300-350 000 years (see also Mania and Vlček 1981: 134). This placement is based on the geomorphology of the site, the fossil fauna and flora from the travertine, and on an amino acid racemisation date of 230 000 years BP (Mania and Vlček 1981). There is, however, much confusion over the placement of the Holsteinian. On a correlation chart for the European Pleistocene, Gamble (1986: Table 3.3) has the Holstein appear in three places (see also Habgood in prep.: Table 5:1).

A number of attempts have been made to obtain

uranium series dates for the site. Harmon, Glazek and Nowak (1980) have obtained uranium series dates for a calcite lens within the fossiliferous lower sandy travertine horizon which provide an age of 228 000 +17 000 -12 000 years BP for the hominid remains. They place the site in the penultimate interglacial, and equate it with oxygen isotope stage 7, which dates between 190-195 000 and 247-251 000 years BP (Hays, Imbrie and Shackleton 1976; Shackleton and Opdyke 1973, 1976).

Svoboda (1987) reports uranium series dates of 179 000 - 301 000 years BP and 335 000 - 350 000 years BP, and amino acid dates that range between 280 000 and 350 000 years BP, while Schwarcz et al. (1988) cite uranium series dates that range from 175 000 years BP to greater than 400 000 years BP. Additional uranium series determinations have yielded ages of greater than 350 000 years BP (Cook et al. 1982; Schwarcz 1982).

Schwarcz et al. (1988) have provided new uranium series dates that range from 319 000 ±40 000 years BP to greater than 350 000 years BP for Bilzingsleben. They feel that the hominid-bearing deposits are 'less than or equal to 414 ±45 ka and no later than 280 ka' (Schwarcz et al. 1988: 13). However, assumptions about the depositional and chemical history of the samples used had to be made, which leaves the dates open to question. Schwarcz et al. (1988) argue that their new uranium series dates differ from earlier dates (i.e. Harmon et al. 1980) because the samples used must have come from different strata, even though geological descriptions place the samples in the same stratum. There clearly remain problems with the uranium series dates obtained for the Bilzingsleben site.

Schwarcz et al. (1988) also obtained electron spin resonance dates of 374 000 +84 000 -55 000 and 424 000 +110 000 -72 000 years BP for the hominid-bearing deposits at Bilzingsleben. Again, major assumptions about the history of the samples were required to obtain these dates and so they must remain questionable.

Based on floral and faunal material a correlation of the site with the warmer oxygen isotope stages 9 or 11 has also been suggested (Cook et al. 1982). These stages date to between 276-297 000 and 336-347 000 years BP, and 356-367 000 and 425-440 000 years BP respectively (Hays et al. 1976; Shackleton and Opdyke 1973, 1976).

This discussion indicates that there are still problems with the dating of the Bilzingsleben site. However, a late Middle Pleistocene age for the archaeological deposits at Bilzingsleben would seem probable, although an earlier date cannot be ruled out.

I will leave it up to my colleagues to ascertain what significance my comments have for the interpretation of the marks on the bone artefacts from Bilzingsleben.

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By **DIETRICH MANIA** and **URSULA MANIA**

To begin with, we cannot help feeling that Habgood's demonstrative title promises more than his remarks on the research results of Bilzingsleben (G.D.R., central Europe) can finally offer. He refers to arguments which are in no way new to us; we have heard them expressed from other, also not much informed, sides. Consequently we are going to respond to them in a brief and concise manner.

In his Comment, Habgood generally refers to the observations made by us on rhythmical engravings on four bone artefacts of Bilzingsleben which we have come to regard as deliberately produced (Mania and Mania 1988). We feel that his Comment is based on an underlying 'ideology': if the engravings are intentional, *Homo erectus* could not have produced them, while the lines cannot be deliberate if the hominids of Bilzingsleben should indeed be representing *Homo erectus*!

We strive not to be influenced in our research by any such preconceived model, and to judge the authentic find complex as objectively as it is possible for us. We have been led by this principle from the very beginning of our investigations.

The question of the precise status in hominid development of the people who produced the deliberate engravings of Bilzingsleben is not so important to us: since they are of Middle Pleistocene age their great antiquity alone is of much more significance. This is not only true for the four bone artefacts themselves, but for the site as a whole, because it provides more detailed and comprehensive information on the development of a Middle Pleistocene, Lower Palaeolithic culture than any other site has been able to offer so far.

Until now, ten skull fragments and seven isolated teeth from three human individuals have been found at the Bilzingsleben site. They have been analysed and described by E. Vlček since 1975. In his most recent reference to this subject (Vlček 1989a) he states that 'according to the morphological features of the single skull fragments it is possible to ascribe Bilzingsleben man to a *Homo erectus*'. In another context we read: 'The best analogies of the Bilzingsleben finds are represented by the Middle Pleistocene finds of Arago, Petralona and, above all, by the Lower Pleistocene find of Olduvai Hominid 9. The form of the frontal and occipital area indicates that Bilzingsleben belongs to the *erectus* form' (Vlček 1989b).

Habgood, however, considers the skull remains to be too fragmentary to determine them properly as a *Homo erectus* form. How is it then that they are not too fragmentary for the identification of an early *sapiens* form? So much for his comments on the physical anthropology.

We are familiar with the two opposing views concerning the presence of *Homo erectus* in Europe. Here again we prefer not to be influenced or impaired by preconceived ideas in our search for objectivity. The finds alone must provide answers. At present, the morphological comparisons carried out by E. Vlček are among the best methods

available to analyse them. Conversely, he attributes the most important skull remains to a *Homo erectus* female. Other colleagues who are not fully familiar with these finds consider them to belong to an archaic *sapiens* male. At the same time they postulate a marked sexual dimorphism for this *sapiens*. In this context we would like to know which internationally accepted taxonomic rules do these physical anthropologists follow?

If we look at the technological manifestations of the Bilzingsleben finds, preferably at the lithic and bone artefact inventory, we recognise a distinct type of industry similar to that of Vértesszöllös, Tautavel and Chou-k'ou-tien. They all are connected with *Homo erectus* finds. In contrast to it, there are the Acheulian handaxe industries generally attributed to the archaic *Homo sapiens* form. This corresponds completely with our concepts of the hominid remains.

Time after time, and now as we see by Habgood as well, doubts have been expressed concerning the geochronological position of Bilzingsleben. The inadequate knowledge of these authors (see Stringer 1981) becomes apparent through their doubts about the validity of the Quaternary geological and palaeontological data from the central European region. Moreover, they are guided by a single $^{234}U/^{230}Th$ date published in *Nature* (Harmon, Glazek and Nowak 1980) which was obtained by Nowak and commented on by himself and the geologist Glazek in an unreliable manner, and without our knowledge. Other dates of series of samples exist which indicate the problems of radiometric dating of Middle Pleistocene travertine (Brunnacker et al. 1983). However, they yielded quite workable values (Schwarcz et al. 1988), considering potential sources of error.

From our experiences with this method we have learnt that radiometric dates must be employed with caution in general. They provide only supplementary information for the relative stratigraphy, and by themselves they cannot give us any reliable absolute values. We therefore decided to be guided primarily by relative stratigraphy. Accordingly we can attribute the travertine of Bilzingsleben together with its archaeological horizon to the period separating the Elster from the Saale glaciation—namely the Holsteinian (Holstein Complex). Additionally it should be taken into account that we work in the region of the classical Quaternary research where the definition of Elster, Holstein and Saale deposits is clear, as is the geochronological position—contrary to Habgood's opinion—which is supported by geological and biostratigraphical evidence. The radiometric and ESR dates provide additional information but are by no means a reliable method for determining the geochronological placement of a deposit.

The excavations and research work at the Bilzingsleben site are still continuing, and they involve the participation of a great number of specialists. We are hopeful that further surprises might still be in store for us.

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By ROBERT G. BEDNARIK

Dr Habgood provides some very useful background information about the Bilzingsleben finds, particularly on the conflicting interpretations of the hominid remains, and on the complexities of the dating results. Perhaps most importantly, he challenges the attribution of the skeletal remains to *Homo erectus*. In their paper, Mania and Mania had not discussed these aspects in any detail, referring readers to previous publications. As the responsible editor (who accepted the paper's title, in which reference is prominently made to *Homo erectus*) and as one of the commenters I feel obliged to respond to Habgood.

His key argument is: 'would the commenters have been more willing to accept' certain assumptions by Mania and Mania about the faculties of the Bilzingsleben hominids if they belonged to early *sapiens*? I cannot speak for the other commenters but for my part I can categorically state that this factor did not influence me. At the analytical level I am inclined to ignore the age, association or postulated cultural context of the objects, just as I analyse all cave markings initially by the same techniques, irrespective of whether they are man made or natural. In my Comment I was primarily concerned with analysing the four bone fragments on their own merits, considering the physical evidence itself, without anticipating what would be an acceptable finding. This I think constitutes a scientific approach.

Habgood believes the commenters were influenced by preconceived ideas about the abilities of a given hominid species. His own bias is shown by his statement: '. . . most of the commenters . . . found it difficult to accept Mania and Mania's assertion that *Homo erectus* had the faculty for abstract thinking'. This is a fundamental misunderstanding on his part: the commenters did not find it difficult to accept the faculty for abstract thinking, they found it difficult to accept *that the evidence presented by Mania and Mania proves such a capacity!* There is a huge difference between these two positions and I suspect that none of the commenters has as clear a concept of what the mental faculties of *Homo erectus* were as Habgood apparently believes to possess. Moreover, some commenters had difficulties with the term or concept of 'abstract thinking'; it is itself an over-economical rationalisation, and particularly Davis offered an excellent discussion of this very problem.

Obviously the type of philosophy that underpins Habgood's thinking—that something cannot be because it runs counter to what is essentially the currently fashionable paradigm—is what leads to excessive conservatism in science; as Bertrand Russell once wrote: 'It is unscientific not to admit the possibility of anything'. There must be a first time for everything, as Virchow, Cartailhac and scores of archaeologists had to discover.

Habgood asks where the remains of all the other examples of European *Homo erectus* are. As a complete novice in this bewildering world of physi-

cal anthropology, I must confess that I had always thought that the Heidelberg mandibel dates from the Cromer interglacial, as do various early lithic industries in Europe. Is the Heidelberg find also of a *sapiens*? Only a few decades ago, Neanderthal was considered to be a species separate from *sapiens*, and the latter's reign was thought to have begun only during the Göttweig interstadial. Now we have embraced not only Neanderthal, but all his Middle Pleistocene predecessors in Europe, as our very own kin (oddly, the hominids of other continents were less fortunate: they were only admitted to this exclusive European club if they had lived on the respectable side of the Riss!).

When considering the palaeoanthropological aspects of Mania and Mania's manuscript I was guided mostly by the documentation in Mania and Vlček (1987), and I reasoned that by the onset of the Riss *erectus* seems to have been replaced by an archaic *sapiens* population in Europe. There would have been intermediate stages of development, individual specimens of which would be difficult to attribute to either species. One would presumably look for diagnostic features, particularly of crania (the most frequently found remains). In the case of *Homo erectus*, these would include the postorbital constriction of the calotte, the cranial capacity, the curvature of the occipital region and, perhaps most importantly, the frontal aspects, notably the continuous supraorbital torus and the low vaulting.

Looking first at the occipital region in median-sagittal section, we see that Bilzingsleben is far more robust than Swanscombe, Steinheim or Ehringsdorf, with a bone thickness of more than double that of the early *sapiens* remains. The angularity formed by the occipital torus appears more archaic than on most of the Asian *erectus* specimens. Comparisons of the *os frontale* section are even more persuasive, there are few *erectus* remains that have a thicker frontal torus than those from Bilzingsleben. The entire frontal section of Bilzingsleben does not remotely resemble that of typical early *sapiens* forms (cf. the well-rounded and very thin-walled vaults of Steinheim and Ehringsdorf), and the continuous frontal torus renders Habgood's identification of Bilzingsleben as a *sapiens* highly suspect. How can he reconcile this feature, and the massiveness of the browridge, with the taxonomic latitude of *Homo sapiens*? The surviving frontal fragments also suggest that Bilzingsleben man had a pronounced postorbital narrowing. The differences between Bilzingsleben and Sinanthropus III are particularly pronounced in the glabellar region, where the former is about twice as massive. Bilzingsleben compares well with Olduvai 9 and Sangiran 17, while lacking any characteristics supporting a classification as *sapiens*. It was on this basis that I accepted the interpretation of Mania and Mania, and Habgood has not convinced me that this was hasty of me.

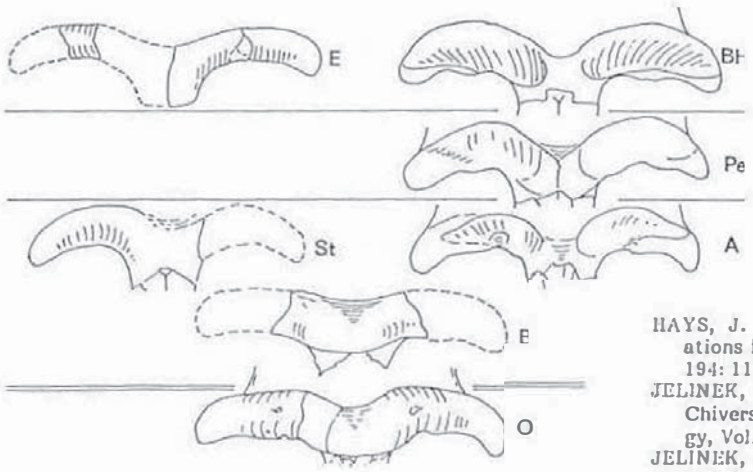
If *sapiens* appeared in Europe hundreds of thousands of years before *erectus* expired in other continents, would this not suggest that he evolved in Europe? After all, *erectus* remains keep popping up in the Upper Pleistocene of Africa and Asia, while almost no pre-Würm *sapiens* remains have been found in these continents. (This scenario still suggests that Europe was first entered by some *erectus* form!) Habgood finds it difficult to reconcile the morphological dichotomy between Bilzingsleben and Steinheim or Swanscombe, while apparently accepting precisely the same dichotomy when it relates to a much greater chronological difference: after all, the Vértesszöllös remains are many times as old as, for instance, those of *Homo erectus soloensis*.

Habgood's ambivalence comes through as he muses over how Bilzingsleben, if it were younger than Vértesszöllös, Steinheim and Swanscombe, could be of a more archaic morphology. Firstly, the morphological characteristics of human populations do not always develop in quite the way the naive models of physical anthropologists predict: even near Habgood's home in Melbourne there was a Pleistocene population of 'gracile' *Homo sapiens sapiens* (at Lake Mungo) of twice the antiquity of the 'robust' people of nearby Kow Swamp, who had a skull thickness of up to 13 mm, prominent brow ridges and receding forehead (for summary see Flood 1983: 55-66). Secondly, why should Steinheim and Swanscombe (perhaps 220 ka old) be older than Bilzingsleben, for which he lists himself a range of radiometric dates spanning roughly 220 to 440 ka? How does he reconcile his identification of Vértesszöllös as 'modern man' with that find's attribution to the Upper Biharian, the Mindelian interstadial (Kretzoi and Vértes 1965), and with its Olduvaian cobble tool industry?

In a most pertinent overview Habgood also summarises the dating attempts at Bilzingsleben, showing essentially how misleading single uranium/thorium dates can be, and how experimental dating methods continue to be plagued by inaccurate results. He omits to mention that one vertical series of dating samples yielded greater ages for sediments overlying strata with younger dates!

In my evaluation of the Mania and Mania manuscript I was guided by what I saw as *real evidence*. The travertine containing the occupation remains underlies the Saale terrace and overlies the Elster gravels. It comprises two phases of which the upper one, the Dömnitz, contains the archaeological horizon. This is fact, not mere speculation—as the radiometric dates are. The most likely age of the remains is therefore, as I have implied in my previous Comment, roughly between 250 and 350 ka. If the little we know about hominid evolution during the second interglacial is a reliable reflection of this process then one would tend to place the finds closer to the latter of these two ages, and this is in agreement with lithic typology, palaeontology and palaeobotany. Most importantly, the geochronological sequence near the river Saale, the namesake of the Saale (Riss) glacial, is of course indisputable—it cannot be seriously challenged by any amount of juggling of experimental dating results.

In summary, the range of dates Habgood lists only serves to confirm the geochronological dating. I conclude that the age estimate by Mania and Mania, however vague it may appear, is the only tangible one we have, and Mania and Vlček's (1987) reliance on the sedimentary record is to be applau-



The formation of the hominid supra-orbital torus according to Mania and Vlček, comparing (from bottom): Olduvai, Bilzingsleben, Steinheim, Arago, Petralona, Ehringsdorf and Broken Hill. After Mania and Vlček 1987.

ded.

On the issue of the attribution of the hominid remains it will be apparent that there would have been intermediate hominid forms between the two species: Habgood seems to see in the Bilzingsleben remains a very early, erectoid male *sapiens*; Vlček attributes them to a late *erectus*, and identifies one specimen as a female. May I, as a lay person, question the validity of a taxonomising process when it has to rely on the sex of a specimen to determine its species. Surely we are splitting hairs when the males of a *sapiens* population are more erectoid than the females of an *erectus* population, and one might even enquire how the specialists determine the sex of a few small cranial fragments when they cannot even agree what species these belong to! Or is it a matter of first deciding on a species, and then determining the sex on the basis of how well the remains fit into the variability range of that species? It is most apt to cite here White's (1986: 208) comment on how our perception of skeletal differences among hominids is determined by preconceived ideas, or by data that should not influence our identification, such as the age of the find. Which brings me back to where I began this response, and prompts me to ask: do we really know enough about the Middle Pleistocene hominids to indulge in the application of preconceived models about what the data should confirm, and in explaining away any deviation we encounter? I doubt it.

Robert G. Bednarik
Editor

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AGE DETERMINATIONS FOR ROCK VARNISH FORMATION WITHIN PETROGLYPHS: CATION-RATIO DATING OF 24 MOTIFS FROM THE OLARY REGION, SOUTH AUSTRALIA

MARGARET F. NOBBS and RONALD I. DORN

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FURTHER COMMENTS

By **MAURICE P. LANTEIGNE**

Unlike most reviewers I have very serious reservations with regards to Nobbs and Dorn's recent article on cation-ratio dating of petroglyphs—not so much with their dating procedures per se as with their application to, and subsequent inferences from, a cultural feature, rock art.

The most serious problem, and the only one I will address here, is their inadequate sampling technique. They have indicated that 'the selection of petroglyphs for analysis was random' (Nobbs and Dorn 1988: 108), thereby suggesting that they were attempting to conform to standard statistical sampling procedures. However, they 'randomly' selected only 24 motifs or 17.5% of the total minimum sample required (N=137) in order to achieve a standard 95% confidence interval from which to make statistically reliable conclusions. This figure is derived from the equation (solving for N): Standard Error of Population Mean = Standard Deviation of Population Mean / Square Root (Population Number) (SAS 1987: 228; Blalock 1979: 215-8; Choi 1978: 93-4; Hammond, Householder and Castellan 1970: 259, 261, 369); where (solving for Standard Error): Estimate Error of True Mean Value (Set at 10% of Sample Mean) = Confidence Interval (Set at 1.96) * Standard Error of Population Mean (SAS 1987: 226); and where: Estimate Standard Deviation of Population Mean = Square Root ([Sample N / Sample N-1] * Sample Variance [or S.D. squared]) (Blalock 1979: 205; Hammond et al. 1970: 261, 271-4, 370).

The only statistically valid conclusion which may be inferred from the proposed 'minimum sample' of N=137 is the date range that the rock art should fall within, given a 95% confidence interval. One cannot extrapolate, as Nobbs and Dorn have, the specific chronological order of the motifs or their categories. In order to do so one is required, again, to set a minimum sample to satisfy the requirements of a 95% confidence interval.

Example 1: For the 3 'bird tracks' (K13, K16, K21) with an average date of 12 466.7 one will need to sample N=572 'bird track' motifs before extrapolating the range of dates within a 95% confidence interval—Nobbs and Dorn provided only 3 or 0.5% of the required sample.

Example 2: For the 6 'macropod tracks' (Frieze K13, K6, K5, K17, K9 and K12) one will need to sample N=498 such motifs in order to achieve a 95% confidence interval—Nobbs and Dorn sampled only 6 or 1.2% of that required.

If one wishes to test for temporal variations one will have to increase sample size accordingly. For example if one were to suggest that each of the 24 motifs were representational of the 'style' of their time period (and here the vacuous term 'style', as noted by Bednarik [1988: 125], creates a theoretical dilemma in terms of statistical modelling procedures) and one wanted to place them in a chronological order (as Nobbs and Dorn have), then one would have to take a minimum sample of each temporal 'style'. Estimating a Population S.D. at 57% of Population Mean, we find that a minimum of N=125 motifs is required in order to adequately describe each of the 24 'temporal periods', for a total sample of N=3000—Nobbs and Dorn provided only 0.8%.

These minimum sample requirements are based upon the assumption that we accept the results of the cation-ratio dating at face value, i.e. that we ignore the same sampling requirements for the procedure itself. If not, then it would require a minimum sampling of N=3836 cation-ratio assays (N=28 assays per motif) just to present a general range date of the rock art of Karolta within a 95% confidence interval. To place them in chronological order, as Nobbs and Dorn have, would require a total N=84 000 sample assays. Then, and only then, can we discuss the chronological relationships between individual motifs and categories of motifs. As it stands, Nobbs and Dorn performed only 72 assays, or 0.09% of the total required.

Although it is unreasonable to expect a researcher to produce 84 000 CR assays (at a cost of about 12,6 million US$), given the enormous range of dates (30 300 years) it is reasonable to expect the researchers to do a more adequate sampling of motifs than was presented, prior to generating hypotheses about their chronological distribution; either that, or restrict inferences to the data available. As it stands, Nobbs and Dorn cannot even provide an age sampling distribution of the Karolta rock art within a 95% confidence limit (i.e. present sufficient statistical evidence to accept the basic hypothesis that 95% of the 1826 rock art motifs at Karolta would fall within the time range of 1400 to 31 700 years), let alone proceed to 'seriate' them chronologically. Until such time as a minimum sample is approached, their conclusion 'the style of the petroglyphs at Karolta has remained largely unchanged for about 30 000 years' (Nobbs and Dorn 1988: 108) can only be regarded as premature and conjectural.

To be fair, however, their cation-ratio dating experiments hold much promise for understanding the phenomena of rock art. I await the results of their continued efforts with anticipation.

Postscript

One possible approach (the least expensive) for Nobbs and Dorn is to modify their data gathering methodology to suit the specific types of questions

they would like to ask—rather than gathering the data first, then asking the questions.

For example: are there statistically recognisable differences between specific categories of motifs, which would indicate changes in 'tradition'? Preliminary T-tests on the three cation-ratio dates provided for each of the 24 motifs (excluding anomalies) suggest that there is no difference between 'bird' and 'macropod tracks' (T=0.2593 DF 20.0 Prob>/T/=0.7980) (Frieze K13 is discarded for the 'macropod' category, due to its repetition in the 'bird track' category), but there is between 'bird tracks' and circles (T=-4.3390 DF 20.0 Prob>/T/= 0.0003) and between 'macropod tracks' and circles (T=-5.8006 DF 26.0 Prob>/T/=0.0001); indicating that, contrary to Nobbs and Dorn, 'contextual parameters' have changed in Karolta rock art.

Such statistical significance between small samples can become quite meaningful, without the need to sample 3000 motifs. The cautionary note is to clearly define what would constitute an adequate total sample size (and a proportional category sample size) given the types of questions (and statistical procedures) one would like to employ—at the same time accounting for both Type I and Type II statistical errors (Blalock 1979: 109-12; Tabachnick and Fidell 1989: 35).

It should be noted that the T-tests are slightly biased in that the three 'uncorrected' cation-ratio dates are used for each motif, rather than the one 'corrected' mean date—to increase sample size. The direction of this bias may be in favour of the circle category due to its smaller Standard Deviation and Standard Error of Mean—relative to its Date Mean. However, this condition appears to be intrinsic to the cation-ratio procedure itself, decreasing 2.1% per +10 000 years for S.D., and 1.1% per +10 000 years for Std.Err. This intrinsic error may be described by the following two equations:

EQUATION A
Standard Deviation Percentage = 0.184 588 87 + (-0.000 0019 * Mean Date). (N=24 S.D.% Mean= 0.154 C.V.=11.8679 R-square=0.503 PR>F=0.0001).

EQUATION B
Standard Error of Mean Percentage = 0.110 964 + (-0.000 0012 * Mean Date). (N=24 Std.Err.% Mean=0.092 C.V.=13.1324 R-square=0.466 PR>F= 0.0002).

The smaller Standard Deviation for older cation-ratio dates was noted in Dorn et al. (1986: 832), but a statistically incorrect inference was drawn from this observation: 'This indicates that older C.R. dates are mutually consistent'. Here the researchers failed to initiate follow-up tests to account for this discrepancy, which I would assume to be standard practice when presenting an 'absolute' dating procedure.

In my opinion the chronological ordering of specific motifs by cation-ratio dating is not economically feasible, given the statistical and theoretical requirements for the procedure. On the other hand, differential assessment of general categories of motifs (and definable 'traditions') is certainly within reach. [1]

[1] The same problem of inadequate sampling procedure plagues Dorn 1983, Dorn and Whitley 1983, Dorn and Whitley 1984, Whitley and Dorn 1987, Dorn et al. 1986 and Dorn et al. 1987. Although the latter two reports attempt to approximate an adequate total sample size (N=167 or 43.2% and N=150 or 77.7% of required sample size, respectively), they fail to provide adequate category sample size. In Dorn et al. 1986, 87% (N=145) of the varnish samples are collected from only two of the six surface sites in south-eastern California. The other 4 sites have N=8, 7, 5 and 2, and therefore should have been discarded from the analysis. Contrary to Dorn et al. 1986 there is little statistical evidence which would indicate an age older than 5000 years for the two larger surface collections, and certainly none to infer a pre-Clovis occupation—based upon only 13 samples.

It is unfortunate that such an innovative and potentially useful dating approach should be crippled at birth by improper statistical sampling procedures.

By JOHN CLEGG

Dr Lanteigne's Comment provides a very new and interesting approach to dating. He gives a formula designed to calculate the number of measures required to produce a statistically satisfactory sample. And the numbers suggested are very large: 137, 572—impossibly large for archaeological populations where the total number of 'tracks' on a surface may be three, not 500! Many old-fashioned tests—I think of Student's t-test, and the Chi-square test—provide some indications of how seriously to take results of calculations which are based on small samples or small populations. These tests refer to their own questions, as opposed to Lanteigne's understanding of Dorn's requirement of a random sample description, and would presumably therefore be unsuitable.

But I do not think we want a date representative of all the engravings, or a date representative of all the 'bird track' pictures. It would not mean very much to me to be able to say 'Olary engravings were made 17 000 ± 17 000 years ago'. That is pretty well the situation with the Sydney engravings about which there is almost no dating information: ordinary intelligent enquirers are not satisfied with an answer that we do not know how old the engravings are, nor that they are something between 200 and 30 000 years old. A much more satisfactory answer is to point out that there are a few engravings which feature objects introduced 200 years ago; that local Aborigines suffered something close to genocide in the first few years of white settlement; that Aborigines have been in Australia more than 30 000 years, but that the intensity of evidence of Aboriginal activity in the Sydney area seems to have increased markedly about 5000 years ago, so the engravings are probably between 5000 and 200 years old.

Up to now my own version of how old the Panaramitee engravings are has been that there are various dates for engravings which are more or less similar (and some consider them less), of more than 10 000, and 13 000 BP, and general acceptance that they are old. Now that Dorn offers the hope of dating individual engravings or engraving types, it is possible to ask very different questions (if the method is satisfactory); it has been established that engravings were made over a very long period —which may or may not be restricted to the Pleis-

tocene. Obvious further questions will be directed towards seeking patterns of change within the long time—the far-from-vacuous enquiries whether recognisably distinct sorts of pictures were made at different times, which Nobbs and Dorn have already begun. Now that we are (as we all hope) able to date individual engravings we no not ask 'how old are they all?' any more than we ask 'how old are people?', when we already know some are old, some young, some in between, and we can recognise people within those broad categories without trouble.

Homologous things happened with the introduction of carbon dating; questions like 'how old is Stonehenge?' or 'how old is some cave site?' were answered in much more detail: Stonehenge was built in many different stages, beginning with the Neolithic; the cave has so many dates of such and such at this and that level: it was intermittently occupied from x to y. How old is the collection of coins in my pocket? The collection itself dates from when I last spent money this morning: the coins are 67, 67, 69, 75, 75, 77, 77, 82, 84, 88, 88 —which can be described using mean, mode or whatever. So one asks more detailed questions when more detailed methods become available. In fact the change is both so automatic and so rapid that I find myself wanting to deny that we ever even asked 'how old are the Olary engravings?'.

I believe Lanteigne was misled by Nobbs and Dorn's use of the word 'random'. I had a similar experience many years ago when a colleague stated that he had sampled dimensions randomly. I got quite annoyed when I discovered that he had merely measured anything that came to mind and was convenient, with no attempt at 'randomisation' or any other behaviour which might justify the use of the term. His whole paper was in a statistical and scientific context which made me think that random meant random. Upon clarification it became clear that the word was intended to mean no more than that the measurements were made *without* thought to their representativeness. And in fact that is just what the dictionary says (Concise Macquarie: 1045): without definite aim, purpose, or reason; in a haphazard way. I suspect that in both cases the word 'random' was chosen partly because it sounds high-class and scientific, thus reinforcing the high scientific standard of the papers, and misleading myself and Lanteigne. If that is what Nobbs and Dorn meant, then presumably they had no intention of obtaining a representative sample, just of getting a few dates, from various sorts of engravings, scattered on the surface.

It is possible that if Nobbs and Dorn used the term 'random' to signify haphazard, rather than representative sample, then Lanteigne's criticisms lose much of their force.

We never try to get a random sample of carbon dates to date a site. Surely the Dorn method implies an equivalent, at the level of measuring the cations in each sample. At that level Lanteigne's comments may be appropriate.

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By MAURICE P. LANTEIGNE

I thank John Clegg for having taken the time to assess the merits of my initial criticisms of Nobbs and Dorn (1988). His Comment provides the opportunity of addressing larger issues in rock art (and archaeological) research which I had felt to be secondary to the more immediate problems associated with Nobbs and Dorn's paper. I will attempt to clarify my position by answering the more specific questions raised by Clegg, within the general context of the proposed cation-ratio (CR) dating procedure.

(1) The large sampling number required to produce a statistically reliable confidence interval for the Karolta rock art (N=137) is indicative not of the 'unsuitable' requirements of statistics, but of the limitations of the proposed CR dating procedure. Without the establishment of a minimum sample, no statistical confidence can be expressed as to how well the procedure actually 'dates' the rock art complex. If the dating procedure had been applied to an adequate 'random' sample, then one could say that the remaining 1689 figures of the Karolta rock art complex should date between X(Min) and Y(Max) years, 19 times out of 20. The failure to establish minimum sampling requirements not only means the failure to establish a Min/Max range of dates for the entire Karolta rock art complex, but also the failure to establish that the CR dating procedure is anything more than a sophisticated random number generator.

(2) I agree with Clegg that only limited information may be gained from a Min/Max distribution. I therefore proposed alternatives in my above Comment. However, the establishment of a minimum sample and a confidence interval for the Min/Max range of dates for the entire Karolta rock art complex is one of the most critical steps towards establishing the non-random authenticity of the results. It is my contention that, during the past seven years of scientific publications the non-random authenticity of the results of the procedure has never been statistically demonstrated.

When Nobbs and Dorn went so far as to chronologically seriate the individual motifs, as if the dates for each figure should be taken at 'face value', they seriously overstepped the boundaries of proper statistical inference. I employed the 'bird' and 'macropod tracks' as examples to illustrate the enormous difficulty in chronologically ordering categories of motifs (such as those based upon morphological classification), as opposed to individual figures, within the CR procedure.

Questions recently raised by Clarke (1989) and Watchman (1989) refer to the technical problems associated with the CR procedure, in taking into consideration the temporal cross-sectional variations of the different layers of rock varnish. Nobbs and Dorn (1989: 66) have indicated that these technical problems cannot be easily overcome. I have suggested above that the CR dates should not be taken at face value because my tests indicated that

the error of estimate for each of the 24 motifs was too great for statistical confidence in the dates proposed. Until a minimum sample of assays per motif is performed no individual rock art figure can be said to have been 'dated'.

(3) How old are the eleven coins in John Clegg's pocket? The question is based upon a sample which equals the 'total' population of coins available in Clegg's pocket. The problem is therefore circular as, statistically speaking, sample mean will approximate population mean as sample number approximates total population number. As this condition arises, error of estimate will reduce to zero, and the probability vector will approach certainty.

The question should be phrased: 'How many coins will there need to be in Clegg's pocket before we can be statistically confident that the sample of these coins is representative of the total population of coins presently in circulation in his neighbourhood?' According to the formulae I have provided, a minimum of five coins will be needed before we can be statistically confident that, 19 times out of 20, all the coins in Clegg's neighbourhood would date between 1967 and 1988. The problem lies not with the data but with the way in which the question is phrased.

Let us suppose, for the sake of the argument, that Clegg has 24 coins in his pocket, each coin corresponding to one of the 24 mean dates presented for the Karolta rock art complex. Let us assume, also, that each of these dates is an 'absolute' date indicating the year the coin was minted. Can we state with any degree of statistical confidence that the 24 coins now in Clegg's pocket are an adequate representation of all the other coins in his neighbourhood? According to statistical requirements, Clegg would need at least 137 coins in his pocket before we can say that his collection of coins is representative of his population group, 19 times out of 20.

Now, let us chronologically order the 24 coins in Clegg's pocket, and assume that they represent the 'stylistic' periods of the various mints which manufactured the coins. In order to be statistically confident that each coin is representative of a specific 'minting tradition', we need to take an adequate sample of each 'minting tradition' which a coin is presumed to represent. Assuming all vectors to be constant to the given sample, Clegg now needs 3000 coins in his pocket to answer our question.

Up till now we have assumed that the dates on the coins are 'absolute', at least in relation to our present position in Cosmic Time. What happens when the dates are not 'absolute', and contain an error of estimate similar to that fraught with the CR procedure. Clegg would now need to have 84 000 coins in his pocket before we could say with confidence that the temporal order and stylistic characteristics of the various minting traditions exhibited by the initial 24 coins in his pocket are representative of all the coins in his neighbourhood, 19 times out of 20.

Admittedly, this is stretching things a bit far, not to mention Clegg's pocket. But practicality and physical limitations have nothing to do with mathematical reality and the laws of statistical probability.

(4) Random sampling: any measuring experiment which requires the power of statistics in order to validate the results must follow all basic rules of statistical theory, or else risk invalidating the very foundations upon which the procedures rest. Random sampling is one such rule. I only made allusions to the procedure, and deliberately chose to ignore the statistical requirements, as I assumed that when Nobbs and Dorn (1988: 108) stated 'random' sampling they had meant 'random sampling' in the fullest statistical sense of the phrase. If not, then further modifications of the CR experiment will be required in order to conform to proper statistical procedures. The process is simple and direct, with random tables available in the appendix of most standard textbooks on elementary statistics.

In a complicated measuring experiment, such as CR dating, and given the enormous theoretical implications which will be inferred from the results, there is no valid excuse for not performing a proper 'random sample'. Along with choosing an adequate sample number, a random sample of this number is the most critical aspect upon which all statistical premises are founded. Without these two factors being assiduously considered, any conclusions generated by a measuring experiment which relies upon statistics in order to validate its results are superfluous.

(5) Radiocarbon dating: the same concerns that I have raised with the CR dating procedures presented by Nobbs and Dorn are equally applicable to the radiocarbon dating procedure currently accepted by most archaeologists. I, along with my colleagues, have been guilty of accepting at face value the dates we have obtained by carbon-14 assessments without understanding the statistical limitations of the results. C-14 assessments must be corroborated by other, independent procedures: analysis of geological stratigraphy, stylistic seriation, comparison with discrete assemblages, to name a few. For the CR procedure there are no independent secondary and tertiary procedures available for corroboration. The importance, therefore, of clearly establishing the results of the procedure upon a sound statistical foundation becomes paramount.

(6) Psychology of the problem: I am slightly concerned that the statistical errors of the CR dating procedure have gone largely unnoticed for almost seven years of publication in very reputable scientific forums. One of my colleagues remarked that the discipline of rock art research is under so much pressure to enter the mainstream of archaeological theory that it has essentially 'psyched itself out' into believing that 'somewhere, out there', a dating miracle really exists. Better theoreticians than I will need to seriously ponder the historical implications of this question.

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METHODOLOGY IN THE ANALYSIS AND INTERPRETATION OF UPPER PALAEOLITHIC IMAGE: THEORY VERSUS CONTEXTUAL ANALYSIS

ALEXANDER MARSHACK

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FURTHER COMMENT

By G. SAUVET

Lewis-Williams and Dowson's *Current Anthropology* paper and Marshack's **RAR** paper are both dealing with 'internal analysis', which becomes thus unintentionally an important point in the debate. Because Lewis-Williams and Dowson have rejected internal analysis too hastily and without proceedings, a defence of this methodological principle was welcome. Unfortunately most arguments presented by Marshack are based on a restrictive concept of internal analysis (often mistaken for the detailed, microscopic analysis of the marks on a single object) and are thus irrelevant. Consequently Lewis-Williams and Dowson feel reinforced in their judgment.

As stated by Lewis-Williams and Dowson in the introduction of their *Current Anthropology* paper, prior to presentation of their neuropsychological 'entoptic' model, two classes of arguments have been used in interpreting Upper Palaeolithic art: *external* arguments based on ethnographic comparisons and *internal* arguments (i.e. coming only from parietal and mobiliary evidence and its archaeological context). This last approach, promoted by Laming-Emperaire and Leroi-Gourhan, and inspired by structuralist principles, aims at discovering structural features in the manifestations of Palaeolithic cave art (e.g. spatial distributions, co-occurrences of motifs, identification of themes, diachronic evolution, regional variants etc.).

Combined with the modern tools of quantitative data analysis, internal analysis is well designed for syntax pattern elaboration (Sauvet 1988), but not for semantic studies, so that Lewis-Williams and Dowson's statement that it fails to induce meaning is correct. Nevertheless, the same could be said about their own model. In their Reply to Marshack they admit that the forms originated in altered states of consciousness 'cannot be understood outside of their social and cognitive setting'. Thus meaning escapes both approaches, and will probably do so forever—as an increasing number of us is inclined to think.

Lewis-Williams and Dowson claim that the evaluation of an hypothesis versus competing hypotheses should be based on the quantity and diversity of data explained, but they did not apply this principle to the case of structural internal analysis. In *Current Anthropology* they only affirm their *belief* that their argument 'reduces the inferential element that looms so large in arguments from internal analysis' (without saying in which way), and in their Reply to Marshack they present only a scathing criticism of Marshack's *contextual* analysis (which is quite different from Leroi-Gourhan's structural analysis, because of its empiricism).

Most of the arguments developed by Marshack are actually more or less irrelevant and Lewis-Williams and Dowson have been able to rule them out easily. It appears that sexual dimorphism, pelage rendering and its (eventual) seasonal symbolism, reuse of paintings etc. do not conflict with the 'entoptic' hypothesis since comparable examples can be found in shamanistic San rock art. Moreover it is true that 'Marshack sees everything as literal'. For example his interpretation of zigzags on bison

as a depiction of a moulting coat is only one possibility. Superpositioning of signs on animals is well documented (grids on a horse in Lascaux, claviforms on a horse in Les Trois Frères, tectiforms on mammoths in Font-de-Gaume etc.). Iconic and noniconic images are syntactically and semantically linked but their associations do not necessarily express a naturalistic relation.

Even though most arguments presented by Marshack could be dismissed it remains to discuss Leroi-Gourhan's structural analysis. Is the 'entoptic' model capable of producing a comparable explanation of cave organisation, or of refuting that such organisation exists (many recent works, though not agreeing with Leroi-Gourhan's views, recognise that caves are 'structured')? One of the positive contributions of structural internal analysis has been to draw attention to the function of communication. The very existence of rock art indicates that a materially perceptible form has been given to mental images. This process of 'externalisation', which makes graphic products available for communication, implies a number of conventional relationships between transmitters and receivers, but is independent of our point of view about the origin of mental images: Jungian archetypes, subconscious cognitive units or 'engrammes' (Gallus 1977) or shamanistic trance visions. I do not believe that Lewis-Williams and Dowson can disagree with this statement, and there is perhaps less discrepancy between the different points of view than it may seem. In fact the different approaches are not focused on the same level. While Lewis-Williams and Dowson's hypothesis refers to the origin of the forms (Section II of their Reply), Marshack as well as Leroi-Gourhan and most archaeologists are concerned with the conscious use of these forms in particular cultural contexts. Therefore the confusion is not between origin and meaning, as stressed by Lewis-Williams and Dowson, but between origin and cultural use of graphic products.

This is probably why Lewis-Williams and Dowson dodge some of the questions raised by Marshack. For example when Marshack argues that 'To assess even the "geometric" imagery . . . to altered states of awareness is to deny the human capacity for consciously creating and signifying forms', Lewis-Williams and Dowson simply admit that graphic products 'could be duplicated even by someone who had never experienced an altered state of consciousness'. Thus the conclusion expressed by Marshack, that Palaeolithic graphic products are 'highly enculturated' (a conclusion also reached by structural internal analysis on different grounds) does not trouble Lewis-Williams and Dowson. Their hypothesis can explain one thing and its contrary as well. It seems that the model is so adaptable that it cannot be invalidated.

The question of disconfirmation has been already put forward as a weakness of the theory. Lewis-Williams and Dowson have admitted to be unable so far to indicate an element that would not conform to their hypothesis. Now they affirm in a sibylline form that 'the way in which the model *excludes* arts known not to be shamanistic encourages our view that the model can be used to evaluate Upper Palaeolithic art'. This is a very important point that would deserve fuller explanation. If confirmed it would be a strong argument in favour of the theory.

Schematically summarised, Lewis-Williams and Dowson's argument runs as follows: 'If the six basic forms and the seven principles of perception found in entoptic phenomena are present in Palaeolithic art, we could hypothesise that they also have an entoptic origin and are related to shamanistic practices'. This hypothesis requires independent, *internal* confirmation. As already pointed out by Bahn, nonfigurative shapes are so numerous in Palaeolithic art that 'it would be amazing if these basic categories were *not* present'. This is also true concerning the seven principles of perception (replication, fragmentation, integration, superpositioning, juxtapositioning, reduplication and rotation): these principles are universally spread, since they constitute the fundamental syntax of every graphic sign system, including pictographic and ideographic writings and . . . our modern Highway Code.

In a series of recent papers Lewis-Williams and Dowson have shown that many otherwise unexplained San rock paintings could be interpreted by shamanism. It is worth to note that their argument works *internally*, i.e. by using only rock art examples and ethnographic reports from San studies. Moreover shamanism is attested in San rock art by the depiction of shamans in trance (as convincingly established by the authors). Nothing comparable exists in Palaeolithic art. Anthropomorphous representations likely to be interpreted as shamans are quite exceptional.

Therefore, unless Lewis-Williams and Dowson are able to state that the human graphic activity was initiated everywhere and at all times by altered states of consciousness, the application of the hypothesis to Palaeolithic art, in the absence of a specific argument that could be used to discriminate between shamanistic and nonshamanistic arts, remains in my opinion a gratuitous exercise.

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A Comment submitted by Dr David S. Whitley (South Africa) has been withdrawn by the author prior to publication. Following consultation with the editor, a Comment by Dr Randall White (U.S.A.) has also been withdrawn, which has necessitated the cancellation of a Reply by A. Marshack. Due to lack of space, a further Comment, by A. R. Willcox (South Africa), is being held over for the next issue of **RAR**, where it will appear together with A. Marshack's response to all published Comments.

BRIEF REPORTS

An Aboriginal view of rock art management

DAVID MOWALJARLAI AND ALAN WATCHMAN

Introduction

To Europeans, *Wandjinas* are painted, human-like figures (for an example, refer front page) on rock faces in the Kimberley region of Western Australia (see map below). To the Aboriginal people, in whose territory the paintings exist, they have a greater significance. To manage and conserve the sites where *Wandjina* paintings occur requires a thorough understanding of their meaning and importance to the Aboriginal custodians. The story of how the *Wandjina* paintings came to be where they are, how they form part of Aboriginal culture and how they should be managed was told by David Mowaljarlai, Chairman of Wanang Ngari Resource Centre, to the rock art conservation students at the Canberra College of Advanced Education in February 1989. Mowaljarlai's story is presented here.

About the Wandjinas

There were human beings living in the Creation time. Everything, including yams, birds and snakes, lived and walked around like humans then. Their collective name was *Mungunumga*.

The *Mungunumga* were hunters. They used to arrange rocks like a fence to trap kangaroos. They would then kill the trapped animals with clubs.

When the Ice Age came, *Mungarari*, it changed all the people. The *Mungunumga* people became *Wandjinas* and left their images on the rocks. The *Wandjinas* left their spirit all over the land. For example, at Beverley Springs a big swamp formed where the Milky Way came down to Earth and left his footprint. At *Ledgemarro* the Milky Way sat down with a shining light and put that shining light on a little island in the creek. After he left his image, the Milky Way *Wandjina, Ngaja,* went back into the sky because a big law exists there. He had to go back to care for it so that Aboriginal people could learn the law from him.

Wandjinas chose the caves in which to put their images. A *Wandjina* saw his image in the water and put it in the rocks. The same water, called *Wulu*, is where the *Wandjina* left spirit children. After the *Wandjina* left his image in the rock he went into the earth and now lives under the water. The images he left behind are seen today as paintings.

Aboriginal people did not paint *Wandjinas* in the beginning.

Wandjinas left their images all over the Ngarinjin, Wunambal and Worora homelands. They put their images in selected places in the rockshelters in the Creation time. This time is called *Yoriyoro*. It is the time when everything in the land became fixed and permanent. The stories and songs of the *Wandjinas* are known today because in the early days Aboriginal people lived with the *Wandjinas*. The laws and ceremonies were all created at that time.

Aboriginal people fear the *Wandjinas* because they are the creators. They may destroy the people if people do not respect them. People look after the *Wandjinas* today, partly because of their fear and respect for them.

Most of the places where the *Wandjina* paintings are found are public places. There are some painting sites where only men can visit. One special place is a shelter containing a stone altar, with paintings on the walls. The men would put bushes on the altar and place their sacred objects there during ceremonies. The women were allowed to hear those objects rattling, but they were not allowed to see them. This place is called *Wulungari* and it is where the *Wandjina* called *Maralwaday* died.

Traditional Conservation Practices

In the past Aboriginal people had ways of making sure the paintings were cared for too, besides repainting them. For many years, in the wet season, flat rocks were piled on top of spinifex that was collected and placed on top of the shelter. The spinifex helped prevent the rainwater from dribbling down the rock and washing the paint off. This procedure is called *Dundamarra* (like a wind break). It was put there on every *Wandjina* painting to protect it. This is in the law. Also in the rainy season, families gathered and stayed close to the big shelters, to stay dry and to be close to the *Wandjinas*. They repainted the figures in the middle of the 'wet', in *wingin* or cyclone time. People stayed in one place because it was too wet to walk around. It is at that time that songs for the country were sung and the *Wandjinas* were repainted.

Once the *Wandjina* was repainted in *wingin* time, people could not touch it until the next year; it became untouchable once the repainting was finished. It is not for fooling around with. If a person

interferes with a painting after it has been repainted, that person will receive a bad shock from the *Wandjina*. The *Wandjina* will get angry and make the rain heavier to frighten the people. They must keep quiet and speak softly in these places.

Traditional Retouching of Wandjina *Images*

There are many *Wandjinas* in the Worora, Wunambal and Ngarinjin homelands. Many Aboriginal people used to live in the country but most of them are now gone. A lot of the *Wandjinas* have not been visited for many years and they are now fading away because there is no-one to look after them. Those *Wandjinas* are going back to their *wungud*, their beginning. They are being sucked back into the land.

Aboriginal people only started touching up the *Wandjina* images after the *Wandjinas* had gone into the ground to live. Men started retouching them. *Wandjinas* told people to start fixing their images because they had lived alongside one another for so long. People consider the images in a rockshelter as a shrine, to be cared for and maintained. Every day is a blessing for the Aboriginal people and so the paintings need to be kept fresh, otherwise the *Wandjina* will get sad that no-one is caring for him. When people lived close with the *Wandjinas* the images were always fresh.

Both men and women repaint *Wandjinas*. This is to ensure a continuity of tradition. If all the men in a family die out, the women must take over the traditions and keep them alive. The women learnt how to retouch the images from their husbands and fathers.

The *Wandjina* sites are also *lalai* or 'increase' places. This is like a place for praying: if you retouch the images of goannas or barramundi the *Wandjina* will increase the numbers of animals around the site where the image is found in the rock.

In the early days only the Aboriginal people who came from the nearby country, where the particular *Wandjina* was found, had the right to repaint the images. It was each family's responsibility to care for the *Wandjinas* in their own country. The country owned by that family is called a *dambun*, like a block of land in a suburb. Different families have different *dambun*, but all families are related. The next-door neighbour can also have the right to paint the same *Wandjina*. People have the right to paint the *Wandjinas* through their relationship in the *wunan* exchange system.

The *wunan* places people in a line, so if one family dies out in a piece of country, the next-door neighbour takes over, and cares for that country and the *Wandjinas* that live there.

Recent Wandjina *Repainting*

One day the paintings will fade away and be gone forever because nobody has looked after them. People will have lost their respect for the *Wandjinas* and the *Wandjinas* will feel sadness for the people. This is very dangerous as the *Wandjinas* could destroy everyone because the people who are responsible for looking after the paintings have neglected them.

Even though time has passed and people have left many of the *Wandjinas* behind, to fade away, people can still go back to the paintings because they have an on-going right to care for them. The present generation of people can repaint them, in the same way that their parents repainted them in the past. Grandmothers have left them in the care of their grandchildren.

Aboriginal people have continued the life-long tradition of repainting *Wandjinas* until the present day. Old people at Mowanjum, Mount Barnett and Kalumburu still retouch these images in their country.

In 1987 an attempt was made to repaint *Wandjinas* using a Community Employment Program grant managed by the Wanang Ngari Resource Centre (Mowaljarlai and Peck 1987). This caused many problems for the Aboriginal people who were merely asserting their traditional rights. It is important that young people are taught how to repaint the *Wandjinas* as they will otherwise not be able to identify themselves. The story and meaning of *Wandjinas* must be told to the young Aboriginal people because unless they identify themselves with the *Wandjinas* they are nobody.

Today the young people grow up in towns, many miles away from their cultural homelands. The law and culture the elders pass on to them must be strong. The young people are desperate to retain their culture and they must be taken back to their own country to be taught about their culture, and to be given a better chance in the future.

The young people without identities live outside the *wunan*. If a man is not initiated, he cannot be part of the law or the community because he is outside the *wunan*. He is nobody for the rest of his life. He might have a wife and children but he is not allowed to take part in any law activities because of the shame he bears from not identifying with a *Wandjina*. Also, when his sons are being initiated he cannot be a witness. His sons may become law men [elders], but he is nothing to them and must take orders from them. This is happening a lot today.

Young people are taken back to the country with their fathers and uncles to give them the opportunity to be instructed in Aboriginal law and culture. It is hoped that new communities will grow up in these homelands from which the people left years ago.

Already there are Aboriginal people living at places like Pantijan, Mitchell Plateau, Mount Barnett, Imintji, Cone Bay and others. It is important to them to return home, no matter what hardships they may suffer. People will continue to use traditional methods of making fire with fire sticks, using spinifex resin to cement spearheads onto bamboo shafts, and studying the entrails of kangaroos to learn what feelings animals have inside themselves. People cannot use these skills living in towns, they must be in the country.

These little communities are growing up now, back in the country where the *Wandjinas* are located and where the culture is being revitalised. Parts of traditional country are now in nature reserves, national parks and on pastoral leases. Small areas are inside Aboriginal reserves.

Management of Painting Sites

Many tourists, *gadiyas*, travel through the resettled country and they want to visit the *Wandjina* places. Tour guides take tourists there already but Aboriginal people are not approached to explain the meaning of the *Wandjinas* to the tourists.

People who want to see the *Wandjinas* must respect them and should not damage these places in any way. It is important for Aboriginal people to be able to visit the *Wandjinas* and to be there, as rangers, to educate the tourists. It is important to work side by side with white men on this. The old Aboriginal people, with the help of white people, can teach the young generation to be rangers.

It might be necessary to protect the *Wandjina* places with special fences to keep the *gadiyas* away. It is important that *gadiyas* see the *Wandjinas* and understand what they are seeing, but they should not touch them. Fences should preferably be kept away from the *Wandjinas* because most fences look incongruous. They are not natural and they do not fit in with the surroundings. Bush fires can burn wooden fences and they need to be frequently replaced. That is another reason why Aboriginal people should be living close to the paintings.

Aborigines and *gadiyas* can share this Australian heritage together; they should help one another. Aborigines want to share in the financial benefits too, so far only *gadiyas* have benefited from the rock art. In this way they could earn money so that their living conditions can be improved.

Sometimes expert *gadiyas*, such as rock art conservators, may need to come to look at the paintings, and there are no problems with that as long as they first inform Aboriginal people of their visit.

Wandjinas are always listening, they have ears. People should whisper in the rockshelters. If *gadiyas* come there and are noisy and disrespectful by touching the paintings, the *Wandjinas* will feel mocked and shrink away.

Custodians talk to the *Wandjinas* when they visit them, introducing themselves and their guests. A language is spoken that the *Wandjinas* understand, they listen without getting annoyed. *Gadiyas* do not know or understand this. They need someone to show them how to act properly. If an Aborigine takes a tourist to see a *Wandjina* he asks the *Wandjina* not to get angry because he has brought a visitor to see him. The tourists are told not to touch the paintings, but they can take pictures. Aboriginal people are proud that tourists like to see the *Wandjinas* and take something back with them to remind them of their visit. It helps them to remember the history and culture of the country. Aboriginal people like to guide visitors to the *Wandjinas*, to tell them of their belief in the *Wandjinas* and to explain that the paintings are precious. *Gadiyas* must understand the importance and significance of the *Wandjinas*, to respect them and to honour life in this part of Australia.

It is important for Aborigines to have these rules for visiting *Wandjinas*, especially in national parks where many *gadiyas* visit. Governments should support Aborigines in the protection of these important paintings by making such rules.

Conclusion

Debate on the merits of repainting *Wandjinas* was recently highlighted at the First AURA Congress in Darwin, in Symposium O, and it has been the subject of articles in the specialist literature (Bowdler 1988; Mowaljarlai, Vinnicombe, Ward and Chippindale 1988). It is an important issue for all Australians, but particularly rock art conservators who may be asked to repaint faded paintings. Further debate will no doubt ensue as more Aboriginal people return to their traditional homelands to renew their activities and to repaint faded paintings, and as the attractiveness of the painting sites to tourists begins to fade with the loss of brightness of the ochres. This article documents the attitude of a Kimberley Aboriginal elder to the *Wandjinas* and to the repainting of rock images.

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Prehistoric rock art of southern Cape York Peninsula

BRUNO DAVID

Introduction

In spite of over twenty years of rock art research, very little is known of the prehistory(ies) of the various regions which make up Cape York Peninsula (defined here as that part of Australia that lies east of the Gulf of Carpentaria and north of 17°30' S). It is therefore in order to situate the rock art of different parts of north Queensland within a broader understanding of the region's prehistoric past. In 1987 the author began a long-term research program on the southern Cape York Peninsula's Aboriginal prehistory (David and David 1988; David 1989; David and Cole in press). This project, its aims and its methodology are outlined in the present paper.

Background

To date, seven occupation sites with rock art have been excavated, dated and published, in part at least, from the southern Cape York Peninsula. Three other sites have recently been excavated by the author, and Morwood (1989) and Mardaga-Campbell (1986) are at the time of writing conducting further archaeological investigations in the region. A pattern is already emerging from the seven dated sites which may further our understanding of the chronology and sequence of rock art forms of the southern Cape York Peninsula. This pattern is outlined below.

(1) *The Early Sites.* All of the excavated sites which have so far revealed late Pleistocene or early Holocene occupation remains contain patinated peckings (petroglyphs) of 'track' and/or nonfigurative forms. These sites include Early Man Shelter near Laura (with peckings dated stratigraphically to over 13 000 years BP; Rosenfeld, Horton and Winter 1981), Green Ant and Echidna Shelters on the Koolburra Plateau (with earliest human occupation dated to 8660±340 BP and 7280±130 BP respectively; Flood 1987; Flood and Horsfall 1986), and Walkunder Arch Cave near Chillagoe (earliest occupation dated to 19 520±170 BP; Campbell 1982; David 1984).

In 1985 the author test-excavated Fern Cave, a painted, stencilled and pecked site immediately north-west of Chillagoe. A radiocarbon date, on a sample of land snail (*Xanthomelon pachystylum*), was recently obtained from the base of the occupation layers, of 26 010±410 years BP (Beta 30403). The species of land snail used to obtain this date for Fern Cave was probably the same as that used to date the early levels at Walkunder Arch Cave (Campbell 1982: 63). In order to assess this early date the original excavation was extended in 1989 so that enough shell, bone and/or charcoal could be obtained for dating, thus not only assessing the initial date but also enabling a sequence of dates to be obtained. Enough bone and shell was collected for dating, but unfortunately the only charcoal fragments present were from a burnt root from one section of the excavation. Importantly, Fern Cave contains a number of extremely patinated 'track'/nonfigurative peckings of the same types found in other early sites from north Queensland, and if the 26 000 BP date is confirmed the antiquity of the site's occupation would offer further support for an association between 'track' and nonfigurative peckings and early human occupation in southern Cape York Peninsula.

(2) *The Recent Sites.* All of the sites excavated in north Queensland which do *not* contain peckings of 'track'/nonfigurative forms have been dated to the late Holocene (and, conversely, all of the late Holocene sites do *not* have peckings). These sites include Echidna's Rest (earliest occupation dated to approximately 3000 BP; David 1987) from Chillagoe, and Endaen and Walaeimini Shelters from Princess Charlotte Bay (dated to 2500 BP and 4760 BP respectively; Beaton 1985).

Discussion and Future Research

David and Cole (in press) have recently argued that the earliest surviving rock art from southern Cape York Peninsula is relatively homogeneous wherever it is found (peckings of 'track' and nonfigurative designs). This does not necessarily mean that no paintings were produced in the same period, but that they have not survived in most cases if they did exist. The later rock art forms, however, conform to two broad stylistic groups: a southern, predominantly nonfigurative tradition (found to the south of the Walsh River, e.g. in Chillagoe, and to the south-west), and a northern tradition where sometimes extensively decorated anthropomorphs and zoomorphs predominate. Considerable regional variation of this common theme exists within the northern group.

The current research program has therefore two primary research aims:

(1) To systematically survey and record the rock art from various parts of the region between the Palmer River to the north and Chillagoe to the south. These surveys include both systematic recordings of all of the sites from selected areas (random stratified surveys), and a systematic (non-random, non-stratified) survey and recording of all art sites found from selected areas between the Palmer River and Chillagoe. The results of these surveys are to be used in a detailed comparative analysis of the rock art of the broader area, with the already existing recordings of sites to the north and south of this area (e.g. Koolburra Plateau, Laura, Bare Hill, Lawn Hill, Mount Isa) forming an extensive data base for broader comparisons.

(2) An attempt to date the various rock art forms from the Mitchell-Palmer, Mount Mulligan and Chillagoe regions through excavation, patination and superimposition studies. Alan Watchman (Canberra College of Advanced Education) has already visited a number of the sites providing the data for this study (including Fern Cave), and collected wall surface and oxalate samples so as to arrive at a better understanding of the processes that have resulted in the formation of surface deposits over paintings and petroglyphs of the region. Other planned work includes excavation of further sites containing what appear to be early and/or late art forms (e.g. Hearth Cave, immediately south of the Palmer River, was recently excavated; it contains paintings of the latest style superimposed over extremely patinated peckings of the early type). Dating of a number of such sites will be attempted to determine whether the pattern briefly outlined above holds true or not. This pattern will subsequently be compared to changes in other aspects, such as lithic typology, resource exploitation strategies (fauna and raw materials) etc., and related directly to current demographic debates in Australian prehistory.

Presently the rock art survey and recording program nears completion, whilst the excavation program is just beginning. Of the three sites excavated, two contain examples of what appears to be the earliest art form in southern Cape York Peninsula (peckings of 'tracks'/nonfigurative designs) (Fern and Hearth Caves); the author expects both to contain pre-late Holocene occupational

deposits (if the 'early' art form predates the 'recent' one). The third site contains only examples of the hypothesised most recent art form (large, sometimes internally decorated, anthropomorphs and zoomorphs), an art form probably attributable to the mid to late Holocene period, as well as hand stencils and prints. It now remains to be seen whether this perceived chronology will hold firm in the light of further dates and excavation results.

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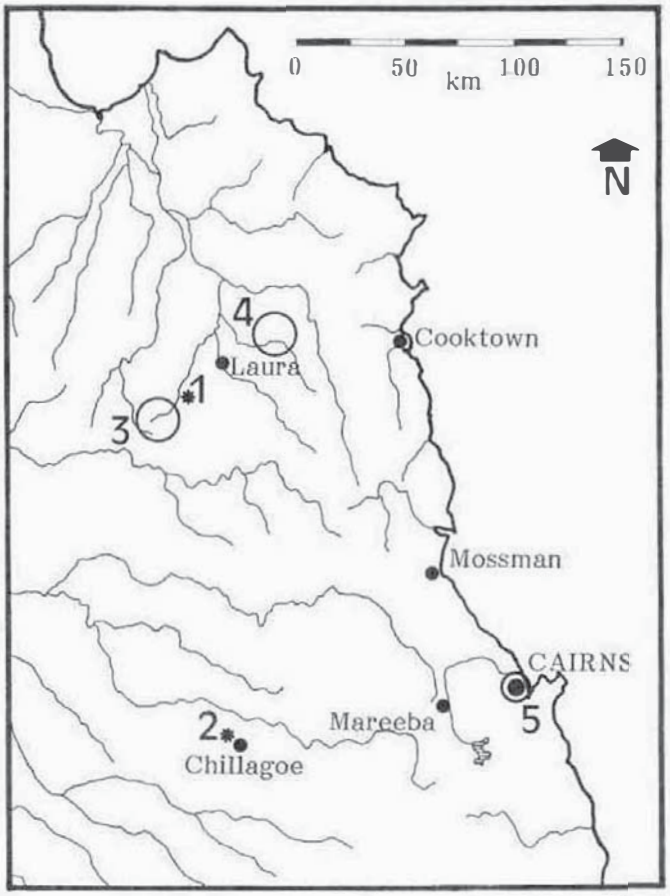
POSTSCRIPT. During the Second AURA Congress (Cairns 1992), Bruno David will lead field trips to the Chillagoe-Mungana rock art region.



Map of south-eastern Cape York Peninsula, Queensland.

1 - Sandy Creek (M. Morwood; see article below)
2 - Fern Cave (Bruno David; see preceding article)
3 and 4 - General areas to be examined by the initial AURA Explorers' Expeditions (see announcement on next page)
5 - Cairns: site of the 1992 AURA Congress

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The Archaeology of Aboriginal Art in S.E. Cape York: preliminary report on the 1989 fieldwork

MIKE MORWOOD

This was the first field season of a three-year project funded by the Australian Research Council, the University of New England and the Heritage Section of the Queensland Attorney-General's Department. The rationale for this research is detailed in an outline published in **RAR** 6(1): 71-2, but the main thrust of the work is to carry out a multi-disciplinary investigation of the prehistory of the Laura-Cooktown Basin, S.E. Cape York, in which the evidence of rock art is integrated with that from excavations (stone artefact assemblages, economic remains), detailed assessment of local resource structures by people from a range of disciplines and local Aboriginal communities, and ethnographic information, to investigate how *systems* of Aboriginal resource use in the area have developed over time. An essential component is the collection of evidence for palaeoclimatic change by the study of pollen sequences from the excavations and local swamps; and the study of silica skins, their age and the conditions of their formation.

Between 1 July and 21 August 1989, archaeological excavations were undertaken at four rockshelter sites on the headwaters of the Mosman and Little Laura Rivers near Laura. Three of the sites,

Red Bluff, Magnificent Shelter and Sandy Creek 1, occur in the Quinkan Reserves, the other, Yam Camp, occurs on adjacent Jowalbinna Station. In addition, an open artefact scatter on Shepherd's Creek was gridded, plotted in detail and sampled for comparison with rockshelter assemblages by Warwick Pearson. Faunal and botanical surveys, coring of swamps, interviews with informants, as well as recording of rock art by Noelene Cole were undertaken concurrently with the excavations and archaeological surveys. Fieldwork ended with reconnaissance in the wider Laura and Cooktown vicinities to identify sites/areas for investigation next year.

A total of 27 people participated in the project this year, clocking up a total of 583 person-days of fieldwork. Although the analysis of evidence collected during this field season is obviously at a preliminary stage, points of interest include:

(a) Some members of the Laura Aboriginal community still have detailed knowledge about the use and availability of local plant resources. In particular the contribution of Laura George in assessing some 250 plants collected by Gethin Morgan and Jenny Terrey during the terrain unit mapping was invaluable.

(b) Five swamps were cored by Dr Lesley Head and Karryn Stephens from the University of Wollongong, and at least one of these, Garden Creek Swamp on the Quinkan Reserves, has yielded pollen throughout. A mid-Holocene date has been obtained for the basal peat in the sample. This means that a pollen record for the locality will be available which begins prior to a period of major change in stone artefact technology, patterns of site use and artistic activity identified in the archaeological record. The Garden Creek palynological sequence will be the subject of a post-graduate degree by Stephens.

(c) Preliminary results from the excavations show clear trends in the association between rock art phases, stone artefact technology and patterns of site use at all sites. Ultimately, these should enable the functional interaction between a range of different cultural components in the regional prehistoric sequence to be demonstrated.

At Sandy Creek 1 the occupational sequence begins prior to 31 900±690 years BP and continues until the European contact period. It is therefore likely to document the entire time span of Aboriginal presence in the area. The uppermost of the many changes in this sequence are also represented at the other excavated sites. A joint paper by Percy Trezise and myself on the results of work at Sandy Creek is in preparation. [A paper dealing with the rock art will be prepared by Trezise - ed.] In addition, excavated bone and stone artefact assemblages will be the subjects of postgraduate degrees at the University of New England.

(d) Alan Watchman's work has shown that a silica skin, which overlies pecked petroglyphs at Sandy Creek 1 (refer Plate 8, Dr Flood's article in RAR 4(2): 103), definitely contains organic material and is therefore dateable. Other silica skin samples from sites in the area are currently being assessed.

In 1990, a similar multidisciplinary approach will be used. It is planned to investigate sites and swamps within the same general Laura-Cooktown region, but lying within very different resource contexts. This should enable the changes evident in the archaeological record to be compared, contrasted and explained in a more finely-resolved way.

The program will also involve larger-scale work at Sandy Creek 1, to increase the sample of 'cultural' material from the deepest deposits at the site and to investigate the stratigraphic relationship between a panel of petroglyphs and the occupational sequence. More detailed work on silica skins by Watchman should provide a minimum age for the panel, whereas further excavations should provide a maximum date.

Finally, it is hoped to incorporate into the synthesis evidence from several sites excavated in the past but never properly published (e.g. Platform Gallery). In some cases this may involve re-excavation.

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AURA EXPLORERS EXPEDITIONS

10-12 days in Quinkan Country, S.E. Cape York Peninsula, Queensland

Expedition 1:
7 to 16 April 1990
Expedition 2:
18 to 29 June 1990
Expedition 3:
24 July to 4 August 1990

Expedition Leader:
Percy Trezise
Assistant Leader:
Mary Haginikitas

Please direct all enquiries to:
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Australia

In preparation for the 1992 Second AURA Congress (see announcements on following pages), Congress Co-chairman Capt. Percy Trezise has decided to lead a series of expeditions into unexplored rock art regions of Cape York Peninsula, the largest remaining wilderness area of Australia.

AIMS: To locate and describe undiscovered rock art sites in unexplored sandstone gorge country south-west and north-east of Laura, from base camps on Pine Tree Creek and Deighton River.

CONDITIONS: Expeditions depart Cairns and return to Cairns; 4WD transport, tented accommodation, all meals provided. Bring only personal gear, back pack and sleeping bag. Participants must be fit enough to undertake four-day back-packing hikes in very rugged and remote terrain.

Share of expedition costs $A1050 per participant. There will be a limit of nine explorers on each expedition.

Further expeditions may be organised later in 1990, or in 1991, depending on the outcome of those announced here.

ORIENTATION

Preparations for the SECOND AURA CONGRESS: first progress report

The major event in prehistoric art studies and cognitive archaeology between now and 1996 will be held in Cairns, north Queensland, about mid-1992 (probably late August). It has been said that 'Darwin '88 will be a very hard act to follow, but if anything can do it, it will be Cairns '92!' (Paul Bahn) The plans for Cairns suggest that the Second AURA Congress will be spectacular indeed!

To begin with, attendance will be at least double that of Darwin (well over 600), and the international participation, which was excellent in Darwin, will increase significantly. The Congress will be preceded by the largest public exhibition of prehistoric art ever assembled, combined with a major media campaign to raise public appreciation of the indigenous cultural heritage, and to foster support for its conservation.

The Congress will be preceded and followed by a field trip program that is to differ from that of Darwin '88. Most field trips will be shorter, groups will be smaller, and in contrast to the 'experimental' program at Darwin, the Cairns field trips will be planned in more detail. Groups should not exceed 20 participants, and will be smaller still for certain sites or site complexes. The program will therefore be a massive logistic exercise—bearing in mind that many hundreds of congress delegates will be in the field at its peak. The Trezises (Percy, Stephen and Matthew) will plan the field trips in every detail, covering all rock art regions of Queensland.

A special feature will be two expeditions into unexplored regions of Cape York Peninsula, one before and one after the actual Congress. Teams of selected people will be led by Congress Co-chairman Percy Trezise, to search for new rock art sites in extremely remote and inaccessible areas. These field campaigns will involve the establishment of base camps in unexplored areas, and the use of helicopters to ferry researchers and equipment. In preparation for these 1992 expeditions, Percy Trezise will lead a series of reconnaissance expeditions, beginning in April 1990, with the specific purpose of establishing distribution patterns of undiscovered rock art in various regions. In July 1989 I conducted aerial reconnaissance with Percy Trezise and Mary Ilaginikitas, over some of the areas earmarked for exploration. Flying at low altitude we saw numerous sandstone cliffs and rockshelters, in fact I could just glimpse large red paintings on one such cliff. I was particularly struck by the frequency of surface water, which we observed not only in the valleys but in a few cases even on the plateaus. It is therefore realistic to expect that major bodies of rock art will be discovered during expeditions now planned (refer announcement on p. 156).

The Cairns Steering Committee met on 22 July 1989 and elected office holders (see *AURA Newsletter* 6/1 for report). The

initial members are Robert Bednarik, Noelene Cole (Vice Chairperson), Elaine Godden, Mary Haginikitas, Robert Reid (Public Relations Officer), Beverley Trezise (Secretary), Percy Trezise (Chairperson) and Stephen Trezise. This committee has recently been strengthened by the addition of two representatives of the Queensland Department of Environment and Conservation, Dr Nicky Horsfall (Regional Archaeologist) and Mr Bruce Butler (Laura ranger and traditional custodian). The Department has offered its assistance for the successful outcome of the Cairns conference, for which AURA is grateful. The committee will recruit further members as required and will meet at quarterly intervals.

In planning Cairns '92, our experience from Darwin will be very valuable, but I think Darwin's most precious legacy is the reputation established for the Congress. It seems unique in the scientific world that such an event can establish itself as a discipline's premier conference the very first time it is held.

R. G. Bednarik

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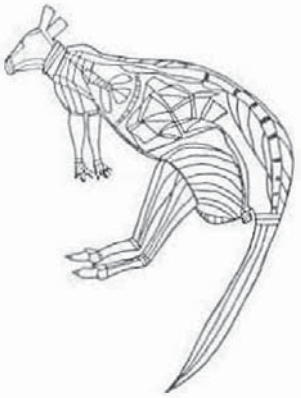
THE AURA EXHIBITION: CALL FOR PROPOSALS

AURA will assemble a public exhibition featuring the 'best' prehistoric art of the world. Proposals and rationales are now invited from individual researchers, institutes and organisations, for exhibits and collections suitable for inclusion in the AURA Exhibition.

AURA has enlisted the help of Australia's foremost museologist, Robert Edwards, and entered into an agreement with the Museum of Victoria to co-produce a 6000 m^2 exhibition on prehistoric art—the largest ever attempted. Featuring rock art as well as early portable art of the entire world, it will open in Melbourne in 1992, and then travel to major Australian cities before continuing abroad. It will be extensively promoted, and seen by millions of people. In view of the outstanding potential of influencing public attitudes to rock art, the AURA Exhibition is anticipated to have a profound effect on worldwide awareness of rock art and the need for its preservation. We intend to explore this potential fully.



AURA plans to solicit corporate sponsorship for this project, and there will be prizes offered for the best exhibits. Winners will be flown to Australia and will have the opportunity of attending the Second AURA Congress as guests of AURA. Entries will be judged by a panel of distinguished specialists, chaired by the Director of the Museum of Victoria, Robert Edwards. Quality of actual exhibits (photographs, transparencies, casts, originals, tracings, serigraphs) will not be the sole basis of appraisal, which will also take into account criteria such as method and integrity of presentation, including innovativeness and standard of accompanying text; conceptual arrangement of exhibits and explanatory graphics; originality and other aspects of the display. Submissions may range from a single outstanding photograph to an entire subject-determined module (for instance, 'Trans-Pacific migrations reflected in rock art'; or 'Methods of dating rock art'; or 'Depictions of altered states of consciousness in rock art' might be plausible titles of display modules). Previously exhibited material may be used, edited or rearranged.

The principal criteria for acceptance will be quality, innovativeness and scientific importance. Submitters should carefully design their exhibits for maximum adaptability and compatibility, to facilitate integration into the overall project. It is intended to guide prospective contributors by announcing 'dominant themes' as they emerge, which should greatly assist co-ordination.

Proposals are invited on this basis, and should provide the following details:

Name and address of submitting individual or institute.
Rationale: interpretational or rational basis for the proposed exhibit's design, topic or intent.
Description of exhibit, stating approximate quantities, size(s), type(s) etc.
Rough outline of proposed interpretative text and graphics.
Other relevant details.

We envisage that existing displays, archival material and museum collections will be utilised for some submissions; other proposals are expected to be in the form of blueprints for topic-based exhibits that are to be assembled over the next two years, illustrating the results of current research work. It cannot be emphasised enough that innovative proposals of presentation are of particular interest to us. At this stage a closing date is not nominated for draft submissions, but an early submission of proposals would greatly assist in planning because these may themselves provide an impetus or catalyst for ideas of others.

No exhibits are to be submitted at this stage. Please send your draft proposal to:

The Editor, AURA, P.O. Box 216, Caulfield South, Vic. 3162, Australia

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Notices

ANNUAL AURA MEETING 1989. Held on 11 and 12 November 1989 at North Head, Sydney, this conference was extremely productive. The Rock Art Dating Consultation announced in the September 1989 issue of *AURA Newsletter* led to a blueprint for future dating work in Australia and to the spontaneous endorsement of a conglomerate project, to be known as the Australian Rock Art Dating Project (AURADATE). About ten different dating methods may be involved, most participating projects will be of geochemical orientation, and the integrated project will be a long term one. A detailed announcement will appear in the next issue of RAR.

SARARA, our sister organisation covering the southern African subcontinent, invites contributions for the organisation's journal, *Pictogram*. Articles should be relevant for southern African rock art students and should preferably be short and well illustrated. Please submit contributions to the Editor, SARARA, P.O. Box 81292, Parkhurst 2120, South Africa.

DARWIN CONGRESS PUBLICATIONS. Editorial work is nearing completion and it is hoped that the first volumes will shortly go to press. Partial funding of printing costs has just been secured for five symposia, and it is hoped that the balance can be approved shortly. Order forms will be circulated in 1990.

The ROCK ART ASSOCIATION OF CANADA (RAAC) has been formally announced, and an interim executive has been formed on 28 September 1989, comprising Professor Jack Steinbring (President), Professor Anthony P. Buchner (Vice President) and Michael Daly (Secretary and Treasurer).

WELLMAN AWARD. The American Rock Art Research Association (ARARA) has established a prestigious award in the name of the organisation's first president: the Wellman Award for Distinguished Service. The first recipients are Dr Frank Bock and A. J. Bock, 'for 15 years of dedicated service in all aspects of ARARA's operations'. Dr Bock edits ARARA's journal, *La Pintura*, and Mrs Bock has been the organisation's secretary and treasurer for many years. RAR congratulates the recipients!

The recently formed ASSOCIATION FOR THE ANTHROPOLOGICAL STUDY OF CONSCIOUSNESS (AASC) is a multidisciplinary organisation of academically oriented professionals and students dedicated to the diverse, rapidly growing and significant field of the anthropology of consciousness. Some current areas of interest include: states of consciousness; shamanistic and spiritual training and practices; non-medical healing methods and philosophies; and paranormal and anomalous phenomena. AASC holds annual conferences and produces a quarterly publication. For information, please write to AASC, P.O. Box 1391, Venice, CA 90294-1391, U.S.A.

IFRAO Report No. 3

R. G. Bednarik

Two more societies have recently joined the International Federation of Rock Art Organizations. They are (together with name and position of principal representatives and official postal addresses):

■American Rock Art Research Association (ARARA)
A. J. Bock (Secretary)
P. O. Box 65
San Miguel, CA 93451-0065
U.S.A.

■Institutum Canarium
Professor Herbert Nowak (General Secretary)
Postfach 48
A-5400 Hallein
Austria

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Professor Anati has advised that the Centro Camuno di Studi Preistorici would join IFRAO soon, requesting clarification of constitutional aspects to finalise affiliation of the Comité International ICOMOS pour l'art rupestre also (Professor Anati presides over both these bodies).

The executive of the Rock Art Association of Manitoba has been appointed as the interim executive of the newly formed Rock Art Association of Canada on 28 September 1989. The national organisation is headed by Professor Jack Steinbring as President, and Professor Anthony P. Buchner takes over as President of RAAM. The incorporation of the new Canadian association will not be finalised until early 1990. At about the same time the founding of the Rock Art Research Association of the Soviet Union is expected to take place at the Academy of Sciences of the U.S.S.R. Besides the applications from these two prospective new members, IFRAO is expecting submissions from at least two other rock art associations in the near future.

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The IFRAO Member Centro Studi e Museo d'Arte Preistorica plans a major exhibition: '**Rock Art in Europe - prehistoric farmers, shepherds, warriors, from the Urals to the Atlantic**'.

The exhibition, under the patronage of the European Community Organizations, will open at the Mountain National Museum in Turin, Italy, in 1990, and then tour Europe. It will be divided according to geographical areas and involve the cooperation of the principal researchers working in Europe. The Centro invites submissions synthesising the post-Palaeolithic rock art of specific European regions, and seeks the collaboration of IFRAO member organisations in securing appropriate exhibits, including cartographic material, drawings, recordings and colour transparencies (slides).

Please forward your enquiries and submissions to:

Centro Studi e Museo d'Arte Preistorica
Viale Giolitti, 1
10064 Pinerolo (TO)
Italy

Dr Piero Ricchiardi, President of the Centro
Professor Dario Seglie, Director of the Museum

The following article is a contribution to the debate initiated by Dr Maurice Lanteigne in the *First IFRAO Report*, on the subject of an international archival data sharing network for rock art.

ARCHIVAL DATA SHARING NETWORK: A RESPONSE

CLEMENT W. MEIGHAN

In the *First IFRAO Report*, in RAR 5: 176, Dr Maurice Lanteigne presented a proposal for an IFRAO archival data sharing network. Our call for debate of this proposal has so far elicited only a few responses. This important proposal for an international, computer-based network will need to be considered and discussed for some time, and further responses are requested from specialists involved in this field, and from readers offering suggestions. Here is a comprehensive discussion of Dr Lanteigne's proposal by Professor C. Meighan, Director of the UCLA Rock Art Archive in Los Angeles.

Since the Rock Art Archive of the University of California, Los Angeles, is a public resource, we are in the business of sharing data and being used by scholars in rock art. This is why this archive exists and what we do on a daily basis. In principle, therefore, we are supportive of the free exchange of scholarly information. I think this requires a lot of discussion and planning, however, to be made operational on the scale envisioned in the IFRAO planning. Our experience in our many requests for data has shown us some of the practical problems and traps in general distribution of information, some of which are recognised in Lanteigne's article and are discussed below. In addition to these, however, I mention the following:

(1) We are restricted in the use of some of our data by the wishes of the donors. For example we have a lot of Australian material which is sensitive because tribal informants would not approve if the information were being disseminated or published. Other donors have publication projects under way and do not want their manuscript notes made available until they have had a chance to see their own work into print. Most of these restrictions are matters of delay rather than suppressing information, but they do involve some responsibility on our part. Transmission of large amounts of data to third parties would jeopardise our ability to meet the terms under which we acquired the data.

(2) We try do do a check on the *bona fides* of people who use our material (they fill out a form, etc.) to prevent site locations from falling into the hands of pothunters, potential vandals and others whose interest in these sites may not be scholarly. We are in fact compelled by state regulations not to publicise site locations except to legitimate researchers. For this reason we do not honour requests from county planning commissions, utility companies etc., who want to have copies of 'all the records' on certain counties or districts. We have no control over records that are in the hands of non-scholars, and we have some evidence that such records are routinely misused.

(3) With the proliferation of contract archaeology, there are now numerous contract projects involving rock art recording and conservation. If the contractors use our records to make profit for themselves we are subsidising their work and receiving no compensation. Individual contractors can get paid more money for a project than our budget has for several years of maintaining the archive. This is very different from a scholar who is writing a research paper as part of his scholarly work, receiving no payment for it. Our policy is that if the person using the records is receiving payment for the project concerned, he or she should pay the appropriate search fees and contribute to the cost of the thousands of person-hours that went into accumulating the data base that is being exploited for profit. Non-paid research does not involve any fees or contribution and here we are happy to serve as a scholarly resource for academicians.

These points all have to do with scholarly responsibility, which is difficult enough to maintain within one's own set-up and could be impossible with massive distribution of files. Certainly a beginning step is to set up some controls based on scholarly ethics to which all participants would be bound.

However any exchange is worked out, it takes time and money to transfer information. If everything is already computerised it takes less time and money but there is still a substantial outlay required, which can be very great if someone has to enter a lengthy manuscript in a computer before it can be transmitted. Ultimately we will have everything computerised but that is a long-term, ongoing job and I doubt whether any rock art archive of significant scope has its data in computer form. For the present, the most economical transfer mechanism is the photocopy (xerox copy), and that works fine provided that the amount of material being moved is not excessive.

Some comments on hardware and software. Initially the various centres need only two things: a modem to transfer text material, and a fax machine to transfer pictures. This would enable any centre to use any computer and would require no standardisation or costly investment. Basically this would be a BBS (Bulletin Board Service) that would allow transmission of data, messages and inquiries. Some more sophisticated set-ups will allow transfer of graphics from computer to computer, but this requires considerable compatibility of both hardware and software.

The problem with computerising rock art material is the graphics: since we are dealing with much pictorial material it is essential that the pictures go with the text. Graphics require a lot of computer space, and the technology is moving so rapidly that I have been reluctant to fix on a particular method of computerising; every year there is something new that vastly increases the options for storage and transmission of graphics. Now we have not only laser disks and computerised videotape, but WORM drives and CD-ROM drives, the latter available for microcomputers and storing gigabytes of information. Whatever is done requires a lot of investment, however, and this needs to be thought through by the individual centres. It seems to me highly unlikely that there will ever be a standard system shared by all centres. For political reasons, not all machinery is the best choice for all countries; what is cheap and easy in the U.S.A. may be difficult and expensive in France, and vice versa.

Finally, of course, the cost of the organisation has to be justified in terms of the results obtained. Is it really valuable or necessary for a rock art centre in Sri Lanka to have copies of all our records on Inyo County, California? We can accumulate huge piles of data but if there is not a clear reason for having it, it may be mere proliferation of paper. Our experience has been that most of our users want pretty specific information and are looking for particular kinds of comparative or regional data. Individual users rarely want copies of more than a couple of hundred pages of manuscript and records, and on that scale the photocopy is still the most economical way to provide the material. If everything were computerised I can visualise a situation in which it would be very valuable to call up all the footprints, say, or some other element of interest, but even here I think the researcher would want selected information, not the whole database. Again, a computer link would perhaps do the job since one could query several centres for data and put together what was needed for any specific research project or program.

There have been at least two or three attempts to develop a more widespread system of storing and transmitting rock art data, which have foundered because of the difficulties of planning and organisation. One of these came up at a conference in Little Rock a few years ago where Arkansas was going to lead the way with some kind of massive laser-disk system which was to integrate all the rock art research. This did not proceed because the money was not made available. It looked great at the time but would now be considered obsolete since better systems are available. Certainly the idea of centralising everything in one institution is not realistic; no one institution is going to support rock art studies (a low-priority budget item) if requests start coming in for massive funding.

The general point of all this is that I am for the idea of information exchange but see a number of hurdles to overcome. I suspect that this proposal will not get very far until the various collections and archives can get together and agree on what it is that they are going to do and how they are going to do it. I also think that this is a multi-phase enterprise which will require some years of commitment and that it would be a good idea to begin with some rather simple procedures and build up to the more complicated aspects of data transfer as the communication links are developed and improved.

Professor Clement W. Meighan
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The Institute of Archaeology
University of California, Los Angeles
Los Angeles, CA 90024
U.S.A.

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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 contain the original, together with one copy, typed in double-space, with a wide margin on one side of each page. Please underline words to be italicised, and identify each page by number and the author's surname. The content of the paper should be outlined by three to five keywords (e.g. 'Petroglyphs - patination - style - Pilbara') placed above the title. The manuscript should 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*; in the few instances where the two disagree the former has precedence. Footnotes ought to be avoided where possible. 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 thicknesses, stippling, lettering sizes etc. must be selected accordingly. Photographs should be black and white gloss prints of high contrast. 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.

There are no deadlines for specific issues of this journal, but announcements intended for an issue ought to be available at least two months before the month of intended publication. Proofs are issued of all articles. Each author, or group of co-authors, receive thirty free copies of the article in question; additional reprints are available at cost.

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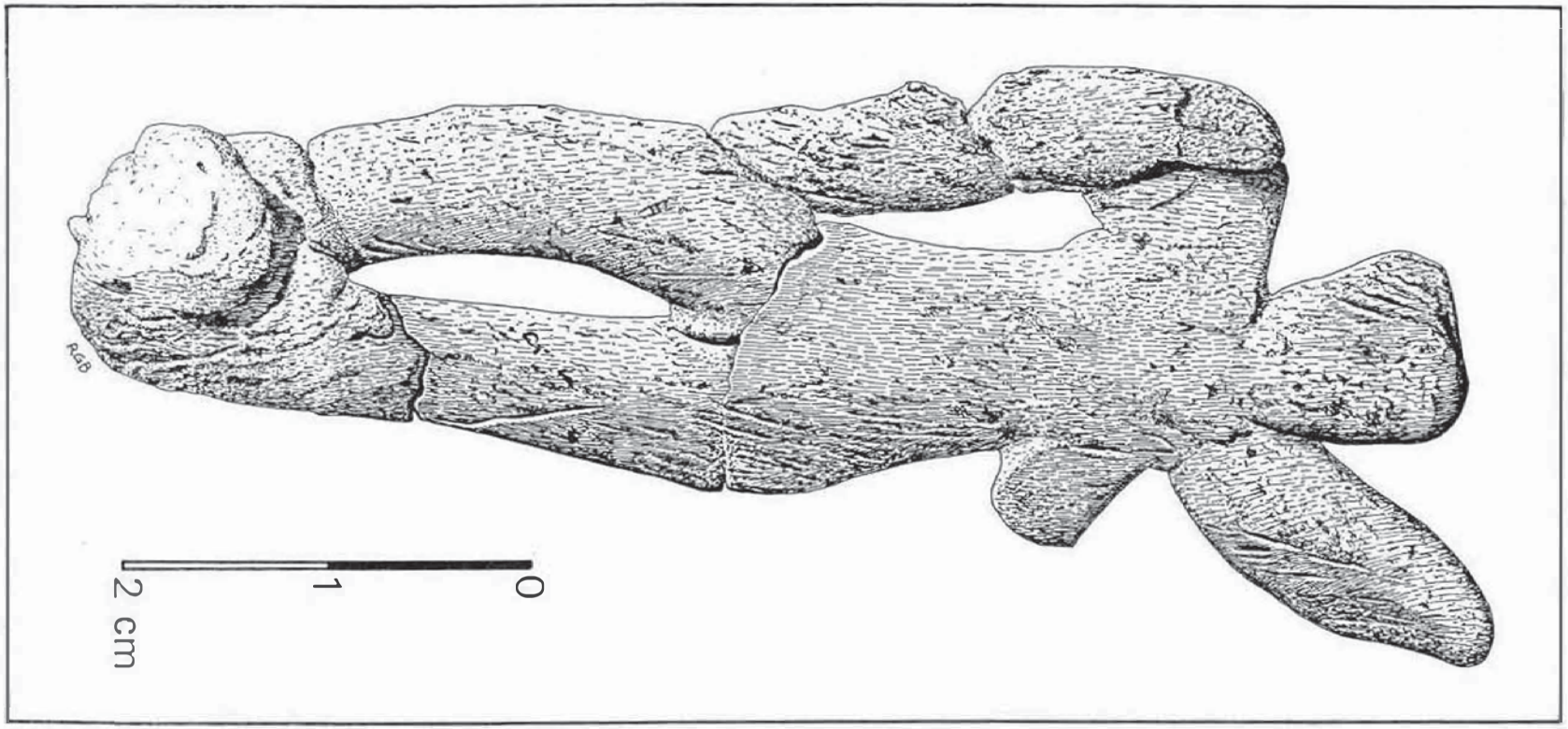

the oldest known stone sculpture