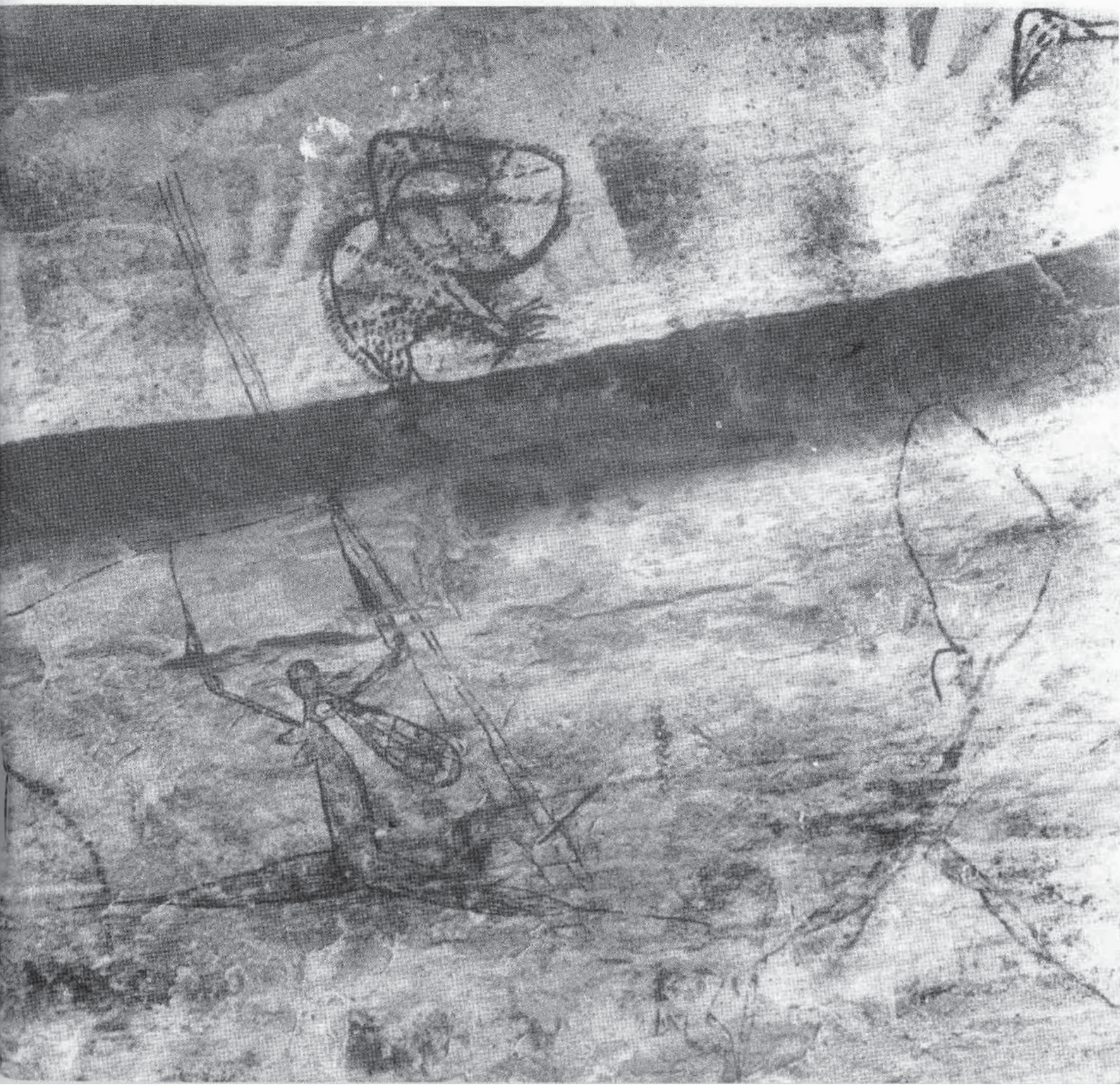
Registered by Australia Post -Publication No. VBP6658 ISSN 0813-0426

THE AUSTRALIAN ROCK ART RESEARCH ASSOCIATION (AURA)

ROCK ART RESEARCH

Volume 3. Number 2 NOVEMBER 1986



The journal *Rock Art Research* is devoted to developing theory and methodology for the systematic and rigorous understanding of this form of human expression. It is supposed that rock art is the major surviving record of the nonmaterial aspect of prehistoric cultures, that which primarily defines any culture. Rock art is believed to be better suited than the study of the material aspects of prehistoric life, for detecting cultural change or continuity.

Although this journal is concerned principally with the Australasian region, the subject served by it is characterised more by its goals and approach than by its geographical bounds. Emphasis is given to communication across the various disciplines related to the study of rock art, and to synthesising related subjects around its focus: the surviving externalisations of prehistoric world views.

Contributions should be consistent with these general goals. Notes for contributors can be found on the inside of the journal's back cover. All articles submitted will be refereed; authors will receive a summary of the referees' comments, plus an editorial view. While final responsibility for the acceptance or rejection of an article rests with the Editor, responsibility for opinions expressed, or data introduced, always rests with the author.

Selected manuscripts will be sent to international or Australian commentators for reviews which may be published in order to promote scholarly debate. Where appropriate, the author may be requested to respond to these comments in the spirit of the involvement and discussion for which AURA stands. In addition to articles reporting original research, the submission of short reports, reviews, abstracts and bibliographical entries is also invited.

Rock Art Research is published twice a year, in May and November. The Australian recommended retail price per issue is $A 12.00

Annual subscription for Subscribing Members of the Australian Rock Art Research Association (2 issues, post paid) is $A 10.00

Full membership with the Australian Rock Art Research Association is **$A 15.00 per annum.** Besides journal subscription and constitutional rights, full membership entitles one to receive the *AURA Newsletter* and to take advantage of reduced prices for the monographs which the Australian Rock Art Research Association may publish from time to time. Please make all cheques payable to Archaeological Publications.

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The Editor
Rock Art Research
P.O. Box 216
Caulfield South, Vic. 3162
Australia

Rock Art Research is owned and published by Archaeological Publications, a non-profit company incorporated in May 1983.

ROCK ART RESEARCH

The Journal of the Australian Rock Art Research Association (AURA)

ISSN 0813–0426

Volume 3, Number 2

Melbourne, Australia

November 1986

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 immovable cultural heritage, particularly prehistoric rock art.

Archaeological Publications, Melbourne

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aura

FRONT COVER: Paintings of the dynamic figures style, Kakadu National Park, Northern Territory, Australia. Running female figure carrying spears and dillybag; above several stencils and the image of an emu, at left the neck of a large emu figure. Pre-estuarine Period (possibly Pleistocene).

KEYWORDS: Palaeolithic sexuality - Iconographic interpretation - Western Europe

NO SEX, PLEASE, WE'RE AURIGNACIANS

Paul G. Bahn

'I often think how comforting life must have been for early man because he believed in a powerful, benevolent Creator who looked after all things. Imagine his disappointment when he saw his wife putting on weight'.

Woody Allen, Side Effects

Abstract. For the past seventy-five years scholars have followed Breuil in interpreting certain early Upper Palaeolithic motifs as vulvae. This paper examines this hypothesis and finds it to be based on erroneous reasoning as well as on wishful thinking. Definite depictions of female genitalia are extremely rare in Palaeolithic iconography, a fact which casts considerable doubt on theories which claim that sexuality played a dominant role in the religion and sign-system of the period. It is argued that the 'vulva' interpretation has been applied indiscriminately to a wide variety of motifs, and is not only subjective but also tautological. Other interpretations are certainly possible, and in any case the motifs need to be treated more objectively, and differentiated rather than lumped together under one simplistic heading.

Introduction

One of the most enigmatic areas of Upper Palaeolithic art is its depiction of women. The fat little statuettes, for example, have caused a great deal of ink to flow, although the homogeneity of their style over vast distances can blind us to the fact that they are relatively few in number, and mostly grouped in a handful of sites. They have inspired numerous theories concerning mother goddesses, and have contributed much towards a belief in the importance of fertility and sexual symbolism in Palaeolithic iconography. One of the linchpins of this belief is the supposed dominance of vulvae among the earliest known Palaeolithic depictions. In this paper I would like to pursue some thoughts inspired by the Dellucs' recent (1978) exhaustive re-examination of these engravings, and to suggest that one subjective interpretation of certain shapes has become an *idée fixe* and one of the most durable myths of prehistory.

Birth of a Notion

The motif in question has been found on stone blocks in seven Aurignacian sites, all located within a fifteen kilometre stretch of the Vézère valley in southwestern France, and indeed many are concentrated in four sites within five kilometres of each other (see Delluc and Delluc 1981). An apparently similar series was found more recently, painted on a wall in the cave of Tito Bustillo in northern Spain (Fig. 1), as will be seen below. The French specimens, on the other hand, consist of wide, deeply engraved lines, clearly made very deliberately.

The first examples were discovered in 1910; troubled by some of the ovoid and subtriangular figures he had found, Didon consulted the abbé Breuil, who decided that they represented vulvae; the term he used was *Pudendum muliebre* (see letter to Didon, 1911, in Delluc and Delluc 1978: 239). Thus Breuil, 'with the completely unique skill in deciphering prehistoric mysteries characteristic of him, recognized vulvas without hesitation, at the first glance' (Stoliar 1977/8: 42); and, since then, scholars have shown a high degree of unanimity in interpreting the meaning of these motifs in exclusively sexual terms:

> To be more specific, they saw in them indubitable representations of vulvas and nothing else. This hypothesis, which has existed for over half a century and become a commonplace in numerous publications, retains to this day its unshakeable status as something self-evident (Stoliar, *ibid.*).

With tongue in cheek, one might point out that a man of Breuil's profession should not be considered an expert on this particular motif; but it is interesting to examine carefully the reasoning behind his interpretation. In his card to Didon (Delluc and Delluc 1978: 239) he claimed that *'En cunéiforme primitif et en Egypte, c'est bien le signe de la femme. Encore dans les classifications zoologiques . . . c'est presque la même chose'.*

The symbols used in modern zoology for male and female can hardly be considered relevant to Aurignacian figures, and in any case I can

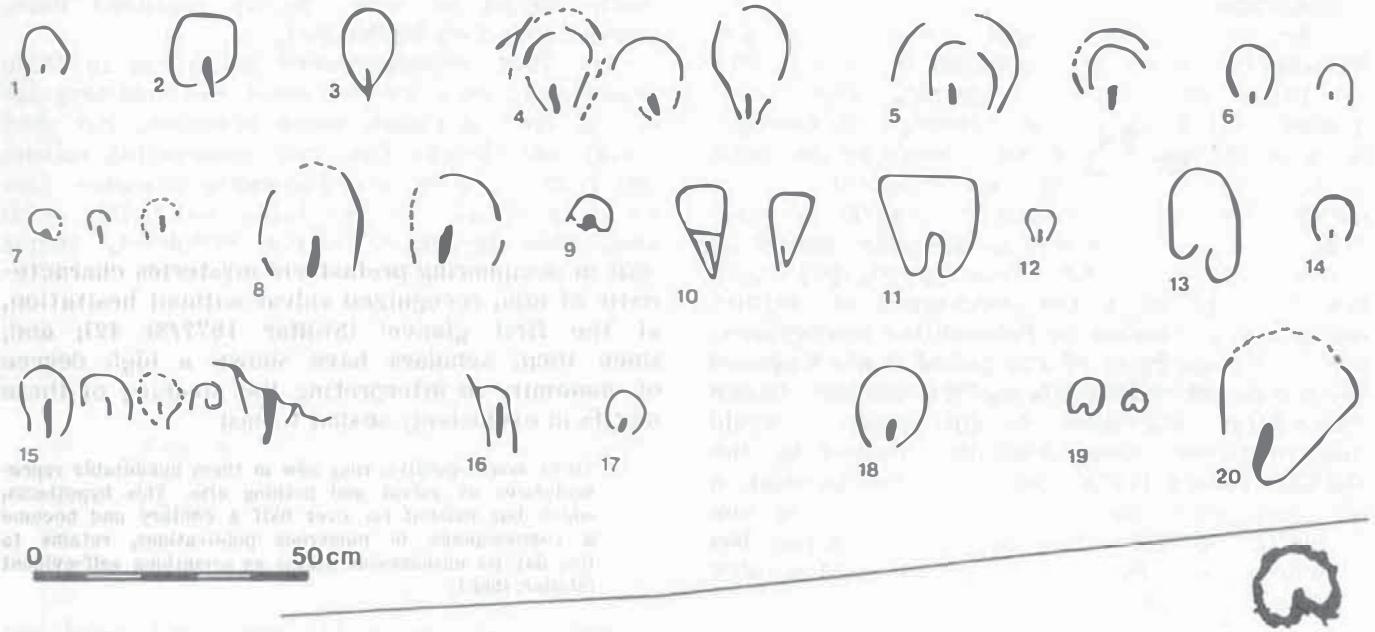
MS received 4 July 1986

see no resemblance. In early Cuneiform, the relevant symbol is an inverted triangle with a median line (A. Millard, pers. comm.), as one would expect. There is nothing remotely similar to the Aurignacian motifs in Egyptian hieroglyphics or even Egyptian art (A. Shore, pers. comm). Indeed, the Egyptians were somewhat reticent about representing this part of the female anatomy, even in blatantly sexual scenes (e.g. see Omlin 1973; Manniche 1977). Of the predynastic figurines presented by Ucko, only nine percent of the female examples are shown with sexual triangles (Ucko 1968: 192), and none with anything like the shapes Breuil was interpreting.

It appears, therefore, that Breuil was talking through his beret; the ethnographic parallels with which he supported his interpretation were erroneous or imaginary. Being only human, he saw what he wanted or expected to see: e.g. he also had a tendency to see stylised fish in a great many Palaeolithic motifs, and it is amusing that, at times (Breuil and de St Périer 1927: 130), he claimed that a fish tail motif also represented the female organ! Examples abound of the subjectivity which crept into his work, as into every scholar's: one such example is his copying and interpretation of certain animal figures in Spanish Levantine art as Pleistocene species, to support his view of the art's date (see Almagro 1952). Such errors detract little from his stature as a scholar, though they do invite caution; of course, one finds the same in the work of his contemporaries.

Peyrony, for example, was a keen supporter of the sexual symbol idea; in addition, as the Dellucs (1978: 267) have pointed out, he always wanted to find better material than Didon. The recent reanalyses by the Dellucs have shown clearly that, even allowing for wear of the blocks since their discovery, Peyrony's *relevés* are often far from accurate. Some are distorted, exaggerated; he saw vulvae, and so, in places, he finished 'incomplete' lines. In some cases (e.g. Abri Cellier—see Dellucs *ibid.*: 345) his interpretations were painted onto the blocks, and many subsequent publications have included photographs of these painted lines rather than of the original engravings. In fairness, Peyrony's *relevés* were described as schematised, but this has not prevented them from being copied and recopied, and thus contributing heavily to the myth of an Aurignacian obsession with vulvae. It is worth mentioning that some of Peyrony's interpretations of animal figures on these blocks are very farfetched, but have also entered standard works on the origins of art. In future, only the analyses by the Dellucs can be considered reliable.

Nevertheless, the Breuil/Peyrony view was taken up unquestioningly by many subsequent scholars, and was a major factor—albeit one of many—in recent theories of male/female opposition in Palaeolithic art, and in particular Leroi-Gourhan's concept of the development and complementariness of male and female signs. It is not my purpose here to attempt a critique



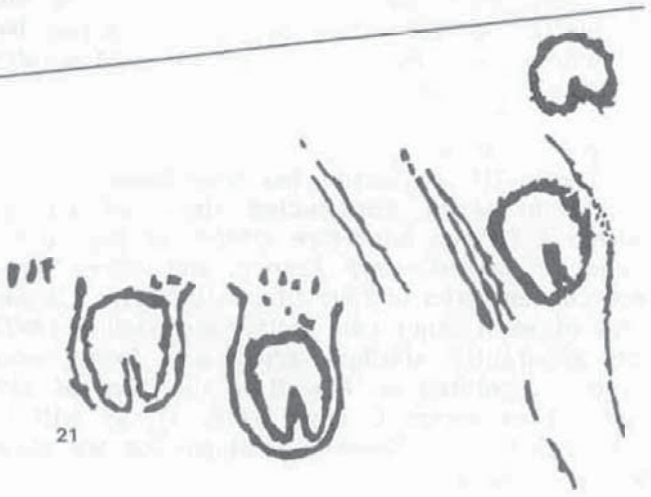


Figure 1.
Alleged Aurignacian 'images génitales féminines':
1 - 4 Abri Blanchard
5 - 6 Abri Castanet
7 - 12 La Ferrassie
13 - 16 Abri Cellier
17 - 19 Laussel
20 Abri du Poisson
All engravings on stone blocks, after Delluc and Delluc 1978. Scale as shown.
21 Tito Bustillo; parietal paintings, after Beltrán 1972. Not to scale.

of such theories, which have been a tremendous stimulus to the field of Palaeolithic art. Despite the extreme scarcity, if not complete absence of copulation scenes (see Bégouën and Clottes 1984; Jordá 1983: 270; Nougier and Robert 1974), and the rarity of direct indicators of sex in human and animal figures (*pace* Guthrie 1984: 59), it can hardly be doubted that sexuality must have played a role in the religion and rituals of Palaeolithic people.

However, the point I wish to discuss is whether it was one of their fundamental preoccupations, and, more specifically, whether the Aurignacians of a tiny corner of France were obsessed by female genitalia. As Onians (Collins and Onians 1978: 11) has pointed out, no later culture, apart from one or two very isolated exceptions, has been known to give such prominence to the vulva; this fact alone should make us cautious, whereas the same author proceeds to wax lyrical about the act of engraving the Aurignacian figures reflecting the 'digital activity of lovemaking around the labia'! His view has been criticised by Gimbutas (1981), who also points out the obvious fact that we do not know the sex of the Aurignacian artists.

The Pudenda Triangle

Breuil's view was not based on decipherment, nor developed through debate. As Stoliar (1977/8: 43) has said:

> The meaning of the signs was not derived by analysis, but was merely proclaimed on the basis of a vague, prejudiced, logically and visually unverified process of association, which, let us admit, remains incomprehensible and mysterious. Everything that followed was then based on these traditional, a priori definitions, which appeared to confirm ideas dating back to the beginning of the nineteenth century as to the dominant role of sexual factors in all human life in primeval times.

There seems to be tautology in the reasoning involved: the figures are assumed to be vulvae, from which an obsession with sex is inferred, the evidence for which is the vulvae. Independent data are required to support the hypothesis; but when one searches, vulvae are remarkably hard to find in Palaeolithic art. If one allows that the only *definite* vulvae are those found in context, that is, in full female figures, then they are very few in number: it is surprising to find that among the 'Venus figurines' (Delporte 1979), often seen as proof of an intense interest in female sexuality, very few have the pubic triangle marked, and even fewer have the median cleft (Willendorf, Chiozza, Grimaldi; see Fig. 2), despite Guthrie's claims (1984: 67). Yet Zervos (1959: 37) claimed that *'Ce qui est mis de leur corps en évidence, c'est le sexe, toujours bien souligné'*, and Collins (1965) 'proves' the interpreta-

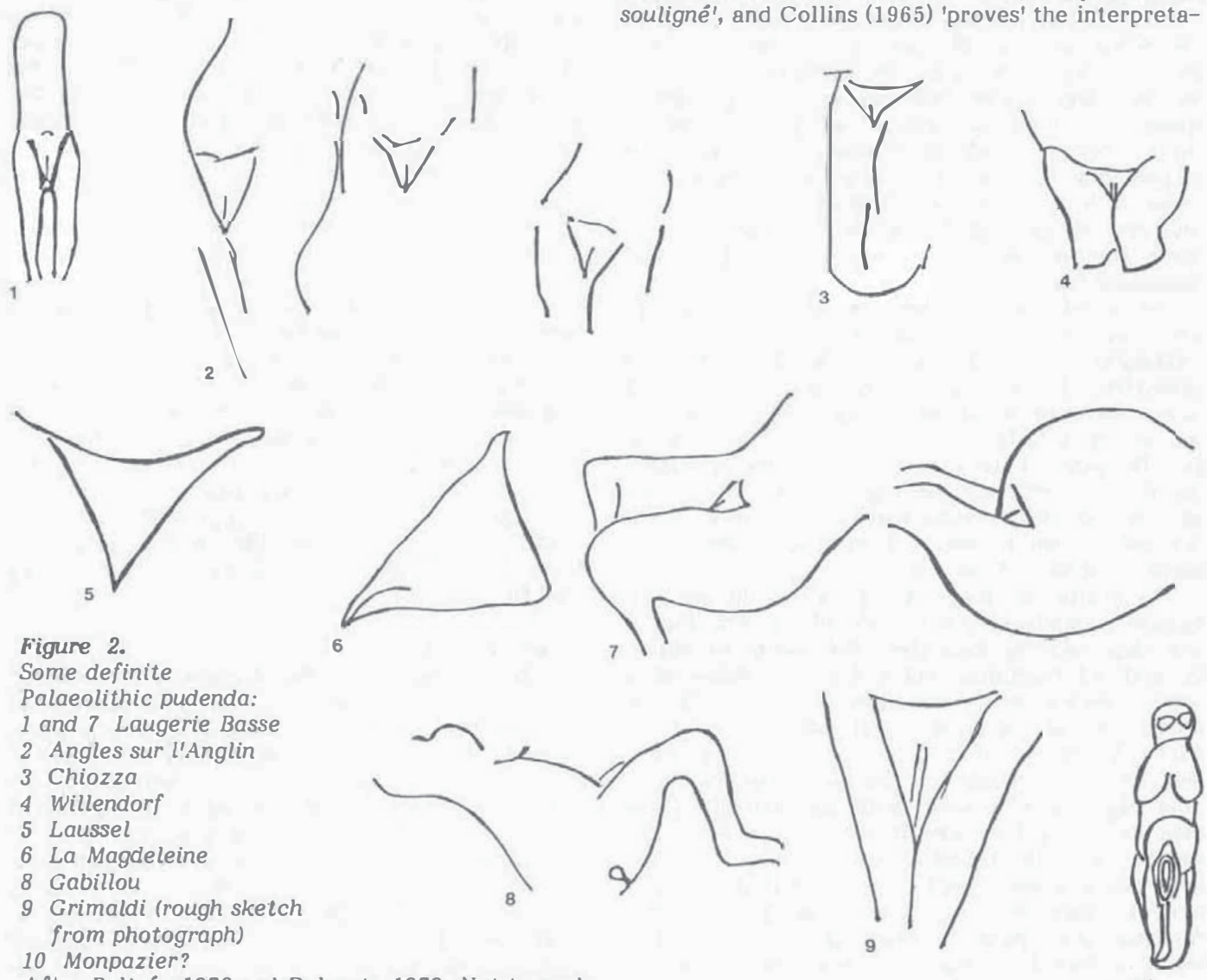


Figure 2.
Some definite Palaeolithic pudenda:
1 and 7 Laugerie Basse
2 Angles sur l'Anglin
3 Chiozza
4 Willendorf
5 Laussel
6 La Magdeleine
8 Gabillou
9 Grimaldi (rough sketch from photograph)
10 Monpazier?
After Beltrán 1972 and Delporte 1979. Not to scale.

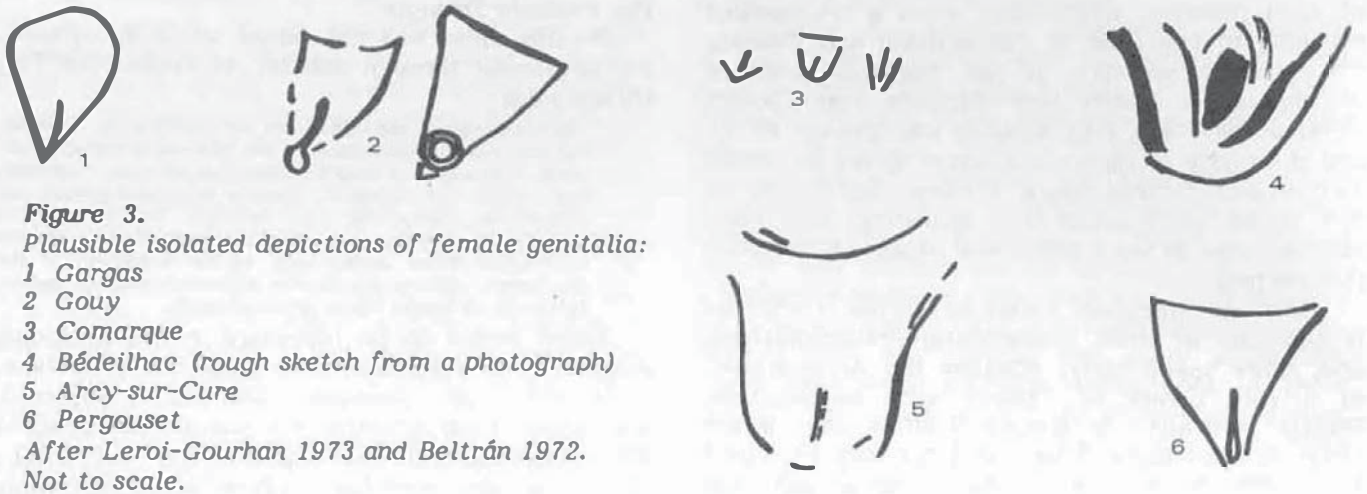


Figure 3.
Plausible isolated depictions of female genitalia:
1 Gargas
2 Gouy
3 Comarque
4 Bédeilhac (rough sketch from a photograph)
5 Arcy-sur-Cure
6 Pergouset
After Leroi-Gourhan 1973 and Beltrán 1972.
Not to scale.

tion of Aurignacian vulvae by reference to the female statuettes, and indeed claims 'it would be pedantic to doubt the connection'.

In fact, these figurines accentuate the breasts, buttocks and hips; they may represent fecundity, but they do *not* draw attention to the vulva. The one exception is that of Monpazier, with its excessively large, gaping oval, but this figurine is very unusual, was found outside of any stratigraphic context, and some doubt still persists as to its authenticity.

Most of the known figurines have no firm chronological context, but on the basis of two or three which do, they are generally attributed to the late Gravettian period, though Breuil (1959: 15-17) claimed that those of Brassempouy came from a Châtelperronian layer. Even in Magdalenian art definite vulvae are very scarce: three or four at Angles-sur-l'Anglin, one at Gabillou, one at La Magdeleine, one on the Laugerie Basse figurine and one on the 'Femme au Renne' engraving from the same site.

In short, where definite, realistic vulvae are concerned, the people of the later Upper Palaeolithic seem to have accorded them scant attention in their art. Could the Aurignacians have been radically different? They were, of course, a totally different culture: indeed, as the Delluc (1981: 14) have reminded us, from Aurignacian art to Lascaux is the same span of time as from Lascaux to us. But why should the earliest art represent such a basic and simple shape in so many bizarre ways?

As mentioned above, almost all of the definite female pudenda known in Palaeolithic art (Fig. 2) are depicted by triangles: the main exception is that of Gabillou, which has the shape of a small droplet. When one looks at female figures from a variety of periods and cultures (e.g. Ucko 1968; Neumann 1963; Gimbutas 1982) one finds that the few pudenda are represented by triangles. This shape is, of course, both anatomically accurate and easy to draw. It is certainly easier to engrave a simple triangle, with or without median line, on rock with stone tools than it is to trace a neat curve, oval or circle. The fact that the Aurignacians chose to draw these shapes—only three or four real triangles are known, at Ferrassie—suggests that they had a specific reason for doing so; it cannot be assumed that they were incompetent at drawing triangles.

A few scholars (e.g. Delluc and Delluc 1981: 18) accept that the 'realistic, easily identifiable' examples are rare, and limited to those at Ferrassie. A variety of excuses has been put forward for the other shapes in a desperate bid to fit them to the chosen interpretation: e.g. 'incomplete vulvae', 'squared vulva', 'broken, double vulva', 'circular vulva', 'relief vulva' and even 'trousers vulva' (Collins and Onians 1978)! Peyrony claimed one figure on a block from Abri Cellier was a *'triangle suspubien à angles adoucis'*, while those on either side of it were more deformed but represented the same thing (quoted in Delluc and Delluc 1978: 345); the Dellucs themselves mention 'unfinished vulvae', 'atypical vulvae' and even (*ibid.*: 353) interpret a single straight line as an isolated vulvar cleft!

However, while explaining away these shapes as variations on a triangle, scholars have also considered them anatomically accurate: thus, for Delporte (1979: 46) *'il s'agit de contours, subtriangulaires ou ovalaires, entourant le relief du Mons Veneris'*, while the Dellucs (1978: 400) are more specific, seeing the ovoid or circular examples as an *'aspect périnéal observé, les cuisses étant en abduction'*: thus the oval, as on the Monpazier figure, would represent the outline of the *labia majora*. Beltrán (1972) interprets lines on either side of a 'vulva' at Tito Bustillo as a female profile, even though the vulva is seen from the front, and is of the wrong size for the profile.

Other Interpretations

One problem with the Aurignacian engravings of stone blocks is that one does not know which way up they were meant to be seen, or even whether it matters. Certainly, on some blocks the figures seem to point in different directions. This does not, of course, apply to the group of four on the wall of Tito Bustillo cave, which all point more or less 'downward': but it is as risky to extrapolate this orientation to *all* those on the blocks as it is to extrapolate the interpretation and the date of the block figures to those in the Spanish cave.

Another problem, applicable equally to the

cave and the blocks, is that of why a single motif should have been made in so many different varieties, side by side, and more or less contemporaneously. To those who support the vulva interpretation, this is a case of 'heads I win, tails you lose'—if all the motifs looked like realistic vulvae, there would be no argument; as it is, those which do not are considered to be incomplete, stylised or simply views from a different angle.

Ethnography offers little help in the predicament. Similar motifs are found in profusion in the Neolithic of northern Iberia, on the Pedra das Ferraduras (Kühn 1971: 75), or on blocks in Bolivia (see Giedion 1965: 143); unfortunately, their makers and hence their meaning are just as dead as those of the Aurignacian 'vulvae'.

In any case it is known from studies of, for example, Aboriginal art that two indistinguishable pictures can have meanings which are totally different, while two drawings of the same thing can, in our eyes, have nothing in common. We have no way of knowing what these motifs meant to the Aurignacians, or whether—or why—they are variations on a theme.

However, it should be noted that alternative interpretations have occasionally been put forward: for example, Bourdier made a very pertinent comparison between some of the Aurignacian motifs and the print made in snow by an unshod horse (Bourdier 1967: 272-3). As shown in Bang and Dahlstrom (1974: 64, 74), in a non-cloven hoof the wall curves round the toe pad or 'frog' (Fig. 4); thus the 'vulva's' cleft would actually be the cleft of the frog. This interpretation is equally applicable to some of the figures at Tito Bustillo, although Beltrán (1972: 121) considers them to be absolutely naturalistic vulvae, indeed the most faithful reproductions of female external genital organs in all Palaeolithic art. Ironically, Breuil himself (1925: 293) stressed the important role in the origins of art among hunting peoples of the observation and intentional reproduction of human and animal tracks; rocks covered in them are well known in southern Africa, America and of course Australia where they are a notable element in the early rock art often described as 'Panaramitee style'.

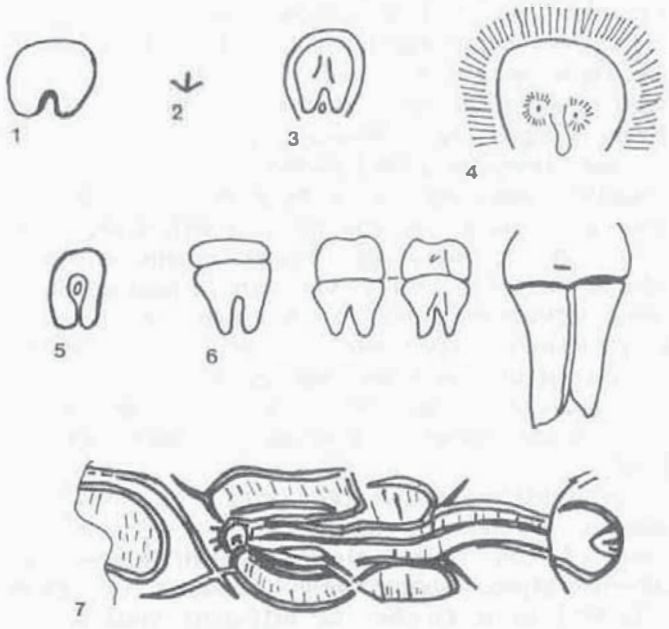


Figure 4.
Some alternative interpretations:
1 Imprint of non-cloven hoof
2 Imprint of a grouse
3 Imprint of a horse hoof in snow
4 Wandjina face, Kimberley, Australia
5 Montespan 'rear view' (?)
6 Engraving from La Ferrassie (on left) compared to 'Venus' statuettes from Dolní Věstonice (on right)
7 Baguette from La Madeleine
After Bourdier 1967, Bang and Dahlstrom 1974, de Saint Mathurin 1976, and Stoliar 1977/8. Not to scale.

Even the apparently indisputable though strangely elongated Ferrassie triangles are not immune from reinterpretation. Both Bourdier (1967: 272-3) and Stoliar (1977/8: 43)—through comparison with the Vestonice figures of females with a horizontal line above their legs—see them as representations of the entire female body; hence the 'vulva's' cleft would actually represent the separation between two legs, which are divided from the torso by the transverse line.

There are other possibilities. Some of the Aurignacian figures have the shape of a breast rather than a vulva (e.g. see the breasts on the Laussel females, or various 'Venus' figurines); some, with their incomplete round outline and median line resemble the faces on Wandjina figures in Australia (Fig. 4); certain scholars have pointed to shields ('scutiforms'), and Glory even suggested some 'vulvae' were *sacs à provisions* (Leroi-Gourhan 1966: 37); ten marl objects at Kostenki I, which Leroi-Gourhan (1973: 94) sees as realistic carvings of vulvae, were merely called 'medallions' by Efimenko and Abramova (Abramova 1967: 130); Stoliar (*op. cit.*: 48) sees the supposed clay 'vulva' at Montespan as the rear view of an animal's anus and hindquarters (Fig. 4); the well-known baguette from the Magdalenian of La Madeleine depicts, in Leroi-Gourhan's view (1973: 94), a bear head, a phallus and a vulva, and is thus one of the keys to interpretation of the abstract signs in Palaeolithic art; this interpretation was originally put forward by Capitan, Breuil and Peyrony, who claimed the vulva was drawn with careful detail—indeed Peyrony said it was half open, and the hair was depicted; Luquet, however, accepted the phallus, but not the vulva, which he saw as an anus and buttocks (see de Saint Mathurin 1976), while de Saint Mathurin (*ibid.*) has most recently suggested that the phallus/vulva may form a very stylised doe's head.

The above discussion highlights some of the problems inherent in motifs which have no context. As at Tito Bustillo, the difficulties are lessened slightly in cases where the motif is parietal rather than portable, and thus its correct orientation is known. In some examples, the simple inverted triangle or V shape, with median line (e.g. in clay at Bédeilhac; engraved at Pergouset, and perhaps also at Arcy, Gouy, Comarque and Gargas)

is very similar, in our eyes, to a normal, common sense depiction of female genitalia, and it seems reasonable to interpret them as such, while bearing in mind that there is no guarantee that the interpretation is valid. Did they look like vulvae to Palaeolithic man? Where triangles have no median line, further doubts creep in: e.g. there are forty-three inverted triangles painted in the Spanish cave of Ojo Guareña (Burgos), but the Dams (1974) have not seen any sexual significance in the motif. There are a great many other signs which Leroi-Gourhan (1973: 103-4) derives from the vulva; indeed his whole system rests on the idea: *'Tout le système que représentent les signes seraient probablement resté incompréhensible sans la présence de figures limitées à la représentation du sexe féminin'* (Leroi-Gourhan 1973: 91). Nevertheless, he finds it hard to fit the Aurignacian blocks into the scheme: *'Le problème du passage des signes explicites de l'Aurignacien aux signes quadrangulaires n'est pas éclairci'* (1966: 42); *'L'origine des signes pleins . . . est encore obscure . . . Il ne semble pas que l'évolution se soit faite à partir de ces figures circulaires, mais plutôt à partir d'une figure ovale ou triangulaire'* (1980: 290).

These blocks represent a localised and apparently short-lived phenomenon. Regardless of their interpretation, it seems an extreme exaggeration of their significance to derive much of the sign system of the next fifteen millennia from them.

Conclusion

As shown above, we owe the 'vulva interpretation' to Breuil; in a way, he had few alternatives in 1911—he could describe (Castanet had already called one a 'heart-shaped figure'), he could say what it reminded him of, and he could seek ethnographic analogies for support. Unfortunately, he did this badly. As Ucko and Rosenfeld (1967: 129) have said, Breuil was more important for the documentation of Palaeolithic art than for its interpretation, yet his supreme standing in the field ensured that his ideas had influence and durability.

Following his lead, many scholars have used the Aurignacian blocks, the female figurines and a wide variety of 'signs' on cave walls to support their views of a Palaeolithic preoccupation with sexuality, views for which the evidence is far from sufficient. Simply by focusing on the Aurignacian motifs, I believe I have shown that this approach is highly subjective, simplistic and tautological.

Scholars studying rock art in other areas are trying different approaches, and Clegg (1981) has stressed that interpretation should be avoided, or at least left until the end of the analysis. He has adopted a symbol (!) which is used as shorthand for describing the shape of a motif without assuming its meaning. It might be useful to adopt this for the Aurignacian '!vulvae', although the variety of their shapes invites a wider set of names. Stoliar, for example (1977/8: 39), has stressed that they are not stereotyped, and has proposed a tentative grouping: horned ovals, triangles, pear-shaped ovals, arched ovals.

Such a system is preferable in that it is more objective, being a description rather than an interpretation. It avoids the problems of faith and preconception which have dominated this field for so long. Similarly, Lorblanchet (1977) calls the motifs 'circular signs with an indentation', and considers them to be nonfigurative, even though they may resemble certain objects or even have been derived from a real model. In other words, one should treat them as purely abstract, whereas most authors have searched for similarities to a variety of objects—tectiforms, scutiforms etc.—and thus treated them as schematised representations. This type of search, which typified the approach of Breuil and many others, did not lead very far. Leroi-Gourhan's analysis, which tackled the abstract signs but still saw them as schematised representations, can be seen as a sort of transition, an extremely important transition, in cave art studies. It remains to be seen how far a more objective approach will lead. We need new ways to model something that is infinitely complex and basically unknowable. Those who aspire to an archaeology of the mind are unlikely to derive much joy from Palaeolithic iconography.

It is already clear that in primitive and in prehistoric art simple elements are joined and related in very complex ways. There are no definite universals in meaning or abstraction. As Forge (1977) shows, we inevitably apply our understanding to what we see; our background, culture and art history come into play, and we try to translate all signs as if they were hieroglyphics, in the vain hope of finding a Palaeolithic Rosetta Stone. Even if signs *are* representational, their meaning is far from straightforward: Munn's study of Walbiri iconography (cited by Forge *ibid.*) stresses that a simple circle can mean a hill, tree, camp site, circular path, egg, breast, nipple, entrance into or exit from the ground, and a host of other things. These meanings do concentrate in a 'femininity/fertility' sphere, while the numerous meanings of a straight line have connotations of maleness, but there are ethnographic examples where these clusters are reversed in terms of sexual connotation.

In short, very simple graphic designs can have a tremendous range of meanings. In the case of the Aurignacian engraved blocks, an interpretation, subjectively chosen, has been attached to a number of different motifs, and has stuck. In order to make it stick, scholars have claimed that engravings were incomplete, distorted, badly drawn, or stylised. It seems more sensible to apply Occam's Razor to these 'vulvae', and assume that their engravers knew what they were doing, were competent artists, and that the forms of the motifs are intentional and meaningful. There is little sense in our lumping together signs with different shapes when Palaeolithic artists took pains to differentiate them, a point which has also been made recently by Vialou (1981: 946).

Rather than choose one meaning, let alone base most of Palaeolithic religion upon it, it

might be more appropriate to classify the motifs on the basis of shape alone, and assign all interpretation to the category of extremely tentative speculation instead of self-evident truth.

Acknowledgments
The author is most grateful to John Clegg, Robert Kruszynski, John Pfeiffer, Mlle Suzanne de Saint Mathurin, Denis Vialou, and Brigitte and Gilles Delluc for advice and discussion.

COMMENTS

By JOHN CLEGG

Dr Bahn's topic promises comic richness, not least through the potential spectacle of male academics solemnly debating the triangularity of the human vulva—and which is the right way up.

Paul's paper effectively demolishes the current myth that archaeologists have established that the people of the European Upper Palaeolithic were obsessed with sex. Concurrently, it raises the possibility of enquiring about that topic. A casual knowledge of the literature suggests that sexual themes are less common in European and American prehistoric pictures and more common in Australian prehistoric pictures, but most common in European pictures and art from the last two and a half millennia. Any enquiry into the relative importance of sexuality which used pictures as evidence would have to come to grips with not only the problems of subject identification, but also the question whether 'sexual obsession' leads to more or fewer pictures of sexual subjects. Certainly there is current a myth that Victorians (the period, not the state) were obsessed with sex to the point of placing a general taboo on all sexual representations visible in polite, or mixed company—a taboo which extended to clothing the upper parts of piano legs.

The paper raises two important points, one about the problem of identification, the other about myth in prehistory. As Peter White often points out (e.g., White and O'Connell 1982, especially p.v), there are many myths which have become a part of received common knowledge of prehistory, even though the basis for them (if there is any basis at all) is very shaky. It is both valuable and healthy for these myths to be examined and questioned. Each should be reassessed whenever it forms a crucial link in a chain of evidence or argument. Bahn draws attention to several examples where such reassessment is lacking. These myths are particularly prevalent in the study of rock art (though we are trying to decrease their incidence in the study of prehistoric pictures), and are one of the factors which contribute to the bad name and misunderstanding which stone-and-bone prehistorians use as an excuse to ignore those studies (which *RAR* has done so much to encourage). Some of the myths are founded on laziness, others on a misunderstanding of what is claimed. Ideas were treated as though they claimed to be hypotheses which had been confirmed through testing.

Old-fashioned science used to progress (or such was the myth) through several stages, to which observation was fundamental. Observation involved both fact collection and the contemplation of facts in a theoretical context. Sometimes this process produced an interesting idea, and some interesting ideas could be formalised as testable hypotheses. The two stages, *generate an idea*, and *test an hypothesis* work on very different standards of evidence and proof. An *idea* need be demonstrated only to a level the courts call 'on the balance of probabilities' although many ideas are well worth following up merely because they look interesting. *Hypotheses* must be tested to 'beyond reasonable doubt', and it is facts 'proven' to that level which should form the basis of prehistories (although they may need to be enlivened with less well-established information). There can be no testing of hypotheses without hypotheses to test; *ideas* are both a fundamental source of knowledge and themselves bases for insights. We cannot afford to discard ideas—rather it is essential to be aware of the differences between ideas and accepted hypotheses. (Other words might be better: 'hypothesis' once meant what I mean by 'idea'; in those days 'theory' meant tested, confirmed hypothesis; now neither word has that simple meaning.) If an idea is promulgated without examination, it may well reproduce itself as myth.

While Bahn constructively demolishes some myths, he accepts others. He seems to accept and accordingly promulgates the Gamble myth that Palaeolithic 'Venus' figurines are significantly similar to each other although they come from places one continent apart. These observations are thought to demonstrate some important communicative continuity across thousands of kilometres. But the only evidence that the Venuses are so much like each other is that Leroi-Gourhan drew geometric lines over pictures of them. To my eye the geometrical constructs of Leroi-Gourhan demonstrate *differences* between the Venuses. A few years ago I came across a charcoal drawing (see Fig. 1), 500 millimetres high, which I traced. The drawing was in a sandstone rock shelter in the Sydney area. The shelter's walls bear a lot of writing which suggests that the shelter was the haunt of adolescent twentieth century males; it is known as Mick's Cave after one of the largest inscriptions. There is no proof that the drawing is not much older than the written inscriptions, but it does not look at all like Aboriginal pictures from the area. What is certain is that the picture is several continents distant from the Palaeolithic Venuses, and that it has several traits in common with them: absence of facial features and feet, so-called 'emphasis' or 'stress' on 'sexual' characteristics. I suspect it would fit just as well into Leroi-Gourhan's constructions as do the Venuses.

The problem of interpretation is fundamental to iconographic study of 'rock art'. It is assumed

that pictures are *of* things, and they cannot be satisfactorily studied until their subjects are identified. There has been remarkably little work on methods of identification. The most common technique is to identify the subject as what the picture in question seems to resemble. The identification is usually made with authority, and often accepted without question—human perceptive processes are partly to blame: it is easy to perceive what one is told is there. If such an identification is questioned, any alternative proposed is usually equally subjective and untestable. Once other prehistorians discover that the study of rock art has such a subjective foundation, they rightly question its scientific value.

I was once involved in a comic but distressing combination of myth genesis and subjective interpretation. The first prehistoric pictures I saw in Australia were at The Art Gallery, Carnarvon Gorge, where there are what seem to be thousands of sets of two concentric ovals carved into a vertical friable sandstone surface. Some of them have two small holes at either end of the innermost oval. I learnt that the literature (Goddard 1941) referred to them as representations of macrozamia nuts, which I took as some strange Queensland circumlocution. I referred to them as vulvae. I have the impression that now any oval in Queensland rock art is identified as a vulva. I do not see any reason now, twenty years later, to prefer one interpretation over the other, for simple ovals; but I believe that it is an error to force interpretation.

At Sturts Meadows (western New South Wales) there are many thousand engravings which look like the tracks of animals. Many of them occur in lines, as though representing trails. Next to a trail of more than twenty !bird tracks is a line of three bisected circles. These could represent the trail of a cow, but cows were not introduced until 1788, and the engravings seem to be millennia older than that. The only current interpretation (which is offered only to those who demand it) is that these engravings represent the trail of some sisterly variant of Imjim Quinkins who move about the country by bouncing on their testes (for a version suitable for children, see Trezise and Roughsey 1978).

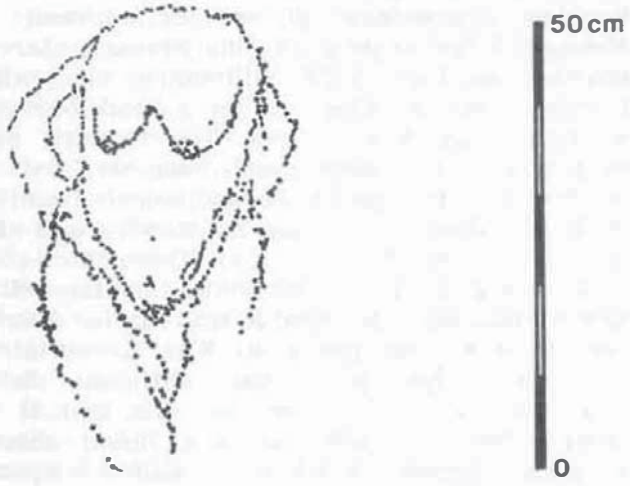


Figure 1.
Tracing of drawing in Mick's Cave, Daley's Point, New South Wales. The dotted nature of the drawing derives from the uneven surface.

John Clegg
Department of Anthropology, A14
University of Sydney, N.S.W. 2006
Australia

By JEAN CLOTTES

Dr Bahn's papers are always fascinating and most enjoyable: very clear, extremely well-documented and scholarly, they are sometimes written in support of some 'outrageous' theories (agriculture in the Palaeolithic, domestication of animals in the same period, relationship between art and thermal springs etc.) which makes them all the more stimulating. This time, though, he does not propound a controversial thesis, but on the contrary, he tries to destroy 'one of the most durable myths of prehistory'.

If his purpose were only, as he says, to discuss 'whether the Aurignacians of a tiny corner of France were obsessed by female genitalia', his paper would have been of limited scope. But in fact he raises several much wider epistemological issues, and I must say that I find myself in agreement with most of his arguments.

In particular, he draws our attention to H. Breuil's and D. Peyrony's subjectivity and he gives several examples to show that their identifications could be erroneous, through their personal tendencies and biases. Dr Bahn is cautious enough to add that the work of their contemporaries and of every scholar is not immune to subjectivity, and he is quite right indeed. This is a useful reminder, which also emphasises that after such masters had expressed their views and published their tracings—whether they be accurate or not—what they wrote and published was reproduced again and again and became an indisputable truth for many decades.

The main point, though, is whether it is possible to identify isolated vulvae as such. Dr Bahn argues that in some cases a few (in our eyes) naturalistic depictions can be so identified, but he adds that 'there is no guarantee that the interpretation is valid' and that they looked like vulvae to Palaeolithic man. This of course depends on the greater or lesser degree of complexity of the drawing. It is a methodological problem and should be treated accordingly: in the case of the Bédeilhac vulva, modelled in clay on the ground, there can be no possible discussion, as not only is the vulva realistically reproduced, but a tiny stalactite has been added, in the appropriate place, to represent its clitoris (it is apparent on a photograph kept in the Bégouën collection, taken shortly after its discovery, and thus cannot be said to have been a later addition).

As to a mere triangle or circle, when out of context, it is impossible to ascribe such a definition to them. In fact, one should very clearly distinguish between what is identification and what is interpretation, and I think the whole

crux of Dr Bahn's paper lies on the muddle resulting from the confusion of the two notions. The Bédeilhac vulva is undoubtedly identifiable. On the other hand, that A. Leroi-Gourhan should interpret rounded, oval or triangular signs as having a feminine value within his interpretative theory of Palaeolithic art is a perfectly legitimate attitude, which is quite different from stating that they depict female genitalia (Clottes 1986). That Leroi-Gourhan's theory is debatable and, in particular, that it may seem difficult to deduce a 15 000-year old tradition from 'a localised and apparently short-lived phenomenon', such as the engraved blocks in the Dordogne, is another problem altogether.

Which brings us to the question of an eventual transition from realistic vulvae to unidentifiable but related signs. Bahn clearly raises this subject when he wonders 'why a single motif should have been made in so many different varieties, side by side, and more or less contemporaneously', but he does not follow up and he concludes that 'ethnography offers little help' and that alternative interpretations have sometimes been put forward. As a matter of fact, finding on the same cave wall or on the same antler or bone one particular theme depicted at various stages of stylisation is far from being unusual in Palaeolithic art. For example in the Réseau Clastres (Niaux), French Pyrenees, a perfectly naturalistic bison is closely followed by a very incomplete one, and the latter by a third which can mostly be defined as a bison by its closeness to the other two. Likewise, most of the Gönnersdorf female outlines might appear abstract and controversial out of context, but can be determined as females because they occur together with some others which are quite characteristic (breasts, buttocks) and with which they share the broader outlines. It may be ironical to recall that Breuil rather intuitively recognised the Lalinde engravings as very stylised female figures long before the discoveries of Gönnersdorf, Gare de Couze and others corroborated his hunch (Pales and Tassin de St-Péreuse 1976: 101).

Realism and stylisation have always coexisted in Palaeolithic art (Lorblanchet 1977), which adds to our difficulties not only in interpreting it but in identifying the simplest depictions, such as vulvae. Besides, it is quite true that many images are ambiguous and that alternative explanations may be propounded. This, too, is a characteristic feature of Palaeolithic artists who were fond of playing with forms, even when they drew animals (Lorblanchet 1986). The main difficulty, when we are confronted with contradictory explanations by different scholars may in fact be to determine whether their interpretations differ because the subject was in itself ambiguous from the first or because of their own preoccupations and biases.

One last point: to strengthen his argument and show that Upper Palaeolithic artists did not draw too much attention to vulvae 'in context', Bahn cites Gravettian figurines and says that very few of them evidence the median cleft: he quotes Chiozza, Willendorf and Grimaldi, but he forgets two statuettes from Gagarino and Moravany (Pales and Tassin de St-Péreuse 1976, Figs 41, 42), and he does not mention that on four out of the six more or less complete ones found at Grimaldi the vulva has been clearly indicated and in some cases stressed ('Le Polichinelle', 'Le Losange', 'La Femme au Goître'; Delporte 1979: 101-9).

Besides, Dr Bahn is clearly embarrassed by the Monpazier figurine, with its wildly exaggerated sex, and thus he says that 'this figurine is very unusual' and that 'some doubt still persists as to its authenticity'. It is true that it was not found in a stratigraphical context, but as the author mentions this applies to most of the known figurines, so why single out Monpazier in that respect? Far from being so unusual, 'it appears as particularly classical of feminine representation in the Upper Perigordian' (Delporte 1979: 78). As to the problem of its authenticity, it was analysed at some length when first published (Clottes and Cérou 1970). We concluded then that whilst it was not possible (as for many of its predecessors) to state its authenticity without any reservation, we had not found the slightest indication to doubt it (*op. cit.*: 436). To the best of my knowledge, the only specialist who took up the subject since was H. Delporte who examined the pros and cons and concluded in favour of its authenticity (1979: 77-8).

These few examples show once again the difficulties of dealing with a period of such immense duration, where some finds may seem to contradict others, and how cautious one must be. Dr Bahn, by bringing an old myth into the limelight and exposing its weaknesses, usefully incites us to still more prudence, and the propositions he makes in his conclusion are well worth endorsing.

Dr Jean Clottes
(Directeur des Antiquités Préhistoriques)
11, rue du Fourcat
09000 Foix
France

By PAUL FAULSTICH

Bahn's paper illustrates the problems associated with archaeological extrapolation. Theoretical archaeology necessitates informed extrapolation, and herein lies a Catch 22; we must rely on inferences, but inferences are inevitably based on cultural predilections.

Bahn illustrates his point well. I would, however, warn against the danger of closing the door to intelligent, well-informed interpretation. The potential value in creative 'answers' to enigmatic questions should not be ignored. What Bahn is proposing, and I agree wholeheartedly, is that the next generation of creative 'answers' should be based less on speculation, and more on solidly grounded synthesis.

I cannot help but wonder how the vulvae interpretation came to be such an accepted and long-lived myth. Perhaps it was not Palaeolithic *Homo sapiens sapiens* who were fixated on female

sexuality, but it is twentieth century men who have such obsessions. The simplistic and mythic analogy between 'primitive mentality' and our subjective subconscious may have encouraged interpretations of Palaeolithic iconography based on contemporary repressed emotions and drives. In searching their own minds for a connection with prehistoric symbolism, researchers found the taboo yet enticing subject of fertility and sexuality. Hence, what Bahn calls 'one of the most durable myths of prehistory'.

Even if the motifs in question do in fact represent vulvae, this is not necessarily indicative of a preoccupation with sex or goddessness. Just because they may represent the vulvar *form* does not necessarily mean that they represent the *concept* of fertility.

Bahn's paper also supports the view that we should abandon the vain search for meaning. We should, at this point, not be so concerned with symbolic interpretation as with categorising motifs based on their forms. In our initial excitement we have jumped over our heads into the ocean of Palaeolithic symbolism, when in fact we should simply be wading.

I have two minor points of contention with Bahn's report. Firstly, he makes an unnecessary comparison between the shape of the breasts on the Laussel females and the shape of Wandjina heads. Unless this likening was made tongue-in-cheek, I question its significance. Visually, the Laussel breasts no more resemble Wandjina than they do extracted teeth or my neighbour's swimming pool. Certainly, Bahn is not attempting to convince us of any imported style, but I wonder just what his point is.

Secondly, I disagree with his position that students interested in an archaeology of the mind will not find much satisfaction in the study of Palaeolithic iconography. Philosophers, art historians, naturalists, psychologists and others interested in the unfolding of human consciousness will inevitably be rewarded through the study of Palaeolithic signs and symbols. Even if their meanings cannot be ascertained, the pieces of the puzzle hold enticing clues to our past.

I commend Bahn on his willingness to slay the sacred cow—the *idée fixe* of Palaeolithic sexual obsessions. His willingness to break the rules and abandon preconceived ideas is at the root of what Taoists refer to as 'killing the Buddha'. Bahn's critical examination of previously accepted interpretations is a step toward attaining his goal: a realistic and enlightened view of Palaeolithic iconography.

Paul Faulstich
Institute of Culture and Communication
East-West Center
1777 East-West Road
Honolulu, Hawaii 96848
U. S. A.

By HEINZ HUNGER

Though I shall arrive at somewhat different views in this essay I am convinced that Dr Bahn's thorough mistrust of the current trend of interpretation of circular and semi-circular motifs (with or without a central marking of the pudendal cleft) as pudenda is justified. 'The supposed dominance of vulvae among the earliest known Palaeolithic depictions' is also in my opinion but a modern Freudian myth.

Nevertheless, I feel that the title of his sparkling article misses an essential point, because his interest is confined merely to the female private parts and leaves the penis out. I trust that by concentrating on depictions of sexual intercourse (Fig. 1) we can answer Bahn's question satisfactorily.

I contend that in contrast to our modern understanding of 'sex' our ancestors in the Stone Age had a basically different concept of everything they enacted with their genitals and depicted, either as isolated genital organs or in the form of genital union, for those ritual purposes we might nowadays attribute to 'superstition'. Nevertheless, I shall accept them as magico-protoreligious manifestations. As a generic synonym for this technical term the adjective 'apotropaic' applies.

A word of caution may be necessary as to the customarily assumed equivalence of 'sexual' and 'genital'. They are to be differentiated throughout, for it is one of the findings of the following reflections that sex has not gained its extraordinary preponderance due to any specific neurophysiological peculiarity of its own, but by its unique human association with morality and religion:

> Sexual functioning is a natural physiological process, yet it has a unique facility that no other natural physiological process, such as respiratory, bladder, or bowel functioning can imitate. Sexual responsivity can be delayed indefinitely. No other basic physiological process can

Figure 1.
Copulation scene of two anthropomorphs with enlarged hands and feet, expressing their superhuman quality. After Gibson and Singer 1978.

> claim such malleability of physical expression (Masters and Johnson 1970: 10).

What we perceive and designate with the word 'sex' is but a product of the previous century (van Ussel 1970: 8-10). The term has been used mostly in a deprecatory sense and is therefore unsuitable to express what and how our primitive ancestors felt and how they reacted to impulses we nowadays register as 'sexy'.

The modern euphemism 'fertility' is a similar abuse of a category of biological imputation. Our progenitors had no comprehension of the mechanism of impregnation, not to mention its hormonal requirements.

To establish a common denominator for the discussion of Bahn's paper dealing exclusively with Upper Palaeolithic art, and my preferred version including rock art of all primitive cultures, I suggest to pick up the thread where it was left by him: 'it might be more appropriate to classify the motifs on the basis of [abstract] shape alone, and assign all interpretation to the category of extremely tentative speculation instead of self-evident truth'.

A word of caution is in order therefore, concerning a too-facile reading of these presumed vulva forms. Jesse Warner (1982, 1983) has called attention to the confusion that exists concerning the varieties of the forms and symbols (Fig. 2a), and the irritatingly different interpretations of these signs by various authors (Fig. 2b):

> There are terms such as deer hoof, bird track, arrow, vulva, rainbow, sunburst, moon, star, shield, scorpion, atlatl [spear thrower], plant form, etc.
> The reader often assumes that whenever he sees such a glyph that's what it means no matter what the context (Warner 1982: 159).

But how often does the complete panel confuse rather than facilitate identification:

> At Rochester Creek (Utah) five vulva symbols occur next to some copulation figures. Those most familiar with fertility sites would have previously probably argued a vulvaform label. However there are 13 birdlike figures in the panel and out of five 'vulvaforms', two or three look equally like bird tracks, one with an intersecting line. The possibility that these may be bird tracks cannot be ignored (Warner, *loc. cit.*).

In order to get away from this tiresome muddle and perplexity of which figures can be proved to have truly sexual connotation and nothing else, I suggest that we first turn to depictions of genital acts and actions that are beyond all doubt: images of human couples engaged in sexual intercourse.

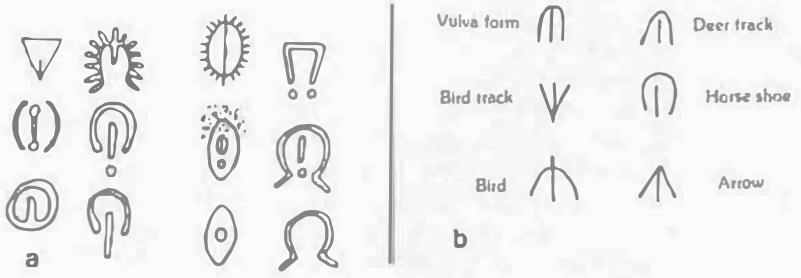


Figure 2.
a - Varieties of vulva forms
b - Different generic terms for the same basic element form from different element lists. After Warner 1982.

Proceeding to this task I start with the oldest pictorial rendering of a copulation as a kind of landmark, the famous 'scene' from Laussel (Dordogne), a relief assigned to the Solutrean and dated to about 18 000 BP (Fig. 3). It shows obviously two figures, one inverse and joined to the other, most likely in genital union.

Unfortunately I cannot present this customary interpretation without a question mark, because André Leroi-Gourhan (1971: 347, 582) felt tempted to tamper with the glyph and 'redraw' the outlines of the copulating pair until he succeeded in converting the composition into an apparently less obnoxious breech delivery.

A similar alternative was offered by K. Castleton (1979/II: 249, 310), a retired professor of medicine, for a few compatible petroglyphs in Utah: 'one figure appears to be having a bowel movement or giving birth'.

From whichever angle one may look at the 'scene' from Laussel, to present human copulation pictorially presents veritable difficulties to the artist. To depict the genital organs of the couple is scarcely feasible. Irrespective of that problem of a satisfactory artistic rendering of human copulation many cultures all over the globe did not give up, endeavouring time and again to portray man and woman in their genital union (Fig. 4).

This leads us to the question of why many primitive societies made not only special efforts to depict human copulation, but also enacted it regularly in public for the welfare of the community as a 'Sacred Marriage' (Hunger 1983: 4-5; 1984), mostly irrespective of the marital status, i.e. promiscuously (Frazer 1922: 139-46, 245,

Figure 3.
'Copulation scene' from Laussel, Dordogne. After Lo Duca 1963.

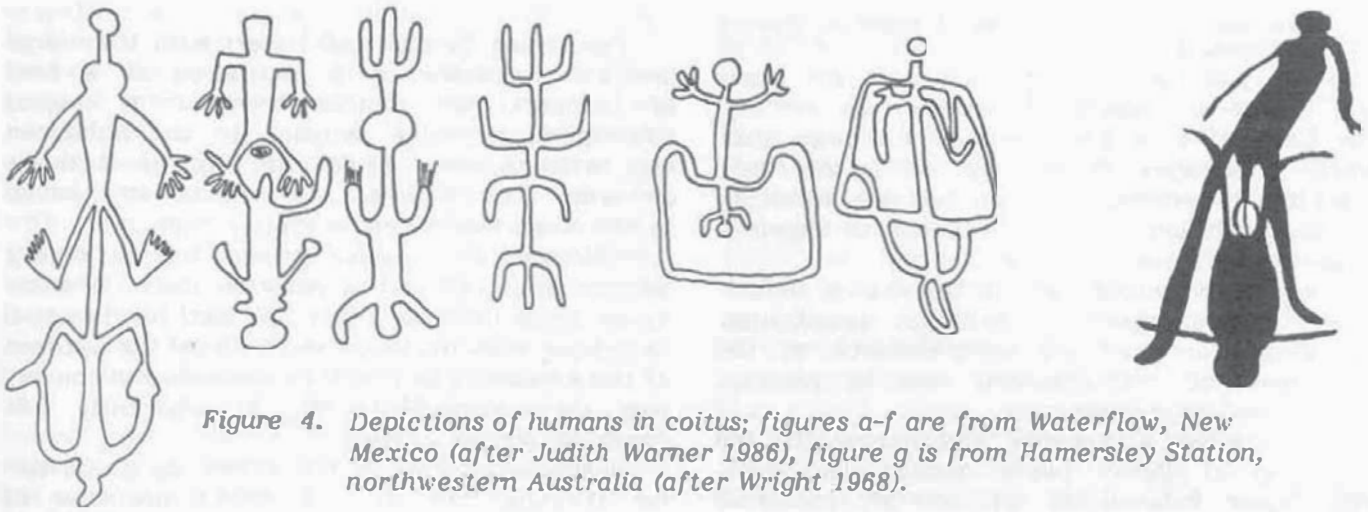


Figure 4. Depictions of humans in coitus; figures a-f are from Waterflow, New Mexico (after Judith Warner 1986), figure g is from Hamersley Station, northwestern Australia (after Wright 1968).

435, 556). Perhaps the depiction of copulation in the Palaeolithic was not intended to portray a secular, ephemeral event but mainly a ritual act connected with primitive religion.

Pictorial renderings of a human copulation in the presence of a shaman or priest, which could prove a religious ceremony, are extremely rare in rock art. One of the best is without doubt the scene in the so-called Cave of Life in Petrified Forest, Arizona (Schaafsma 1980: 159):

> The entire sequence depicts a chain of closely interconnected anthropomorphs. The copulatory nature of these unions cannot be mistaken. The top figure seems ceremonial, with its two staffs (Bertsch 1983: 26).
>
> . . . the religious background and framing is finally expressed by a twice encircled cross, above the copulating couple and again connected by a power line. The four points of the cross often symbolize in Indian folklore and healing rites the four cardinal points, i.e. the cross represents the universe . . .
> Such meeting and mating of two selected partners has nothing to do with romantic love à la Romeo and Juliet or hormonal sex drive, but is a public duty which these two have to perform whether they like each other or not (Hunger 1983: 3-4).

The dominating figure of this public copulation scene is the priest (Fig. 5). His resemblance to an older colleague from Lascaux, Dordogne, is remarkable. He also shows an inconspicuously small ithyphallus, conjuring a bison, he also carries a wand crowned by the body of a bird. The Anasazi Indians lived in Arizona until A.D. 1300; the Magdalenians in France are dated to about 13 000 B.C.

Figure 5.
Depiction of 'ritual coitus'. Cave of Life, Petrified Forest National Monument, Arizona. After Bertsch 1983.

This coincidence (and there are more!) may indicate the incredibly long persistence of certain pictorial associations and motifs during prehistory, but also that these may have originated independently at various places and in various periods.

Depicted ritual copulation, as I understand it, does not pursue a sexual goal in itself, but is primarily a magic-protoreligious activity. Such a ceremony has never been employed to induce pregnancy to the woman involved, nor did it aim at any bizarre sexual gratification for the couple concerned.

There are a number of problems to be addressed before the relief at Laussel can be accepted as the ideal rendering or even earliest idol of a human copulation; especially the depicted position of the partners, and the absence of explicitely depicted genitals, which is contrary to the majority of later portraits of human copulation (Figs 1, 4). It cannot be explained why the artist from Laussel did not picture the organs clearly in their genital unification. The supposition of a first manifestation of bashfulness appears arbitrary in the face of the numerous documents of the following millennia.

Though the 'inverse' coital position as represented at Laussel (Fig. 3) is the preferred and most frequent model for portraying human genital interaction in prehistory, this posture is unlikely to portray the way these societies used to have sex ordinarily. In my opinion it represents a sophisticated version expressing the significance these primitive peoples attributed to the unification of the genitals. The observation that the 'inverse' coital posture of the partners provides the least sexual gratification is sexologically important.

Notwithstanding this, some esoteric Tantric sects in India recommend this technique of a *coitus reservatus* particularly to avoid ejaculation. They wish to protract the genital union for at least half an hour's time and praise it as a sublime way of their transcendental meditation of sexo-yoga (Mookerjee 1971: 112, Fig. 76; Rawson 1973: 102, Fig. 70; Mookerjee and Khanna 1977: 126, 144, 163-6). In all probability their concept

of sexuality was essentially different to our modern doctrine of biology. And the same can be said of our early prehistory. It will partly confirm Bahn's reservation as to a sweeping identification of geometrical designs with sex symbols.

Bahn's scepticism as to the sexual meaning of certain 'abstract' configurations of the Palaeolithic is justifiable, particularly when considered in a wider context. Neither the act nor the portrayal of intercourse was aimed at any sexual arousal in the sense of *l'art pour l'art*, or to serve the more distant ends of procreation or fertility. The base of ethnographic public copulation was to conform with apotropaic rituality: in the negative to drive away the ubiquitous evil spirits and demons and in the positive to safeguard the property and prosperity of a man and his community.

I believe that genital activities and interactions in prehistory were innocent of any sex in the modern understanding of psychoanalysis; they were in principle 'sex indifferent'. To conclude, I shall illustrate this with an example: the Hopi Indians of Arizona consider a mere sexual thought to be 'an excellent remedy to relieve . . . hiccoughs', clearly caused by a witch (Talayeswa 1942: 170).

Emeritus Prof. Drs Heinz Hunger
Braunsbergstrasze 35
D-4400 Münster
West Germany

The Eye is Not As Clever As it Thinks it is

By **ALEXANDER MARSHACK**

Bahn has performed a major service by bringing attention again to the century-old problem of the vulvar images in the European Upper Palaeolithic. These images appear in many forms and on diverse materials: on rock walls in the caves, on stone blocks in the habitation site and carved on artefacts of bone and antler. As Bahn notes with amusement these images have incited a host of conflicting theories, a fact that again raises the perennial problem of subjectivity in observation and interpretation. Having presented us with the problem, however, Bahn is faced with precisely the problem he criticises in others. Though he has apparently seen many of the vulvar images and has gleaned and compared much of the literature, he has *not* performed a careful, first-hand internal analysis of the artefacts, images or traditions. He is arguing primarily from his reading and his personal, thoroughly subjective viewing of some of the images. This, unfortunately, results in what might be termed a form of literary criticism and a discussion of merely surface appearances.

Only the Dellucs (Delluc and Delluc 1978, 1981) have to date published a careful study of one group of these vulva images, those in the Dordogne region of France. They have not addressed many of the broad problems concerned with aspects of the tradition they have not studied at first hand. This is a major part of the methodological contribution and strength of their work. Bahn, by contrast, has leaped to the problem without instituting the necessary slow, methodological research. The problem is in fact more complex than he perceives.

In my book (1972) I presented an early Magdalenian (c. 14 500 B.C.) antler baton from the site of Le Placard, France, which vividly depicts a three-dimensional, realistic vulva between two truncated legs (Fig. 1). The upper body is suggested by the handle. This vulva is intentionally isolated and stressed as a significant 'human' image. From a far earlier Aurignacian level at Pair-non-Pair, France (c. 28 000 B.C.), there comes the fragment of an antler baton upon which an ovate 'bell-shaped' vulva is carved, just below the baton hole, as in the Le Placard

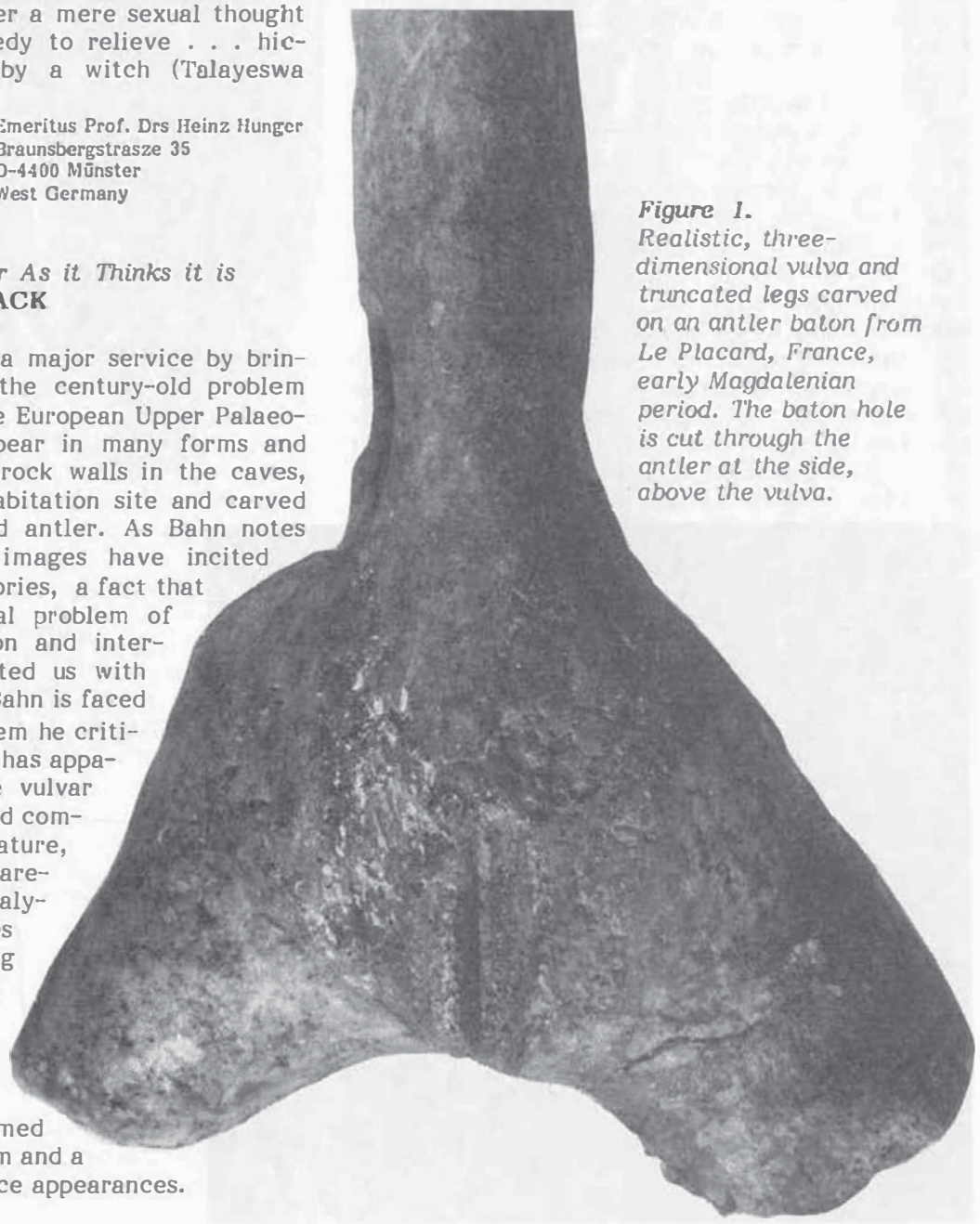


Figure 1.
Realistic, three-dimensional vulva and truncated legs carved on an antler baton from Le Placard, France, early Magdalenian period. The baton hole is cut through the antler at the side, above the vulva.

example. The vulva is in the same style as one finds carved on stones in habitation sites of this period (Fig. 2).

The microscopic analysis of an engraved stone that comes from the period between these two examples, from a Solutrean level (c. 21 000 to 20 000 B.C.) in the Pyrenean site of Isturitz, revealed the presence of a phallus *inside* a vulva (Figs 3a, 3b). The excavators (de Saint-Périer and de Saint-Périer 1950), without benefit of microscopic examination, thought the image looked like a set of bovid horns ('*cornes en lyre*'). The microscopic analysis was one among thousands performed to double check the published literature, since I had found quite early that many published illustrations were incorrect. Some years after studying the Isturitz stone, Bosinski (1975) published the same type of image, an engraved phallus inside a vulva incised on a stone from the Magdalenian site of Gönnersdorf, Germany, c. 14 500 B.C. (Fig. 4). Gönnersdorf also provided hundreds of incised schematic female images on slabs of slate (schist), one of which depicted a woman giving birth with the fetus still attached to the female by an umbilical cord (Marshack 1975).

It is clear, then, that vulvae were depicted in the Upper Palaeolithic across a wide expanse, both temporally and spatially. Despite Bahn's assertion, a microscopic analysis of the images from the Ukraine, including the so-called 'medallions' carved in marl, revealed that they too were vulvae and were part of a complex and diverse regional tradition of rendering and *using* vulvae in many forms (Marshack in press).

One need not concern oneself with 'the myth of Aurignacian obsession' but one must recognise the reality of the Upper Palaeolithic depictions and uses of the vulva within the broad context of Ice Age symbolism. Significantly, the phallus was also isolated as an image and symbolised

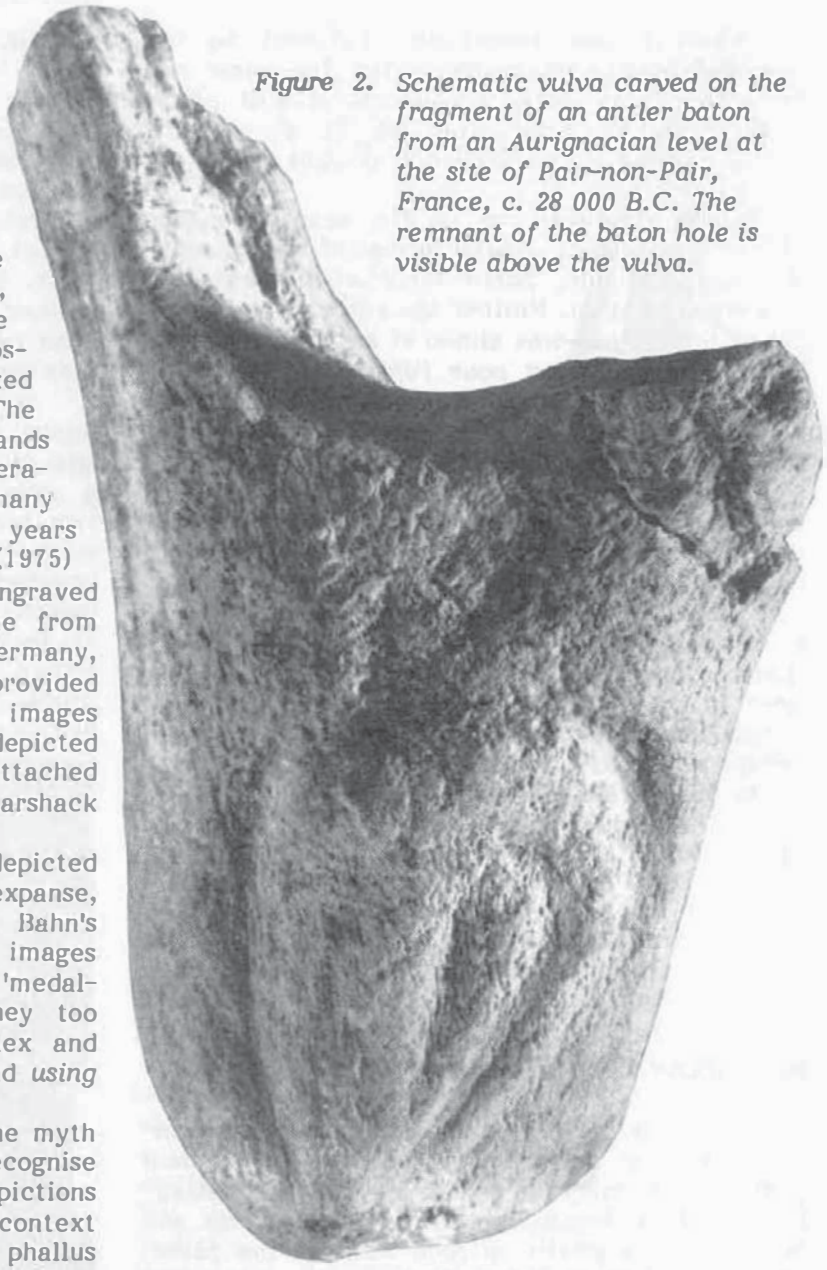


Figure 2. Schematic vulva carved on the fragment of an antler baton from an Aurignacian level at the site of Pair-non-Pair, France, c. 28 000 B.C. The remnant of the baton hole is visible above the vulva.

Figures 3a, 3b.
A phallus inside a vulva incised on a stone from the Pyrenean site of Isturitz, France, c. 21 000 to 20 000 B.C.

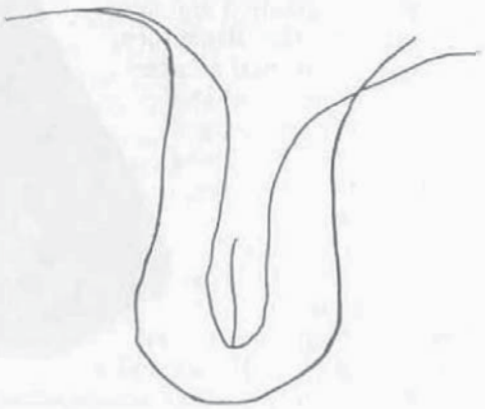


Figure 4.
A phallus inside a vulva incised on a stone from the Magdalenian site of Gönnersdorf, Germany, c. 14 000 B.C. After Bosinski (1975).

Figure 5.
One face of a fragmented bone incised with fish tail vulvae, from the site of La Madeleine, France, Magdalenian period. After Breuil and de Saint-Périer (1927).

in diverse ways, both in rock carvings and on bone and antler, yet no one has yet implied a 'phallic obsession' to these cultures. Yet the problem is not this simple and we are dealing with far more than mere depiction.

Bahn quotes Stoliar's criticism of Breuil for not having conducted a careful analysis of all the materials. But neither Stoliar nor Bahn, nor any of the others that Bahn quotes or cites, has conducted such analyses. Each argues largely from the published literature or from a visual surface scanning of the images. Bahn, for instance, criticises Breuil for claiming that a 'fish tail motif also represented a female organ' (Fig. 5). Bahn does not indicate that the fish tail vulvae from the site of La Madeleine are associated with a clearly rendered phallus, a fact that probably influenced Breuil in his interpretation. Breuil made numerous errors in rendering images, but he was closer to the truth in his understanding of these fish tails as *also* vulvae than Bahn realises.

From a Magdalenian level at the site of Laugerie Basse at Les Eyzies, France, on the Vézère river and a short walk from La Madeleine, also on the Vézère, there comes an antler baton on which there is carved a reindeer head on one face and a horse head on the other, each head being associated with a realistic fish tail, including rays, that is *also* a vulva (Fig. 6a, b). A profound question is raised by these two neighbouring

Figure 6b.
Close-up photograph of the carved and engraved fish tail vulva on the baton from Laugerie Basse. The tail is finely marked with lines indicating the rays.

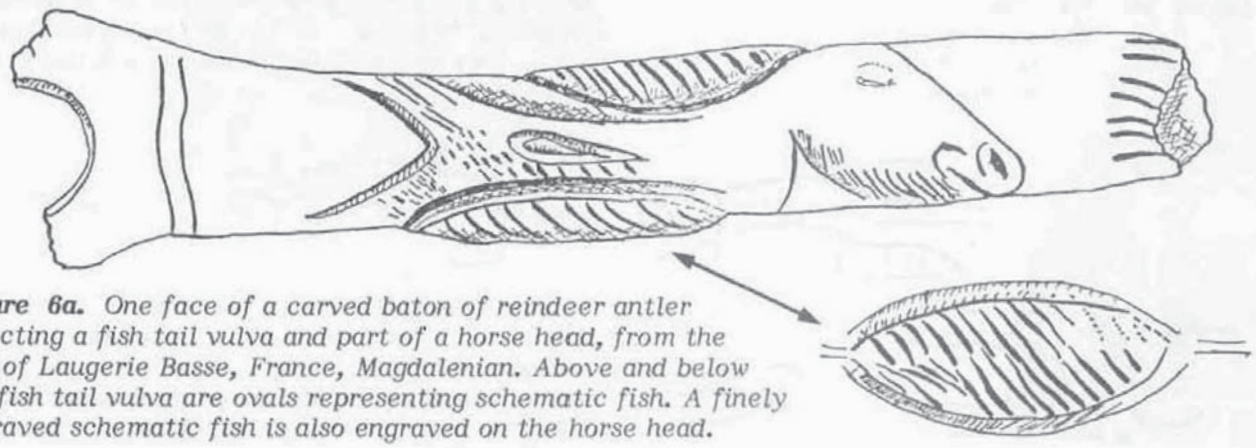


Figure 6a. One face of a carved baton of reindeer antler depicting a fish tail vulva and part of a horse head, from the site of Laugerie Basse, France, Magdalenian. Above and below the fish tail vulva are ovals representing schematic fish. A finely engraved schematic fish is also engraved on the horse head.

Figure 7.
Head of a male salmon in the time of spawning and egg laying, incised on a fragment of bone, La Vache, France, terminal Magdalenian.

and contemporaneous examples, a question not only about the fish tail/vulva but about the associations and contexts in which it is found.

If the fish tail vulvae schematically or abstractly represented the female salmon in the time of spawning, the period when reindeer cows and mares were calving, then these fish-tail/vulvae may have been seasonal images that were conceptually related to the many clearly depicted male salmon of the spawning period, with the kipe or hook on the lower jaw that the male acquires only at this time (Fig. 7) (Marshack 1970a, b; 1972, 1975). There is in fact the image of a male salmon with the hook on its lower jaw carved on the limestone ceiling of the Abri Poisson in the Gorge d'Enfer only a five-minute walk from Laugerie Basse and the Vézère, and not far as well from La Madeleine. The fish tail/vulva may therefore be an abstracted and schematic image of a type different from any that has yet been conjectured or discussed in the literature. That the vulva in the animal world was recognised, imaged and used symbolically was indicated in this journal in my analysis of the many vulvae that were added to a lioness in the Pyrenean cave of Trois Frères (Marshack 1985).

Still another question is raised: were the depicted phalli and vulvae at Isturitz and Gönnersdorf representations of *human* sexual organs or were they generically schematic representations of the concept of male/female fertilisation in the natural world? I have rather profusely documented the Upper Palaeolithic depiction of the dimorphic sexual and seasonal characteristics and behaviours among different species. If the general concept of sexual fertilisation was present, do we consider the images of the vulva and phallus to be 'erotic' as such images are considered at times in the modern Western world, or are they more aptly images of the male/female principle in procreation and fertility, as the *yoni* and *lingam* are considered in historic India? Logic, reasonableness, ethnographic analogy and *a priori* assumption cannot provide the answer. I am not discussing Leroi-Gourhan's thesis concerning a male/female opposition in all Upper Palaeolithic iconography.

The complexity of the problem is indicated in still another of Bahn's illustrations (Fig. 4:7), the engraving on antler from La Madeleine that Leroi-Gourhan used as his admittedly 'crucial' example in hypothesising a pervasive sexual polarity in all Upper Palaeolithic symbolism. It is interesting that both Leroi-Gourhan and Bahn render this crucial composition incorrectly. Microscopic analysis reveals an extraordinary complexity. I present the composition (Fig. 8a, b) both as engraved (Marshack 1972) and as exploded to indicate the discrete elements involved. We have *two* phalli, one facing a bear head and the other inserted into a vulva. The vulva at left seems to be attached to or to continue into the ovaries. The larger phallus at right is facing and is connected to the bear by an emanation from its nose. What we have are not isolated depictions of phallus and vulva, but a complex set of associations in some sort of culturally defined context. Are these, then, the *human* phallus and vulva or do we have a composition in which the intended reading is not derived from the recognition of any one element but from the *set*?

If we turn to the Upper Palaeolithic cultures to inquire about possible aspects of the Magdalenian tradition, we find that there are a number of images of bears that have been ritually or symbolically killed. Some are depicted with blood streaming from the nose and mouth or from body wounds. In one instance, at Mas d'Azil in the Pyrenean foothills, there is the engraving of an ithyphallic man apparently spearing a standing bear (Marshack 1972). That image had raised

Figure 8a. The engraving on a fragment of antler from La Madeleine, France, Magdalenian period, depicting two phalli, the smaller within a vulva. The bear head has an emanation from its nose and a barbed form at the neck which may be the spinal column of a killed bear or a harpoon.

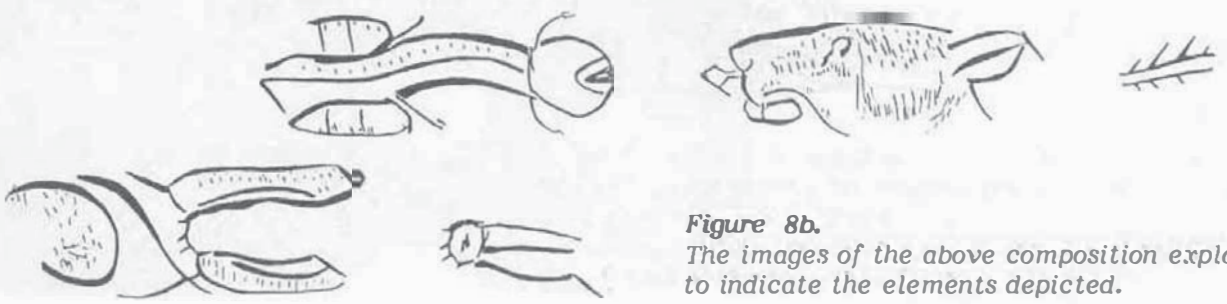


Figure 8b.
The images of the above composition exploded to indicate the elements depicted.

a question and I had discussed the problem with François Bordes, one of France's leading archaeologists at that time. I had asked, could the ritual or sacrificial killing of a bear in the springtime when it awakens from winter hibernation be related to the generalised renewal of nature in the springtime? Bordes commented that such ideas concerning possible ritual and symbol could never be proved. A short time later, however, in 1973, he sent me an article from a French journal which described at great length a historic Basque bear hunt and ritual killing, performed by men as an annual springtime ceremony of renewal within the French Pyrenees. I am not claiming a derivation from an Upper Palaeolithic tradition, but I do wish to indicate the depth and difficulty of the problem involved. The bear in the La Madeleine composition may be depicted as being 'killed' by a harpoon thrust in its neck. Harpoons are depicted in late Magdalenian art and weapons are depicted in animals. The emanation from the bear's nose could then be the spiritual blood of the life force which symbolically invigorates the male principle and the procreation and renewal of nature that comes with the spring. This is not intended as a reading or interpretation of the composition, but an indication that probing internal analyses of the materials and traditions raise questions at many levels. I touch again on the problem of 'eye-balling' versus analysis.

Figure 9.
Close-up photograph of the carved bell-shaped vulva on the 'Venus' of Willendorf, Austria, Upper Perigordian (Gravettian) period, c. 26 000 B.C. The vulva is similar in shape to many found in the Aurignacian period in France and the Magdalenian period at Tito Bustillo and Castillo, Spain.

Bahn mentions the possible lack of authenticity for the crude 'Venus' from Monpazier with its huge carved oval vulva. When I studied the piece by microscope soon after it was found, I ascertained that there were barely visible specks of pure red ochre embedded in the crevices at the rear. Red ochre is found often on the early 'Venus' figurines—on the 'Venuses' of Willendorf in Austria, Laussel in France, Grimaldi in Italy, Dolní Věstonice in Czechoslovakia etc.—but the fact is not generally known among amateurs who might fake a piece. In any case the flecks of red ochre would disappear in the making of a mold to prepare a cast. This is a class of evidence that cannot always be determined by scanning for 'style'.

Bahn also throws doubt on the bell-shaped painted vulvae in the Spanish cave of Tito Bustillo. It is clear that he has not examined the corpus of Upper Palaeolithic vulvae at first hand. There is an exquisitely carved 'bell' vulva carved on the 'Venus' of Willendorf (Fig. 9). The Magdalenian bell vulva of the Tito Bustillo type occurs also in the Spanish cave of Castillo (Marshack 1972) but this time closely associated with a large plant image. (A study of the Upper Palaeolithic plant images and the nature and range of their associations is being prepared for publication.) A comparable association of a plant with surrounding vulvae is engraved on a rock in Baja California, Mexico (Fig. 10). In these American cultures the carved vulvae and cupules on rocks have been ethnographically reported to be associated with human fertility rites and seemingly also to fertility of 'mother' Nature. The presence of such data raises again questions concerning the complexity and possible significance of the *set* and the *association* rather than merely stressing the shape, form or style of the vulva image itself. It is unfortunate that Bahn's critique is based so largely on his reading of the literature and his scanning of individual images. He discusses, for instance, the horizontal lines that cut across the hips and buttocks of the carved and fired-clay figurines from Dolní Věstonice, Czechoslovakia (Fig. 4:6) and questions that they represent vulvae. Had he carefully studied the body of these images he would have found that the lines represent *hip belts*, a costume item that is common among hunting-gathering peoples. One example in fact is carved as a three-dimensional plaited cord.

Figure 10.
Engraved plant surrounded by vulvae on a rock surface in Baja California, Mexico. After Crosby (1975).

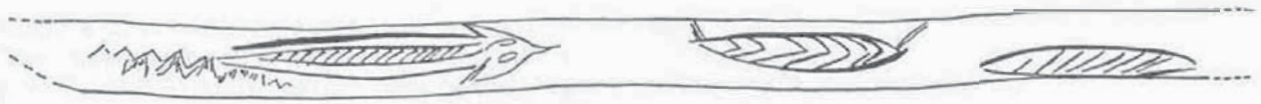


Figure 11. Three engraved fish on a fragment of bone, depicted in a sequence of increasing schematisation. The fish at right is shaped like an oval. The fish at left has schematic water beneath it. Petersfels, Germany, Magdalenian, c. 14 000 B.C.

As a result of his surface study of the images Bahn lends support to the suggestion of Stoliar and Lorblanchet that one should be content with the simple classification of the images based on their shape or 'style'. This seems like objectivity but is the most profound form of observer subjectivity, the search for recognisable geometric forms or elements without attempting a deeper inquiry into their derivation, development, variation, range of associations and contexts and modes of use. I am not asking for interpretation, but for deepening modes of methodological analysis. I present a final example of the fundamental problem raised by Bahn's paper. Bahn criticises Breuil for suggesting the derivation of certain schematic motifs and for suggesting that oval motifs in certain Magdalenian compositions represent fish. Bahn has apparently not studied the body of such imagery for Breuil was right in many of his instances. I have published (Marshack 1972) the analysis of a Magdalenian composition in which a realistic fish is schematised by a series of steps into a final striped oval (Fig. 11). Two striped ovals of this type appear with the fish-tail vulvae on the baton from Laugerie Basse. The schematic fish oval is found in the dispersed Magdalenian, from Poland and Czechoslovakia to Petersfels in Germany and as far south as Santander, Spain. Such horizontal ovals on the Magdalenian bone artefacts are often intended to schematically represent schools of fish. In addition, within the body of engraved materials in the Franco-Cantabrian area, both the vulva and the fish are often used as associative or conjunctive motifs in complex compositions.

We have a profound problem then in the study of such imagery. Merely categorising images and motifs by their appearance and style can help in studies of chronology and image dispersal, but categorising them by their shape and disassembling them as 'design elements' without reference to or inquiry into their cultural derivation, their modes of use, the classes of artefact on which they appear or the range of their associations often leads to the purest form of subjectivity. Leroi-Gourhan (1965) categorised the Upper Palaeolithic signs and motifs on the basis of their 'geometrical' appearance; he then tried to interpret them on the basis of the similarity in groups of signs. He never undertook a profound study of the different derivation of the many classes of sign and motif. His categories and his interpretations were mainly subjective. More recently M. Conkey (1977, 1980a, 1980b, 1981, 1982) has attempted to disassemble motifs and images 'geometrically' on the basis of what she saw reductively as 'design elements' without probing the derivation and development of these images within the dispersed Magdalenian cultures. The Petersfels and Laugerie Basse ovals were assigned primarily to the cave of Altamira. Within such modes of analysis the vulvae can become 'vertical ovals', 'marked triangles', 'marked circles', 'marked bell-shaped figures', just as the fish can become 'horizontal ovals', 'striped ovals', 'hatched ovals', 'sets of ovals', without recognising their common derivation or ultimate reference.

Bahn is one of the most innovative and creative of the new generation of researchers into the complex Upper Palaeolithic cultures. He has made significant contributions and has raised new and intriguing questions when he has taken a problem in hand and has applied himself to careful, direct research of the materials. Quoting authority, criticising authority and relying on the subjectivity of surface classification is not the road to deepening science, no matter how it is wrapped in theory and seeming logic and reasonableness.

Alexander Marshack
(Peabody Museum of Archaeology and Ethnology, Harvard University)
4 Washington Square Village
New York, N.Y. 10012
U. S. A.

REPLY

'Cherchez la Femme'

By PAUL BAHN

I welcome the positive and amiable nature of all these replies, which prove that one can make serious points in a light-hearted way, and that this in no way detracts from subsequent debate. I will endeavour to be brief, in order to help save the editor some time, and the world some trees. First, a few minor points:

I agree with John Clegg that the homogeneity of 'Venus figurines' has been much exaggerated by Gamble and others, but they do nevertheless constitute a recognisable category of objects from the Upper Palaeolithic, like spearthrowers or perforated batons, and to that extent it is justifiable to treat them as a group, while bearing in mind that they may well be scattered throughout the period.

Jean Clottes mentions other 'Venuses' with the vulva marked, and I am grateful for the reminder, although my list was not meant to be exhaustive—I was merely stressing that relatively few of these figurines have the genitalia marked at all, let alone as the focus of the composition. Indeed, Praslov has recently pointed out that a new figurine from Kostienki XIII differs from the others in the region precisely because its genitalia are clearly represented (1985: 185).

I am by no means 'embarrassed', in any sense, by the Monpazier figurine. Having never seen it myself, I was unable to give a personal opinion as to its authenticity, but I have the greatest respect for the verdict of scholars such as Clottes, Marshack (whose comments on the figurine in his reply are of great interest) and, especially, Léon Pales in this matter. Nevertheless, it remains true that some colleagues remain sceptical about its authenticity, and I believe that the principal reason for this is not so much its total lack of context (it was found in a ploughed field) but the treatment of the vulva which is completely 'un-Palaeolithic'—it is therefore the exception which proves the rule.

Paul Faulstich quite rightly criticises my point about the archaeology of the mind—I should have made it clearer that I was referring only to the new breed of 'cognitive archaeologists' who aspire to reconstructing past thought processes from archaeological data with a considerable degree of accuracy; he is correct that Palaeolithic iconography's content and location can give much food for thought, not only to archaeologists but also to scholars in many other disciplines. However, he seems to have misread one part of my paper—I did not compare the Laussel breasts with Wandjina heads; what I said was simply that some of the Aurignacian motifs have a breast shape, while others resemble Wandjina faces!

Heinz Hunger notes that I 'left the penis out'; this was partly through subjective interest (I have a female colleague who for some time has been planning to produce a critical review of the phallus in Palaeolithic iconography), and also because the phallus has been accorded far less importance than the vulva in Palaeolithic studies. Leaving aside the question of the 'signs', where anything which is longer than it is wide has been categorised as phallic, there are fewer definite penes, fewer motifs which have been claimed to be penes, and fewer definite male figures than their female equivalents (precisely because the genitals are not usually depicted—see again Praslov [1985: 186] for problematic male figurines). The supposed Palaeolithic obsession with sex has been based almost entirely on the 'Venuses' and the 'vulva' motif (though breasts have also been included at times—see Cornaggia Castiglioni and Calegari 1975).

Turning finally to Alex Marshack, I have the highest regard for his perhaps unique experience in examining Palaeolithic images, and particularly mobiliary art, at first hand. In most of his recent published comments, and this one is no exception, he seems to state that anyone who has not done a similar amount of 'slow, methodological research', or 'careful, first-hand internal analysis' of the original objects should not bother to voice any opinions, since these opinions will inevitably be subjective and based on a mixture of eyeball viewing and a knowledge of the literature. I have three points to make about this. First, it makes little sense for all other specialists to desist from argument until they have repeated or emulated Marshack's analyses—there would be silence for twenty years! His work will gradually be repeated and checked, like that of Breuil, by future generations of scholars armed with new techniques. Second, I do not accept that it is always necessary to study an object at first hand to have pertinent views about it: a handful of men have trodden the lunar surface and they have a special and unrivalled knowledge of it; but there are far more earth-bound scientists who know just as much, if not more, about the moon from the analysis of photographs, samples, print-outs, reports and so forth.

Third, I do not believe that viewing through a microscope always enhances objectivity. It merely provides additional data of various kinds. Marshack talks disparagingly of 'visual surface scanning' or 'eye-balling', as opposed to the use of the glass eye of the microscope—but there is an eyeball at the other end of the microscope, and behind it a brain which needs to assess what it is seeing. The entire discussion therefore boils down—as Clottes perceptively remarks—to the difference between interpretation and identification, or, in Clegg's terminology, between ideas and accepted hypotheses.

As I stated in the paper, the only definite vulvae—or penes—are those depicted in the context of human bodies. In every other case the attribution remains unproven. Occasionally, identification is probable: I mentioned the clay vulva in Bedeilhac, which, as Clottes says, is almost certainly correctly identified—but one must bear in mind that it is a three-dimensional model; were it simply engraved or painted, identification might be far less straightforward.

This is not to say that depiction must necessarily be complete, detailed or three-dimensional for identification to be possible. Clottes also cites the example of the three bison painted in the Réseau Clastres; the first is naturalistic, the others are very incomplete. But I believe that, even without the proximity of the first, the two incomplete specimens could confidently be identified as bison, through comparison with complete examples elsewhere: the shape of the dorsal line and the position of the 'mane' are distinctive. Similar identifications are often made from the dorsal line of horses or mammoths. Such figures are not so much stylised as abbreviated, perhaps a kind of 'Palaeolithic shorthand'. Likewise I agree that most of the stylised female figures are recognisable, though here again it is necessary to have more complete examples for reference.

Clottes and (indirectly) Lorblanchet also raise the crucial point of the possible ambiguity of some motifs, implying that the artists were playing with forms, perhaps making visual puns. It is interesting that the Dellucs have also shifted to this position: in a recent article (1985) they suggest that a 'vulva' carved at Laussel may *also* represent a horse hoof print, and a 'vulva' engraved at Comarque may *also* depict a bird track. There is, of course, no way to prove purposeful ambiguity in cases of this type, but the hypothesis is at least an improvement over an unquestioning interpretation in sexual terms.

Ambiguity is also apparent in the Breuil/Mar-

shack concept of the 'fish tail vulva'; leaving aside tasteless jokes regarding the aptness of such a visual pun, I find it somewhat harder to accept. It is apparently derived exclusively from the La Madeleine engraving (Breuil and St Périer 1927: 130); it seems a reasonable hypothesis that the central motif here is a phallus and thus equally reasonable to suppose that the other two might be fairly realistic vulvae. But fish tails? Would they spring so readily to mind if Breuil had not included the engraving in a book of fish pictures? Even if this were a purposeful visual pun, the same does not necessarily apply in Marshack's other example—his engraving from Laugerie Basse—where neither a vulva nor a fish tail seems very clear. And even if one were to give this engraving the benefit of the very considerable doubt, Marshack is building castles on sand in his speculations about schematic representations of spawning salmon.

He has every right to make such speculations, and they often lead to useful insights; but the point is that they are ideas based on interpretations, not on identifications. Looking through the examples and illustrations in his reply, I cannot see anything apart from the animal heads and the Willendorf vulva (found in context) which can be positively identified. The Placard baton *may* represent a 'vulva between two truncated legs'; the Pair-non-Pair baton *may* display a 'bell-shaped vulva'; the Isturitz and Gönnersdorf engravings *may* represent a phallus inside a vulva—but these are all interpretations, assumptions. Other notions are possible and it is noteworthy that the St Périers thought the Isturitz example was a set of bovid horns! I fail to understand how microscopic analysis of objects such as the Russian 'medallions' can 'reveal' that they are vulvae—only the artist can reveal this. The microscope can only provide additional support to one hypothesis or another.

This point arises again with regard to the La Madeleine composition featuring the bear head. In my paper I noted the differing opinions as to this engraving's contents, even among those who supported the sexual interpretation. The *relevé* which I gave is not significantly different from that of Marshack, and I simply do not understand how his own leads him to the confident identification of not one but two penes, one of them inserted into a vulva, and even ovaries! But even the 'penis' facing the bear may be nothing of the kind; one could, for example, assert that it represents a phallic fungus (I did once follow this up with an expert on fungi, but he felt that the engraving was not sufficiently realistic to be sure) and that the picture thus presents a 'complex set of associations' involving hallucinogenic mushrooms and a bear cult!

Similarly, Marshack believes that a number of bear images have been ritually killed and have blood streaming from nose/mouth/wounds; again, he may be correct, but it is one interpretation among many (see Barandiarán 1984)—he himself later refers to 'the spiritual blood of the life force', which may indeed be a more likely explanation (Smith, in press). But he also sees the image by the La Madeleine bear's neck as a harpoon; I also note that, in his own response in *RAR* 3 (1) p. 69, he mentions depictions of 'darts with shafts', and weapons of various types from highly realistic to abstracted—but 'there are also marks within animals which are not darts or weapons'. This looks very much as though he is putting names to motifs which remind him of something, and leaving unidentified those which do not.

We all do this to some degree, of course; this is how we identify the animals depicted. But I stress again that for isolated motifs such as those under discussion here, only the original artists can reveal what was depicted. All the ideas we put forward are mere interpretation. Marshack feels that his own are well-founded because of his experience and knowledge of the derivations of different motifs (though I would argue that derivation is a very shaky concept in many cases—Breuil's sequences of degenerating motifs were placed in what he quite subjectively considered to be the correct order); but they remain interpretations nonetheless, whether it be the 'bell-shaped vulvae' in Tito Bustillo or Castillo, or the 'plant image' in the latter: I have myself published a review of plant images in Palaeolithic iconography (Tyldesley and Bahn 1983) but I consider very few of the examples to be solid identifications rather than speculations.

Marshack's engraving from Petersfels has a figure which certainly looks something like a fish to the twentieth century eye (or microscope); the other two figures could be a great number of things, if indeed they were meant to represent something specific. He claims that the fish has been 'schematised by a series of steps into a final striped oval'; once again he may be right, but I see no proof. The two ovals do not resemble the fish in the same way as, for example, the two incomplete bison of the Réseau Clastres resemble the complete one. It is a considerable and dangerous step to proceed from examples like this to the claim that all striped ovals are schematised fish. And I find it preposterous that Marshack can state that 'Such horizontal ovals . . . are often intended to schematically represent schools of fish'; had he used the words 'may sometimes, in my opinion, have been intended to represent', I would not argue with him.

As Paul Faulstich says, we must not close the door to intelligent and well-informed interpretation; if we limited ourselves to the figures which can be positively identified, we would be able to discuss only a tiny fraction of rock art. But we must be careful to differentiate what is definite, or very widely accepted for solid reasons, from that which is mere supposition. And I repeat that, where the 'vulva' motif is concerned, it does not matter how many examples you see or how long you gaze through a microscope—if it is not inside a female body, you are merely saying what it looks like to you. If it closely resembles one of the definite vulvae, your case is stronger; but it is never proven. I am grateful to Hunger for bringing Jesse Warner's work to my attention—his element lists, like

Munn's data from the Walbiri, underline how many interpretations are possible from simple motifs of this kind. (As a digression here, I should point out that the intriguing similarity between the Lascaux shaft scene and the Arizona ithyphallic figure with the bird stick in Hunger's illustration was first pointed out by Pericot [1962], and merits further study.)

Even where one can accept the interpretation that genitalia are depicted, it does not follow that Palaeolithic people were overly concerned with their own sexuality; Marshack makes the useful point that general sexuality rather than human may be involved; or, as Hunger states, the motifs may have an asexual purpose, and thus denote gender rather than sexuality. I should add, however, that I am far less sure than Hunger that his Laussel figure depicts a copulation—I see no proof of this whatsoever.

It might be thought that the 'sex-maniac' view of Palaeolithic man had reached its peak with the article by Guthrie (1984) which, although it has its good points (Bahn 1985), nevertheless greatly exaggerates the importance of sex and genitalia in Palaeolithic iconography. Now I find that Kurtén (1986) has taken the idea to extremes. Like Guthrie he compares illustrations of certain Palaeolithic images with modern pin-ups; and he makes some staggering claims: 'Female figures often [*sic*] appear in sexually inviting attitudes, which may be quite the same as those seen in the most brazen pornographic magazines. There are also anatomically detailed pictures of the vulva, showing the female sex organ sometimes frontally, sometimes inverted and from the back, open to penetration' (Kurtén 1986: 114-6). I can only say that apart from the reclining females of La Magdelaine I cannot think of any such 'poses'. Kurtén has a vivid, not to say lurid, imagination; and it is the diffusion of views like this in popular books on the Palaeolithic which justifies the writing of my paper.

Dr Paul G. Bahn
428 Anlaby Road
Hull, HU3 6QP
England

Résumé. Durant les soixante quinze années précédentes, les savants ont suivi Breuil en interprétant certains motifs Paléolithiques Supérieurs comme des vulves prématurées. Ce journal examine cette hypothèse et trouve qu'elle est basée sur un raisonnement erroné ainsi que sur des désirs pris pour des réalités. Les peintures définies des organes génitaux féminins sont extrêmement rares en iconographie Paléolithique, un fait qui jette un doute considérable sur les théories. Ce qui affirme que l'interprétation de la 'vulve' a été appliquée sans discrimination à une grande variété de motifs, et n'est pas seulement subjective mais aussi tautologique. D'autres interprétations sont certainement possibles, et en tout cas les motifs ont besoin d'être traités plus objectivement, et différenciés plutôt que les englober sous un en-tête simplifié.

Zusammenfassung. Für nunmehr fünfundsiebzig Jahre sind Gelehrte Breuil, in seiner Deutung gewisser Motive des frühen Jungpaläolithikums als Vulvae, gefolgt. Diese Hypothese wird im vorliegenden Aufsatz untersucht, wobei sie sich als auf Trugschlusz und Wunschvorstellung beruhend herausstellt. Eindeutige Darstellungen weiblicher Genitalien sind auszerordentlich selten in paläolithischer Kunst, was beträchtliche Zweifel zuläszt bezüglich Theorien, denenzufolge Sexualität in der Religion und den semantischen Systemen dieser Zeit eine Hauptrolle gespielt haben sollte. Es wird erörtert, dasz sexuelle Deutungen wahllos für eine Vielfalt von Motiven herangezogen wurden, was nicht nur subjektiv, sondern auch eine unnötige Wiederholung ist. Andere Auslegungsmöglichkeiten bestehen, und auf jeden Fall sind die Motive mehr objektiv zu behandeln und zu differenzieren, statt unter einer einzelnen Rubrik zusammengefaszt zu werden.

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KEYWORDS: Stochastic - Emblemic - Panaramitee - Simple figurative - Social interaction

STOCHASTIC VS EMBLEMIC: AN ARCHAEOLOGICALLY USEFUL METHOD FOR THE ANALYSIS OF STYLE IN AUSTRALIAN ROCK ART

Natalie R. Franklin

Abstract. The analysis of stylistic variation has always been a preoccupation of archaeologists. However, the term 'style' has assumed numerous meanings which have not been distinguished from each other, resulting in a great deal of confusion. This paper enumerates some of these coexistent definitions and models of style and endorses the views that style is a measure of social interaction and the boundaries between social groups. Within the latter model two concepts of style are expounded: *emblemic styles*, which deliberately mark and maintain the boundaries between social groups, and *stochastic styles*, which reveal their own patterns of distribution and variation, and do not necessarily coincide with socially defined boundaries. The archaeological usefulness of these two concepts of style is evaluated through an examination of Australian rock art styles. It is concluded that an approach using the concept of stochastic styles, which does not predetermine the kinds of explanation one can propose for stylistic variation, allows a greater insight into and understanding of the data.

The analysis of stylistic variation in prehistoric artefacts has played a significant role in archaeology since the beginning of the discipline. However, in the extensive literature dealing with this subject, the term 'style' has assumed a seemingly infinite number of meanings, resulting in a great deal of confusion (cf. Plog 1983 for references). Generally, there has been a failure to distinguish between the broad quality 'style' and particular manifestations of that quality on prehistoric artefacts. Therefore, assumptions implicit in particular models of style are often not clearly articulated with the concrete archaeological data they attempt to explain, and this has merely added to the confusion associated with the analysis of style. This paper is an attempt to remedy this situation. It presents an evaluation of two models of style in use in the literature through an analysis of a particular set of archaeological data—a sequence of Australian rock art styles proposed by Maynard (1976, 1979). Style in this discussion is considered as a 'highly specific and characteristic manner of doing something' (Sackett 1977: 370), or a particular effect produced on an artefact, which is peculiar to a specific time and place. Models of style which can be used in the archaeological analysis of Australian prehistoric pictures are considered more valuable than those necessitating a purely ethnographic approach.

Style as a Measure of Social Interaction

Whallon (1968) has observed that style has many aspects and levels of behaviour which can be analytically distinguished and measured, and that each aspect demands a distinct method of analysis. He states that (1968: 126):

> ... the nature of the diffusion of stylistic ideas and practices both within and between communities, will be determined by the nature and interaction among artisans. The aspect of style concerned, the rate of diffusion, and the directions and limits of diffusion, will be conditioned by the kind, frequency and channelling of interaction among the products of the stylistic material.

Therefore, in this social interaction model of style, two aspects have been stressed in attempts to infer the characteristics of prehistoric social organisation by measuring stylistic variation:

(1) The similarity of stylistic attributes in different areas of a site or at different sites within a region.
(2) The greater the interaction between social units the higher will be the stylistic similarity between sites, and the lower the degree of stylistic homogeneity within sites (Plog 1980).

The latter aspect is important in a consideration of the change within Australian prehistoric rock art from the Panaramitee style to the simple figurative styles as will become apparent later in this paper.

Stochastic vs Emblemic: Style as a Measure of Social Boundaries

Style has also been used to measure *social boundaries*, and as such has assumed two distinct forms. Style is a conscious statement of group solidarity or identity, expressed in items of material culture (Conkey 1978). This form has been described by Wiessner (1983) as *'emblemic style'*, represented by, for example, flags or emblems

MS received 5 March 1988

that transmit a message of group identity. At the unconscious level, style relates to the individual's perception of the world from a culturally-shaped perspective (Conkey 1978). I have labelled this form *'stochastic style'*, whereby each cultural trait has its own pattern of diffusion and variation, and only occasionally coincides with an ethnographic boundary of some sort. Stochastic styles are randomly changing, since there are no influences which encourage change in one direction only, just as, for instance, increasing efficiency in function lends direction to the evolution of functional artefact types. It is envisaged that human conservatism results in styles changing slowly, so that the concept of stochastic styles is intended to imply *random and directionless*, but not rapid, change. However, the initial change from one style to another might not be random, as, for example, with the change from the Panaramitee style to the simple figurative styles. Stochastic styles would form a clinal distribution (cf. Wobst 1977), whereas the other form, which I will call *'emblemic styles'* after Wiessner (1983) would form a distinct, clear-cut pattern.

In practice, how does one recognise from archaeological remains which sort of style one is dealing with, since they may both manifest themselves in identical circumstances? Use of stochastic styles, as opposed to emblemic styles, avoids this problem since it recognises that styles have their own patterns of variation and distribution which do not necessarily coincide with a particular social group. The work of Clarke (1978) and Hodder (1978) also indicates the greater archaeological usefulness of stochastic styles. Clarke (1978) recognised the problem of correlating the different hierarchical aspects of material culture with those of linguistics, social organisation and genetics, and argued that this was not surprising since all of these entities are 'arbitrary horizons of unspecified definition' (p. 367). However, he also states that lack of a 1:1 correlation between different entities did not imply that there was no correlation whatsoever—it merely stressed the complexity of the relationship (p. 365).

Ethnographic work undertaken by Hodder (1978) in the Baringo district of western Kenya found that the tribal groups studied were each characterised by distinctive styles of dress, and distributions of items of material culture indicated clear breaks at the tribal boundaries. However, despite the fact that each group maintains distinct styles and customs, individuals were permitted to move across tribal borders, and on marriage into another tribe usually changed their dress accordingly. Thus, although distinct styles can be discerned within the material culture of the Baringo district, distinct, sealed tribes cannot.

The concept of stochastic styles, then, appears to be more acceptable in archaeology, since it acknowledges the complexity of the relationships between different classes of archaeological evidence. It does not assume what causes the styles, as with emblemic styles, which assume that particular 'tribes' are responsible for the artefactual variation observed.

USE OF THE MODELS IN AUSTRALIAN PREHISTORIC ROCK ART

How might these two models, the 'social interaction' theory of style, and style as a measure of social boundaries, be used in analyses of Australian prehistoric rock art? However, before an evaluation of the two models can be undertaken, it is necessary to place the discussion within the context of Australian rock art styles—that is, the synthesis of Australian prehistoric pictures proposed by Maynard (1976, 1979).

A Sequence of Styles in Australian Rock Art

Maynard (1976, 1979) has presented a simple synthesis of rock art styles in Australia:

> ... there are within the whole corpus of Australian rock art, three major identifiable styles which can be placed in a relative sequence . . . I have called the three major units, in the order in which I believe them to have been used in Australia, Panaramitee style, Simple Figurative styles, and Complex Figurative styles (Maynard 1979: 91-2).

Maynard's styles are described as follows:

The Panaramitee style - pecked engravings of macropod and emu tracks, human footprints, circles, dots, crescents, spirals, radiate designs and only a small proportion of figurative motifs other than tracks. They are found over a large area of south and central Australia, in western New South Wales, at Laura in Cape York and in Tasmania (Fig. 1). The most significant feature of the sites in south and central Australia is the consistency of the proportions of motifs, as indicated by Edwards (1966). By contrast, the engravings of Laura and Tasmania do not parallel the south / central Australian results in terms of the proportions of different motifs, but since they display identical techniques, forms and range of motifs, they too are included within the Panaramitee style (Maynard 1976, 1979).

Simple figurative styles - figurative motifs, in engraved or painted, solid or outline form. Decorative details, although rare, consist of stripes, bars, dots and the like as infill, and different colours are used in the outlining of painted solid figures. Motifs usually consist of simplified silhouettes of human or animal models.

Maynard claims (1979: 99):

> Most portrayals are strongly standardised. Human beings are depicted frontally, animals and birds in profile, snakes and lizards from above. Normally only the minimum visual requirements for recognition of the figure are fulfilled by the shape of the figure.

Simple figurative styles occur around the northwestern, northern and eastern edges of Australia (Fig. 1), and the sites in western New South Wales constitute the furthest inland examples.

Complex figurative styles are extremely diverse, but 'their common characteristic, and that which distinguishes them from Simple Figurative styles, is that they are, in some respect, more sophisticated than crudely naturalistic' (Maynard 1979: 100). They are found exclusively in coastal regions of the northwestern corner of the continent (Fig. 1), and examples are the Mimi and X-ray paintings of the Arnhem Land escarpment, the Bradshaw and Wandjina figures of the Kimberley region, and the engraved Wood-

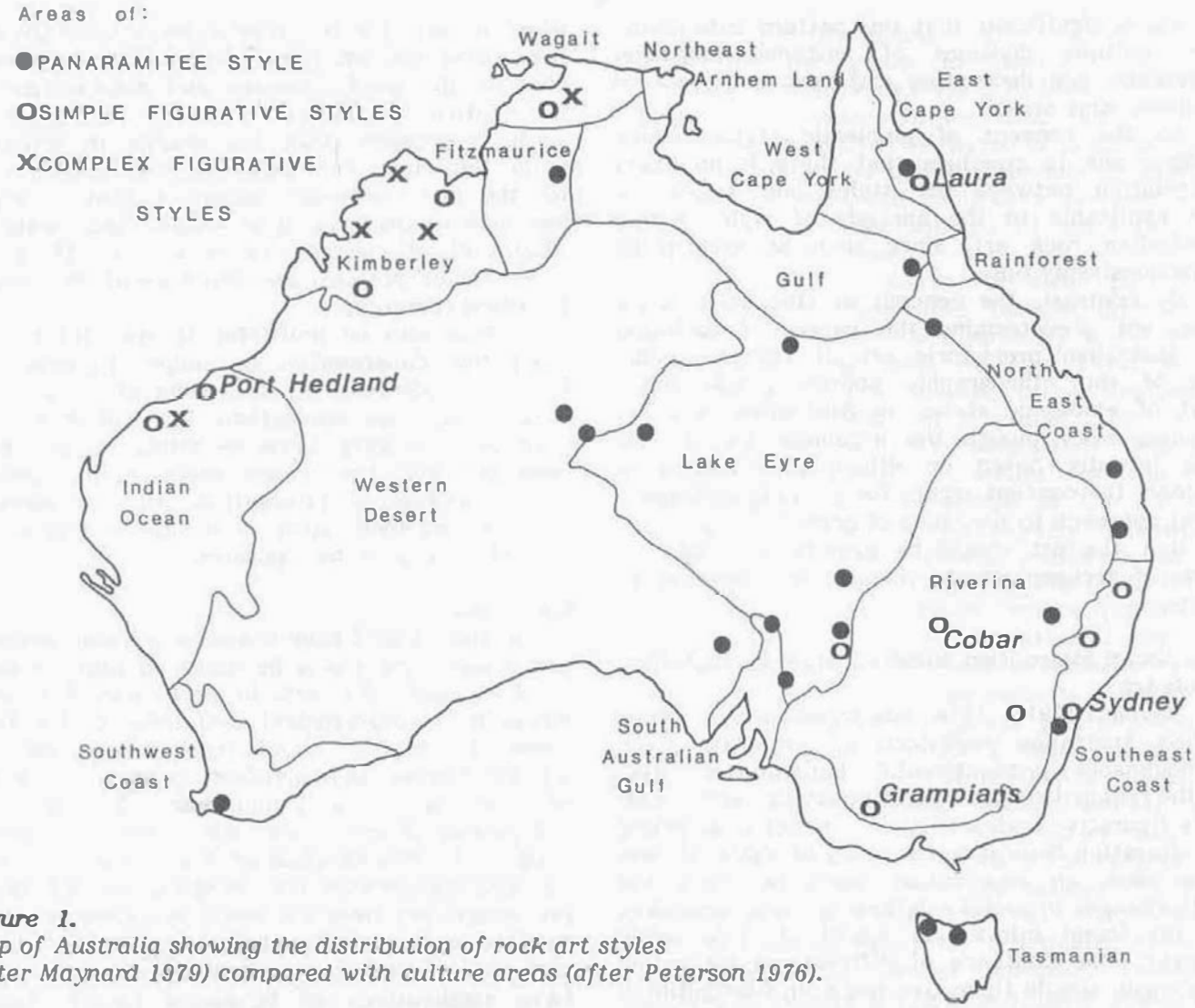

Figure 1.
Map of Australia showing the distribution of rock art styles (after Maynard 1979) compared with culture areas (after Peterson 1976).

stock style (formerly 'Kurangara' figures) found at certain sites in the Pilbara (Maynard 1976, 1979).

Maynard has stressed that the diversity of the simple figurative and complex figurative styles is in contrast to the uniformity and widespread distribution of the Panaramitee style:

> Throughout Australia, the end product of these changes in rock art was a pattern of considerable regional diversity among the most recent styles, in contrast to the uniformity of the ancient Panaramitee Style. Again, this situation parallels that of Australia's prehistoric stone tool industries - the wide and homogeneous distribution of the generalised early assemblages contrasting with the more regionalised distribution of later specialised types. It would be foolish to imply, at this stage, that a historical connection existed between specific styles and stone tool industries . . . but the similarity in the tendency toward specialisation and regionalisation may suggest the operation of similar cultural processes.

Having described Maynard's hypothesis (1979: 109) of rock art styles within Australia, it is now possible to evaluate the two models of style discussed above.

The Social Boundaries Model of Style in Australian Rock Art

It is clearly demonstrable that in Australia there is no 1:1 correlation between art styles and any other Aboriginal group as defined by the ethnography. White and O'Connell (1982) have noted that coherent recognisable clusters have not been derived from attempts to correlate such cultural features as art styles, languages, artefact forms, legends, or social and ritual practices. For example, in the highlands of south-eastern Queensland, three culture-areas, North-east, Riverina and Lake Eyre, which are based on drainage basins (Peterson 1976), join. Yet in terms of language, rock art style and communication networks as defined by the ethnography, the highlands form a distinct area in their own right (White and O'Connell 1982, Fig. 1). So, since different, distinct clusters cannot be recognised within different classes of material evidence for the recent past, we have little hope of recognising such clusters in prehistory.

It is also apparent that certain kinds of 'stylistically and technologically similar archaeological data' (White and O'Connell 1982: 102) occur over larger areas than are occupied by any Aboriginal group. This phenomenon is exemplified in Australia by the Panaramitee style, which, as noted above, is found over a large area of South Australia and central Australia, as well as in western New South Wales, Queensland and Tasmania (Maynard 1976, 1979), regions which conform to several culture areas (Peterson 1976, see Fig. 1). Edwards (1971) has made some important observations regarding four 'constant features' of Panaramitee style engraving sites, i.e. their proximity to regular water supplies, their association with occupation sites, their advanced weathering and heavy patination, and consistent relative proportions of motifs. He states (1971: 363):

'It seems significant that this pattern cuts across the multiple divisions of customs, language, artefacts, and decorative and cave art recorded in these same areas'.

So the concept of emblemic styles, which *obliges* one to conclude that there is an exact correlation between art styles and 'tribes', is not applicable to the analysis of styles within Australian rock art, since such an assumption is demonstrably false.

By contrast, the concept of stochastic styles does not predetermine the causes of variation in Australian prehistoric art. It argues against use of the ethnographic approach, and hence that of emblemic styles, in Australian rock art studies, even though the argument for its use was initially based on ethnographic premises. Instead, the concept argues for a purely archaeological approach to the study of prehistoric pictures, in that the art should be examined as another form of archaeological evidence, like stone artefacts.

The Social Interaction Model of Style in Australian Rock Art

Maynard (1976, 1979) has argued for a change within Australian prehistoric art from the more homogeneous pancontinental Panaramitee *style* to the regional simple figurative *styles* and complex figurative *styles*. If she is correct in asserting an alteration from *style* (singular) to *styles* (plural), then such an observation could be connected with changes in social relations or communication, as the social interaction model of style would predict. The existence of different styles within the entity simple figurative has been substantiated by a number of multivariate analyses (Franklin 1984). Therefore we need to ask: does such a change from homogeneity throughout Australia to diversity and regionalisation imply social change? Binford (1968) has noted a contrast between the absence of stylistic differences in the Acheulian and the presence of *style clines* in the Mousterian and *style zones* in the Upper Palaeolithic, and implies that some sort of social change has occurred to result in the increasing visibility of style zones in the archaeological record. She states:

> The function of style is thought to be a means of either group or individual identification with a product or class of products (Binford 1968: 275).

Similarly, Gamble observes (1982: 105):

> By concentrating upon a class of non-utilitarian display items, my intention has been to show that the significance of style lies in the possibilities that its investigation opens up for the study of palaeolithic social change.

He has noted that the homogeneity and pan-European distribution of the Upper Palaeolithic Venus figurines, his 'class of non-utilitarian display items' contrasts with the more localised occurrence of cave art.

Within Australia, Morwood (1984) has argued that the appearance of a distinctive art style in central Queensland, which he in effect equates with the simple figurative styles (1979), appears to have coincided with the onset of the Small Tool Tradition at 4300 BP (1984). He observes that the technological and artistic innovations which occurred at this time in the central Queensland highlands were associated with widespread changes in social relations and mechanisms for information exchange.

It is probable that the change in Australia from the more homogeneous Panaramitee *style* to the heterogeneous simple figurative *styles* has implications in terms of social change, although what sort of change this entails is difficult to ascertain at present, and must await the results of future research.

It may also be profitable to view the change from the Panaramitee to simple figurative in terms of a decrease in the extent of social interaction and communication. As noted earlier, stylistic similarity between sites, as has been observed for the Panaramitee style, implies a high degree of interaction, while differences in style between sites, as occurs in the simple figurative, implies the opposite.

Conclusions

In this paper I have discussed several concepts and models which may be useful in future studies of Australian rock art. In particular, I wish to stress the archaeological usefulness of the term 'stochastic styles', which does not assume the causes of variation in artefacts as does the concept of 'emblemic styles', and which indicates the complexity of the relationships between different classes of archaeological evidence. This discussion has also documented the change within Australian prehistoric art from the more homogeneous Panaramitee style to the heterogeneous simple figurative styles, and has suggested that this might have implications in terms of social change. It is noted that similar explanations have been offered for similar changes in European prehistory, and so in future consideration of Australian art styles within a broader perspective is held to be profitable.

COMMENTS

By WHITNEY DAVIS

As a formal statement of the particular respects in which different artefacts are similar to each other, a stylistic description does not necessarily have to make any predictions about the rate, tempo, direction or causes of variability, nor does it inherently call for any particular correlation between stylistic entities and historical (social, cultural, linguistic) entities (Davis 1987). To extract these predictions or effect these correlations—to use style as archaeological or historical evidence as well as a descriptive and critical tool—archaeologists and art historians must build further assumptions, by stipulation, into the general theory of style itself. For instance: *rate*—for Meyer Schapiro (1953), styles exhibit a 'constancy' of form and expression over measu-

rable (often considerable) periods of time or geographical distances; *tempo*—for Alfred Kroeber (1957), among many others, styles have a life-historical rhythm of birth, maturity, and senescence; *direction*—for George Kubler (1962), styles occupy a predictable 'form-class' trajectory governed by the constraining 'rule of series'; *causes*—for Giovanni Morelli and Bernard Berenson, differences among works result from the unconscious idiosyncrasies of different producers (see Hill 1977). All of these particular versions of the general theory of style are vulnerable to theoretical objections and to empirical falsification in some identifiable range of cases (see Ackerman 1963; Goodman 1978; Wollheim 1979; Sauerländer 1983). However, they are, of course, designed to answer specific archaeological or art historical questions which have been deemed worth asking.

Franklin rejects some assumptions of some versions of the general theory of style while accepting others which seem to me to be equally dubious. I am not sure that the distinctions put forward in her paper can be sustained in detail.

If we interpret stylistic similarity as indicating 'social interaction' among the producers of artefacts, then we must simultaneously interpret it as *also* indicating a 'social boundary'—for where similarity decreases, by hypothesis we are seeing less social interaction, and at the extreme, a social boundary between producers. The 'boundary' and 'interaction' interpretations of stylistic variability are therefore really the same thing, differing only in foregrounding the low or the high degrees of stylistic similarity: map the low levels, and we map social boundaries; map the high levels, and we map social interactions. This bounded-interaction model probably does work best when it is handled 'stochastically' (we are dealing with multi-modal or clinal distributions of shared or similar attributes of artefacts) and when a one-to-one correlation between the style and a group of producers is not expected to be constant through time or universal across space (the group itself might change its composition etc.). However, absolute differences among or barriers between producers should result in relatively clear-cut bimodal distributions. The bounded-interaction model of stylistic variability therefore should not privilege either bimodal or clinal distributions *a priori*.

Described 'stochastically' or not (a decision the typologist makes pragmatically), it seems to me a completely separate question whether the style-identified-as-sphere-of-bounded-interaction is *emblemic*, that is, not only is *caused by* interaction but also is *symbolic of* interaction (flags, national costume), or, in an even stronger sense, is the very guarantee, justification, or source of interaction (as when total strangers recognise each other's Masonic handshake). We cannot prove that a style is emblemic just by discovering a correlation with a well-defined social, cultural, or linguistic group. Conversely, the absence of a correlation does not prove the style is *not* emblemic; if it is just and only the shared use of the style which identifies the group to itself in the first place, then we cannot expect to identify the group independently anyway. To determine whether a style *is* emblemic (expressional, communicational), we must examine how it functions socially. We have to scrutinise its form and content, for messages (emblems, expressions) can be expected to have a certain order and use which other sources of variability in production will not have.

Whatever its 'stochastic' or other particular nuances, as it stands the bounded-interaction interpretation of style (see variously Wobst 1977; Conkey 1978; Plog 1980; Wiessner 1984) is no more plausible than several other competing accounts of variability in artefacts. The fundamental assumption that similarities indicate interaction (and lack of similarities indicates a boundary) is very unreliable in the absence of independent evidence for exactly that which we are trying to prove—namely, *similarity-causing interaction* among producers of artefacts. Although many of our similarity-terms ('copy', 'prototype') prejudge the case, a similarity-relation of whatever kind does not in and of itself necessarily reveal the *cause* of the relation between two artefacts. (Taxonomy does not automatically give phylogeny.) We have to prove on formal and contextual grounds that the relation is that of imitation, quotation, forgery, or what-have-you. Furthermore, although the presence of a similarity-relation may (or may not) be evidence of the historical relatedness of artefacts, its absence is *not* evidence of their *un*relatedness: social interaction between producers could lead just as easily to stylistically different as to stylistically similar products. Therefore we seem to require a characterisation of interaction itself—as, for instance, of producers sharing a plan, quoting a model, imitating a master, revising an ancestor, rejecting a competitor, following a tradition etc., including such possibilities as reversing oneself or forging another—which will serve as the *explanation for* the stylistic taxonomies we have developed. The study of these questions is the bread and butter of art history (see the systematic review in Hermeren 1975) and now of the 'ethnoarchaeological' work of some contemporary archaeologists (see further Sackett 1982; Wiessner 1984).

I conclude that Franklin has done a service by applying one version of the general theory of style to problems of rock art. Her suggestions on the interpretation of Australian styles and on possibilities for further research are justified in the light of the hypotheses she has adopted. However, these hypotheses require major reformulation to be 'archaeologically useful'. This problem should not be blamed on the author but on the unresolved state of the debate about the general theory of style itself.

Dr Whitney Davis
(Getty Fellow in the History of Art and the Humanities)
Department of History of Art
University of California, Berkeley
Berkeley, CA 94720
U. S. A.

By JOSEPHINE McDONALD

Franklin's article highlights and substantiates the problems inherent in the analysis of stylistic variation, but fails to address the problem in more than an elementary fashion. The means of differentiating between *stochastic* and *emblemic* characteristics in the archaeological record is not elucidated. The use of 'the Australian case' as support for the stochastic approach is not convincing. While agreeing wholeheartedly that the archaeological approach to art analysis will be 'in the long run more profitable than the purely ethnographic method'—this has been substantiated in much recent and current rock art research—I do not see that the approach advocated by Franklin fully addresses the problem theoretically. Nor can I envisage an easy application of the concept to the archaeological record.

Before further illustrating my problems with her 'model' I would like to comment on some points made and used by Franklin. Obviously this paper is brief and as a consequence very generalised. There are, however, several generalisations which are inconsistent with current research findings, in particular in relation to the Panaramitee 'style'. As Franklin relies heavily on the 'homogeneity' of this style as disproving the applicability of the emblemic concept in Australia, this point is quite important.

Of the four 'constant features' of Panaramitee (Edwards 1971) only one is directly related to style. That is the consistent relative proportions of motifs. While advanced weathering and heavy patination appear to support an ancient and contemporaneous event, the other two 'features' relate more to site location—sensible, considering the generally arid areas of this style's site locations.

Since Edward's work in the late 1960s, and Maynard's thesis in the late 1970s, there has been much research which indicates that there is *NOT* a consistent proportion of motifs in Panaramitee sites across Australia (Clegg 1983; Forbes 1982; Morwood 1979, 1980; Rosenfeld *et al.* 1981; Rosenfeld 1982; Nobbs 1984; McDonald 1985: 20). This work has been completed over a wide area of Australia and in many ways is only the beginning of the *quantification* of the Panaramitee style. The methods used by Edwards in his vast survey in the 1960s involved a loose sampling of sites—more an intuitive recording than an objective count. Maynard's thesis (1976) relied heavily on the then known 'state of the art' and only limited (Australia-wide) fieldwork (Maynard pers.comm.). It is thus only as a result of this more recent fieldwork and analysis that the heterogeneity of Panaramitee sites has been recognised. Franklin's apparent ignorance of this work is partially contradicted by her reference to Morwood's work in the text and bibliography.

On this basis then—of the Panaramitee style's homogeneity crosscutting known cultural areas—one cannot argue (certainly not emphatically), that the concept of emblemic styles 'is not applicable to the analysis of styles [sic] within Australian rock art'.

Problems with this argument are exacerbated by Franklin's unexplained assumption that the cultural areas defined by Peterson (1976) existed when the art was created—at least 9000 years ago (Sturts Meadows) and probably much earlier. Such an assumption would appear to be contradicted/complicated by Franklin's later conclusions about major social change with the influx of the simple figurative styles and the Small Tool Tradition.

Franklin's statement that stochastic style does not assume the causes of the variation in artefacts as does the concept of emblemic style, is contradictory (see her descriptions of this concept on page 124). It would appear from this description that Conkey's definition of style as being 'the individual's perception of the world from a culturally-shaped perspective' (Conkey 1978) is being applied by Franklin—which makes the assumption of cause at the individual if not also the cultural or group level. I would also like to know just *how* this concept will be useful if the cause of variation is not assumed. How does one interpret prehistory if not by assuming that there is a reason for variability in the archaeological record?

My main problem with the concept/model discussed here is that I do not think that it is yet a model. What are the theoretical constructs? What are the postulated causal relationships?

I would also like to know how will this concept be applied to the archaeological record and the general analysis of art? Will it depend on identifying 'stochastic motifs' as opposed to 'emblemic ones'—or is it a combination of artistic elements which will indicate the stochastic patterning—or is this concept purely an interpretative device?

And why is it not possible for both concepts/influences (emblemic and stochastic) to be applicable at the same time in any one body of art?

I remain unconvinced (or even to understand) just how the stochastic concept will approach the problem of stylistic variation and similarity. Will it, for instance, be possible to use this concept for spatial as well as temporal variation? And how does it explain *similarities* in the simple figurative *styles* as far removed as Sydney, Port Hedland and Laura?

Franklin's article certainly indicates that debate on style analysis in Australia is not dead, and that hopefully advances in this area are going to be made in the near future. As it stands, Franklin's article does not convince. Her work in this area is in its infancy however, and it is to be hoped that further work will develop the concepts presented here into an archaeologically useful method.

Josephine McDonald
(Consultant Archaeologist)
40 Railway Avenue
Stanmore, N.S.W. 2048
Australia

By KELVIN OFFICER

Stochastic vs Emblemic is a challenging paper. It appears within a period of increasing theoretical introspection and perhaps a lost innocence for many researchers of prehistoric style. There is growing impatience with the apparent conundrum that art must be artefactual and yet also describe prehistoric social structures. Models appear which seem to fit these requirements but are invariably found to be too simplified and unrealistic. Yet despite these passing theories art has maintained its latent suggestion of a ripping yarn a cut above the standard bone and stone story. ►

Happily, Franklin appears to be a firm believer in this potential and is more than a little brave in attempting to move the reality of style-as-artefact closer to a battle scarred and irritable collection of high level theories and jargonisms. Unfortunately much of this paper's challenge is in understanding the concepts and theoretical argument. I found that review of the cited references was essential in order to understand Franklin's sequence of theoretical conclusions. However, beyond obscure leaps in cognition, the paper presents a set of theoretical proposals which demand discussion and are therefore constructive in the formulation of an analytical framework for the methodology of prehistoric pictures.

As I understand them, Franklin's main proposals can be summarised as follows:

EMBLEMIC		STOCHASTIC
expression of group identity	-	expression of culturally shaped
collective perception	-	individual perception
conscious	-	unconscious
nonrandom change	-	random change
direct causal relationship	-	directionless and assumes no causality
based on ethnography	-	not dependent on ethnography
recognises only one determinant	-	recognises multiple levels of variability and diffusion
distinct spatial distributions	-	clinal spatial distributions
archaeologically invisible	-	archaeologically realistic

Franklin's argument is underpinned by two observations. The first recognises that stylistic traits may reflect multiple and complex patterns of social interaction and will probably intervary at differing levels, rates and spreads. This is an observation about the *mechanics* through which archaeologically visible stylistic traits may have operated. In the past, models have tended to concentrate on single variables such as cultural boundaries or interpersonal communication. This recognition of complexity is then an appropriate foundation on which to base any new understanding of stylistic variation.

The second is an analytical separation of style into the conscious and unconscious. The conscious sphere is directly equated with emblemic functions. This is of prime importance to Franklin's argument, but the division fails to fulfil the analytical role in which it is used.

There are some major contradictions. Both the concept of emblemic style (Wiessner 1983), and stylistic variation according to social interaction (Deetz 1965; Longacre 1970; Whallon 1969) are based on studies of living societies and rely on peculiarly anthropological data such as historical and artefactual context, methods of production and use. If, as Franklin argues, the basic social interaction can be incorporated with a stochastic model, then the contention that stochastic styles 'argue against use of the ethnographic approach' is contradictory.

The major strength of the stochastic concept is the recognition given to the multivarious mechanics of stylistic traits and the potential for diverse causal factors to be represented: '. . . it acknowledges the complexity of relationships between different classes of archaeological evidence'; '. . . each cultural trait has its own pattern of diffusion and variation'. However, the adoption of the stochastic concept is based primarily on arguments of contrast which propose emblemic motives to be the main factor effecting formal variations. This contradicts the former recognition given to the likelihood of a multivarious character in stylistic variation.

The use of strictly archaeological or nonethnographic methodologies for the analysis of prehistoric styles is now well developed in Australian prehistory (Maynard 1977, 1979; Clegg 1977, 1979; Morwood 1979, 1980). Common to all these approaches is an attempt to separate causal interpretation from classificatory identification within the bounds of human analysis and the aims of the analysis. Franklin stresses that stylistic theory must be compatible with this level of objectivity. The rejection of any identifiable causality in the stochastic concept appears to make it methodologically very useful: 'Stochastic styles are randomly changing . . . It does not assume what causes styles . . .'

Nevertheless, the concept of stochastic when developed into a theoretical model has some major inadequacies. The argument for random variation is based on the failure of any direct relationship such as efficiency or ethnic boundaries to explain the multivariance in style. For a discipline which tries to reconstruct past human behaviour, a conclusion of randomness based only on the failure of direct and limited models seems a little extreme and self defeating. Is 'random' a tag for a complex situation which previous models have been unable to penetrate?

The concept of random and directionless variation also has hidden causal implications.

If one accepts that all artefacts are a product of the manipulative strategies, and therefore culture, of their makers and users, then random variation in an artefact must to a large extent imply random objectives within a culture. The stochastic model cannot therefore present a clear division between style description and style interpretation.

The concept of random variation, or stylistic traits that co-operate outside of conscious formal boundaries is not new. Binford's concept of stylistic 'drift' explores some of these ideas (Binford 1963; Cleland 1972) and the continuing discussion toward a psychoarchaeology of prehistoric pictures (Bednarik 1984) reveals increasing sophistication in this area. What is new about Franklin's model is the attempt to be holistic in application.

It is quite probable that there are random aspects to stylistic variation and that these may have a role in temporal and spatial changes. In any such exploration of stylistic traits, however, the relative scale of diagnosis must be defined. The discovery of variation, both in extent and type is generally a question of how hard you look. Stylistic variation has been related to various social variables from the individual (Hill 1977; Wiessner 1984), to collective sociopolitical networks (Deetz 1965; Longacre 1970; Wobst 1977; Plog 1980). It is not unreasonable to assume that what is recognised archaeologically as style may incorporate variation due to a large combination of such determinants. Their identification and interpretation will depend on the appropriate use of criteria and means of identifying at which level these variants become formally significant. Franklin provides neither of these beyond the observation that stochastic is all those traits that are multivarious, archaeological and not emblemic.

Franklin's use of the emblemic and social interaction models no longer reflects current attitudes in the present literature and research. Emblemic is assumed to represent a holistic paradigm capable of explaining a broad spectrum of variation. It is discarded in favour of the stochastic model because at this scale it is clearly inadequate. Using this reasoning, the maxim fundamental to the social interaction model of multiple avenues of interaction and diffusion becomes very attractive.

The expression of social and larger cultural boundaries via style in artefacts is now considered to be restricted by specific prerequisites:

> Only under certain conditions, which promote the frequency and/or intensity of comparison, will style take on collective associations and thus have the potential to delimit the boundaries of social groups (Wiessner 1984: 226).

Theories advocated by Wobst (1977) and others (Hodder 1977; Plog 1980; Davis 1983; Conkey 1980) predict an active (but not necessarily conscious) role for style in the negotiation of social relations depending on the functional and potential contexts of the artefacts involved.

The major premise of the social interaction model was that the degree of stylistic similarity between the artefacts of individuals or collective groups was directly proportional to the amount of social interaction between them (Plog 1980; Whallon 1968). Subsequent discussion of results has found this to be too simplified and the modelling of stylistic dynamics too dependent on the ceramic ethnography on which it was based (Watson 1977; Plog 1980, 1983). Inverse or even indirect relationships between style and social interaction are not considered. Styles could conceivably differ fundamentally in a formal (and archaeological) sense but may have operated within their original social context in a complementary fashion, reinforcing social links via mutual recognition.

Any model of style and its variation must recognise its inherent complexity as a medium of 'structural' and social dynamics. Variation appears to be definable at differing but related magnitudes and relatable to many cultural variables. Much of the criticism levelled at the social interaction model, and many diffusionist studies of style, has grown from a concern about the sociological context or social function of artefacts (Hodder 1977; Davis 1983; Plog 1980; Binford 1986). It is now postulated that style will vary according to the potential communication offered by the context of an artefact (Wiessner 1984; Wobst 1977; Plog 1983). Or to put that another way, prehistoric art should be considered in a context of manufacturers (or donors), and potential consumers (or receivers) (Davis 1983). In this way, the intended function of style must be considered in any model of style.

Unfortunately the stochastic model seems to sidestep this issue. Change in the social function of pictures is just as likely to cause style variation as stochastic or social interactive elements. The dramatic change from the Panaramitee to the simple figurative styles is too often considered only in terms of major cultural changes which ignore social function.

Sackett (1977) has constructed a model of style which attempts to differentiate functional aspects of artefactual variation from cultural variability. Functional variability, or the physical requirements for technical efficiency, is not seen to be 'peculiar to a specific time and place', and therefore rejected as being unstylistic. Such a division is untenable in the analysis of art for the intended function of a picture must also have an impact on its form and context. Observations of stone tool manufacture and their use within existing communities are now also questioning this division for stone tool analysis (Binford 1986). Correlations between function and style appear impossible in the stochastic model which defines a randomly determined sequence of events. If a truly archaeological approach is wanted, interpretive divisions such as these should not be involved in the description or primary modelling of style and the mechanics of its variation.

The term style can be used with various intentions. In the archaeology of art there appear to be two basic contexts. The first attempts to tightly define those residual artefactual elements with which prehistory can be made using prehistoric pictures. Maynard's (1977) definitions are typical of the expressly formalistic approach which looks to the remaining physical information

contained by the art. The second is used to refer to particular pictorial characteristics which are interpreted as diagnostic for the creation of major groupings. Thus the Panaramitee 'style' includes various sites because they include pictures with specific 'Panaramitee' qualities.

There is little to no consensus within the literature as to how these two contexts should be related. The first would be most successful if considered an interpretive transformation of data collected by the second. This division is often blurred in theories of style or not defined at all. Sackett's definition of style as nonfunctional formal variation makes the analysis of social variables such as function within art very difficult. Franklin's incorporation of this division in her initial definition of style and the dismissal of function as a primary component of style in the stochastic concept present major obstacles for the use of the model as a tool in understanding the prehistoric social contexts in which style operated.

Franklin's model of stochastic styles would benefit from tighter theoretical definition. For example is this model about the interpretation of the mechanics of stylistic variation or is it attempting to model something more fundamental about style? How is the stochastic model and the social interaction model related? Aspects of each are equated in Franklin's paper but inherent contradictions are never resolved. Social interaction implies direction in formal complexity whilst the stochastic concept specifies randomness.

Franklin's views are constructive in directing attention toward the inadequacy of existing models in relating stylistic data to broader theoretical sociological themes. Many of the shortfalls Franklin identifies are real and major considerations in the analysis of prehistoric art, however I feel that the stochastic concept has only limited success in meeting those challenges.

Kelvin Officer
1 Dodgshun Court
Emu Ridge
Belconnen, A.C.T. 2617
Australia

By ANNE-MARIE PESSIS

The article by Natalie Franklin represents an important contribution to analytical methodology in rupestral art studies. In this field, the absence of logically structured procedures reduces the possibility of doing systematic and very precise research which is the only way of obtaining results based on controllable scientific constructions.

The author's starting point is Sackett's definition of style: 'highly specific and characteristic manner of doing something'. This definition is based on the technical specifics of producing rupestral art. It seems to us to be the right solution to define the styles on the basis of techniques which can be objectively compared, and much more useful than the ethnographic approach which defines relations of homology—frequently without being able to demonstrate whether these homologies derive from an inherited influence or simply from convergent phenomena. As a general rule, the ethnographic approach is weakened by its neglect of spatial and temporal factors and of the problems of identity between signifier and signified.

If the social factors implicated in the practice of prehistoric art are considered, new perspectives of analysis and interpretation appear and new data can be recovered, other than those gathered through the analysis of graphics which are only the material basis of a system of communication.

Franklin's paper calls for a few reflexions. First one must underline the necessity of establishing a model of description on a technical level, which precisely defines the categories to be taken into account. This will make it possible to undertake stylistic descriptions in a systematic way. One should always bear in mind that prehistoric art consists of three systems, which correspond to three levels of analysis: the technical system, the system of representation and the system of interpretation. The technical system only comprises the aspects related to the physical creation of the graphics: this statement corresponds to Sackett's definition. Therefore, when defining the styles, one should avoid taking into consideration characteristics of the figures other than those which derive from the technique, such as traits which allow their identification or the analysis of the themes, and which are elements of the system of representation. In the course of the analysis, technical factors and thematic factors should be treated separately as they are related to two different levels, to two different aspects of the same problem.

With the technical system and the system of representation we have better methodological tools to tackle more accurately questions regarding social interactions, by comparing the data from the various systems.

The author introduces very subtly the question of the opposition which appears in the analysis of rupestral art with the use of the concepts of stochastic style and emblemic style. The use of the definition of style, to study other aspects of social life or the limits of the distribution of social groups, offers various new possibilities. Franklin's suggestions for the definition of 'stochastic' and 'emblemic' are very relevant but it seems to me that the way the term 'style' is used can be a source of confusion. In fact in the examples which are given, taken from Australian rupestral art, the styles are described via their subjects which is contradictory to the initial definition. A decision should be made as to the use of this term. As a matter of fact, Franklin raises the problem at the very beginning of her article. A style is either characterised by various attributes including those relating to the technique of production, in which case the weight of each attribute should be specified; or style is really defined as 'a highly specific and characteristic manner of doing something', in which case the only relevant criteria are the technical procedures used in the creation of a pictorial ensemble. The process, the 'manner', is then taken into account, and not the identification, the subject ('something'):

therefore, in Sackett's definition, the motif does not have to be identified, only the way of producing it is relevant.

Description of style in Australian rupestral art (Panaramitee 'style', simple figurative styles and complex figurative styles) shows that these styles are defined mostly by their thematic aspects. I believe that in these instances it would be better to speak about *rupestral art traditions*, within which the styles would be defined according to their technical characteristics.

As a matter of fact, the characteristics which should be taken into account in the technical system have yet to be established. I am referring to the techniques of use of the pictorial space, the techniques of perspective which are extremely important elements in the study of the technique of creation of rupestral art panels. These shortcomings enhance the relevance of the author's reflexion as well as that of the application to a rupestral art as rich as that of Australia.

It seems to me that, in the study of rupestral art throughout the world, the notions of 'stochastic' and 'emblemic' presented by Franklin can be usefully considered within a single stylistic class defined only by the technical criteria of creation. Without referring to any specific rupestral art, it is possible to find in a single style both emblemic and stochastic graphic components. Rupestral art in northeastern Brazil is rich in such examples.

It is urgent that we define precise levels of analysis and criteria of classification for the analysis of rupestral art, as a basis for more in-depth research which could lead to regional comparisons. With the latter, and the archaeological record of territorial data it will be possible to differentiate centres of creation and the rythms and characteristics of their evolution. In this spirit, Franklin's paper is a serious contribution and of great methodological value.

Dr Anne-Marie Pessis
Groupe de Recherches d'Anthropologie
Préhistorique d'Amérique du Sud
Ecole des Hautes Études en Sciences Sociales
44, rue de la Tour
75016 Paris
France

By PAUL S. C. TAÇON

The problem of what constitutes a 'style', like the problem of what is 'art', is something that has confronted, confused and confounded most people engaged in rock art research and will continue to do so in the future. These basic questions are fundamental to the discipline and must be resolved by all researchers interested in writing about or analysing prehistoric creative manifestations or manipulations at the start of their projects. In the past many people initially grappled with the problems of 'art' and 'style' in a vacuum. This led to much scratching of heads and succeeded in producing what Franklin has described as 'a seemingly infinite number of meanings, resulting in a great deal of confusion'. This confusion was avoided by some by merely adopting the terms and definitions provided by favourite mentors but this practice has not always been fruitful as it has often led people to encounter the same traps and pitfalls as their predecessors. Franklin is to be applauded for firing the first shot in a new round of what should prove to be a stimulating and productive debate. It is unfortunate, however, that her shot missed the target!

The target we all are shooting for is 'what is style in the context of Australian prehistoric pictures?' and 'how can the notion of style enable us to learn about other prehistoric processes?' Maynard (1976, 1979) has provided one of the most innovative models to help answer these questions but it was overextended, over-generalised and is too simplistic to be overly productive. It provides only a framework, overview or skeleton and tells us little of the driving force behind the organism or body of prehistoric Australian art. Franklin's wholesale adoption of Maynard's system without refinement, comment or criticism is perplexing. In western Arnhem Land, for instance, Maynard's system makes little sense as it fails to describe the true complex nature of the rock art of the region as it was produced over the past 18 000 or more years. It also falls short in other geographical regions as some other researchers have noted and it fails to relate form to function or to consider the iconic nature of the images produced. It is unnecessarily evolutionary in conception and fails to account for the numerous exceptions that plague the system. In the recent period of the western Arnhem Land rock art continuum, for instance, the equivalent of several simple figurative and complex figurative 'styles' can be shown to have coexisted. In reality, they are all part of a larger art tradition or style complex and the different forms seem more likely related to function and meaning than to different styles and groups of people. As well, when one examines the entire body of art from the region, one finds some forms of the so-called 'complex figurative' preceding certain forms of 'simple figurative'. Much of the confusion and lack of understanding has arisen from the misuse and overextension of the terms 'Mimi' and 'X-ray'. George Chaloupka (1983, 1984, 1985) and Darrell Lewis (1983) have attempted to remedy the situation, concentrating much of their attention on defining early styles and forms of art and have wholeheartedly and wisely advocated the abandonment of the term 'Mimi'. My own current program of work is focused on the recent, 'X-ray' end of the spectrum and I have found the term 'X-ray' to be just as misleading (Taçon 1986).

In any discussion of style the relationship between form and function should never be forgotten and often it is the key to understanding the coexistence of several types of creative manifestations as well as the nature of changes (or lack thereof) in styles over time (see Chaloupka 1984: 46 and Vastokas 1967). The form an object takes makes a statement in and of itself and if it is important enough it will be adopted into the overall style of an area or region. In turn, the local style may also influence the form and there is a continual interplay between form,

style, meaning and function, all of which are further influenced by the sense of aesthetics shared by the artists and audiences at both conscious and unconscious levels. To simply say that styles are either 'emblemic' or 'stochastic' ignores the true complex nature of what makes up and influences particular styles. Furthermore, to conclude that the styles exhibited in Australian rock art are all stochastic and that these change in a '*random* and *directionless*, but *not rapid*' (emphases mine) way tells us nothing useful at all. Indeed, it implies that it is futile to study change in Australian rock art as it is necessarily random and directionless! Human beings do not operate on a purely random basis, however, whether they be European, Aboriginal or otherwise.

Franklin concludes that 'in future consideration of Australian art styles within a broader perspective is held to be profitable' but her definition of stochastic and her conclusions negate this. Franklin seems to be seeking a simple solution to a complex problem but such a solution may not have the best fit at either the specific or general levels. It is my contention that each body of rock art from each area has to be examined at both the specific, individual level and the more generalised, regional or pan-Australian level in order to fully appreciate and understand its significance. One may find that the social boundaries model *and* the social interaction model *both* are applicable models for the data at hand and that the one does not exclude the other. Other forces may also have influenced art styles and forms and these, too, should be examined. In order to do this one has to study rock art from a variety of perspectives and approaches. Franklin, by the very nature of her definition and sweeping, unfounded conclusions has ignored this and has made it easy for herself by stating that 'the concept argues for a purely archaeological approach to the study of prehistoric pictures, in that the art should be examined as another form of archaeological evidence, like stone artefacts'. But this is simply not true! Rock art is very *different* from stone tools and can tell us more and different things about the people responsible for its creation. It is true that rock art needs to be studied from an archaeological approach but not to the exclusion of other approaches, such as the ethnographic or art historical. Working in North America, Joan Vastokas has long advocated this:

> Instead of our simply accumulating lists of pictorial elements and plotting their distribution on maps, an investigator concerned with the wider context of rock art avoids these arbitrary classifications, recognises that the study of rock art is not limited to the identification and charting of isolated pictorial designs, elements or so-called morphs, but necessarily takes into account the positioning of those images on the particular site, explores the physical and even psychological character of the site itself, examines the geographical relationship of the site to the wider environmental setting and investigates as far as possible the significance of the site and its images in the context of its archaeologically and ethnographically known culture. In thus striving to understand rock art in any given region from every conceivable point of view—environmental, economic, social, religious, calendric, ritualistic and cosmologic—the contemporary rock art investigator brings to bear whatever interpretive forces he can muster upon his elusive data (Vastokas 1978: 22-3).

The data we work with is elusive by nature as Vastokas notes and is consequently much more challenging to interpret and understand than most other forms of archaeological evidence. For this reason a simple approach that ignores the vast majority of interpretive tools available is not sufficient and to say that stylistic change is random and directionless contributes nothing to comprehension. One has to look at art and the nature of style from a variety of perspectives in order to understand it more completely and correctly. Furthermore, art has a unique attribute that distinguishes it further from other forms of archaeological data as Forge (1971), Vastokas (1978) and others, particularly art historians such as Irvine Panofsky, have noted. Works of art exhibit systematic symbolism and yield meaning 'without reference to outside phenomena' (Vastokas 1978: 23). We will never be able to retrieve all of the overt meanings in a given body of art but by studying it as a whole and by analysing the arrangement of symbols within we may be able to retrieve some of the hidden meanings, meanings often hidden from the artists and audiences themselves (Forge 1971: 297; Vastokas 1978: 23-4). In the process a clearer understanding of style and stylistic change in rock art will also be revealed and archaeological interpretation will become more complete.

In conclusion, Franklin's paper is welcomed as an important stimulus towards discussion. She has boldly begun a debate on a contentious issue and all of us will benefit from the comments appearing in *Rock Art Research*.

Current address (until February 1987):

Paul S. C. Taçon
Department of Prehistory and Anthropology
Australian National University
G.P.O. Box 4
Canberra, A.C.T. 2601
Australia

By BRUCE WRIGHT

The confusion which has accompanied concepts of 'style' in rock art research has not abated as a consequence of Franklin's paper. The concept of 'emblemic styles' does *not* force the conclusion that there *must* be an 'exact correlation between art styles and tribes'. An emblem may transmit a message appertaining to 'group' identity, but the 'group' need not necessarily be a 'tribe'.

Sociopolitical units, or tribes, may unite under one or several emblems, but some members or sections of such groups may participate in additional emblems. These 'extra' emblems may be subtribal or crosstribal, particularly when totemic affiliation or religious belief motivates participation. Consequently the researcher must not assume that affiliation with *all* emblemic styles followed 'tribal' lines.

Indeed, Australian Aboriginal art, generally, was functional, high in ideological content, and produced in accordance with strict regulations

(Edwards 1971: 357; Morphy 1977; Moore 1977: 318; Wright 1985). Art, ritual and 'myth' are known to have been components of a cognitive set. Aspects of social, political, historical, religious, and/or other ideological information was encoded within, and between each cognitive medium. Encoded symbols could convey a multitude of messages relevant to group identity, but the group could be tribal, multitribal, subtribal, and/or crosstribal in composition, depending on which 'message' or 'messages' the individual participant was able to discern.

'Public' messages conveyed by the cognitive set can be said to have had emblemic significance for one or several tribes and/or subtribes. At the same time certain 'signifiers' present in each medium, when associated, could convey many separate 'inside' emblemic meanings (Morphy 1977: 5). The emblemic significance of these 'inside' messages was known by, and restricted to, a limited number of individuals. Participation in these 'inside' emblems depended on the level of initiation attained by the individual, and was crosstribal in affiliation. Some individuals travelled great distances to participate. Indeed, considerable prestige was attached to the acquisition and maintenance of access rights to myths and rituals, at least.

As emblemic 'signifiers' distributed between the three cognitive media needed to be associated in order to convey a specific identifying message for the group, access rights to emblemic art 'styles' must have been necessary if the message was to be understood. Further, the importance placed on the 'correct' portrayal of art during pantomimic performances of 'myths' suggests that such access also included, at least for some participants, the right to produce the associated artistic 'styles'. It is highly likely, then, that at least some emblemic art 'styles' were disseminated over great distances.

Unfortunately the extent of crosstribal 'emblemic' affiliation is not fully documented. Consequently the distributional pattern for emblemic art 'styles' cannot be said to be 'distinct' and 'clear-cut'. In addition, the pattern of 'emblemic style' distribution could also have been affected by trade.

The acquisition of access rights to myths and rituals by trade was so important, so prestigious, that new 'owners' would travel far and wide in order to teach and present pantomimic performances of them (McCarthy 1939: 82-6, 179; Petrie 1904; Roth 1897; Spencer and Gillen 1904). As art was an important component of the cognitive set, and as the 'correct' production of art was emphasised during pantomimic performances of myths, emblemic artistic 'styles' (those conveying 'public' identifying messages at least) could also have been traded over long distances (see Macknight 1972 for one example of how trade could, at least, affect art). In summary, then, the probability that emblemic art 'styles' enjoyed a 'distinct' and 'clear-cut' distributional pattern is highly unlikely. Rather, 'emblemic styles' could well be expected to exhibit a 'random' or 'clinal' distributional pattern. What sort of pattern, then, might we expect to accompany the distribution of 'stochastic styles'?

When we say that art is 'stochastic' in style, we are in effect claiming that while it is doubtful that the art even has 'style', given the laws of probability, it most likely is stylistic. These 'probable' styles, in accordance with Franklin's definition (and cf. Conkey 1978), are those which reflect an *individual's* 'unconscious' 'perception of the world from a *culturally-shaped* perspective'. In other words, if a particular artistic work is to be classified as 'stochastic' rather than 'emblemic' in style, then it must have been the product of an *individual* artist acting *out of concert* with the 'group' ('group' here taken to mean any, and indeed all groups with which the artist was affiliated), whose cultural perspective shaped the 'perception' being portrayed. To say otherwise would involve a definitional contradiction as 'stochastic styles', i.e. *individual* perceptions, cannot be produced by, or for, the group without invoking some sort of 'signifier' and 'signified', or identifying and emblemic relationship.

If 'stochastic styles' do exist, and if they can only be the work of an individual artist acting out of concert with the 'group', then their production must be restricted to the 'cultural district' (i.e., the territories 'owned' by the various groups with which the artist was affiliated and had acquired access rights) which shaped the perception being portrayed. Further, strict rules would have prohibited the production of 'stochastic styles' in those parts of the 'cultural district' that the artist was only entitled to enter by way of his acquired access rights.

In those areas where right of entry was acquired, the artist would only be able to produce the 'correct' 'emblemic style' traditionally associated with the local totem. Production of a 'stochastic style' in such areas could have offended the totem and, as a serious offence, would have been punishable. If an artist had any right to produce 'stochastic style' rock art, then it would have had to have been in the territory 'owned' by his own subtribe. Even in that territory the strict nature of the rules may well have prohibited the production of nonemblemic 'style' art. Certainly the production of a single hand stencil, his own personal mark which could be used to denote 'ownership' of a rockshelter (Moore 1977), was emblemic in 'style' in that it transmitted an identification message to, and which was understood by, other members of the group.

In summary, then, particular 'stochastic styles', if they exist, would not be distributed as randomly as Franklin suggests. Their distribution would be more 'distinct' and 'clear-cut' than 'emblemic styles' in that particular examples would be found to have an 'exact correlation' with the 'group', or subtribal territory. In other words, the distributional patterns exhibited by 'emblemic' and 'stochastic' art styles could be the reverse to that which has been assumed by Franklin. It is unlikely, then, that rock art research would be significantly enhanced by adherence to the concept of 'stochastic style' in preference to the concept of 'emblemic style'.

The emblemic nature of art, as a component of a cognitive set, indicates that a valuable and major research input can be provided by a variety of disciplines (i.e. psychology, sociology, students of religious phenomena etc.). A *'purely* archaeological approach' to the study of rock art would be unwise, even though an archaeological perspective can be useful (see Jochim 1983: 212; Manhire *et al.* 1983; Van Ryssen 1985). The integration of archaeology and rock art research certainly enabled Morwood (1979, 1984) to conclude that artistic and technological changes in central Queensland, c. 4300 years BP, were related to widespread modifications to the method of exchanging information and the extent of social interaction. However, if there was a shift from a single and homogeneous pancontinental 'style' of rock art, to several heterogeneous and regionally specific 'styles' (Maynard 1976, 1979), then the implications for models of social interaction may be more complex than Franklin infers.

The presence of an earlier, homogeneous pancontinental 'style' indicates that participation in an exchange network was not only widespread, but also implies that communication and social interaction was so intense that homogeneity was maintained. The widely disseminated and fairly contemporaneous appearance of new stone working technologies indicates that the intense interaction continued during the period when heterogeneous art 'styles' are thought to have appeared. The shift away from a homogeneous art 'style' might therefore reflect increased selectivity in the types of items and ideas that were exchanged by way of such interaction. If so, then the important questions are:

(1) What was the prime mover which caused the introduction of selectivity, or greater selectivity, into the exchange and social interaction network?
(2) Was there a greater emphasis on tribal identity and, if so, what was the motivating force?

Much more research is necessitated before the complex nature and extent of the changes (social, political, religious, or otherwise), which apparently occurred during that phase of Australian Aboriginal *history*, can be clarified. It should be noted that my use of the phrase 'Australian Aboriginal history', rather than the word 'prehistory' is deliberate. The word 'prehistory', even though I have previously used it, is a misnomer. It implies that the Australian Aborigines had no history and, as such, ignores the fact that at least some 'myths' and rock art, as part of a cognitive set, encode historical information. I therefore propose that the Eurocentric and chauvinistic term 'prehistory' no longer be used to refer to the period prior to European settlement of Australia.

Bruce Wright
P. O. Box 242
Broadway, N.S.W. 2007
Australia

REPLY

By **NATALIE R. FRANKLIN**

There is a marked '. . . similarity between the problems of interpreting the archaeological record and those of interpreting the written one. Most would agree that the archaeological record does not speak for itself; it is rarely admitted, however, that this is also true for the written record . . .' (Binford and Chuan Kun Ho 1985: 436). It is apparent from some of the comments submitted here that no matter how hard the author tries to make arguments clear, one's own judgment is never enough to ensure that the ideas presented are understood by all readers. Only when other scholars are given the opportunity of publishing comments on particular papers does it become obvious that the written work fails to reflect the reality of the author's thought. The forum provided by *RAR*, where there is an exchange of views between authors and readers, allows this situation to be to some extent rectified and offers a chance to produce more meaningful literature and discussion.

The 'target' I set myself in this paper was to attempt to clarify some of the definitions which have been attached to the notion of 'style', extract some of the models which have been used in the literature and make them more explicit, and examine how some of these models might be used in the analysis of rock art in general and Australian art styles in particular. It was *not* an attempt to settle the style debate, as it would appear some of the reviewers have expected me to do.

Both McDonald and Taçon have criticised my use of the 'Australian case'—the sequence of prehistoric rock art styles proposed by Maynard (1976, 1979) as evidence for the utility of 'stochastic styles', so I shall answer their comments on this aspect first before turning to the other specific points raised here. Taçon states 'Franklin's wholesale adoption of Maynard's system without refinement, comment or criticism is perplexing'. However, Taçon has ignored that this is still the most comprehensive synthesis of Australian prehistoric rock art available. Despite Maynard's statement that her sequence was a hypothesis to be tested by further research, few detailed examinations have appeared in the literature of the complete framework she proposed. In particular, quantitative investigations of each of the styles or groups of styles elucidated have yet to be published. Elsewhere (Franklin 1986) I have attempted to test by multivariate techniques the second part of Maynard's style sequence—that there are a series of different styles within the entity 'simple figurative', an observation which was in fact confirmed by the analyses undertaken. As more and more (unpublished) regional analyses are completed, it is becoming apparent that the intricate details of the sequence proposed by Maynard may no longer hold. This result is to be expected in that Maynard's work was pio-

neering and was offered as an alternative to the frameworks proposed by McCarthy (1962) and Lommel (1970). In my paper I have just made use of the data available in the literature. When a more comprehensive sequence appears I shall also be happy to work with that and incorporate it into my hypotheses.

McDonald and Taçon each have different reasons for objecting to Maynard's system. McDonald notes that recent research has indicated that there is not a consistent proportion of motifs in Panaramitee sites across Australia, and that the style is therefore not as homogeneous as Maynard claimed. She cites a number of regional analyses as evidence for her case. But the situation remains that as yet there has been no Australia-wide investigation of the Panaramitee 'style' to determine whether or not it is as homogeneous as was originally stated. My own current research is an attempt to do just this.

There is, to some extent, a problem with terminology in that the term 'homogeneous' tends to imply that there is an exact identity between the different sites which make up the Panaramitee 'style'. McDonald appears to have interpreted Maynard's original claims in this way. However, neither I nor Maynard have ever argued that Panaramitee sites across Australia are identical to each other, simply that the Panaramitee sites are more similar to each other than the simple figurative or complex figurative sites are to each other. Indeed, as McDonald is no doubt aware, a preliminary analysis of the variability of the Panaramitee sites compared with that of the simple figurative sites indicated that this was the case (Franklin 1984).

McDonald declares that I am ignorant of the regional analyses she has cited, but the same accusation could be levelled against her. My reference to Morwood's work does not, contrary to what McDonald thinks, contradict my original thesis. Indeed, it tends to substantiate it. Of early rock engraving assemblages in central Queensland, Morwood (1984: 363-4) states:

> It is significant that these early art assemblages have a number of highly specific parallels with pecked engraving assemblages over a wide area of Australia. All have a motif range in which tracks and a restricted range of geometric motifs predominate . . . In her comprehensive survey of Australian Aboriginal rock art, Maynard (1979) termed this art the Panaramitee style after the type site of Panaramitee in North-east South Australia.

Thus Morwood is supporting Maynard's claims for similarities between Panaramitee sites across Australia.

Taçon also has several objections to the style sequence proposed by Maynard. He argues that it has failed to account for the 'numerous exceptions that plague the system'. But Maynard herself recognised that her sequence did not include all '. . . important sites, styles, and even whole rock art areas . . .' (1979: 108) in Australia, and rightly pointed out that this did not mean that the system was wrong, only that it was not comprehensive. She even listed the areas that did not fit into her proposed synthesis—(1) the finger markings in Koonalda Cave, to which can now be added the finger flutings in the south-east of South Australia (Bednarik 1986); (2) abraded grooves, which are found extensively throughout Australia associated with a wide variety of other rock art; and (3) stencils, which are widely distributed both temporally and spatially in Australian rock art (Maynard 1979: 108-9). The results of future research must show the relationship of these examples which do not 'fit' to the rest of the sequence; Maynard herself cannot be blamed for the existence of exceptions to her sequence, especially since she was aware of her own system's shortcomings.

Another of Taçon's criticisms of Maynard's synthesis is centred around its failure to account for the complexity of the rock art of western Arnhem Land. Here Maynard is being blamed for failing to take account of work which was not available to her at the time she completed her thesis (1976). Current research must be directed towards refining the system, since it seems to be agreed that it provides a framework, and not just unfairly shooting it down in flames. Taçon also provides no substantiation for his claims that the rock art of western Arnhem Land was produced over the 'past 18 000 years or more', that in this region the equivalent of several simple figurative and complex figurative styles coexisted, and that 'In reality . . . the different forms seem more likely related to function and meaning than to different styles and groups of people'. Such statements are in direct contradiction to the ideas put forward by Maynard (1976, 1979), yet no justification is given for them by Taçon.

Having replied to the objections raised by two reviewers as to my use of Maynard's synthesis of Australian rock art, I shall now answer specific points raised by each of the writers.

McDonald's opening comments on my paper suggest that she has entirely missed the point I was trying to make, since she states that I have not elucidated the means of distinguishing between stochastic and emblemic characteristics in the archaeological record. The point of my article is to indicate that one cannot differentiate between stochastic and emblemic attributes in archaeological artefacts, since the two types of style may manifest themselves in identical circumstances. We are also unable to determine from the artefacts alone, in the absence of any ethnographic context, what sort of style we are dealing with, and the attributes of each type of style may in fact be identical. Therefore, an approach which recognises the complexity of the relationships between different classes of evidence, i.e. 'stochastic styles', is held to be of more use. As Clarke has succinctly pointed out (1978: 365):

> The complexities of the problem of equating archaeological entities with social, linguistic and racial groupings rapidly become apparent. There is no *a priori* reason why the different aspects should equate exactly one with another . . . simple and naive equation of these differently based entities is not possible and is demonstrably false, but lack of exact correlation does not mean that there is no correlation whatsoever—it simply emphasises the complexity of the relationship.

Clarke (1978) realised that the problem is

due to dealing with different hierarchical aspects, and deciding which levels to compare with which aspects, and he listed the choice of levels we are forced to deal with in the dimensions of material culture, linguistics, social organisation and genetics. He presented the possible rough range of correlations between these dimensions in a diagram (see his Fig. 75), and argued that the difficulty of isolating correlations is understandable in view of the fact that all of these entities are 'arbitrary horizons of unspecified definition' (Clarke 1978: 366-7).

McDonald also claims that I have 'failed to address the problem in more than an elementary fashion'. However, as I have pointed out previously, the attempt in this paper was to clarify some definitions and models of style which have been used in the literature, and examine how some of these models might be given a concrete application to the archaeological record. It was *not*, as it appears McDonald has expected, an attempt to settle the style debate, which I believe will require a great deal more research before we can hope to provide any answers.

McDonald has dismissed the major basis I have used for rejecting the concept of 'emblemic styles' in the analysis of Australian rock art—that is, the homogeneity of the Panaramitee style crosscutting known cultural areas. I have already answered her reservations over the homogeneity of the Panaramitee style, but I would like to point out to her that this is not the only evidence I have used to argue for the greater archaeological usefulness of 'stochastic styles'. I have also made reference to the work of Clarke (1978), Hodder (1978) and White and O'Connell (1982). In the Baringo district of western Kenya, Hodder found that, although distinct styles could be discerned within the recent material culture, distinct, sealed tribes could not, since individuals were permitted to move across and marry across tribal borders. I shall highlight the points being made here with some further examples. One of the notions which is inherent in the literature is that style is 'caused' by culture, a term which suffers from the same sort of confusion over its definition as style, and thus there appears to be a notion of emblemic styles implicit in the literature. The application of this notion, however, results in a seemingly paradoxical problem. According to this view, a culture may be inferred from a particular style, but rarely has an ethnographically observable culture been found to coincide with a particular style. For example, Schapiro (1966) observes that the geographical distributions of art styles do not fit neatly with boundaries of physical types or with recent national boundaries. It follows, then, that although there was a Gothic style of architecture which was spread widely throughout Europe, there was never a Gothic 'culture' or 'society'.

Another example from Clarke (1978) will further serve to make my points explicit. Clarke refers to the work directed by Kroeber between 1930 and 1940 which attempted to record the total list of cultural 'traits', both material and social, of each of the hundred or so California Indian tribes. These 'traits' are equated mostly with artefacts or artefact type entities (p. 374), but could equally well be particular styles. The following associations between the different classes of evidence were detected:

(1) Thirty percent of each list of artefact types may be shared between assemblages from tribes with similar economic systems in similar environments. These tribes may lack significant political, ideological or linguistic relationships, an association which may reflect nothing more than a common technocomplex background (pp. 377-87).
(2) A level of about seventy percent of shared specific artefact types apparently only occurs between very closely linked tribes, usually united linguistically within an entity roughly equivalent to the archaeological culture. However, there are some exceptional cases where tribes sharing seventy percent of their artefact types are not tribally or linguistically related (p. 378).
(3) An average of about sixty-five percent linkage best coincides with that between assemblages from tribal clusters, but this may also add assemblages from unrelated tribes (p. 378).

Clarke concludes that the entities defined by archaeology are simply different to the tribal, linguistic or historical sets (p. 378), and thus there is no 1:1 correlation between archaeological and sociocultural entities. I hope that these further examples have served to clarify my case for accepting the greater archaeological usefulness of the concept of 'stochastic styles'.

McDonald suggests that there are further problems with my 'unexplained assumption that the cultural areas defined by Peterson (1976) existed when the art was created—at least 9000 years ago (Sturts Meadows) and probably much earlier'. I would agree with her that there is a problem with projecting Peterson's cultural regions back into the past, but I would stress that it was just one of several possible ways of applying the concepts I have proposed. Besides, Peterson's system would appear to be based on more static principles than other Australian cultural groupings proposed by other workers, for instance Tindale (1974), in that it is based on physical features of the landscape—drainage basins—which it would appear have not changed greatly over the last 10 000 years or so. McDonald goes on to argue that the assumption of contemporaneity between Peterson's cultural areas and the creation of the art styles contradicts/complicates my later conclusions about social change with the introduction of the simple figurative styles and the small tool tradition. I do not see how this follows, since I have not specified the sort of social change which might be involved. McDonald appears to assume that the social change I have postulated incorporated the establishment of the cultural areas proposed by Peterson (1976).

The remainder of McDonald's Comment indicates that she has not understood the main point raised by my article—that one *cannot* distinguish between emblemic and stochastic styles, or em-

blemic and stochastic motifs, since such motifs could manifest themselves in identical circumstances. Use of the concept of 'stochastic styles' does not oblige one to conclude that particular 'tribes' or groups are responsible for the variation observed, but instead leaves the way open for the archaeologist to interpret it according to established archaeological models. For example, with the similarities between simple figurative styles as widely separated as Sydney, Port Hedland and Laura (although I do not think that they are particularly similar, except in terms of the broad characteristics described by Maynard), the concept of 'emblemic styles' forces one to conclude that the similarities are the result of the artistic manifestations of the same social group. By contrast, 'stochastic styles' would allow one to interpret the similarities as due, for instance, to similarities in the medium used, as a result of independent invention, or even as the manifestation of the same 'tribe', although it does not *force* one to conclude this.

The opening comments made by Officer indicate that he has clearly understood the distinctions I have drawn between 'stochastic' and 'emblemic' styles, and I find his table for illustrating these distinctions most useful. He has in particular noted my recognition of the complexity and 'multivarious' character of stylistic variation. However, he has drawn attention to what he sees as some major contradictions in the ideas I have put forward. For example, he states that the concept of emblemic style and the social interaction model of style are based on ethnographic studies, that I have argued for the incorporation of the stochastic approach and the social interaction model of style, and that this consequently contradicts my recommendation for an archaeological approach. However, I would like to stress that I have not attempted to incorporate the notion of stochastic styles with the social interaction model of style, and that style has assumed two distinct forms—'emblemic' and 'stochastic'—within the *social boundaries* model of style. I have merely suggested how the two models of style might be used in the analysis of Australian rock art—they are two separate things (although see Davis as to how the two models, social interaction and social boundaries, might be usefully incorporated). I have also stated that the concept of stochastic styles 'argues against use of the ethnographic approach . . . even though the argument for its use was initially based on ethnographic premises'. The evidence adduced for the greater archaeological usefulness of 'stochastic styles' stems from the failure of the attempts made by White and O'Connell (1982) to derive coherent recognisable clusters of recent Aboriginal cultural features and to correlate these with Aboriginal groups as defined by the ethnography.

Officer has observed that in any analysis of stylistic variation, the 'relative scale of diagnosis must be defined', and that the identification and interpretation of the social variables involved in the stylistic variation will depend on the 'appropriate' use of criteria and means of identifying at which level variants become formally significant. He declares that I have 'provided neither of these beyond the observation that stochastic is all those traits that are multivarious, archaeological and not emblemic'. These statements sum up very nicely the whole point of my paper—I never intended to provide such criteria. Indeed, I am uncertain how such criteria can be determined.

Officer has stressed that in any model of style the intended function of style must be considered, and that the stochastic model seems to have sidestepped this issue. However, while I recognise that style must have had some sort of 'social' function, one cannot, when one is dealing with archaeological artefacts such as rock art, know what sort of function is involved.

In reply to the questions raised by Officer at the end of his Comment, I would stress that my paper was not intended to be taken for an elaboration of the general theory of style—the term 'theory' is too strong for the ideas expounded here. The paper was intended to indicate how rock art might be analysed as an archaeological artefact, and in the process extract some models and meanings of style from the literature. But it has also suggested how the mechanics of stylistic variation might operate and what this might tell us about prehistoric human behaviour. As has been mentioned above, the stochastic model and the social interaction models of style are not related—the two have been applied separately to the body of Australian rock art styles as defined by Maynard (1976, 1979).

Taçon's comments open with the assumption that in any art study we are all aiming for the same target, i.e. 'what is style in the context of Australian prehistoric pictures?' I think the target should be first 'what is style?' and then the second follows. Indeed, both of these questions can be subsumed under the even larger question 'what can rock art as an archaeological artefact tell us about prehistoric human behaviour?' I think that this is the target we should all be aiming for. My paper may have missed the target that Taçon seeks but I contend that it has not missed the target I had set for myself.

Taçon notes that the relationship between form and function, in any discussion of style, should always be taken into consideration. However, since no definitions are given for the terms 'form', 'function' and 'style', his statement about what influences style also 'tells us nothing useful at all', which he suggests is a fault of my definition of 'stochastic styles'. Furthermore, when one is dealing with prehistoric rock art, determining its function is not as easy as Taçon appears to believe.

The majority of Taçon's comments make it obvious that his main sources of disagreement with my paper stem from the differences between our approaches to the analysis of rock art. I have never claimed that rock art is identical to stone tools, nor that it can tell us exactly the same things about the past and the people who created it. I believe that rock art has the potential to tell us a great deal about past human

behaviour that cannot be derived from stone tools (and *vice versa*), but the archaeological approach to the analysis of rock art is still developing. I did not intend to claim that rock art is the same as stone tools, only that the art should be treated as another class of archaeological artefact. Clegg (1971: 41) has long been advocating such an approach: 'A drawing is just as much an artefact as a Bondi point is, and, as such, should be just as illuminating for prehistory'.

Taçon has ignored the consideration that ethnographic analogies cannot be directly applied to Australian prehistoric rock art. His quotation from Vastokas in fact lends support to my recommendation of an archaeological approach, in that a number of the procedures she lists are included in any analysis of archaeological sites. Vastokas notes that the researcher of rock art brings to bear '. . . whatever interpretive forces he can muster . . .' and that he '. . . investigates the site and its images in the context of its archaeologically and *ethnographically known culture*' (my emphasis). But the ethnographic culture of Australian rock art sites that may be thousands of years old is not and cannot be known.

As Maynard has observed (1979) it is often not recognised that the vast bulk of Australian Aboriginal rock art is prehistoric (*contrā* Wright). The impression which is given in the literature is that the art is part of the living culture of modern Aborigines, but the number of known occasions within this century on which Aborigines have actually executed rock paintings or engravings is extremely small. Furthermore, virtually all of these instances have been done at the instigation of Europeans. The areas of Australia where Aborigines can still interpret the motifs made by their ancestors is small compared with vast regions where the significance of the rock art is completely lost. For example, in the Sydney-Hawkesbury region where thousands of rock engraving sites are found, European colonisation resulted in the destruction of all memory of Aboriginal culture except for the largely superficial observations of the first settlers. None of those who wrote about the new land ever recorded what the engravings meant to the local Aborigines or why they were made. Despite the fact that the engravings are not believed to be much older than a few thousand years at the most, and the youngest of them are less than two hundred years old, they are all as prehistoric as the Upper Palaeolithic paintings of France and Spain.

On the other hand, Panaramitee sites in South and central Australia, which are generally agreed to be several thousand years old, have been incorporated into traditional religious beliefs. Edwards (1966: 36) has observed:

> . . . They consider these to be part of the 'dreaming' site and claim that they were made when the site was 'created'. Any suggestion of the living people of present or immediate past generations having made the engravings was met with surprise and incredulity. The aboriginals denied any knowledge of the living people having anything at all to do with making the engravings and insisted that they formed an integral part of the ceremonial site and 'have always been there'.

Since the circumstances of the manufacture of these engravings have apparently been forgotten, contemporary Aboriginal explanations of the significance of the engravings probably bear only a limited relation to the original artists' intentions. Similar observations can be made for Aboriginal interpretations of rock art in the north of Australia.

Further arguments against an ethnographic approach to Aboriginal rock art are derived from the even more fundamental difficulty that little information about the meaning of modern or recent Australian rock art has been collected by anthropologists. All of these factors tend to support one of the basic contentions of my paper—that the application of archaeological techniques to the analysis of Australian Aboriginal rock art is the most useful way of extracting information from this source. Thus, Taçon's criticisms of my paper can be shown to stem from fundamental differences in our approaches to the study of rock art, and it is apparent that on this basis we shall probably never agree.

In sum, my approach may appear to Taçon to be a 'simple solution to a complex problem', but it was intended as such—to simplify and explicate a vast body of confused, inexplicit, muddled and unclear literature. I believe we need to start with a simple and archaeologically useful model and then recognise that there are complexities involved, otherwise we will not be able to see the forest for the trees.

Wright has objected to my equation of 'emblemic styles' with particular 'tribes', and has described instances known from the ethnographic context where 'emblemic' art styles in Australia have been found to crosscut two or more tribal groups, to be subtribal in affiliation, or to coincide with one tribe only. First, the original definition of 'emblemic styles' refers to style as a conscious statement of group solidarity or identity, expressed in items of material culture, for example flags or emblems that transmit a message of group identity. Throughout my paper I have used the term 'tribe' for simplicity as an example of one possible social group—I did not intend to imply that this was the only type of social group possible. As Wright has observed, 'emblemic styles' may represent tribes, subtribes, or two or more tribes, but also nations and larger sociopolitical networks.

Second, the examples that Wright has listed seem to me to offer good evidence for the utility of the concept of 'stochastic styles'. The sorts of styles that Wright has described and the related distributions of the social groups indicate what would be labelled in Clarke's (1978) terminology polythetic distributions. When one is dealing with prehistoric rock art divorced from the ethnographic context, as is the case with much Australian rock art, one only has the evidence of the art as an archaeological artefact with which to make inferences. Therefore one cannot know whether the styles are tribal, crosstribal, multitribal or subtribal, and this situation may be further complicated by the trading of motifs and rituals, as Wright has observed. Contrary to what Wright declares, it seems to me that the sorts of 'emblemic styles' he describes would result in a distinct,

clear-cut distribution. However, the distribution would be more complex than a 1:1 correlation between art styles and tribes, it would result in a correlation between art styles and subtribes, art styles and tribes, art styles and several different tribes etc., distributions which would all be superimposed upon each other. Therefore, it is apparent that there is some sort of relationship between art styles and social groups, but the archaeologist cannot say what this relationship is from the evidence of the art on its own.

Wright's comments concerning the nature of 'stochastic styles' demonstrate that he believes that all art is emblemic and that each individual has exactly the same perception of the world and would therefore have depicted things in an identical fashion—this is the implication of his words 'an individual artist acting out of concert with the 'group' '. I see it more as different individuals producing art within a prescribed set of rules but placing their own interpretation on those rules. Thus the work of an individual within a set of artistic productions would result in a random 'noise'. Even the specific example of an 'emblemic style' enumerated by Wright—the hand stencil—would only have been identifiable within the group concerned, i.e. internally. They are not externally identifiable by the researcher, the archaeologist, nor probably by social groups widely separated from the group concerned.

Davis has made some useful suggestions as to how the sorts of concepts and models I have elucidated might be elaborated. For instance, he is probably correct in stating that the social interaction and social boundary models of style are really different aspects of the same interpretation of stylistic variation. However, it seems to me that the two models have been treated separately in the literature (e.g. the social interaction model is inherent in Whallon 1968 and Gamble 1982 and further described by Flannery 1976, while the social boundary model of style is inherent in Conkey 1978 and Binford 1968) and it was considered that clarity is an advantage in a field of inquiry which is already in a state of some confusion. So, for clarity I have separated the two models in my paper. Davis has shown how they might be linked, although it is not stated how one determines or maps high and low degrees of stylistic similarity.

Davis notes that I have rejected some assumptions of some models of style while accepting others which seem to him to be equally dubious. But as he himself states, I have applied just *one* version of the general theory of style, one which appears to have been extensively used in the literature and which seems to at least enjoy some measure of agreement. From the comments published here, it appears that there is some agreement that style indicates something about the past social behaviour of human beings, although there is disagreement as to how to interpret the archaeological record to extract this information. Davis has done a service by pointing out some limitations in the version of the general theory of style which I have used in this paper.

I like the idea presented by Davis that a characterisation of social interaction itself is required, a point seldom made in the literature, although I am still not sure how one will determine whether 'producers are sharing a plan, quoting a model, imitating a master, revising an ancestor, rejecting a competitor, following a tradition etc. . .', characterisations which will serve as explanations for the stylistic taxonomies developed. How does one choose between or even demonstrate any of these alternatives? And how does one obtain independent evidence for that which we are trying to prove, i.e. 'similarity-causing interaction', in the archaeological context? However, I do feel Davis is correct in suggesting that independent evidence is required. The concept of 'stochastic styles' is appropriate, then, in that it demands examination of the art on its own, and recognises that styles have their own patterns of distribution and variation which may or may not coincide with a social group of some kind.

Pessis has provided some useful ideas and an interesting approach to the analysis of style in rock art. Her division of rock art into three systems—the technical system, the system of representation and the system of interpretation—further helps to clarify a field already fraught with confusion. It could perhaps further be stated that the first two systems are empirical aspects which can be observed in the art itself, whereas the third is a system imposed by the investigator on the art.

I would like to clarify some points raised by Pessis as to Maynard's definition of Australian rock art styles. Pessis is not entirely correct in stating that the styles are defined by their 'subjects' or motifs, although it is apparent that this is an important factor of Maynard's descriptions. The styles are defined in terms of the five levels in Maynard's classificatory system for Australian rock art (1977)—technique, form, motif, size and character. Pessis' comments tend to highlight the importance of specifying how much weight should be placed on each descriptive level, a consideration which was not elucidated in Maynard's classificatory scheme. For example, a major distinction of the Panaramitee 'style' from the simple figurative and complex figurative styles is, as Pessis has observed, 'subject' or motif in Maynard's terms. However, that between the simple figurative and complex figurative is largely based on differences in the descriptive level of form and in the level of character. The use of Sackett's definition of style by Pessis to stress the distinction between the motif and the way in which it is produced or depicted may prove to be very useful. For example, it is one possible way of getting around the problem that tracks, although nowhere as dominant as they are in the Panaramitee 'style', are a hallmark of Australian rock art traditions generally—it is perhaps the way in which tracks are depicted, and not the fact that they are depicted at all, that is significant. However, Sackett's definition of style was formulated largely to take account of the possibilities of analysing style in stone artefacts. This approach explains his contention that style in stone artefacts can be analysed

by comparing two artefacts of the same function, the 'something' with each other, style being found to reside in 'the highly specific and characteristic manner' of manufacturing artefacts of the same function. The 'something' ('subject' or motif) in rock art may very well, however, reflect a cultural choice on the part of the producer.

It is significant that Pessis points out that the characteristics which make up the technical system have yet to be established—indeed this is true of the characteristics which make up style generally. The possible examples she gives—the techniques of use of the pictorial space and the techniques of perspective making up art panels—are of potential use in the analysis of Australian rock art. I wish she had elaborated on the examples of emblemic and stochastic graphic components in the rock art of northeastern Brazil. In summary, Pessis has advocated a useful way in which levels of description and analysis might be distinguished, and how the concepts I have proposed might be extended and used.

Natalie R. Franklin
Department of Archaeology
La Trobe University
Bundoora, Victoria 3083
Australia

Résumé. L'analyse de variation de style a toujours été une occupation des archéologues. Cependant, le terme 'style' a assumé de nombreuses significations qui n'ont pas été distinguées les unes des autres, résultant à une grande confusion. Ce journal énumère quelques unes de ces coexistentes définitions et models de styles et endorse les vues que style est une mesure d'interaction sociale et les limites entre groupes sociaux. A l'intérieur du dernier model deux concepts de styles sont exposés points par points: styles d'emblêmes, qui délibérément marquent et maintiennent les limites entre groupes sociaux, et styles stochastiques qui révèlent leurs propres motifs de distribution et variation, et ne coincident pas nécessairement avec des limites sociaux définies. L'inutilité archéologique que ces deux concepts de styles est évaluée à travers un examen de styles de l'art rupestre Australienne. Il est conclu qu'un approche faisant usage du concept de styles stochastiques, qui ne determine pas ces sortes d'explications qu'on peux proposer pour une variation de styles, permet une plus grande pénétration et compréhension de l'élément d'information.

Zusammenfassung. Die Analyse stilistischer Abwandlungen war schon immer eine archäologische Vorliebe. Der Begriff 'Stil' hat aber zahlreiche Bedeutungen erhalten, die nicht voneinander unterschieden worden sind, was zu beträchtlicher Verwirrung führte. Die vorliegende Abhandlung führt einige dieser gleichzeitig in Verwendung stehenden Begriffsbestimmungen und Modelle von Stil an, und befürwortet die Ansicht, derzufolge Stil ein Masz für sozialen Kontakt ist und für die Grenzen zwischen sozialen Bevölkerungsgruppen. Zwei Konzepte von Stil werden vorgelegt: emblematische Stile, die absichtlich die Grenzen zwischen sozialen Einheiten andeuten and erhalten; und stochastische Stile, die ihre eigenen Verbreitungsräume und Abwandlungen haben, welche aber mit sozial bedingten Grenzen nicht übereinstimmen. Die archäologische Brauchbarkeit dieser beiden Konzepte von Stil wird in einer Untersuchung australischer Felskunststile erprobt. Daraus ergibt sich, dasz der Begriff von stochastischen Stilen, der nicht vorausbestimmt, welche Arten von Erklärungen man für stilistische Variationen vorschlagen kann, mehr Einblick in die gegebenen Daten schafft.

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KEYWORDS: Tourists - Cave art management - Southern France

A PLAGUE OF LOCUSTS, OR MANNA FROM HEAVEN?

TOURISTS AND CONSERVATION OF CAVE ART IN SOUTHERN FRANCE

D. Dragovich

Abstract. 'Rock art tourism' was examined in relation to cave management at two sites in southern France. The risk of cave art deterioration has been addressed by a variety of measures aimed at preventing visitors from touching the art, supervising and guiding them in the caves, encouraging visitors to consider cave art as important to the cultural heritage, and by excluding visitors or limiting their numbers when adverse effects on art become apparent. Large numbers of fee-paying tourists can generate substantial funds for site management and conservation, but their simple presence can often lead to accelerated rock art deterioration in caves.

Introduction

A recent stay in the cave region of southern France provided a striking introduction to the world of 'rock art tourism', and the problems (and possible advantages) of rock art conservation under conditions of extreme visitor pressure. The general growth in both tourism and interest in rock art suggests that problems of site management will become increasingly common. In southern France, numerous rock art sites coexist with a substantial tourist industry, and attempts have been made to reduce the potential conflict which could arise between rock art preservation and viewing by the public of a part of its heritage.

Cave art in southern France provides an interesting example of controlled access and viewing by large numbers of tourists, in environmental conditions where strict inaccessibility can be imposed at any time. The presence of much rock art within caves, rather than in positions more directly exposed to the atmosphere, has allowed for a high degree of preservation of paintings and engravings, due partly to decreased rates of weathering and partly to long periods of human exclusion caused by the fortunate collapse of cavern entrances in several important areas.

The long-term rate of deterioration of rock art is influenced by physical and chemical properties of the rock and of any painting materials, which are subjected to weathering in the natural environment. In many cases, however, deterioration of paintings and engravings is accelerated by deliberate human intervention: graffiti are not sufficiently rare, and small or detachable rock fragments with engravings are liable to be removed. Conservation of rock art can be difficult under such field conditions if public access is largely uncontrolled: where control is possible, however, tourists can become an important if reluctant source of funds for site management.

MS received 11 April 1986

Pech Merle and Cougnac

Two limestone caves in southern France will be used as examples of rock art site management: Pech Merle and Cougnac, both of which contain numerous paintings and a much smaller number of engravings (see Figure 1 for locations). Roof collapse in each cave had resulted in obstruction of the main passageways to those chambers where rock art was present, so that rock art was only discovered in 1922 at Pech Merle and 1949 at Cougnac. (This compares with Padirac—a major cave system with a subterranean stream but without rock art—which was already open to the public in 1898.) Because gaining access to Pech Merle and Cougnac was so difficult, art had been preserved and tourist access subsequent to discovery could in large measure be controlled. In turn, such control is possible if funding is available to employ supervisory staff; and tourists contribute to their own regulation by the payment of entrance fees (in 1985, equivalent to about $ 3.00 at Cougnac and $ 4.50 at Pech Merle).

The extent to which site management responds to tourist demand is evident from variations in monthly opening hours of the caves. For caves containing rock art, as for many other cultural heritage items in France—like chateaux and castles—entry is often not possible during the winter months, when the sites may be officially closed and normal access barred by signs and locked security doors. Opening hours are usually longest in July, August and September when tourist demand is at its peak: most places, if open, remain so for seven days a week.

Before presenting cave art for public viewing, considerable preparatory work had to be done

at Pech Merle and Cougnac, including the removal of collapsed rock to allow for easy movement of large numbers of people. The enlarged entrances were made secure with heavy locked doors, pathways were smoothed in the cavern floor, and electricity installed in the viewing areas and along pathways. At both Pech Merle and Cougnac, there are interesting natural phenomena to view in addition to rock art—especially stalactites, stalagmites and columns, of varying colours depending on the nature and amount of impurities present in the rock and groundwater. Visits to the caves can only be undertaken in the company of a guide who explains the main features of rock art and the cave themselves. As unaccompanied visits are not permitted, the system allows for informal supervision of tourists while in the caves.

Protective Measures

Apart from the considerable degree of protection afforded rock art by the 'accompanied visitor' system, other measures have been taken to reduce possible damage to, or deterioration of art. Tourists are kept within viewing, but beyond touching, distance of rock art by physical restraints like single low wooden railings placed well out from the decorated cavern walls. Such restraints are cheap, relatively unobtrusive, and effective. They also allow sufficient room for guides to stand in front of tourist groups and explain the rock art present at that point.

In high-wall or ceiling situations where barriers stopping people from standing close to the art would prevent them viewing it at all, wire mesh has been attached over the decorated sections and at some distance outward from them (that is, the wire is pegged on at the ends and does not touch the decorated section of rock). This form of protection is aesthetically displeasing and furthermore makes it difficult to obtain an uninterrupted view of the rock art being protected. Its only advantage lies in the fact that people are able to see the art at very close range, although their complete view of any art assemblage or large individual decoration is severely impeded by the wire mesh. Not all the meshed-in art appears to be in positions requiring such protective treatment: in at least one case, a wooden railing barrier would have been more appropriate.

In France, the prevention of direct physical contact between the tourists and rock art does not extend to the on-site guides who accompany them. Although within reach of decorated surfaces, however, guides do not touch rock art but point to specific features by using a torchlight beam which is directed towards the section being referred to. The extent to which light from torches and general electric lighting affects cave paintings is not clear, but torchlight beams are used only to indicate the specific feature being described, with the torch being switched off while discussion continues; and general lighting is designed with a series of switches between caverns so the lights are switched on when entering a particular cave section and extinguished on leaving for the next part of the cave complex.

It is now known that the presence of large numbers of people in caves will affect temperature, humidity and carbon dioxide levels of air in the cave; and increasing carbon dioxide levels are especially important for rock weathering in limestone areas because the complex series of chemical reactions involved in limestone dissolution are accelerated by high levels of carbon dioxide. Temperature changes may also affect the bonding of paint to cave walls (Villar et al. 1984), and the marked changes in total environmental conditions may lead to algal growth on rock surfaces. The combination of naturally poor ventilation and the presence of up to 2000 visitors per day in the Lascaux cave, probably the most famous cave for prehistoric art, led to serious problems with the formation of calcite crystals and the spread of algae within the cave (Delluc 1984); public entry to Lascaux ceased in 1963, less than twenty years after it was opened to the public. General interest in the Lascaux site was so great, however, that a replica of the original cave was produced nearby and completed for public viewing in 1983. The rapid deterioration of the original paintings at Lascaux points to the need for monitoring the condition of rock art in other caves which are the objects of tourist attention; and the detailed research being undertaken by Villar and coworkers on air temperature changes generated by tour groups of different sizes (five, ten and twenty people) will assist in making decisions about rock art preservation and cave management. At Pech Merle, group sizes were limited to twenty-five people in 1985, with a maximum of 700 visitors daily. Even with these restrictions, a total of 70 660 tourists visited the cave last year. The impact on weathering of such numbers needs to be evaluated at regular intervals.

The management of many sites to which the public has access is often made easier if visitors can be persuaded to consider the site's attractions as both important and valuable; vandalism and inadvertent damage may be reduced, and supervision of visitors becomes less costly both from the financial and conservation aspects. At both Pech Merle and Cougnac caves, items relating to each cave's attractions are available for purchase: postcards, books and slide sets dealing with the site. Although these serve an educational role, they are also bought as souvenirs; but their usefulness in influencing pre-viewing attitudes and thus on-site behaviour is limited, as purchases of such materials often occur after the caves have been visited.

At Pech Merle, considerable effort and expense has gone into the provision of educational facilities: an on-site museum contains attractively displayed descriptions and models of archaeological excavations, along with examples of excavated material, annotated photographic copies of art found in the cave, and old photographs relating to the discovery of the cave. Diagrams illustrate proposed sequences of superimposition of the figures represented in the rock art. As well, there is a theatre with a film presentation describing prehistory in the 'cave art region' of southern

Figure 1.
Location of some caves, with and without rock art, in the rock art region of Dordogne and Lot, southern France.

France, along with photographs and interpretations of major cave art works. The combination of museum displays and audio-visual presentations provides a general background to the nature and importance of cave art, as well as offering enough information of different kinds to satisfy (at least partly) the visitor's natural curiosity about prehistoric cave art, and to impress upon the tourist the significance of the site and thus the need for its careful treatment.

Pretourist Deterioration of Art

Not all paintings were in good condition when cave art was rediscovered (mainly this century). In some places rock weathering has led to partial loss of paintings, which now appear incomplete; there are also cases where figures were probably never completed by the original artist. In a few instances, seepage lines have increased weathering loss on the rock surface at that point and paintings have not survived. Variations in factors like degree of carbonate saturation, frequency and turbulence of water flow, and carbon dioxide content of circulating water has led to some paintings being destroyed by water flow, and others being preserved by it. Preservation occurs when redistributed calcite is deposited as a thin film over cave art; such films can, however, become a disadvantage when they become thicker and reduce the clarity of outline of paintings.

The few engravings examined were in excellent condition, with sharp, well-defined edges and no evidence of small-scale weathering losses. The effectiveness of a nearly-closed cave environment in preserving rock engravings becomes apparent when a comparison is made with the rates of weathering of limestone in above-ground situations: limestone quarried for building construction shows severe weathering losses over periods of less than 500 years, with blocks, columns and carved stones often being so weathered as to require replacement. It is therefore not certain on grounds of rock weathering whether most prehistoric art was located in caves or shelters or whether these were simply the optimum sites for preservation. Sieveking (1979) did note that engravings in shelters are more vulnerable to weathering loss than those in caves.

Discussion

The risk of cave art deterioration as a result of tourism has been limited at Pech Merle and Cougnac by a variety of measures aimed at preventing visitors from touching the art, supervising and guiding them in the caves, encouraging visitors to consider cave art as important to the cultural heritage, and by excluding visitors or limiting their numbers when adverse effects on art become apparent. Most of these management procedures are only feasible if tourists themselves pay for their cost; and in turn, these costs can only be met if the tourist population is large enough.

In the case of southern France, tourists are not in short supply and the considerable number of caves developed for tourist purposes allows for a spreading of visitor pressure; but the seasonal concentration of tourist numbers during July and August still creates difficulties. At least some of the caves which have become tourist destinations are found within economically less well-endowed regions of the country, and authorities are aware that any discouragement given to viewing cave art would probably diminish overall tourist spending in such regions. The response has been to regulate rather than to deter tourists, and site conditions have lent themselves to this approach: caves can be locked to prevent unauthorised entry, and they are not places where the general public would seek to wander about unaccompanied.

Management strategies in relation to tourists at rock art sites involve attempts to resolve the conflicting objectives of conserving art (or at least of not hastening its deterioration) on the one hand, and, on the other, of making an important part of the cultural heritage accessible to the general public. The desire of people to view rock art sites cannot be deflected by provision of books, photographs etc.; and the management questions then become practical ones, such as: how can damage caused by tourists be minimised (assuming that total damage prevention is rarely possible)? How much damage is 'acceptable'? Can some sites be 'developed' for tourist purposes in order to protect other sites? And if so, what criteria for site selection should be applied? And to what extent can tourists be made to fund the cost of their own regulation at rock art sites?

The rising numbers of visitors to rock art sites, and the inclusion of such sites on 'group tourism' itineraries, makes increasingly urgent the need to design management strategies for site conservation. The traditional procedures of very careful recording of rock art before

any tourist-related environmental modifications are made become even more important when the buildup of tourist numbers occurs quickly and unpredictably. Once tourists appear in large numbers, close and frequent monitoring of rock art for signs of deterioration would provide an indication of the level of supervision/guidance that would be consistent with maintaining tourist-accessible rock art in an acceptable condition. Major sites containing several subsites, and subject to heavy tourist usage, may have an advantage over other areas in that the naturally uneven visitor rates noted by Gale (1985)—in relation to caves at Uluru—could be encouraged: either by physical barriers preventing access to protected subsites or, more positively, by passive (signposted) or active (guide-led) directing of tourists to the preselected art site. As the behaviour and attitudes of guides could affect the on-site behaviour of tourists (Gale and Jacobs 1986: 8-10), it appears desirable that guides be trained by and attached to the site management group, as in France, rather than be 'tour leaders' who happen to be showing rock art to their charges.

There are certain similarities in the concerns evident in France in relation to preservation of cave art, and the situation for rock art in Australia. In both cases, the general public is encouraged to consider rock art as part of the cultural heritage, and to behave in a way consistent with conserving this heritage. The 'touching' problem is more readily controlled in the cave art environment described at Pech Merle and Cougnac than for rock art in Australia which is commonly in more accessible positions; and visitor supervision is often not feasible in Australia, where various alternatives have been adopted, including screening off (closing) underground caves in southeastern South Australia (Aslin, Bednarik and Bednarik 1985); erection of a locked wire mesh 'cage' around painted shelters at Mount Grenfell (Walston and Dolanski 1976) and in Victoria; keeping confidential the precise location of some art sites (for example Aslin, Bednarik and Bednarik 1985); the construction of wooden walkways at Mootwingee and Ubirr (Gale 1984); and the provision of an official presence by the installation of visitors' books (Sullivan 1984).

Conclusion

The presence of 'group tourism' at rock art sites has been recognised in France and other countries as constituting a possible threat to the preservation of rock art. Although increasing awareness of the importance of such sites to the cultural heritage has contributed to the rise in 'rock art tourism', and such heightened awareness is to be applauded, it is clear that special precautions need to be taken to prevent accelerated deterioration of rock art, which can just as easily be destroyed by too many admirers as by a single vandal. Adequate guidance and supervision of fee-paying visitors is assisting in conservation of rock art in the access-controlled caves of southern France.

COMMENTS

By PAUL G. BAHN

Dragovich's paper is timely in that, over the next few years, increasing numbers of people are going to wish to visit rock art sites in Australia and other countries where, so far, they have been largely the domain of the specialist; however, I do not feel that the two French caves discussed are of much relevance to the problem elsewhere; it would have been more instructive to study the situation at popular concentrations of open air art such as Val Camonica or the Vallée des Merveilles.

The French decorated caves and, to a lesser extent, the Spanish, are unique because many of them cluster in areas which would in any case attract hordes of tourists for other reasons (scenery, climate, food and wine, etc.); and since they have been known and studied for decades the general public is aware of their existence. Therefore a visit to a painted cave has become one of the 'things to do' during a holiday in one of these regions, akin to looking round a stately home or going to a bullfight. In my experience, many of the tourists who join tours of the major caves are not interested in them, derive little benefit from the experience, and tend to remember the concretions with funny names rather than the art. One answer lies, as Dragovich says, in introductory displays like those at Pech Merle or Lascaux II, to help the tourists understand and appreciate what they are about to see; higher prices may help to deter the utterly uninterested type of visitor, but penalise the genuine enthusiast. In addition, of course, there is the fundamental problem which afflicts many branches of archaeology: the public has a right to see as much of its heritage as possible, and its desire to see it should be satisfied if at all feasible, whereas archaeologists perceive the need to preserve this common heritage for future generations. Hence the necessity of permitting public access to certain caves whose shape and size permit it, but also restricting the numbers of visitors in the caves.

Of course, accidents can and do happen—a guide cannot watch everyone all of the time; accidents can also happen during visits by archaeologists, of course, though daily visits by the public greatly increase the chances of involuntary damage. Smaller groups and less frequent visits can help to ease the threat. Such measures also keep to a minimum the inevitable effects on the cave's microclimate. In particular, the number of children in the groups needs to be monitored, since they give off more heat than adults. Current studies of many French and Spanish caves are indicating the optimal frequency, size and duration of visits, given each site's circumstances (e.g. see Fernandez *et al.* 1986 for the results at Altamira). However, analysis—for example at Niaux (Ariège), where water which suddenly began to flow down the walls and destroyed several

paintings was at first thought to have been caused by the number of visitors—has shown that on the whole these visits are not climatically dangerous to paintings since the slight rises in temperature they cause are very short lived: after the visits the temperature rapidly goes back down to normal. It is now known that the water flow at Niaux was caused by a modification of the vegetation cover on the surface above—the abandonment of pasture at the end of the last century led to the build-up of a thick soil layer and, thus, a big reserve of water. It is clear from traces in the cave that such events also happened previously during history. Consequently, French and Spanish specialists are having to closely monitor and control the vegetation in different seasons on surfaces above decorated caves, using infrared aerial photos which reveal the thermic zones. In addition, the use of these surfaces needs to be checked—for example, modern deep furrows in the cultivable land above caves such as Pair non Pair, Font de Gaume and Les Combarelles have affected the rates of water infiltration; while the arrangements for tourists around Lascaux II (cleared vegetation, parking areas, tarmac roads), and the accompanying pollution from vehicles, litter and human excrement have all posed a potential threat to the original Lascaux, which is only about ten metres below the surface.

Even more serious, however, is purposeful damage to sites by visitors. France is again a special case here, because ever since the start of prehistoric studies it has been plagued by amateur private collectors; and whereas in other countries their clandestine activities have declined considerably over the decades, or have been diverted into the use of metal detectors, France still has plenty of people bent on acquiring a private collection of flint tools or, better still, *objets d'art*. When visiting an unprotected cave, rock shelter or open site there one very often finds traces of recent digging by such people, ranging from crude holes to neat sections, the latter clearly the work of well-informed amateurs. Unfortunately there are all too few cases where they have been caught red handed, and even then punishment is rarely severe.

Where caves are concerned, the problem is exacerbated by amateur speleologists. Over the years a good relationship has been built up between the local archaeologists and the official speleological groups, involving close consultation and co-operation; this is necessary since nearly all the new decorated caves and galleries are discovered by the speleologists rather than the archaeologists. It is necessary to maintain these close ties since the membership of potholing groups tends to have a rapid turnover, caving being such a vigorous and time-consuming activity, and so the new members have to be informed of the correct procedures to follow with regard to discoveries of prehistoric occupation or decoration.

However, there is an entirely different set of people, akin to the clandestine diggers, who force their way into caves and do accidental or wilful damage. They refuse to be kept out, and constantly break locks and even doors in their efforts to gain entry. As soon as a door is installed at the entrance to a French decorated cave, one can confidently expect it to be broken within a couple of weeks. Many cave doors need replacing several times a year. Crowbars, cutting equipment, oxyacetylene torches and even explosives have been used by the trespassers. The need to maintain the cave's climate limits the design of the doors, so that they often incorporate bars or even gaps in the surrounding concrete.

The cave of Montespan (Haute-Garonne) had such gaps in the wall beneath its door—too small for a normal person to attempt entry, but no barrier to a potholer with any experience. On one visit to this cave some years ago I was able to see the destruction caused by a recent trespasser—including a large lump of clay deliberately stuck onto the engraved ceiling; its removal inevitably detached part of a bison figure. In this cave, too, a parietal engraving has been deliberately wiped off the wall by someone who had to climb onto a rock to do it—this was no accidental brushing by a passing body. In other caves, recent visitors have written their names on top of figures, using a variety of methods; or generations of visitors have applied chalk or colour to engravings to improve their photographs. In the cave of Baume Latrone, people broke in, lit fires, wrote on the walls, and even bombarded paintings with clay.

What can be done to prevent this sort of thing? In France, funds are inadequate to combat it really effectively, since there are only four *'surveillants de grottes'* to police the sites, and there are well over a hundred decorated caves; nevertheless, the frequent visits by these guards seem to be cutting down the vandalism quite considerably (see Roudil 1984). Another solution is being sought in the schools, by teaching youngsters about their heritage and how to respect it; but informed vandals can be the most dangerous, as can the informed private collector. Australia is fortunate here, since the precise location of its decorated caves can be withheld from popular publications, as is being done for the Mount Gambier sites.

It is also fortunate that its numerous rock shelter and open air sites are often in remote areas and, indeed, are still being discovered, so that there is no long tradition of public knowledge of, and interest in, these treasures (I will not touch on the factor of Aboriginal rights and wishes concerning sacred art sites, which would take me too far from the subject in hand). France has little to teach Australia here, since its rock shelters are still a headache for the protectors: isolated caves and shelters such as those of the Rhône valley are very vulnerable to undesirable visits, while a lunatic private collector was caught a few years ago in the act of removing (and thereby destroying) a magnificent sculptured bison head from the frieze in the shelter at Angles-sur-l'Anglin. In Spain, the numerous shelters housing Levantine art have always been prey to vandalism (and excessive application of water for clearer photographs), despite their frequent inaccessibili-

ty—see Dams 1984 for the sad litany of damage. The lesson for Australia is therefore to keep secret the precise location of as many sites as possible, and to police those which are 'sacrificed' to public visits. Fences in front of the decorated surfaces should deter all but the irresponsible from causing damage, but there cannot be complete protection against the really determined vandal, the disobedient child or the stupid parent. Hence all sites open to the public must be very thoroughly recorded and published before the inevitable consequences begin.

Finally, Dragovich is right in saying that we do not know whether most prehistoric art was located in caves/shelters or whether these were simply the optimum sites for preservation; but recent finds such as the Nalón sites in northern Spain (Fortea 1981); the Portuguese engravings of Mazouco (Jorge *et al.* 1981), and especially the remarkable discovery by Abélanet and Sacchi of the engravings of Campôme—located on a rock, protected from the wind, on a Pyrenean mountainside (Bahn 1985)—make it almost certain that it is the second option which is correct; the artists were active out of doors, and further proof will no doubt be forthcoming now that we know that the evidence can survive in exceptional circumstances.

Dr Paul G. Bahn
428 Anlaby Road
Hull, HU3 6QP
England

By JACQUES BRUNET

Dr Dragovich dans l'article intitulé 'Conservation of cave art in southern France' a très bien appréhendé les problèmes de conservation des cavités ornées préhistoriques à partir de deux cas précis: Cougnac et Pech Merle. Elles sont, somme toute, assez représentatives des cavités françaises même si celles-ci paradoxalement sont caractérisées par leur diversité et leur homogénéité. Dans une communication lors du colloque international d'art pariétal de Perigueux en novembre 1984, nous soulignions à leur propos:

- une grande hétérogénéité des conditions de propriété et d'exploitation:

- avec les grottes appartenant à l'Etat, gérées par la Caisse Nationale des Monuments Historiques et des Sites (Font de Gaume - Combarelles),
- les propriétés communales gérées par une régie municipale (Pech Merle) ou par un concessionnaire (Gargas et Bedeilhac),
- les grottes privées gérées en S.A.R.L. (Cougnac),
- les grottes privées géréees directement par leur propriétaire (grotte du Cluzeau à Villars et grotte de Rouffignac).

- une grande hétérogénéité morphologique:

- grotte sous-cutanée - Lascaux
- grotte de flanc de falaise, située à une vingtaine de mètres sous le plateau - Font de Gaume
- grotte de fond de vallon - Combarelles
- grotte de 'montagne' - Niaux
- grotte, réseau fossile associé à un paléo-drainage, aujourd'hui disparu - Rouffignac.

- une grande hétérogénéité de dimensions:

- Lascaux est une grotte descendante de petites dimensions
- Combarelles est un étroit couloir au tracé en zigzag, déterminé par des directions de diaclases
- Rouffignac est un réseau digité aux très nombreuses ramifications
- Niaux a de très grandes galeries faisant partie d'un vaste réseau complexe.

- une grande hétérogénéité de conditions de visites:

- au niveau de la fermeture anuelle: un mois à Font de Gaume et Combarelles, plusieurs mois, de l'automne au printemps, pour Villars, Cougnac et Gargas (sauf pour les rendez-vous)
- pour l'effectif maximum des groupes, avec cinq personnes par jour à Lascaux, cinq jours par semaine, vingt par groupe à Font de Gaume, moins de dix à la grotte de Combarelles, vingt-cinq en principe par groupe depuis 1984 à Pech Merle; à Rouffignac, la visite se fait par deux rames de wagonnets.

Pour les cavités visitées par le public, se pose le problème de concilier les impératifs de la conservation tout en facilitant ou maintenant l'accès aux touristes. A partir de ces deux extrêmes que sont conservation et fréquentation touristique, aux actions antagonistes, il convient de trouver une solution les prenant en compte et les satisfaisant toute les deux.

Cette fréquentation touristique a nécessité depuis de nombreuses années et pour quelques cavités lors de leur découverte de nombreux aménagements. Ainsi pour Lascaux, mentionnons que depuis sa découverte, elle a subi de nombreuses vicissitudes associées à un programme d'aménagement touristique en plusieurs phases dont la finalité nous apparaît, avec le recul du temps, assez éloignée des meilleures conditions de conservation. En voici, brièvement, l'historique:

- de septembre 1940 à 1941: découverte et premiers aménagements. Creusement d'un puits dans les éboulis d'entrée pour accéder plus facilement à l'intérieur. Les premières visites ont lieu.
- de fin 1941 à 1948: de nouveaux aménagements sont prévus et réalisés. Nouveau creusement du cône d'éboulis à l'entrée, électrification de la cavité, mise en place de la porte externe et, déjà, d'un sas.
- 14 juillet 1948: début des visites touristiques.
- 1958: l'afflux des visiteurs entraîne de nouveaux travaux importants, pour éliminer le gaz carbonique et la vapeur d'eau ainsi produits. Conditionnement de l'enceinte souterraine;

machine à recycler l'air de la cavité; mise en place de gaines dans le sol pour la distribution d'air plus frais et, à nouveau, creusement du cône d'éboulis.

Dans d'autres grottes les aménagements ont pu se traduire par la mise en place de parcours touristiques entraînant l'abaissement des sols (quelques fois hélas sans véritable contrôle de leur contenu archéologique) la pose de barrières bien souvent peu esthétiques. Ces barrières sont malheureusement nécessaires car les réactions d'autodiscipline du public sont bien moindres qu'en Australie. Cependant nous nous efforçons de conseiller des aménagements internes plus en harmonie avec les sites; il y a encore beaucoup à faire dans ce domaine.

Toujours dans le cadre de ces aménagements, nous conseillons un meilleur éclairage mettant mieux en valeur ce patrimoine esthétique et assurant par souci de conservation un temps d'utilisation moindre.

Après avoir agi sur le cadre général de la cavité, nous nous efforçons d'agir sur le déroulement des visites qui est une des données clefs des problèmes de conservation liés à l'afflux touristique décrit par Deirdre Dragovich. Ainsi, là aussi, demandons-nous aux responsables des cavites (propriétaires - gardiens - conservateurs) une organisation de visites tenant compte du seuil de fréquentation à ne pas dépasser. L'exemple de Font de Gaume traité dans les commentaires de deux articles de Fay Gale intitulé 'Monitoring visitor behaviour at rock art sites' et 'Identifying high risk visitors at Aboriginal art sites' et à paraître dans *Rock Art Research*, illustre ces réflexions.

L'art rupestre préhistorique a été découvert tardivement par le grand public et nous devons saluer le considérable effort (comme le remarque Dragovich) fait dans le site de Pech Merle pour mieux informer les visiteurs grâce à la conception et à la réalisation du Musée A. Lemozi. Michel Lorblanchet et les responsables municipaux de Cabrerets sont à l'origine de cette action permettant aux visiteurs de mieux connaître et comprendre l'art prehistorique restitué dans son contexte et son environnement; et donc de satisfaire sa légitime curiosité des problèmes de conservation.

Ce qui a pu nous parvenir des manifestations esthétiques de cette époque ne doit représenter qu'une faible partie de l'activité artistique des hommes du paléolithique supérieur. Les abris, les cavités ornées n'ont été préservés que grâce à l'heureux concours de facteurs favorables et ont pu échappé totalement ou partiellement à l'érosion, au lessivage des eaux infiltrées, au concrétionnement. Paradoxalement, nous devons reconnaître que dans quelques cas le concrétionnement a pu avoir un effet bénéfique. Citons une série de représentations de la rotonde des chevaux de la grotte de Villars admirablement protégés par un épais voile de calcite. Les contours des animaux ont été dessinés par de larges tracés noirs pris eux-mêmes depuis lors dans le concrétionnement qui s'est formé.

Avec juste raison, Dragovich met en évidence les objectifs opposés que sont d'une part la conservation de ce patrimoine artistique et d'autre part l'idée généreuse de l'accès de ce patrimoine au plus grand nombre. Le véritable engouement dont fait l'objet l'art rupestre attire l'attention sur le délicat équilibre qu'il faut maintenir entre conservation et fréquentation touristique. Une solution serait peut être la réalisation de copie, de fac-simile très fidèle, mais il ne faudra surtout pas exploiter les cavités comme des usines à touristes.

Les études faites dans les cavités ont montré que toute grotte possède son climat propre qui est plus ou moins directement lié aux variations climatiques externes. Les températures, l'humidité de l'air et des parois dépendent de la propagation des ondes thermiques à travers la roche, des précipitations extérieures dont la partie qui s'infiltre joue un rôle important et des échanges gazeux entre l'intérieur et l'extérieur. Pour les grottes aménagées s'ajoute la perturbation due aux visiteurs. Ces différents paramètres que sont l'hygrométrie, la température, le taux de gaz carbonique, la ventilation des galeries déterminent pour chaque caverne suivant sa morphologie un régime climatique particulier. Ce régime n'est pas stable, il évolue tout au long de l'année et revient à des valeurs comparables pour des périodes identiques dans le temps; un cycle régulier tend donc à s'instaurer. C'est ce climat souterrain, cette stabilité des paramètres, qui ont permis de conserver les oeuvres préhistoriques et bien des exemples ont montré que cet équilibre est fragile; dès qu'une brusque variation des paramètres cités se produit, l'agression des parois est irrémédiable.

Le taux de gaz carbonique doit, dans les cavités creusées dans les roches calcaires, être en principe le plus bas possible pour limiter l'agressivité des eaux de condensation, fréquentes en période touristique. Dans l'air humide, riche en gaz carbonique, la corrosion, donc l'attaque des roches calcaires, peut s'exprimer par la relation chimique globale:

$$CaCO_3 + CO_2 + H_2O \rightleftarrows Ca(CO_3H)_2$$

Solide dissous dissous

Selon cette réaction, l'apport de gaz carbonique provoque la formation de bicarbonate soluble; le départ de ce gaz entraîne la décomposition du bicarbonate et la précipitation du calcaire sous forme de carbonate. En présence d'eau de condensation, le gaz carbonique représente donc un danger pour les figures rupestres par la dissolution qu'il peut produire ainsi que le dépôt de calcaire par évaporation [1]).

La cavité de Font de Gaume est dans un environnement soumis aux influences de l'extérieur et, à certaines périodes de l'année (hiver-printemps), quelques parties des parois ornées et concrétionnées sont exposées aux ruissellements des eaux d'infiltration; à d'autres périodes (été),

[1]) Note: Evaporation, however, plays only an insignificant role in the formation of speleothems, particularly in deep caves (ed.).

à part les pluies d'orage dont l'influence est limitée par l'évapotranspiration, seul le phénomène de condensation constitue une menace par le risque de dissolution. Il y a là un antagonisme entre l'attaque de la paroi à une époque et le dépôt d'un concrétionnement à une autre époque. Il est donc nécessaire d'agir sur la qualité de CO_2 contenu dans l'air, en diminuant beaucoup sa concentration dans le premier cas de figure, et en ne l'abaissant que très légèrement dans l'autre cas. Ainsi, nous y éliminons le CO_2 en été et restreignons la fréquentation touristique. A l'inverse, en hiver, nous laissons la cavité retrouver naturellement un équilibre qui a été le sien durant des millénaires. L'élimination du gaz carbonique a été favorisée par l'établissement d'une légère dépression qui facilite les échanges d'air entre la section décorée et la partie d'accès avec les deux entrées; le déficit d'air est compensé d'une manière diffuse et se renouvelle progressivement par la première partie et les fissures de la roche.

La mise au point d'une politique de gestion des sites archéologiques est délicate; en fait, c'est un problème général et mondial. Les monuments deviennent peu à peu les premières victimes de l'intérêt qu'ils suscitent et je crois qu'il faut s'interroger sur l'opportunité de développer le tourisme sur certains sites. Le choix, la décision dans ce contexte sont difficiles quand on connaît les impératifs économiques qui existent, mais c'est grâce à une attitude ferme que nous pourrons assurer la protection et la conservation des sites préhistoriques.

Jacques Brunet
Director of the Section 'Cave Art'
of the Laboratoire de Recherche des Monuments Historiques
Ministère de La Culture
Château de Champs-sur-Marne
29, Rue de Paris
77420 Champs-sur-Marne
France

By JEAN CLOTTES

As Dr Dragovich very rightly says, in France as in all countries with rock art we are indeed faced with a dual and 'conflicting' objective, that of conserving art and of 'making an important part of the cultural heritage accessible to the general public'. Whatever solutions are found, they can never reach a perfect balance between these two necessities.

Dr Dragovich's view of the way those difficulties were met in Pech Merle and Cougnac covers most problems: close protection of the paintings and engravings by means of wire meshes or metal railings (certainly not *wooden* railings, as wood rots very quickly in a cave); limitation of the number of tourists allowed in both caves; supervision of tourist groups by well-informed guides; restriction of lighting; considerable effort, at any rate in Pech Merle, to educate the general public; problems created by tourist pressure in the summer months etc.

However, the title she chose is a bit misleading, as her paper deals much more with the preservation of cave art in Pech Merle and Cougnac than in southern France. This is not just a casual remark, because out of the 135 caves with Palaeolithic art known in France, most of which are situated in southern France, twenty-four are open to the public, each case being different and requiring different solutions. For instance, Rouffignac (Dordogne) is visited by means of an electric train which combines two advantages from the point of view of preservation: it restricts the number of people to the capacity of the train, and it keeps people grouped and safely away from the wall art.

Southern France, besides, also includes the Pyrenees in which some of the most important and famous cave art is located (cf. the caves of Tuc d'Audoubert, Trois Frères, Portel, Marsoulas, Montespan, Gargas, Isturitz, Niaux, Bédeilhac, the latter four being the only ones open to the public in that area), as well as the lower valley of Ardèche which numbers a dozen painted or engraved caves. Equating 'the cave art region' of southern France with Périgord and Quercy, as the author repeatedly does, is therefore an over-simplification, as there exist several cave art areas, each with its own problems.

The case of Niaux (Ariège) will illustrate my point and, moreover, will give a good example (in the Pyrenees) of the way people's attitude to preservation has evolved over a quarter of a century. In the 1960s a road was built to facilitate access to the cave, previously visited by about 1200 people a year. Their number was immediately increased tenfold, before reaching between 20 000 and 25 000 a year. At the same time a tunnel was constructed to create an easier, artificial entrance to the cave. The number of visitors per group was then only limited by the number a guide chose to lead. No permanent electric lighting was installed, in order to prevent the growth of algae (the Lascaux crisis was then on), but visitors were handed portable carbide lamps. As a result, over the years many graffiti appeared on the walls and some other acts of vandalism occurred. Gradually the conditions of visiting were bettered: groups were restricted first to thirty-five, then to twenty people a group and to a maximum of eleven groups a day in the summer, i.e. no more than 220 persons a day; portable electric torches replaced carbide lamps. In the meantime, a major study, funded by the Ministry of Culture, was engaged on the conditions of preservation of the drawings. In 1978-1979, some of them were seriously damaged by an overflow of water. Scientific analyses could prove that it was an entirely natural accident, in no way caused by tourist numbers, and such as must have happened a number of times in this or other caves. It was possible to stop the phenomenon, and the cave has since been closely watched by a team of specialists.

Niaux is a cave which is managed by the local authorities of the Ariège, but, being classified as a historical monument as nearly all the major caves in France are, it is subject to the rules and prescriptions defined by the Ministry of Culture. Its Commission Supérieure des Monuments

Historiques is specifically in charge of the preservation of cave art.

Rightly or wrongly, I got the impression from Dr Dragovich's paper that she believes, firstly, that with the caves under study, 'tourist access subsequent to discovery' was a sort of necessity; secondly, that a way of dealing with the problems of preservation was to make tourists pay for their visits and thus for the guides that control them; and thirdly that such a policy was defined by the managers of the caves.

In fact, it has always been the other way round, with all the caves ever opened to the public, whether in France or in Spain. The primary motive has always been economic. Such caves bring money and tourists to the area where they are situated, as Dr Dragovich is well aware, and that is the main reason why they were originally opened to the public. As a consequence, even if their owners (whether they be private persons or local authorities) are quite eager to protect them, as is generally the case, they are also eager not to deter tourists from visiting them and to allow as many in as they can. So, most of the restrictions were defined, and in some cases imposed, outside or even against the owners' will, by the Ministry of Culture, whose legal basis for action is the law protecting historical monuments.

As mentioned above in the example of Niaux, the problems of cave art preservation are considered nowadays in quite a different light: thirty years ago, when a new cave with art was discovered, its owners' first reaction was often to study ways and means to open it to the public and, if it was materially possible without entailing too much expense, that is exactly what happened (Rouffignac, Cougnac). These days, there exists a widespread belief among specialists that enough caves can be visited to satisfy people's natural curiosity and legitimate interest about cave art, and that new discoveries should be kept as they are, because opening up a cave always entails a number of modifications which drastically change its microclimate, such as Dr Dragovich remarked about Pech Merle and Cougnac (levelling of the ground, removing collapsed rocks, opening new passages, enlarging entrances etc.). We just do not know what the long-term consequences of such changes may be, as we are dealing with paintings which are between 12 000 and 25 000 years old.

Therefore a close scientific watch must be kept at all times in such caves: it is generally done by specialists from the Laboratoire de Recherche des Monuments Historiques.

To close one of them is legally possible, and was done in the case of Lascaux (and Altamira in Spain), when the tourist pressure on the environment seriously threatened the paintings, but it is an extreme measure that cannot be taken lightly owing to its economic consequences. In fact, if they had been discovered now, most of the caves which were opened to the public in the past would not be allowed to be, and if they were, much more care would be taken not to change their natural conditions.

It is true, though, that in a very few cases, some farsighted owners who happened to be prehistorians, not only did not seek any gains from their caves, but took a personal and extreme care of their preservation, and such caves rank among the best preserved in Europe (for example, Tuc d'Audoubert and Trois Frères by the Bégouën family, Portel by J. and J. Vézian, Gabillou by Dr Gaussen).

On the other hand, caves which are just visited a few times a year by prehistorians are difficult to watch over and, even when closed with strong metal railings or doors, may suffer from vandalism. To lessen that risk, some specialised wardens were appointed in 1982 by the Ministry of Culture and attached to the major cave areas (Dordogne; Pyrenees; Ardèche); they go from one cave to another, check the doors, occasionally show some caves to visiting archaeologists etc.

As can be seen through these few examples, the protection of cave art, be it visible by the general public or not, is a complex problem, with a long history and many different local solutions. Not knowing, except by hearsay, the situation for rock art in Australia, I cannot express a view about any similarities in its preservation, but I strongly suspect that many more similarities exist with our own open air rock art sites, such as Mont Bego, or with the painted shelters in the Spanish Levant. In any case, Dr Dragovich's accurate account of what is being done in two of our caves will usefully acquaint **RAR** readers with some of the major problems faced in the preservation of cave art.

Dr Jean Clottes
11, rue du Fourcat
09000 Foix
France

By FAY GALE

Dr Dragovich's paper on aspects of visitor management at art sites in southern France raises important issues facing rock art conservation in Australia. The situation is quite different in France but some principles apply in both countries.

Dragovich's conclusion highlights our need to learn from the French experience—'it is clear that special precautions need to be taken to prevent accelerated deterioration of rock art, which can just as easily be destroyed by too many admirers as by a single vandal'. The French were forced to take the extreme remedy in the case of Lascaux where visitors were damaging the rock art just by being there. It was not wanton vandalism which led to the closure of Lascaux but interested, well-behaved tourists.

Australia has much to learn from this experience and we need to learn before it is too late. We cannot take the extreme remedy which the French took with Lascaux because our art shelters do not lend themselves to closure as do the European caves. The rapid deterioration of the paintings at Lascaux within twenty years of its being opened to the public forced French authorities to revise the whole process of tourism and protection

and to institute very careful management controls. Furthermore the controls are being continually monitored. But in Australia we are still trying to 'boom' tourism and only where dedicated individuals have taken a lead has any attempt at tourist management begun to be monitored.

Dragovich quotes a figure of 70 660 visitors to the Pech Merle Cave in 1985 but in many art areas in Australia such as the Flinders Ranges and Uluru National Park we have already surpassed this figure without instituting any management procedures. Thus the numbers visiting our major tourist sites are more than sufficient to adopt some kind of charging mechanism. It is clear in all overseas tourist areas that tourists pay for the privilege of visiting sites and that payment is essential to meet the costs of protection.

I would go further than Dragovich in this matter and say that charging is not only an economic reality, it is also a valuable management strategy. The cliché that people appreciate what they pay for is probably very true in relation to art protection. Furthermore my research suggests that the souveniring instinct is very strong. People want to take something away from a place as a reminder of their visit or as a gift to share with friends or relatives. Thus virtually all heritage sites in Europe sell a wide array of material not only to make money but also undoubtedly to satisfy the souveniring instinct; better to sell a poster or replica than have visitors chipping off a piece of rock art or carving their names on it as a reminder of their visit.

Dragovich says that the general public in both France and Australia is encouraged to 'consider rock art as a part of the cultural heritage'. This is certainly true in France, but I doubt its applicability to Australia. Most non-Aboriginal Australians view their cultural heritage as rooted in Europe, not Australia. Our experience of studying tourists at rock art sites suggests that most see the art as definitely not part of their culture, in fact disparaging references to its primitiveness and to Aborigines in general made us realise that we have a long way to go to achieve appreciation of this art as a part of Australia's cultural inheritance.

The French realised very early in their tourist development that education plays a significant role in protection. In Australia, individuals like Dan Gillespie and Grahame Walsh have utilised this concept but it has not yet been applied to major art areas in real danger.

In France the caves are privately owned but the management strategies are State determined by a central authority. The few art areas which are privately owned in Australia certainly fare better in terms of protection than those in freely accessible national parks. The booming of the latter for tourist purposes has had serious consequences for art protection except in areas where conscious planning has paralleled the development. To date awareness of this need seems to be the exception rather than the rule.

Professor Fay Gale
Department of Geography
University of Adelaide
Box 498, G.P.O.
Adelaide, S.A. 5001
Australia

By JANE JACOBS

Dr Dragovich's paper is an example of the communication needed in the field of visitor management at rock art sites. All too often management proceeds in a vacuum, ignorant of the innovations and failures which have occurred elsewhere in Australia and overseas. Dr Dragovich provides a résumé and appraisal of some of the tools now basic to site management in France but to which there has been some resistance in the Australian scene.

Many of the management strategies used in the French caves are already being applied at Australian art sites. Wire meshing and low wooden fencing, which allow visitors to view the art but not touch it, are commonly used at the more heavily visited art sites in Australia. These are well accepted management strategies which are based on making modifications to the site. The most significant aspect of the French system, however, is that management is directed towards modifying the visitor population.

The French scene is highly regulated: limited opening hours (seasonal and daily), restrictions on overall levels of visitation as well as the number of visitors in a site at any one time, and guided tours. Each of these controls is aimed at modifying the visitor population rather than the site being visited. In France the key to this regulation is the pay-as-you-enter system. Not only does this provide a source of revenue which can support site maintenance, but it establishes a valuable point of contact at which the various restrictions on the scale and nature of visitation can be enforced.

The French system also strives to make long-term modifications to the visitor population through education and interpretation. Basic to this is the emphasis placed on the art as part of the direct heritage of the French people. In Australia, too, considerable effort is being put into the interpretation of art sites for public education. Here, however, there is a significant cultural gap between what the majority of touring Australians see as 'their' heritage and Aboriginal art which is often perceived as foreign to that heritage. The long-term challenge of interpretation in Australia is the development of a sense of pride and respect for Aboriginal art among the touring public.

As Dragovich rightly points out there are significant differences between the French caves and Australian art sites which may hinder the application of some of the regulatory principles used in France. One difference is the type of site. The French art is in confined caves which lend themselves to entry regulation. The other difference is the scale of visitation which in France is much higher and can sustain the costly, intensive controls implemented. These differences do not preclude the adoption of some of the French mechanisms at those sites in Australia

that are under intensive visitor pressure. However, the transfer of management strategies developed in one set of circumstances to other sites always needs to be undertaken with caution. Special care is needed to ensure the imported strategies suit the site, suit the problem at hand and, once implemented, are in fact working.

Probably the most controversial principle described in this paper is the idea of fee-paying visitors. While we happily accept that visitors pay to see items of our European heritage there has been considerable resistance to charging entrance fees at Aboriginal art sites. Some national parks which contain Aboriginal art do charge an entry fee at the main entrance but this does not have the same effect as charging at the site itself. The success of pay-as-you-enter schemes depends on the visitors knowing what they are paying for. In France visitors pay at the entrance to the art site. They pay for the privilege of seeing the art while the interpretive material and guides ensure that the visitors feel they are getting 'value for money'. Indeed the process of paying an entrance fee adds to the visitor's sense of seeing something valuable. What is now seen by many visitors as a free, added extra on their visit to an area like Uluru could become a valued part of their visit to the area.

Dragovich's paper will hopefully mark the point at which we seriously begin to consider adopting some of the more innovative and successful strategies working in French caves. There are many broader issues relating to the application of these strategies in the Australian scene which remain undebated. Is there really a conflict between fee-paying for particular attractions and the concept of a national park? What are the long-term consequences of tying tourist capital to heritage conservation? Can visitor numbers in Australia adequately sustain the required level of funds needed for site conservation or is entry payment simply a supplementary income? Is the income obtained from entry fee the main reason for adopting the strategy or is it the regulatory side effects which a ticketing system allows? What are the foreseeable logistical problems of establishing a pay-as-you-enter system at some of the open and remote art sites in Australia? These and many more questions need serious consideration in the light of both conservation ambitions and the reality of a burgeoning tourist industry which relies on and seeks out Aboriginal art sites as a tourist resource.

Jane Jacobs
Department of Geography
University of Adelaide
Box 498, G.P.O.
Adelaide, S.A. 5001
Australia

Lascaux

By DAVID LAMBERT

In looking at the public presentation of rock art, the overseas experience is worthy of consideration. After all, the French have been displaying rock art for a considerable time. Similar problems are experienced; i.e. graffiti, touching, visitor control etc. Accordingly any work carried out at these sites in relation to visitor behaviour similar to that described by Gale and Jacobs (1986) may be of considerable value.

The factors which have influenced site deterioration resulting from high visitation, however, bear little relevance to the Australian situation. The sites described in southern France occur in underground limestone caves with very stable atmospheric conditions. In such cases temperature, humidity, carbon dioxide and light levels are critical in site preservation. The conservator must be very sensitive to fluctuations in any of these factors because of the weathering and micro-organic processes peculiar to this situation. The same factors, however, do not apply to the open shelter site experienced over most of Australia. Here normal diurnal fluctuations of temperature, humidity and light levels would be far greater than those created by high visitation levels in limestone caves. Similarly, the carbon dioxide levels are less likely to be important because of the well-ventilated nature of the sites.

The guided tour system described at both Pech Merle and Cougnac is typically reminiscent of limestone cave tours which are in operation in many parts of Australia. These combine the tour fee, cave guide, locked gates, footpath, hand rails, sectioned flood lighting etc., often with an upper limit of tourist numbers for each tour. No doubt our thinking has been influenced by this type of cave experience; however, its relevance to the manager of today who is considering opening sites for public visitation must be questioned.

By way of example, in my own area near Gosford, New South Wales, there is a group of several painting shelters occurring within several kilometres of each other. Their present visitation is extremely low but their potential for future public viewing is extremely high. It is considered that even a very low level of promotion would generate considerable public interest. The sites have the advantage of being within easy reach of Sydney to cater for overseas visitors and to display local prehistory for the large Sydney population. The dilemma of the manager is how best to cope with the dramatic increase in visitation. The isolation of the sites, combined with the open shelter situation, severely limits visitor control. The decision in such cases is to maintain the status quo, leaving such sites unvisited.

A second site at Maroota near Sydney, New South Wales, has been more thoroughly considered as a major public visitation site with an on-site museum and walkway proposed to display large figure Sydney engravings. Currently the site receives a low level of visitation. The manager is therefore in the luxurious position to fully

plan the development without any existing constraints. In such a case, the studies by Gale and Jacobs (1986) become very useful, particularly with reference to the 'visitor type' classification. That is to say, it would be logical to predict the type of visitors expected at the site and thereby both cater to their needs and plan protective measures according to their predicted behaviour.

David Lambert
National Parks and Wildlife Service
P. O. Box 1393
Gosford, N.S.W. 2250
Australia

By MICHEL LORBLANCHET

I enjoyed reading Dr Deirdre Dragovich's paper, and I think she has perceived the main problems in both the conservation of rock art and the management of caves opened to tourism in southwestern France. I particularly appreciated what she said about Pech Merle. Dr Dragovich has correctly outlined the aims of the new museum built at the cave entrance. As its curator for six years I was so much involved in the creation of that museum that I am delighted to read such relevant comments on it. However, as with any open-minded and interested visitor, Dr Dragovich could catch only a part of the whole situation. The reality is more complex, and other serious problems are evident at Pech Merle.

I would like to emphasise the following points:

(1) *The ownership of Pech Merle and Cougnac:* most of the archaeological sites in France are on private property. The Commune of Cabrerets owns Pech Merle. Cabrerets is a small village of 200 people; its mayor is responsible for the management of the cave. Cougnac is owned jointly by the four speleologists who discovered it.

Pech Merle and Cougnac have both been declared 'Historic Monuments', so they are under the control of the Ministry of Culture. When a site is threatened, the Ministry advises the owner about the best means of protection. It can even impose its view. In 1963, for example, the Ministry decided to close Lascaux cave and acquired it.

In France only three painted Palaeolithic caves open to tourism belong to the State: Lascaux, Font de Gaume and les Combarelles in Périgord. The Ministry of Culture is advised by the High Commission for Palaeolithic Art Sites concerning any management and conservation problem, as well as on rock art research.

In practice the action of the Ministry of Culture in protecting private caves is always difficult. The economic consequences of any measure must always be taken into account and the Ministry always tries to convince the owners of the caves, rather than imposing its authority as it did at Lascaux.

(2) *The conservation problems at Pech Merle since the beginning of its exploitation:* Pech Merle has been damaged by tourism and most recently has been even more threatened by a spectacular increase in the number of visitors. The cave was opened to tourism in 1924. Since then visitors have touched the paintings, and routes through the cave were changed almost every year without any archaeological supervision.

During the last seven years I have recorded all the figures both in Pech Merle and Cougnac and also the actual state of the painted walls. I found that there is not one untouched painted surface in the visited part of Pech Merle. *All* the figures have marks left by visitors: usually finger marks but also graffiti, clay balls thrown onto the figures, scratches, dust settled on the paintings (for example on the lower part of the dotted horses) and algae.

The algae (of different species including the same as those in Lascaux) appeared in 1970 on the 'Hieroglyphs' ceiling (on the finger marks with mammoths and female figures).

The spread of the green algae stains on this clayey ceiling was provoked by a combination of an already high level of visitation and the installation of strong quartz lighting which warmed the rock even further. The Ministry of Culture ordered that the lighting be changed but the tourism level continued to increase.

The algae are not extensive but are now endemic in the cave. Consequently it is feared that a slight change in the cave atmosphere such as that caused by an increase in light and temperature linked to a heavy visitation of the site will allow the algae to spread as suddenly and as extensively as it did at Lascaux.

Moreover the successive adaptations of the passages to tourism seriously disturbed the whole cave: concrete stairs were sometimes built on groups of markings, a switch was fixed directly into a mammoth engraving, large pits were dug in the ground with pick and shovel under the main painted panels etc.

I did eight test excavations in different parts of the cave: everywhere I found a disturbed ground with buried wires, electric cables, fragments of rubber and of plastic, concrete blocks etc. Nowhere could I find more than half a square metre of undisturbed prehistoric ground. And the tiny pieces of Magdalenian charcoal I gathered for carbon dating were unfortunately contaminated.

The conclusion is that a painted cave opened to tourism is somehow lost. Half a century of tourism was enough to irremediably damage a 20 000-year old heritage.

Of course the Commune of Cabrerets was partly aware of the damage caused by tourism and sometimes endeavoured to prevent people from reaching the wall, by installing fences and wire mesh for example. But the fact is that the art is now irremediably damaged.

At Cougnac the damage is less extensive. The ground was certainly disturbed, trenches were dug out and stalagmites cut so that it is no longer possible to see the cave the way the prehistoric users did, to see where they stayed

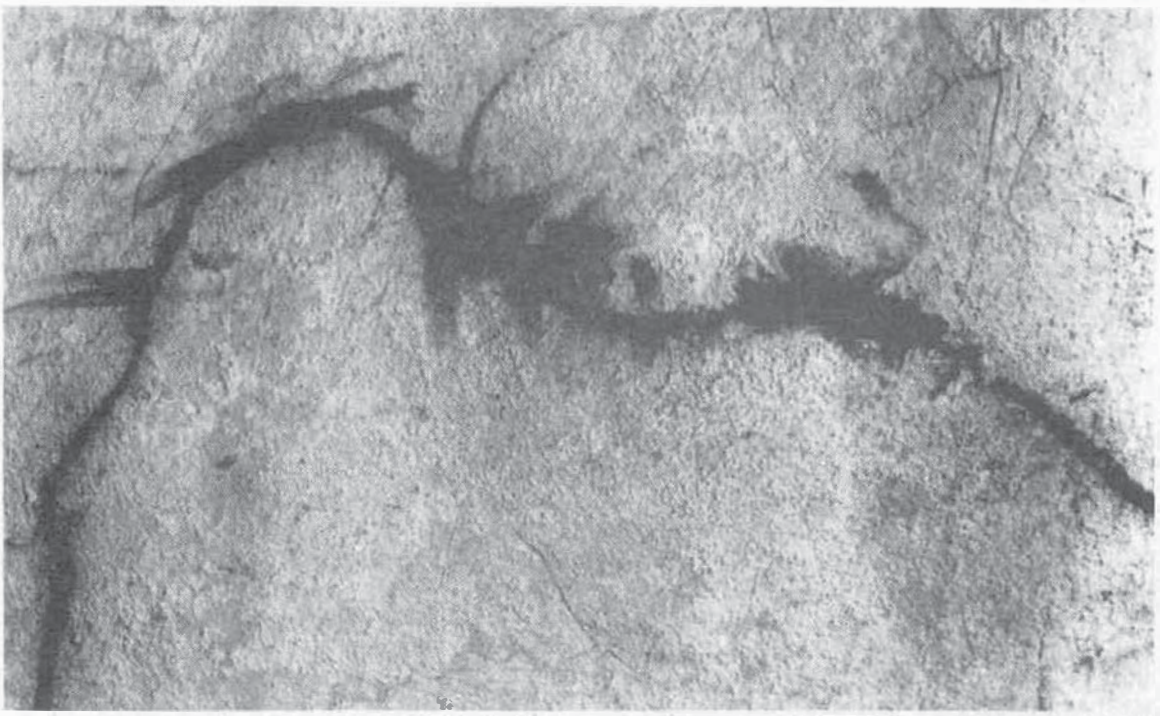

Figure 1. Finger marks made by visitors on the back of a mammoth outline of the 'Black Frieze', Pech Merle. The section shown is about forty-five centimetres long.

Figure 2. Clay balls thrown on prehistoric finger markings and two long scratches (arrows) made by visitors; 'l'Ossuaire', Pech Merle. Length of section about sixty centimetres.

and walked or to determine which stalagmites they themselves removed to make their paths.

On the other hand the painted walls are untouched (except for one tiny scratch on an ibex). The cave is much smaller than Pech Merle and the smaller groups of tourists were easier to control.

During the last few years the continuous increase in the number of visitors in Pech Merle has become a new threat to the paintings which could soon render any protection ineffective. Between 1979 and 1983 the number of paying visitors increased from 58 000 to almost 70 000 per year. That is to say the increase in four years was greater than twenty percent (see graph). The opening of the museum in 1981 may have slightly contributed to this increase since it made Pech Merle a much more comprehensive attraction.

At present Pech Merle is the most-visited

Palaeolithic site in Europe (the second in France is Rouffignac with about 50 000 visitors per year, who are perfectly controlled as they sit during their visit in a small electric train carrying the light).

(3) *What was done at Pech Merle to protect the paintings during the last six years:* the museum was built in order to present the site to the visitors and to locate it in its archaeological context, to show up the importance of the 'sanctuary' and to avoid the direct and quick invasion of the cave by tourists. Almost ninety percent of the building cost was provided by the State and the region, the rest by Cabrerets. The museum plays the role of a waiting room: while providing information necessary to an understanding of the prehistory of Pech Merle, it can also regulate the tourist flow. Moreover it was planned to become both a centre for research into prehistoric art, and the museum of prehistory in Quercy.

Having explained to the Commune of Cabrerets the risks of increasing tourist frequentation and having asked the Commune to limit the number of visitors, the Ministry of Culture had to impose a limit of 700 visitors per day and twenty-five people per guided group, as recommended by the High Commission for Palaeolithic Art Sites. At the same time the State allowed the Commune to increase entry fees (by eight percent) in order to compensate for any eventual financial loss. The Commune was also asked to install at the entrance of the cave a basin filled with formaldehyde solution, which would destroy any algae on the shoes of the visitors, as at the Lascaux entrance. The solution was also spread in the cave twice a year.

After one and a half years of limited frequentation, the following trend has emerged: in 1985 there were 68 699 visitors, that is to say 1145 visitors less than in 1984 when their number was not limited. But in 1986 it seems to have risen again despite the limitation. For example in July (which is with August the main season for tourism) there were 16 633 visitors in 1984 (without restrictions), 16 890 in 1985 and 17 816 in 1986 *despite the restrictions.*

Moreover there was no financial loss, in fact the reverse happened: the entire income from the entrance fees, both to the museum and to the cave, that is to say to the prehistoric centre of Pech Merle as a whole, is about 1.5 million francs per year. But thanks to the increase allowed by the State, the income was raised by 80 000 francs between 1984 and 1985 despite limiting the number of visitors.

(4) *The actual conservation problems at Pech Merle:* it is obvious that the arbitrary limit of 700 people per day is still too high. The access to almost all the other European Palaeolithic caves is much more limited: five people per day at Lascaux, thirty-five at Altamira, 220 at Niaux (with a volume ten times higher than that of Pech Merle), 350 at Combarelles and Font de Gaume, 500 at Rouffignac etc. The only cave with a level of frequentation probably comparable to that of Pech Merle seems to be La Pileta in southern Spain, but I do not know the exact figures.

The main effect of such limitation at Pech Merle is to slow down the continuous increase in the number of visitors, but it does not entirely prevent it. The limitation has also cut maximum daily visitor numbers: before the restrictions sometimes more than 1000 people per day visited Pech Merle in August. Such a pressure clearly endangered the paintings.

The French Research Laboratory of Historic Monuments (J. Brunet and P. Vidal), which is studying the atmosphere of the cave, will probably propose a still lower limit of visitation. The limit of 700 visitors per day does not solve all the problems: for example the doors of the cave are open for about two hours per day to cope with visitor traffic. The outer atmosphere invades

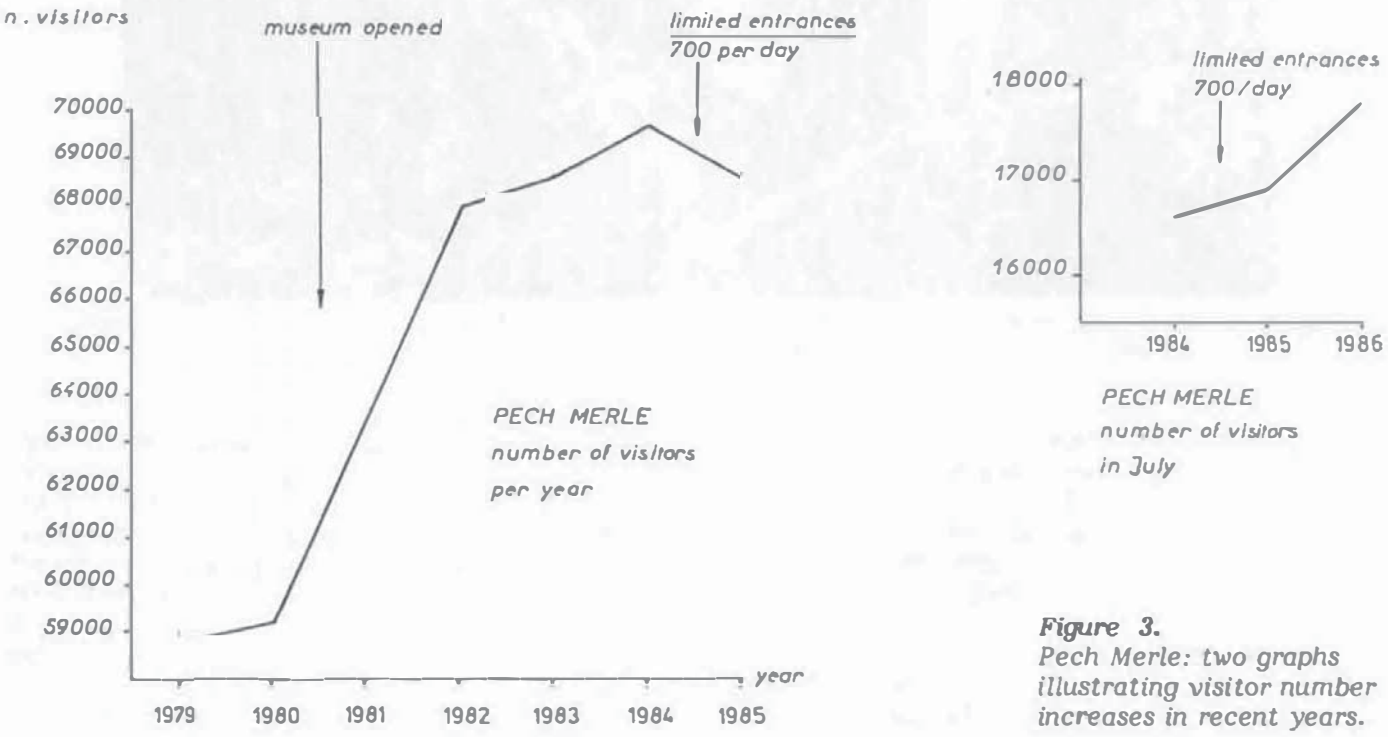

Figure 3.
Pech Merle: two graphs illustrating visitor number increases in recent years.

the cave, leading to a daily variation of temperature which can provoke phenomena damaging to the figures (concretion, condensation etc.). 700 visitors still bring in dust, algae and rubbish. Every day the vacuum cleaner in the museum collects a lot of dust but of course it cannot be used within the cave. The accumulating litter is an ideal hotbed for bacteria and algae.

Moreover, despite the increase in income, the introduction of visitor limits was not well received by the Commune of Cabrerets and by the guides of the cave! They resent the Ministry's intervention in the business of Pech Merle. Thus the owners of the cave do not do anything to warn the visitors of access limitations at Pech Merle. Every day in July-August several hundred people are rejected, many of them from far away or from abroad. Despite all the efforts of the museum employees, many do not understand why the access to Pech Merle is limited, and the Ministry is regularly accused.

Ideally, the museum should inform visitors about rock art conservation; with the collaboration of J. Brunet I had proposed to produce a short film on that topic which would be screened in the museum's theatre every afternoon when the 700-limit is reached. But up to now Cabrerets has not agreed and the museum does not fully play its role.

(5) *What could be done?* To facilitate conservation the art sites should belong to the State, as in other countries. Guides should be officially trained both on prehistoric art and conservation. To my mind the limit at Pech Merle should be reduced to 300 to 400 visitors per day. Entrance fees could be raised, and the access to a Palaeolithic 'sanctuary' should be restricted to the genuinely interested people—to be regarded as an exceptional privilege. Lastly, a regional prehistoric art centre should be created in the museum. Replicas of painted walls of Palaeolithic caves of the Quercy not open to tourism (seventeen sites) would be a valuable addition given the experience of the copy of Lascaux ('Lascaux II') which is visited by 300 000 people per year! Access to the prehistoric art centre of Quercy would not be limited, and this may be the only way to reconcile the conservation of a precious heritage, the continuation of tourism in the Pech Merle Sanctuary and the increasing interest of the public.

Dr Michel Lorblanchet
(Member of the High Commission for Palaeolithic Art Sites)
Centre de Préhistoire du Pech Merle
46330 Cabrerets, Lot
France

By FRANÇOIS SOLEILHAVOUP

Dans une étude courte, mais bien documentée, D. Dragovich expose clairement les principales contraintes exercées par le tourisme de masse sur les sites d'art rupestre en milieux confinés: les grottes d'art paléolithique du Sud Ouest de la France. Deux exemples ont été pris par l'auteur sur lesquels reposent son analyse et ses arguments: les grottes de Pech Merle et de Cougnac.

Il est certain que *l'évaluation des risques* que peuvent subir une grotte ornée est extrèmement difficile à faire: trop de variables interviennent dans les processus d'altération naturels et anthropiques, même si maintenant les spécialistes connaissent bien les gradients d'actions et les effets des principaux paramètres: taux de dioxyde de carbone, d'humidité, circulations d'air, évolution thermique etc. Cette évaluation des risques devient quasi-impossible à faire dans les sites de surface, en plein air et tout particulièrement pour ce qui concerne l'impact du tourisme. Par contre, malheureusement, *le constat et l'inventaire des détériorations* faites par les visiteurs peuvent être réalisés aisément. Prévenir le crime est impossible, le constater est facile.

Lorsque Dragovich conclu que le fait de guider les visiteurs payants et de les surveiller de façon appropriée aide à la conservation de l'art rupestre dans les grottes ornées, je ne suis pas sûr du tout que cette solution soit valable, même si elle est couramment adoptée dans la plupart des pays où l'art des grottes est présent.

Pour les milieux éminemment fragiles que sont les régions de Karst souterrain, avec ou sans art rupestre, la présence de l'homme, même contrôlée, payante ou pas, surveillée ou non, est toujours un immense danger.

Notre système économique dont dépend directement le développement de l'industrie du tourisme et des loisirs est la cause première de la dégradation du Patrimoine Culturel. Pour celui des grottes ornées paléolithiques, par exemple, les remarquables travaux des équipes du Laboratoire de Recherche des Monuments Historiques (L.R.M.H.), particulièrement les études appliquées de J. Brunet, J. Marsal, P. Vidal et J. Vouvé, ont heureusement permis de sauver la grotte de Lascaux que le tourisme avait condamné et, en même temps, à cause ou grâce à la fermeture au public de cet admirable sanctuaire, l'idée, la conception et la réalisation d'un fac-similé furent mené à bien.

Ne pouvant enrayer la croissance fulgurante de l'industrie touristique, trop d'intérêts financiers sont en jeu, la multiplication des fac-similés devrait permettre la sauvegarde à long terme d'autres témoins majeurs de l'art paléolithique telles que les grottes de Cougnac ou de Pech Merle par exemple, qui subissent actuellement une surcharge touristique, à la condition, bien entendu, de stopper le flux des visiteurs dans les originaux. Mais ceci est assez utopique compte-tenu des droits de propriété du sol et du sous-sol et de la législation en vigueur.

Il faut noter que la visite des sanctuaires paléolithiques s'accorde fort mal avec les structures touristiques telles qu'elles fonctionnent actuellement. Comment en effet apprécier pleinement un art aussi lointain, dont la charge symbolique est considérable et dont l'esthétique est aux fondements mêmes de la nature humaine, lorsqu'on ne dispose que de quelques minutes d'éclairage, derrière une masse plus ou moins compacte de visiteurs, avec parfois des piaillements d'enfants, des réflexions plus ou moins vulgaires des touristes et les injonctions pressantes

du guide à ne pas rester à la traine, en arrière du groupe?

La présence dans les grottes de beaucoup de visiteurs, pendant la pleine saison touristique, implique nécessairement très peu de temps de visite. L'art paléolithique n'est décidemment pas fait pour être vu par une masse de vacanciers. Mais chaque individu, élément du troupeau de touristes, est fortement motivé et il souhaite, même un court instant, connaître ce choc, cette émotion esthétique et culturelle.

Il est bien difficile en réalité de concilier les impératifs *a priori* formellement contradictoires du développement économico-touristique d'une région et de la conservation de l'héritage culturel qu'elle recèle. Dragovich, dans son analyse de la situation pour les grottes ornées françaises soulève implicitement ce problème.

De mon côté, j'ai assez longuement travaillé dans plusieurs hauts lieux de l'art rupestre du Sahara, comme par exemple le Tassili-n-Ajjer ou de France, comme la Vallée des Merveilles, autour du Mont Bégo, dans les Alpes, pour constater que la maîtrise ou simplement le contrôle de l'art rupestre de plein air (A.R.P.A.) vis-à-vis du tourisme, quelque soit le milieu géoclimatique de son gisement, sont quasi-impossibles à réaliser, en tout cas infiniment plus difficile que pour l'art des cavernes. Je comprends parfaitement les soucis des collègues australiens cités par Dragovich: F. Gale, K. M. Sullivan, G. S. Gibbons etc., pour ce qui concerne l'immense patrimoine à ciel ouvert de ce continent.

La clef de la protection et de la conservation des milieux naturels se trouve potentiellement dans le public lui-même, c'est-à-dire dans la formation, l'éducation et l'enseignement des valeurs du Passé et de leur fragilité. Rendre responsable chacun du Patrimoine Commun, c'est peut-être rendre responsable le groupe, la société toute entière. Et c'est donc d'abord *dans l'Ecole* et non pas à travers les moyens audio-visuels périphériques (télévisions, radios, expositions)—excellents par ailleurs—que doivent débuter la connaissance et la sauvegarde de l'art rupestre, autant que celles de tout autre vestige.

La Groupe PACT (Techniques physiques, chimiques, mathématiques et biologiques utilisées en archéologie) de l'Assemblée Parlementaire du Conseil de l'Europe a pris récemment l'initiative d'amorcer un large mouvement inter-disciplinaire pour la sensibilisation des enfants et des adolescents à l'existence, l'étude et la sauvegarde du patrimoine culturel archéologique. Deux réunions internationales se sont déjà déroulées en Italie, à Ravello (11 - 14 juin 1985 et 26 - 30 mai 1986). Les conclusions de ces importantes rencontres sont ventilées dans tous les gouvernements, ministères, organismes et services culturels. Même si le chemin doit être encore long pour intégrer pleinement l'enseignement du Patrimoine à tous les échelons, depuis l'Ecole Primaire jusqu'à l'Université, la volonté de parvenir à cet objectif est maintenant clairement affirmée par un nombre croissant de personnes, enseignants archéologues, conservateurs, administrateurs. D'ailleurs, la sensibilisation des jeunes au Patrimoine est faite, de façon ponctuelle, depuis longtemps, par nombre d'enseignants qui *aménagent* dans ce sens leurs programmes scolaires officiels.

On peut donc avoir bon espoir que, progressivement, une mentalité et un comportement nouveaux se développeront chez les futurs touristes.

L'ouverture au public de gisements d'art rupestre de plein air dans le monde, ou bien l'implantation plus ou moins bien contrôlée de structures commerciales d'exploitation touristique des sites entraînent inéluctablement le développement d'un cortège de nuisances, pollutions et effets négatifs sur l'environnement et sur l'art, quelques que soient les précautions prises par les autorités.

De nombreuses agences de voyages utilisent une grande variété de formules: véhicules 4 x 4, randonnées pédestres avec bivouacs, camps de toile, camping 'sauvage' etc. Depuis les découvertes des peintures et gravures néolithiques au Sahara par les équipes de l'explorateur français Henri Lhote, entre 1956 et 1972, et malgré l'indépendance de l'Algérie en 1962, la prolifération du tourisme est devenue en grande partie dramatiquement incontrolable dans ces régions immenses. Jamais les sites rupestres sahariens et les gisements préhistoriques de surface n'ont été plus menacés qu'actuellement, entre autre à cause de la vogue des expéditions en véhicules tout-terrains; parfois ce sont de véritables commandos de pillards qui visitent les sites archéologiques dans le but de faire commerce de documents rupestres, de pièces lithiques ou céramiques.

Dans ce sens, les sites de plein air du monde entier, même ceux qui sont classés parcs ou réserves nationaux et qui bénéficient théoriquement d'un statut de protection, sont beaucoup plus menacés que les cavernes.

Il y a moins d'une décennie qu'à l'échelon international, une prise de conscience s'est manifestée pour les dangers naturels et anthropiques qui menacent le patrimoine commun d'art rupestre. Une des toutes premières réunions internationales débatant de ces problèmes s'est tenue en 1978 à Alger et au Tassili-n-Ajjer sous l'égide de l'UNESCO, avec la participation d'organisations non gouvernementales: ICOM, ICCROM, ICOMOS. Elle fut suivie depuis de plusieures autres, en Italie (1981, 1982), au Canada (1983), en Espagne (1983), en France (1984); d'autres sont prévues dans les années à venir, par exemple en Australie (1988).

Ces rencontres permettent de mieux cerner les problèmes et de définir des accords de coopération scientifique, des stratégies de protection et de conservation. Les travaux de l'équipe du LRMH citée plus haut et les expertises conduites par ses membres, MM. Brunet, Vidal et Vouvé, vont dans le sens des discussions et des accords internationaux. L'accélération des échanges formatifs et informatifs entre les spécialistes européens et ceux d'autres continents, particulièrement avec l'Australie me paraît extrèmement significative dans la mesure où, à la globalisation des problèmes pourra succéder une globalisation des solutions.

Mais il faut aller vite maintenant: l'art rupes-

tre souterrain ou en surface ne doit pas devenir la proie des marchands de voyages et de dépaysements tarifés.

Professor François Soleilhavoup
Laboratoire de Préhistoire du Muséum
National d'Histoire Naturelle UA 184 du
Centre National de la Recherche Scientifique
Institut de Paléontologie Humaine
1, rue René Panhard
75013 Paris
France

REPLY

By D. DRAGOVICH

It is very gratifying that eight people with interest and expertise in the 'rock art tourism' question have commented on my brief (if misleadingly titled) paper. Rather than responding to each reviewer individually, I will just make a few comments in summary.

Reviewers have emphasised the variability in rock art site conditions, both in relation to between cave differences (Brunet, Clottes) and between cave and open air conditions (Bahn, Gale, Jacobs, Lambert, Soleilhavoup). Nevertheless, all sites have a common requirement for considerable attention being given to people management, because visitors constitute a major source of direct physical deterioration of rock art through vandalism and theft (Bahn, Clottes) as well as contributing indirectly to rock art destruction through the environmental changes they engender simply by their presence (Soleilhavoup). Variations in ownership and visitor patterns between caves (Brunet, Lorblanchet) and over time (Clottes) have also influenced rock art preservation. In relation to Pech Merle, Lorblanchet's detailed knowledge of the site allowed him to make positive suggestions for the possible resolution of conflict between rock art tourism and conservation.

In Australia, it seems to me that a major current issue in management of rock art sites is whether visitor fees should be introduced, and whether such fees would make for additional controls on visitors—through the sites they are allowed to visit, the 'guide' conditions which attach to site entry, and their behaviour once on site—which would be of benefit to rock art preservation. The 'sacrifice' of some sites to controlled tourism may be the unfortunate price to be paid for the preservation of other sites. It was pleasing to note that both Gale and Jacobs, who have made detailed studies of visitor behaviour at Australian rock art sites, strongly support the pay-as-you-enter system despite any possible problems associated with the implementation of such a scheme. Educating the public to consider rock art as valuable is another approach to rock art preservation, and ideally this should proceed regardless of what other site management techniques are adopted, as stressed by Lorblanchet and Soleilhavoup.

Although no prescription for preservation of Aboriginal rock art is attempted here, the brief examination of cave art management at two sites in southern France does at least provide a further basis for discussion of rock art conservation in the face of increasing tourist impact. Clearly, the protective procedures followed at one place are unlikely to be applicable *in toto* at another site but, as noted by Soleilhavoup, continued international exchange of information provides an important possible avenue towards finding solutions to the problem of rock art preservation. Specific local problems may become closer to being solved if the diverse experiences of other rock art workers can be drawn upon and modified in a manner appropriate to local circumstances.

Dr Deirdre Dragovich
Department of Geography
University of Sydney, N.S.W. 2006
Australia

Résumé. 'L'art rupestre touristique' fut examinée en relation avec l'administration des cavernes dans deux sites au sud de la France. Le risque de détérioration de l'art rupestres dans les cavernes a été exprimée par une variété de mesures ayant pour but d'empêcher les visiteurs de toucher aux oeuvres d'arts en les surveillant et en les guidant dans les cavernes, et en encourageant les visiteurs de considérer l'art rupestre dans les cavernes aussi importants à l'héritage culturel, et en refusant l'admission de visiteurs ou en limitant leur nombre quand les effets adverses sur l'art rupestre deviennent apparents. Un grand nombre de touristes qui payent des droits d'entrées peut engendrer des fonds substantiels pour l'administration et la conservation du site, mais leur simple présence peut souvent conduire à l'accélération et la détérioration de l'art rupestre dans les cavernes.

Zusammenfassung. Felskunsttouristik wurde im Zusammenhang mit der Verwaltung zweier Höhlen in Südfrankreich untersucht. Der Gefahr einer Verschlechterung des Erhaltungszustandes der Höhlenkunst begegnet man mit einer Zahl von Masznahmen, die ein Berühren der Kunst durch Besucher ausschlieszt; durch Beaufsichtigung der Besucher und Führung in den Höhlen; indem man sie dazu anregt, die Höhlenkunst als wichtiges Kulturerbgut zu sehen; und indem Besucher ausgeschlossen werden oder ihre Zahl begrenzt wird, wenn schädliche Auswirkungen auf die Kunst deutlich werden. Grosze Zahlen von gebührenzahlenden Touristen können beträchtliche Geldmittel für Verwaltung und Erhaltung beistellen, doch bereits ihre Anwesenheit kann zu einer beschleunigten Zerstörung der Höhlenkunst führen.

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ROCK ART PROTECTION PROJECT
Directed by the
Australian Institute of Aboriginal Studies

The Minister for Aboriginal Affairs, who recently joined AURA, has provided funds for the protection and preservation of Australian rock art, to be administered by the Institute of Aboriginal Studies during the current financial year. A Rock Art Working Group has been established which held a meeting on 5 December 1986 in Canberra to make recommendations on the disbursement of the funds.

Thirty funding applications had been received despite the very short notice, totalling about three times as much as the funds available. Since most applications were worthy of support the Working Group had to determine which of the projects would provide the most cost-effective results, and try to stretch the available funds to benefit the largest possible number of these. Only twelve projects were approved, after several hours of tense debate, negotiation and voting; not a single one was approved at the budgeted figure, all were scrutinised and trimmed, and some had to be reduced significantly in scope.

No doubt some unsuccessful applicants will be disappointed, particularly those who are not attached to some public agency; planning of the projects and preparation of applications would have involved much work for them and perhaps they held high hopes for their proposals. As a member of the Working Group I should like to report that I could detect no bias against any category of applicants: for example, the A.I.A.S. strongly supported some projects by private groups or individuals, while receiving some applications by government authorities rather critically. What I did find significant was that the high approval rate among Aboriginal-initiated projects reflected their eligibility—not partiality on the part of committee members: four of six were approved, being easily among the ten most worthwhile proposals. Does this indicate an increased awareness among Aboriginal communities of their role, or potential involvement, in rock art protection?

It is relevant to reflect upon long term considerations: there is no government commitment for further funding in future years, so the best policy for securing funds for proposals not approved this time was to take the greatest care in distributing the limited funds in such a way as to achieve the most obvious and impressive short term results. Hopefully this will demonstrate the effectiveness of this flexible type of funding, and the more ambitious long term projects will find their justification in the early results.

In the light of three unsuccessful applications to establish a national rock art research centre it may be worthwhile to consider alternative means of funding and implementing consistent rock art conservation policies. Annual funding of selected projects on a permanent basis could well be an effective system of achieving optimum results with limited funds, a system from which the most beneficial method would evolve by carefully monitoring the results. Besides, the establishment of a centralised agency would be in conflict with the recommendations of the 1981 Unesco Rock Art Consultation in Italy, according to which regional rock art centres are preferable (Recommendation 1.2 [viii]). There are existing bodies in Australia which could be utilised, upgraded or extended for this purpose.

R. G. Bednarik

PUBLICATIONS ON ROCK ART
INSTITUTE OF ARCHAEOLOGY
UNIVERSITY OF CALIFORNIA, LOS ANGELES

A discount of 20% is available to AURA members on all publications of the Institute of Archaeology, University of California, Los Angeles. The following rock art volumes are currently listed:

Ancient Images on Stone: Rock Art of the Californias. Edited by JoAnne Van Tilburg (1983). 128 pp. ISBN: 0-917956-40-0. US$20.00.

A richly illustrated catalogue from the National Park Service photo exhibit, 'Shaman's Song: The Rock Art of Western America'. Included are 11 essays by the foremost researchers in California rock art studies, as well as 22 full-colour plates.

Ilovenweep Rock Art: An Anasazi Visual Communication System. By Nancy Olsen. Occasional Paper 14 (1985). xvi-153 pp. ISBN: 0-917956-47-8. US$15.00. (See review in *RAR* Vol. 2, pp. 167-8.)

Messages from the Past: Studies in California Rock Art. Edited by Clement W. Meighan. Monograph XX (1981). i-185 pp. ISBN: 0-917956-24-9. US$10.50. (See *RAR* Vol. 1, p. 53.)

Petroglyphs in the Guianas and Adjacent Areas of Brazil and Venezuela: An Inventory with a Comprehensive Bibliography of South American and Antillean Petroglyphs. By C. N. Dubelaar. Monumenta Archaeologica 12 (1986). 327 pp., 77 linedrawings, 184 photographs. ISBN: 0-917956-50-8. US$35.00.

In the wake of the erosion and destruction of the petroglyphs of South America, Dutch scholar C. N. Dubelaar presents 30 years' worth of petroglyph documentation with extensive illustrations. The history of previous petroglyph investigations in the area, a survey of sites, and a comprehensive bibliography of South American and Antillean petroglyphs, over 1000 entries, is also included. (*RAR* review forthcoming.)

Pictographs and Petroglyphs of the Oregon Country, Part 1: Columbia River and Northern Oregon. By J. Malcolm Loring and Louise Loring. Monograph XXI (1982). 325 pp. ISBN: 0-917956-35-4. US$18.50. (See *RAR* Vol. 1, p. 53.)

Pictographs and Petroglyphs of the Oregon Country, Part 2: Southern Oregon. By J. Malcolm Loring and Louise Loring. Monograph XXIII (1983). xi-355 pp. ISBN: 0-917956-43-5. US$22.50.

Part 2 of the Lorings' study of the rock art of the Oregon Country, with primary data on 150 sites in southern Oregon, including brief descriptions, bibliographical references, and 460 illustrations.

Prehistoric Indian Rock Art: Issues and Concerns. Edited by JoAnne Van Tilburg and Clement W. Meighan. Monograph XIX (1981). ii-66 pp. ISBN: 0-917956-24-9. US$6.00. (See *RAR* Vol. 1, p. 53.)

Institute of Archaeology Publications, University of California, Los Angeles, 405 Hilgard Avenue, Los Angeles, CA 90024, U.S.A.

RAR DEBATES

Rock Art Research 1986, Vol. 3, No. 1, pp. 30-61.

PARIETAL FINGER MARKINGS IN EUROPE AND AUSTRALIA

Robert G. Bednarik

FURTHER COMMENTS

By ELERY HAMILTON-SMITH

One of the important lesser issues raised in Bednarik's comprehensive review and synthesis is the selectivity of those markings left for us to study. It is no accident that known finger markings in Australia are, other the exceptional site at New Guinea 2 Cave, confined to the soft aeolianites of Australia's south-westerly coast (as mapped by S. White 1986) or the underlying Tertiary carbonates and the chalky limestones of the Nullarbor. Both of these provide many surfaces readily able to be marked and, when those surfaces are within caves or sheltered sites, likely to retain those markings. New Guinea 2 Cave, by a happy accident, has a lining of mud which provides for this. But most Australian caves are in hard Ordovician, Silurian or Devonian limestones, the readily accessible surfaces of which are washed clean of deposit and are much too hard to accept finger markings, although, of course, some have other forms of early markings.

So, White's map provides a quick overview of those regions which might most profitably be further searched. Similarly, one might identify other regions of the world where a close search is most likely to be repaid with new discoveries simply by mapping the distribution of aeolianites and other soft limestones.

More centrally, it seems to me that there are three questions that might be raised in respect to ancient finger markings:

(1) Why did human beings mark more-or-less flat surfaces at all?
(2) Why did the markings take the form which they did?
(3) Why did the finger markings cease to be made?

My reading of Bednarik's argument suggests to me that he has more adequately responded to the second question than the first. However, in reflecting upon his work I find it useful to consider the potential relevance of neural arousal theories, as I believe they provide a valuable support and complement to the Phosphene Theory, and even contain a response to the third question.

Students of arousal are many, but for simplicity here I will rely solely on the work of D. E. Berlyne (University of Toronto), undoubtedly the most important contemporary theorist, although in places drawing upon the useful summary of his ideas by Ellis (1973). Berlyne's work is widely supported by experimental studies on man and other animals, as well as extensive conceptualisation.

In outline, Berlyne (1960: 46-51) firstly draws upon neurophysiology, pointing out that the Reticulate Arousal System (RAS) of the lower brain serves on the one hand to carry external sensory input to the cortex and thus bring about arousal, and on the other to inhibit arousal messages from the cortex to the peripheral central nervous system. Thus an organism's level of arousal depends upon the balance between the arousing influence being exerted by the RAS and the inhibiting influence of the cortex upon the RAS.

Next it is argued that any organism will strive for an optimal level of arousal. This is obviously in part a biological imperative related to survival, in that an unaroused organism would all too easily fall victim to a predator. However, Berlyne (1960, 1973) demonstrates that optimal arousal also has a hedonic effect, and so is intrinsically meaningful to the organism. If the organism's state of arousal is sub-optimal, then behaviours will be emitted which 'are most likely to produce sensory or cognitive stimulus events that have impact or potential for being selected from the ongoing but familiar events in which they are embedded' (Ellis 1973: 91). The common attributes underlying stimulus-arousal behaviours will include some degree of novelty, uncertainty or dissonance and complexity (Berlyne 1960: 18-44).

These behaviours include forms of exploration and curiosity leading on to epistemic or knowledge-seeking behaviour (Berlyne 1960, 1966); ludic behaviour, including humour and much of what is commonly known as play (Berlyne 1960, 1968) and aesthetic appreciation (Berlyne 1971). Thus stimulus-arousal theory provides a framework for integrating our understanding of a number of aspects of human behaviour which otherwise remain puzzling. Ellis (1973: 108-9), in relating play behaviour to stimulus-seeking, points out that both '. . . are accompanied by positive affect; they both involve exploration, investigation and manipulation of the environment or the symbolic representations of experience; and such stimulus-seeking and play behaviours that are observable are both emitted with higher frequency by the young of a species.'

So, in turning to consider finger markings, the evidence is totally congruent with the idea that the act of producing such markings is an arousal strategy, creating events which can be selected from the setting in which they occur. They serve to make a simple phenomenon (a flat surface) more complex, and hence more likely to provide stimulus. They also translate tactile behaviour and experience into a visual experience which not only has intrinsic hedonic value, but provides cybernetic feedback on the impact of one's behaviour.

Further, they provide the opportunity to explore a specific medium and to gain knowledge about that medium. Given that any organism will strive to maintain optimal arousal, finger markings are clearly a most appropriate behavioural strategy. Bednarik's observation that a high proportion of markings have been made by children further supports the idea that finger markings are a stimulus-arousal strategy. If finger marking is stimulus-arousal behaviour, theory would predict that children would exhibit it more than adults. Finally, the notion of finger painting as a lowly step on the ladder of cultural evolution fits neatly with Berlyne's approach to the evolution of the aesthetic sense. As a forerunner of other kinds of art, it again is what Berlyne might well have predicted—a first step in making simple environments more complex. In fact, one can only be amazed at the goodness-of-fit between finger markings and Ellis's summary of stimulus-seeking behaviour and play cited above.

Again given the striving for optimal arousal, the first artists had to draw upon inspiration for design, just as today's artists must. Bednarik's Phosphene Theory provides the answer to this question—not bear scratchings or water symbolism, but the translation from internal awareness to external stimulus .

It also seems easy to explain the cessation of the finger marking tradition. Once more effective technologies became culturally available to serve the stimulus-arousal function, then these would obviously be adopted. So finger markings would give way to engraving or painting, just as rock media have given way to papyrus, paper, and today, electronic media.

In summary, I believe that arousal theory supports and adds considerably to Bednarik's formulation. Perhaps his suggested archaeopsychologist has already arrived in the guise of an arousal theorist. If so, then arousal theory is firmly rooted in a positivist epistemology, but at the same time as I point this out I must conclude by saying that I reject the notion that positivism necessarily provides any more adequate construction of reality than a more subjective paradigm.

Elery Hamilton-Smith
Department of Leisure Studies
Phillip Institute of Technology
Bundoora, Vic. 3083
Australia

By JEAN CLOTTES

The comprehensive way in which Bednarik studies such a wide-ranging phenomenon as finger markings is particularly useful and enlightening. He says that his 'objective in comparing all sites' is to 'consolidate all we know about it, admitting only plausible deductions' (p. 47). To help somewhat in furthering his aim I should like to raise three points, based on my close association with Pyrenean cave art in the past fifteen years.

(1) If it is true that 'Surfaces of cave clay that have endured since the Upper Palaeolithic times . . . are exceedingly rare', they are not 'always extensively corroded', and many more than 'a few human footprints are known from them' (p. 39). To the three French caves quoted (Niaux, Pech Merle—where twelve human footprints have been studied and not just one; cf. Duday and Garcia 1983—and Montespan), one may add: in France Tuc d'Audoubert (Bégouën 1927), Fontanet (Delteil *et al.* 1972; Clottes 1973), Réseau R. Clastres (Clottes and Simonnet 1972), l'Aldène (Cathala 1948); in Italy Tana della Basura (Pales 1960); and in Spain Ojo Guareña (Duday and Garcia 1985: 36). In all the caves mentioned a number of footprints are perfectly preserved, often covering extensive areas, which allows specialists to study the activities of those that left them, to distinguish between adults and children, and sometimes even to evaluate their sizes and other details (Clottes 1985).

Moreover, besides footprints, many drawings on the ground have been found in various caves; not surprisingly several of them (Fontanet, Tuc d'Audoubert, Niaux, Montespan) are caves with footprints. These drawings take various forms: series of dots done with the tips of the fingers, engravings by means of some tool, or even finger tracings. As in wall art, naturalistic animal figures, signs of various kinds and seemingly unorganised lines have been discovered. The latter category seems to be quite different from the finger markings on walls or ceilings: for example, even when they are numerous, as in the Salle des Talons in the Tuc d'Audoubert, they were not made with several fingers, but just with one, or they were engraved.

Therefore, we have no proof that finger lines such as described in Bednarik's paper were similarly executed upon various soft substances, and we may even doubt it, considering that clay (or sand, for that matter; cf. Réseau Clastres) could undoubtedly retain and preserve marks and often has, and that 'macaronis' are not so far known among the rather extensive repertoire of ground engravings.

(2) The problem of dating noniconic finger lines (Bednarik, pp. 41-2) is indeed arduous, and if, since H. Breuil, they have been ascribed to the earlier phases of the Upper Palaeolithic and been considered as one of the sources of art, it is mostly because of their coarse character rather than through factual proof. If some of

the early ones may safely be ascribed to the Gravettian it is because they are closely associated with animal heads or outlines whose exact parallels were found in well-dated Gravettian strata (Gargas; or, by comparison, the so-called 'Galerie Aurignacienne' in Les Trois Frères). This particular sort of drawing, though, is known from throughout the whole Upper Palaeolithic (Brézillon 1984: 31). For example, it exists in two caves whose art is perfectly homogeneous and typical of the Magdalenian and in which not a single trace of an older human presence has ever been discovered (Tuc d'Audoubert, Réseau R. Clastres).

(3) The Tuc d'Audoubert macaronis, which Breuil curiously omits in his syntheses (1952; Bégouën and Breuil 1958), perhaps because they did not fit into his chronological scheme, are typical meanders on a ceiling, whereas the Réseau Clastres ones are of a different kind altogether and their meaning is probably entirely different too. They are situated at least 400 metres from the nearest possible entrance (Clottes and Simonnet 1972) and consist of three successive horizontal bands (total length 5.6 metres; between one metre and 1.2 metres above the ground). At one point a second band follows the first and partially crosses it. First two fingers, then four were used to imprint these marks in the *Montmilch* covering the wall. They start where the passage narrows, compelling one to pass along the wall. From their location and their 'technique' it was possible to find out that they were made by somebody who used his right hand, trailing it across the *Montmilch* as he walked; somebody else imitated him (or her), which accounts for the overlapping bands (Clottes and Simonnet 1972: 316-7).

In this case the finger marks are not intricate drawings, but record the seemingly casual trailing of a hand along a soft surface by somebody walking past. Explanations such as water symbolism or the influence of cave bear claw marks no more apply than a utilitarian objective. The most plausible hypothesis would be an instinctive (or deliberate?) desire to leave a trace of one's passage inside a remote cavern, as cave explorers used to do some decades ago. It could be compared to the wanton breaking of stalagmites in the Tuc d'Audoubert, which is also a deep cave (Bégouën and Clottes 1981: 184), and would serve to assert one's personality in an alien and hostile milieu (Camps 1972: 144).

Dr Jean Clottes
11, rue du Fourcat
09000 Foix
France

By PAUL FAULSTICH

The recent discoveries of Pleistocene finger markings in Australia has presented a multifaceted challenge that Bednarik is boldly addressing. The striking chronological, technical and possibly psychological parallels between the Australian and European finger line traditions should not be glossed over, and I commend Bednarik on his initial inquiry into the nature of these markings.

I do not propose herein to comment on any minor problems with Bednarik's report, for to do so would only serve to distract us from the far-reaching implications of his important research. I agree with, and accept, the vast majority of Bednarik's observations and analyses. His broadly based approach encourages multidisciplinary debate, which in turn can only guide us closer to comprehending the significance of the markings. I will take this opportunity to explore a little further the psychological, developmental and evolutionary implications of Bednarik's observations.

Searching for parallels in rock art across wide geographical and cultural boundaries is often a fruitless and shallow task. Bednarik's report, however, strikes me as one of the very few efforts at comparison which succeeds in illuminating an important and perhaps universal stage in the cognitive development of hominids. Bednarik's article casts a fresh, albeit controversial, light on the developmental processes which are encapsulated within the finger markings.

The European and Australian sites are related to each other through the common frame of the *Homo sapiens* mind and body. Bednarik's long-range intent seems to be to map out the cognitive and psychological changes that took place in the evolution of hominids and which transformed them into a less intuitive, more rational animal. He is interested in the oeuvre of nonperishable Pleistocene images as clues and evidence that can help us to reconstruct this unfolding. Bednarik is correct in realising that the parietal finger markings hold exciting research possibilities, not only within the field of rock art, but within the wider arena of what he calls 'archaeopsychology'. Using such an interdisciplinary approach, the creative and resolute researcher can more effectively delve into the theoretical and philosophical aspects of hominid development. This approach would prove especially valuable in examining the emergence of artistic consciousness and cosmological awareness.

The concept of universal archetypes may have some significance here. The (presumed) autochthonous development of the finger line tradition in Europe and Australia suggests that these are primary markings of the most accessible type. They are primordial patterns which have a striking immediacy, both visually and strategically (i.e., marking techniques).

Bednarik poses the idea that the 'psychograms' may have been a precursor to advanced linguistic expression. It strikes me as quite plausible that the finger lines illustrate a unique period in the development of the modern mind. The markings, perhaps, represent a quantum leap in *Homo sapiens'* expanding penchant for abstract thinking and manipulative control over the environment. This visual expression may well have preceded complex verbal expressions of *Homo sapiens'* relationship with 'the Other'. As primal symbols they may have been more exploratory than communicatory, and served as precursors to the verbal manifestation of new, developing thoughts. Thus, we are confronted with the question of what the role

of art is in defining and expressing *Homo sapiens'* place in nature. Art (image making) is a communally accessible way of transmitting and synthesising new thought. This 'new thought', encapsulated within the finger lines, may not have been so much an advance in cognitive ability as it was a dramatic shift in cognitive emphasis. This shift appears to have been toward externalising, manipulating and classifying.

Bednarik's suggestion that animal claw marks may have provided an impetus for the so-called artificial human markings is important. From this perspective, the finger markings may be evidence of a sequence in cognitive development which enabled *Homo sapiens* to process and reshape natural signs. Modern physics has shown that the physical world is governed by laws and structure—a structure which exists independent of human interaction. The human mind, however, also is structured, and is structuring. It wants to process sensory information, to structure it, and to shape it into a form of workable, accessible knowledge. The mind finds structure 'out there' and re-works it so that it will make sense to the human organism. Perhaps the organising principle of the human mind discovers structure in nature because its own structure is the product of nature. The holographic model of brain function, as pioneered by the neuroscientist Karl Pribram, further supports this relationship.

There appears to be a universal organising principle of the human mind. This universality of the taxonomic sorting process lies at the root of our consciousness and our humanness. On this common ground lies our ability to name and recall, to review experiences, to plan and predict, to investigate, to consciously manipulate, to imagine, to mimic, to contrast and to organise. Classification is that order-finding analytic device used by the human mind through which the objective world can be organised and reflected upon.

If the artificial finger markings were a response at some level to the natural animal claw markings, then we can see how a visual similarity allowed for one group of images (human markings) to represent another (animal markings) while transcending themselves (that is, they may have been more than simple mimicry; they may have been an expression of control, inter-relatedness, order-finding and order-creating). Art may have begun and continues to serve as the means by which the gap between the natural order and the human order is bridged. It is not surprising, therefore, that animal markings may have been critical players in the unfolding of human artistic development.

The psychological factors influencing the creation of long-lasting markings should be addressed as well. Images fashioned in most malleable surfaces, such as soil and sand, will soon be obliterated through natural processes (wind, surf etc.). In the *Montmilch*, however, was found a medium which could be permanently altered. This, perhaps, was psychologically instrumental in allowing *Homo sapiens* to establish a new level of control over the environment in a long-lasting and largely effortless way. It is not difficult to imagine how these images could quickly come to represent more than mere marks; they may have been viewed, on some level, as expressions of the hominids' struggle with gaining control over their surroundings. If there was either a conscious or unconscious desire to create a lasting image (as opposed to one that would soon vanish), then there were complex cognitive and/or psychological forces at work.

By investigating marking strategies in the better preserved examples of finger markings, we can perhaps gain insight into the psychological factors involved in marking. For instance, were the marks made swiftly and exuberantly, or slowly and deliberately? The research potential of the type of internal analysis pioneered by Marshack is great, and if applied to the finger markings may provide valuable results. By closely examining the remodelled *Montmilch* it may become possible to identify physical aspects of the marking strategies. This type of research could lend important clues as to the motivations behind the markings. Investigating the motivations is the essence of my interest in the markings, and I suspect that it is at the root of Bednarik's as well.

Bednarik's article has brought to the surface a multitude of questions and observations. The quality of debate which has thus far been generated by his report is indicative of its importance. Bednarik is calling for a new approach in investigating Pleistocene markings; he is asking us to abandon the futile search for meaning and to pursue our investigations with creative and deductive reasoning rather than interpretive speculation. I hope that his message is widely heard.

Paul Faulstich
Institute of Culture and Communication
East-West Center
1777 East-West Road
Honolulu, Hawaii 96848
U. S. A.

REPLY

By ROBERT G. BEDNARIK

If the quality of the debate generated by my paper were a measure of its importance, as Faulstich suggests it is, it would indeed be a paper of overwhelming importance. Whilst the validity of that correlation is not apparent to me, the very high standard of debate I have already commented upon in my Interim Reply continues right through the present responses, and it becomes quite clear from comments such as those by Hamilton-Smith and Faulstich that this debate is achieving a significant broadening of our theoretical base. The reviewers' almost unanimous endorsement of my overall approach and their immensely valuable feedback provide this project with a fresh impetus, and the revitalisation that has already become apparent in recent months promises further progress. I see in the

sum total of these comments quite explicit terms of reference for archaeopsychological inquiry.

This is already well exemplified in the first Comment, by Dr Davis, which I find most stimulating and extremely pertinent. Examining several aspects of my paper in the light of his own, certainly very advanced work on the general subject, but especially on iconicity and the 'discovery' thereof, Davis provides the constructive criticism my ideas are so much in need of. Space does not permit me to give his response the kind of attention it deserves, and I shall just briefly focus on points that seem important to me. I should emphasise, however, that I will endeavour to enter into a more detailed debate at the first opportunity.

First, an elucidation of what Davis has called self-sufficient marks, such as can be produced by nonhuman primates. This subject is of importance to the question of symbolic dimension, the introduction of semantic value. Many animals produce cave markings and there is no doubt in most cases that such markings are purely fortuitous. Originally the sole purpose of considering in the Parietal Markings Project animal scratch marks had been to master the distinction between anthropic and natural cave markings. Many natural rock markings appear to be more complex, sophisticated and 'man made' than some authentic petroglyphs, and discrimination is certainly an expert task. In a most comprehensive treatise on natural cave markings (Bednarik, in press a) which is still to appear in print, I have devoted a whole chapter to the parietal claw marks of cave bears. I have examined them in numerous caves in central and western Europe where they occur in massive numbers, distinguishing eleven types of them, and I shall single out one of them here to assist in the present debate. It consists of often dense concentrations of long, vertical sets of claw marks on certain selected walls, executed almost meticulously, by applying even pressure over their full length. Differing so clearly from the violent or frenzied strokes accounting for at least three other types of ursine scratches, these baffling marks seem to be done in a *purposeful* manner. Noting that they were sometimes near hibernation pits I eventually proposed (after giving the matter much thought) that they might be territorial marks, originally designed to warn potential intruders. This mechanism of avoiding conflict was perhaps introduced into caves during the telocratic phases of the stadials, when the hibernating animals were attracted by the stable cave temperatures. Yet in the darkness of caves they had no function.

Here we have an example of an *impulse* to make marks which were, strictly speaking, not even intended to be seen, and which surely required no 'symbolic intentions'. I would argue that these marks were made without *conscious* intent at all, they were *reflexive*.

'Purposeful' markings are produced by various species (and the word 'purposeful' has a wide range of meanings in this context), and in the case of *Pongidae* I am tempted to attribute their markings to exploratory behaviour or mimicry—areas in which these species certainly excel. These markings seem to lack the co-ordinated strategies evident in the 'meanders' Marshack has considered, and Davis's division between 'self-sufficient marks' and semantic marks requires careful consideration. After all, it is crucial to the central question posed by Davis: how did mark making acquire a symbolic dimension? One can highlight some aspects of the issue by asking: what is the status of the kind of doodles a modern business executive produces on his writing pad whilst searching for inspiration? One could focus on the degree of control a mark producer has over his or her activity: to what degree can marking strategy be modulated at will? But how much stylistic latitude could a producer of archaic art with a very limited repertoire of geometric motifs have? Even the selection of motif may be regulated by cultural conventions or codes, and there may be very little scope for conscious modulation. This train of thought can be followed further, leading to the realisation that arbitrary designations, while of course necessary to facilitate debate at this very early stage, must be handled with extreme care, and that a conscious effort must be made to prevent their becoming entrenched prematurely.

Another volatile issue is that of terminology. Davis seems to imply that if a marking is not 'self-sufficient' he would expect it to be of semantic value, in the sense that it is a symbol or sign: it stands for something; it is a representation. Davis asks whether digital flutings could have signified one thing in Europe and another in Australia. Most certainly they *could* have—if in fact they signified anything at all. I have concluded that the various semantic interpretations that have been advanced (including those contributed by myself) could not be adequately substantiated. More importantly, I have tried to show that semantics are among the trivial aspects of early marking traditions, that derivation is the real issue.

The term *representation* has been most eloquently characterised by Professor Brook in this journal (Brook 1985) and whilst there is every possibility that finger lines (or any other form of rock art) *represent* something, the proof for such a connection is certainly lacking (as it is in most rock art). The crucial attribute of a consciously produced or modulated marking is that it gives external existence to a neural pattern or model—it is an *externalisation*. We can say so without assuming that it must have a meaning, it can be produced spontaneously, through 'a creature's interest or pleasure', in Davis's words. In my view the distinction between 'representation' and 'externalisation' is not just an exercise in terminological pedantry, it is most fundamental to rock art studies because it corresponds to the difference between meaning and derivation.

As we seem to be unable to determine whether a marking had semantic content for its maker (and are likely to remain so forever) we should perhaps abandon a categorisation based on this criteria, in favour of one using objective criteria. For example, we can assess early markings with

the aim of hypothesising about their origin or psychological content, and I believe we can do so in a quite scientific manner. If we permit ourselves the liberty of assuming that the evolution of subjective perception is reflected in early markings, as I have done, we will admittedly have to proceed most carefully. But if we are to have any hope of ever extracting the truly significant information available in early art we cannot neglect to investigate its articulation with the origin of the human model of reality.

I am therefore particularly grateful to Davis for taking up the subject of the Phosphene Theory—a subject lacking a response from almost any other author so far. Davis has several queries but then answers them quite satisfactorily himself. The major problem he raises is: why did the makers of archaic markings 'depict' only some phosphene experiences, but omitted to record others, specifically those that are particularly complex. I will keep my responses to this very constructive query as brief as possible:

(1) This neither weakens the theory, nor would the same be strengthened if all entoptic phenomena were depicted faithfully: that Davis uses only some words of the English language does not alter the fact that his Comment is in that language.
(2) Davis rightly implies that even sophisticated mark producers such as ourselves are unable to reproduce any of the complex phosphene patterns some of us experience, probably partly because they are often highly dynamic. Since the producers of the most archaic rock markings were no doubt significantly less dexterous in this respect it would be more appropriate to marvel at their ability to produce at all any consciously modulated markings. To comprehend the concept of 'circle' may have been a major intellectual achievement for them—an achievement of a magnitude that is likely to be beyond the capability of most or all contemporary humans.
(3) Phosphene experiences vary somewhat from one subject to the next, but to have communicative value the externalised and adopted motifs had to be recognised by most or all members of a participating group. Thus the motifs one would expect to find in the art record are those that are the most universal, the most readily identifiable and those that are easiest to reproduce.
(4) Naturally one could argue that the phosphene experiences of Pleistocene hominids may have differed slightly from ours, or have been of a more restricted range. But I admit that this is not a convincing argument: entoptic phenomena are determined by the physiological structure of the neural system of vision (optical nerves and cerebral centre of vision) which suggests phylogenetic longevity of phosphene types (Bednarik 1984).
(5) The phosphene experiences of Pleistocene man were of course not electrically induced (as those in the experiments by Knoll and others) and quite probably not drug induced, therefore it would seem reasonable to assume a smaller range of types or patterns.
(6) Perhaps most importantly, the externalisations of archaic rock art found nowadays can be safely assumed to be part of an already well-established tradition or convention. Its individual motifs would have already undergone conventionalising and selective processes at the time they were produced.

Summarising these points it should become sufficiently clear that the record of archaic rock art could not possibly be expected to present us with a complete range of faithful 'naturalistic' depictions of all phosphenes experienced by modern subjects. Not only is this technically impossible, we do not demand or find such a perfectly complete record in any other archaeological evidence, and can even less expect to find it here. In my view the degree of correspondence is incredible as it is and it would be more appropriate to ask whether there are—as Davis suspects—any archaic (prefigurative) markings that *cannot* be 'paralleled somewhere in the range of entoptic phenomena'. My answer is that I am not aware of *a single one* among the hundreds of thousands of Australian motifs that can be reasonably attributed to the Pleistocene, which include the archaic petroglyphs of northern Queensland, the early part of the Panaramitee series, the cave art along the southern coast and the Tasmanian rock art; and much the same seems to apply to the earliest art of western Europe. Incidentally, even if a small percentage of markings without phosphene parallels would be found among early traditions this would not invalidate the theory of a relationship.

The Phosphene Theory's plausibility is not solely derived from the (admittedly striking) correspondence between archaic rock art and (perhaps conventionalised) phosphene types, it gains a lot of strength from a very fundamental need for a logically consistent, *empirical* explanation for the emergence of the human ability to consciously experience what we perceive as reality. Chlichés such as 'gradual awakening of the human spirit' hold no promise of a precise formulation (besides failing abysmally when required to explain the vast discrepancy between reality, and that three-dimensional make believe world our anthropocentricity provides us with), as it should be possible to find for any process in the physical world. The Phosphene Theory not only offers such a promise, it could even restore one's faith in empiricism, by providing an empirical explanation for the subjectivity of empiricism.

Much of Davis's excellent elucidation of the Phosphene Theory is remarkably similar to two unpublished texts by myself. In the present context I just like to add a few points. Children appear to experience phosphenes so commonly that they conceivably play an important role in the early infantile environment. Significantly, the ability to scribble recognisable (e.g. geometric) patterns does not exist in a child under the age of three years, but then it develops so rapidly as to suggest the activation of preformed neuron

networks in the visual system. In most people, susceptibility to phosphenes declines with adolescence, perhaps as a result of progressive superimposition of adaptive-cognitive systems over genetically fixed neural structures. This would attribute to early hominids a high degree of susceptibility to phosphene experiences, which has prompted me (Bednarik 1985) to observe that phosphenes and tracks were the most vivid two-dimensional visual stimuli experienced by early humans. It should also be remembered that spontaneous phosphene experiences are caused by various kinds of stress and deprivations, such as prolonged deprivation of light (consider European cave dwellers during the severe winters of stadial peaks; consider dietary deficiencies, and many other plausible possibilities) or simply of external visual stimuli.

Which brings us to a particularly valuable contribution to the present debate, the Comment by Hamilton-Smith. By introducing the concepts of stimulus-arousal theories Hamilton-Smith not only demonstrates the profound advantages of the *RAR Debates*, he also reminds us that the phenomena under consideration, parietal finger markings, must have a neuropsychological dimension, that there must be a rational way of accounting for them that can in the final analysis be reconciled with all available evidence. He establishes a remarkable correspondence between the ideas of arousal theorists and my evidence. Moreover, he relates how arousal would have survival value, by assisting an organism to be alert and avoid falling victim to a predator. This reminds me of my own reaction to Davis's (1986a) recent theory on the origin of image making (which appeared in print only after Hamilton-Smith wrote his Comment), when I asked myself how one could develop a line of reasoning that would support or disprove the proposed model. Davis's central premise is that the property of iconicity of noniconic features was discovered by accidental visual misinterpretation, and that this discovery led to figurative art. Considering the neural processes involved in visual misinterpretation I realised that they would have favoured objects that dominated the visual system of these hominids. I argued that these would have been the objects that evoked the most profound fears or desires in early man: large mammals. Not only are the earliest iconic lines suggestive of such objects, they seem to depict those aspects of large mammalian species that are likely to be 'imprinted' in a taxonomic visual system.

But in this same context I also arrived at a curious observation: ambiguity of perception would have had a survival value during the Pleistocene. 'In the neuropsychological sense it is the opposite of being deceived by camouflage: whereas the disguise of a predator or quarry, to be effective, requires that the deceived subject perceive an insignificant object in place of a significant one, in the effect considered by Davis a significant (e.g. threatening) object is seen where in fact an insignificant one exists' (Bednarik 1986a). Or more pragmatically: there was a survival value in starting to run every time the silhouette of a cave lion was perceived, even if nine times out of ten it turned out to be just a boulder or a bush. There appears to be a correlation between this concept, and the idea that optimal arousal is a biological imperative related to survival, as Hamilton-Smith states.

It must be recognised that Davis's theory of the origin of image making and mine on the involvement of phosphenes in archaic art are complementary: each demands an explanation for the phenomenon addressed by the other. And while each model is capable of standing alone we find that by involving stimulus-arousal theories we can connect all three sets of ideas and produce *one single theory* that provides a logically consistent albeit still rudimentary explanation for the emergence of art; an explanation that is firmly rooted in neuropsychology, while enjoying the complete support of the evidence that will always have the last word: the record on the rocks. Hence Hamilton-Smith's Comment provides not only cogent support for my interpretation of the finger markings, but also the means for a considerable broadening of our conceptual basis for hypothesising about the origins of art.

Although I retain the accepted usage of the term 'iconic' as defining an externalisation that is modelled on visual perception (i.e., a so-called naturalistic image; for an excellent discussion of 'object-recognisability' refer Davis 1986b), I should clarify that, strictly speaking, motifs depicting phosphenes could also be considered iconic. This interesting argument may well lead to the realisation that all art is ultimately iconic—not a new idea, incidentally. But while it is true that phosphenes are experiences of the visual sense, they are autogenous and subjective, they are incapable of external verification. Moreover, in their depiction verisimilitude is more likely to have been subordinated to principles of design, to conventionalisation, than in the depiction of objects from the physical world. What is often described as abstract or geometric in art should therefore continue to be defined as noniconic.

It should also be mentioned here—although the Phosphene Theory as such gains no support from this—that several authors have proposed a derivation from phosphenes in respect of comparatively recent noniconic rock paintings. Lewis-Williams (1986) has recently attributed noniconic elements in southern African rock art to trance-induced phosphene experiences (and he arrived at his views quite independent of my work); much of the vivid rock art of the Chumash, in the southwestern United States, is suggested to portray hallucinogen-induced entoptic phenomena (Wellman 1981; Hedges 1983); and similar claims come from other parts of North America as well as from Brazil, Colombia and Ecuador (I shall return to this subject after my impending tour of Latin American countries).

Dr Bahn's support for a connection between finger flutings and water symbolism is not surprising in view of his premise of a correlation between 'abnormal water' (mostly thermal or mineral springs) and parietal art sites in the French Pyre-

nees and in part of Cantabria (Bahn 1978). While he has argued rather convincingly in favour of the latter relationship his interpretation clearly does not extend to other regions such as the Dordogne or, for that matter, the U.S.S.R. or Australia. He acknowledges himself (1978: 132) that his hypothesis was only designed to represent a single contributory factor of the art, which is consistent with his rejection generally of concepts of homogeneity and continuity in cave art.

The stream-like appearance of some meandering designs in Palaeolithic art has been noted long ago (e.g. Capitan and Bouyssonie 1924) but interpretation based on modern semantic systems is clearly unacceptable when we deal with archaic rock art which was produced by people that could not possibly have possessed the same systems. To resort to such subjective and indeed ethnocentric expedients results in seemingly logical and reasonable assumptions—the kind that forms the basis of so much of what we 'know' about the cave art of Europe. Marshack's stern warnings against 'reasonableness' in interpretation are most relevant. I have already addressed the question of objective evidence relating to water symbolism (p. 45), without finding any support for this explanation. If we consider the hydrological regimes of the caves in question the statistical evidence becomes even more unfavourable. As I have explained elsewhere most of the finger markings occur in caves that are very close to the surface, for example near the crest of a hill, while water is far more likely to be found in lower-lying caves. The idea of a water symbolism would be far more eligible in the Australian context than in Europe (consider the importance of water sources during the dry and hot oscillations of the late Pleistocene) but nearly all Australian caves with finger lines are features of flat karst plains that may have been drained of their water then, probably even lacking accessible phreatic deposits.

Koonalda Cave is the principal example of a cave containing both finger markings and lakes, yet the latter are in the main passage from which no prehistoric evidence has been reported, while the markings and quarry sites occur together in a different part of this huge cave system. A correlation between the mining and the marking activities suggests itself so persuasively that one is tempted to accept it as an established fact, especially as the finger flutings are also accompanied by silica mining traces in six of the Mount Gambier caves. Yet I maintain that there is no definite evidence suggesting such a connection. Since the case for a connection with water symbolism is very much weaker it obviously does not even approach the standard I set for accepting an interpretation.

Bahn touches upon the subject of a Palaeolithic bear cult, mentioning the work of Koby. Whilst there is clearly no objective support for the 'osteal culture', and the evidence for intentional deposition of bear remains in central Europe is still a contentious point, there can be no question about the charred and smashed cave bear bones that have been recorded at numerous cave hearths, the worked bones and teeth of *Ursus spelaeus*, or the hornfels flake found embedded in the os *frontale* of a cave bear skull in the Rotes Feld Cave, near Trieste. This is an extremely complex subject that simply cannot be debated without citing thirty or forty authors, and I shall restrict myself here to mentioning that most of the evidence seems to come from the Olschewian rather than the Mousterian; if it were not for Schmid's chronological endorsement of Bächler, the cultural affiliations proposed by most other authors (e.g. Zotz, Bayer, Horusitzky, Malez, Brodar, Movius, to name just a few) would be preferable.

Apart from these rather peripheral issues, Bahn still supports the bulk of my paper, especially my general approach. The *complete* approval expressed by Dr Cairns comes as no surprise to me, after all, my paper is a practical application of the concepts he advocates in his preceding paper. But what is particularly gratifying for me is that Cairns clearly appreciates the major philosophical consequences of the 'extremely complex scientific reflection' to which my approach inevitably leads. I know from my discussions with him that he shares my concern about that incongruous, absurd, gaping hiatus that has been allowed to develop, between orthodox science on the one hand and advanced concepts of reality on the other. Cairns appreciates that I am trying to bridge this by probing the origins of anthropocentricity—that shadow that has given humanity the substance of consciousness, but which we now seem unable to jump over . . .

Cairns' enthusiastic approval is as much a great encouragement as is the endorsement of Australia's own pioneer in the field of archaeopsychology, Dr Gallus. Let me make it quite clear how much I owe to his work. It was Gallus's 1977 paper, one of the most important and provocative, and least-understood archaeological articles that ever appeared in Australia which prompted me to take up this subject in earnest. Gallus has been the intellectual *enfant terrible* of Australian archaeology for a couple of decades but now the tide is slowly turning in his favour. Less than a decade ago his endeavour to introduce neuropsychological reasoning into rock art research was met with bewilderment and apprehension, and it has remained largely ignored since then. But recent developments in Australia and abroad fully vindicate his perseverance.

Never running out of surprises, Gallus presents us with new, hitherto unpublished data from Koonalda Cave which corroborate his earlier chronological model. I have asked Professor R. V. S. Wright, whose interpretation is challenged by the new evidence, to respond to the same. But he has so far not reciprocated and it would not be proper to debate Gallus's version until Wright has had adequate opportunity to respond. But it is obvious now that Wright can no longer discount the date of V-82 and that Gallus should not be obliged to reconcile a discrepancy Wright perceives in the chronology. By coincidence, my own interpretation of the Koonalda chronology was only published shortly before the present

debate began (Bednarik 1986b). In it I argued that stratigraphical consistency is lacking in the dates; that all dates are incompatible at one standard deviation; and that Wright's chronological model and other aspects of his report are debatable. I found myself supporting Gallus's construct, and the new evidence is fully consistent with my own, independent interpretation of the site.

As nearly all commentators are largely in agreement with my paper, or have raised only peripheral objections, I am relieved to see that Moore introduces some very fundamental ones. This promises to put my ideas to a severe test but his objections are too easy to rebut to have any real impact, particularly as he contradicts himself frequently. For example, while on the one hand acknowledging the immense age of the finger flutings, and the absolute dilemma of interpreting such a chronologically distant phenomenon, he compares them elsewhere in his comments with recent markings, such as those on wooden objects. Yet there is no more relationship between the finger grooves in Koonalda Cave and the flutings on a Western Desert spear thrower, as there is between the finger grooves in Baume Latrone and the flutings on the Roman columns of the nearby Nimes arena. Today's Aborigines are as remote from their Pleistocene forebears as today's French are from the Palaeolithic cave artists. To think otherwise would be to deny that the Aborigines experienced spiritual, intellectual and cultural evolution.

If making marks on soft cave walls were a 'natural instinct' of man, as Moore suggests it is, how could we reconcile this view with the assumption he makes in his immediately preceding paragraph: that all finger flutings were made by fully modern humans? If a behavioural characteristic were restricted to extant hominids, how could we simply attribute it to 'instinct'? If it was not present in *Homo erectus* then it is clearly an acquired characteristic, and perhaps the principal issue in my paper concerns the very question: how, by what means or processes, was this characteristic acquired? Were it not for this one question the study of archaic rock markings would be practically a waste of time. If we explained these finger markings as the predictable result of a natural instinct we would have to say the same about other rock art or, for that matter, any culturally acquired behaviour. And we would have to abandon scientific inquiry.

Actually both of Moore's contradictory assumptions are probably misconceptions. On the one hand there is a fair chance that our not quite modern Neanderthal cousins also indulged in the pleasures of finger marking. On the other, the concept of a universal tendency has already been addressed by me (refer p. 39 of my paper).

Although I have not implied anything to the contrary, Moore emphasises that only modern hominids have inhabited Australia. While essentially a correct statement it should be remembered that considerable dimorphism existed among Pleistocene Australians. Grazile and robust colonisers coexisted for tens of millennia—both members of the same species of the genus *Homo*. Yet one recalls R. White's (1986) recent observation on anatomical differences among hominids, and how our perception of them is determined by preconceived ideas. Besides, evolution is a constant process, species do not emerge overnight, and Pleistocene Australians were no less phylogenetically dynamic than other populations.

There is more in Moore's Comment that I disagree with. The 'elementary incised spirals' in Koonalda he mentions are in fact two sets of four curvilinear markings which suggest concentric circles. Indeed, I would find it most exciting if the spiral motif was to appear in Australian cave art. Moore suggests that stone implement manufacture necessitates language; another completely unsubstantiated argument favoured by some archaeologists. Then he propounds the currently fashionable communication model of prehistoric art, which will probably lose some of its appeal now that Marshack (1986) and Lewis-Williams (1986) have so effectively dismantled it. In any case, why expect finger lines to have any communicative content when their production is explained as the result of a human instinct to make such marks?

Again demonstrating his ambivalence, Moore disputes that all finger flutings represent similar patterns of activity. If they were as reflexive or instinctive as he thinks, why should they possess more differentiation than the cave bear marks I mentioned above? Actually, Moore's concern is shared by Davis, but I think both assume that I referred to the activities *associated with* the activity of marking the wall. I should emphasise that I referred purely to the movements of hands and fingers, and certain conscious or nonconscious strategies of relating these movements to existing physical constraints or latitudes. The extant physical traces of these activities permit us to recognise patterns at various levels, and these can be compared between sites and regions. I have found that differences between French and Australian sites are well within the range of intrasite variations in each region. The similarity of the two traditions is not limited to morphological correspondence, I attach more significance to contextual analogies of which there are several.

A fundamental difference in outlook probably accounts for my inability to agree with many of Moore's comments: I cannot reconcile an ethnographic perspective or a search for meaning with an analytical assessment of very ancient art. Such an approach, I believe, can only be valid where one deals with comparatively recent art (Bruce Wright's 1985 paper in an earlier volume of this journal is a superb example of the convincing utilisation of ethnography in a rock art study). I fear I would probably consider as speculation much of what Moore would describe as scientific examination, and *vice versa*.

Moore would like to see statistical methods applied to finger flutings. I have collected statistical information at most of the sites investigated and while this has resulted in the accumulation of vast amounts of metrical and other data it has facilitated little progress, as I stated in my paper. Realising the futility of the statistical

approach, I adopted what Faulstich describes as an approach of creative and deductive reasoning.

I can completely agree with Moore concerning the prospects of finding similar finger markings elsewhere in the world, a point also raised by Hamilton-Smith. Having for some time considered the potentially worthwhile regions (e.g. India and the Near East) I agree with Hamilton-Smith that the eligibility of an area can be theoretically determined; geographic, geological and climatic factors would have to be considered.

Dr Clottes clarifies a few specific points in my paper, and I am particularly grateful to him for demonstrating that Pleistocene footprints are more common in European caves than I had suggested. Indeed, Clottes himself has summarised the subject only recently (Clottes 1985). However, to consider them as 'perfectly preserved' I would expect to find impressions of dermal ridges in such human tracks—as I have observed them even on a track claimed to be that of a 'sasquatch', in the forest soil of western Canada (which I must assume was a fake). Most surviving Pleistocene footprints are quite poorly preserved (e.g. Clottes and Simonnet 1972: Figs 2, 3, 5, 8, 9; refer also other publications by Clottes), examples permitting clear identification of toes (or finger tips) as in Fontanet remain rare (Delteil *et al.* 1972: Figs 1-3; also Clottes 1980: Figs 2-5). Finger markings such as those I have described occur in about four times as many caves as footprints, and in vastly larger numbers when in fact they should be outnumbered many times. The point I was trying to emphasise in my paper is the long term mechanical resistance of desiccated or calcified *Montmilch* (Bednarik, in press b) which could only be produced in clay by firing it.

I intend to address the dating problem, briefly touched on by Clottes, in a different context at the first opportunity. Suffice it to note here that some French 'dating' of cave art would not be accepted in Australian rock art research; for instance, an iconographic similarity of wall art and portable art is certainly no proof for contemporaneity. The entire chronology of Europe's Palaeolithic art might have to be subjected to precisely the same stringent reassessment as that Bahn demands for aspects of interpretation (see article in this issue), to separate fact from myth. It is clear from my paper that I would not regard finger markings associated with iconic elements as being among 'the early ones', as Clottes assumes them to be; they are not of the digital fluting type and may be considerably younger.

Clottes' Comment provides also a response to one of Moore's points, when he corroborates my view that there are different conventions of finger markings in French caves, adding that those found on floors are different from the wall markings I described: they were made with only one finger. Clottes mentions another set of only two fingers, and I have listed the use of less than four fingers as one of the characteristics of macaronis (as distinct from digital flutings). Therefore it is obviously not 'natural', as Moore thinks, to use all four fingers. Clottes then mentions the occurrence of finger markings in post-Aurignacian contexts (Altamira could be added) but these are always of the type for which I have retained the term 'macaronis', which actually comprise figurative depictions in some cases (p. 36). A variety of other finger markings to be found in caves includes even some modern ones, e.g. in Rouffignac (of the 'trailing' type, as in Réseau Clastres; conversely, the Réseau appears to be part of the Niaux cave system, and whilst Clottes has consistently maintained that the two caves are separate other scholars do not agree with him on that point). Modern finger markings exist also in five Australian caves. All these markings differ from the earliest type, and they are as a rule easy to distinguish.

Clottes' conjecture about an 'instinctive' or deliberate desire to leave a trace of one's passage raises interesting possibilities. Visitors may find complex cave systems quite intimidating, which could have been also the experience of Palaeolithic people. They would have memorised natural features along their paths for orientation, and perhaps they added artificial ones to add texture that could be memorised. A tendency to appreciate the reassuring effect of such artificial markings would lead to more markings, and their function would be to appease the human longing for feeling safe, or the mark makers' desire to assert themselves in a threatening environment, as Clottes suggests. This explanation appears most plausible and provides a very convincing psychological model of motivation. However, it finds no support in the evidence: the distribution of the flutings within the caves fails to corroborate this idea in nearly all cases. And by assuming that the Palaeolithic visitors found the interior of deep caves 'alien and hostile' Clottes advances an ethnocentric premise that would be hard to substantiate. The evidence tends to support the opposite view—that these people felt quite at home in the caves; they frequented them regularly in both Europe and Australia, and in some cases established their camps hundreds of metres underground. The potential interpretation provided by Hamilton-Smith is far more persuasive: not the assertion of humans in an alien environment, but the assertion of the exploring, stimulus-seeking human mind.

Faulstich loses no time over questions of semantic significance, ignores all the peripheral issues, and immediately addresses the principal topic: the developmental processes fossilised in archaic rock art. But what makes his Comment particularly relevant from the reader's point of view is that he is the only one of the reviewers who examined any of the caves at Mount Gambier prior to writing his Comment (Bahn has also studied several of them—but this was *after* writing his review). Faulstich does not even mention that he inspected eight of the caves (having travelled half way around the world to do so) a few weeks before writing his response. Such restraint should make his remarks doubly valuable

and we have every reason to contemplate his observations with the consideration they deserve.

Faulstich accepts without hesitation the notion that the finger lines can be related to a crucial stage in the intellectual development of hominids, and he proceeds to speculate about the possible role of this art in cognitive evolution. His arguments contrasting a human order with a natural order, the function of art in this framework, and the psychological effects of acquiring new levels of control are certainly valid in our own time; it would be rash not to grant them the same validity in the context of hypothesising about the earliest art. Faulstich's consideration of art as the determinant of the 'human order' reminds one of the continuing inability of empiricism to define art satisfactorily, and at the same time of the logical explanation for this dilemma.

Like Bahn, Faulstich favours animal claw marks in the caves as the factor that initially prompted finger markings, presumably because he has seen how very numerous they are at Mount Gambier and how frequently they occur together with anthropic wall markings. It is most instructive to review the potential role of natural scratches in the light of the stimulus-arousal theories cited by Hamilton-Smith, because these would appear to support the idea of an initial visual impetus in developing conscious mark making.

Faulstich's meaningful Comment conveys a fundamental agreement with my central proposition, of a nexus between art, archaic rock markings, the origins of advanced intellect and conscious perception, and the intellectual impotence of empiricism when forced to deal with questions of its own origins. His ideas about the direction future analysis of the markings should take are derived from his first-hand experience; I agree with them completely. In fact Faulstich is too modest to mention that progress has already been made with his help and that soundly-based expansion of our concepts is proceeding. New developments are taking place in two areas: Marshack-style internal analysis of minute, but telling details; and the evolution of a theory of marking strategies that may have to be considered in the area of the origins of art. I expect that it will be possible to present convincing evidence that would explain how the concept of 'single motif' was acquired and how this might have prompted the comprehension of a reality composed of elements (objects), a reality that can be analysed into its basic parts and that lends itself to classification.

I had not attributed as much importance to my paper as the reviewers evidently have done, regarding it merely as a 'state of the art' report about the work on ancient cave markings. In considering this phenomenon I cited my Phosphene Theory but this was only a side issue—the finger flutings alone would certainly not provide adequate support for it. The paper's purpose was really to assemble a solid base from which we could explore further, and the ensuing debate shows how well it has served its purpose. It is gratifying that the reviewers recognised the important issues raised and largely ignored the trivial ones. I thank them all for contributing so much to the progress of my work.

Robert G. Bednarik
AURA
P. O. Box 216
Caulfield South, Vic. 3162
Australia

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Rock Art Research 1986, Vol. 3, No. 1, pp. 3-19.

IDENTIFYING HIGH-RISK VISITORS AT ABORIGINAL ART SITES IN AUSTRALIA

Fay Gale and Jane Jacobs

FURTHER COMMENTS

By JACQUES BRUNET and PIERRE VIDAL
[Translated from the French by Colette Mrowa.]

Both the article by Fay Gale entitled 'Monitoring visitor behaviour at rock art sites' and that by Fay Gale and Jane Jacobs entitled 'Identifying high-risk visitors at Aboriginal art sites in Australia' visualise the problems of visitation at open air rock art sites.

The rapid increase in the number of tourists visiting these sites must make us fear the disastrous consequences for their conservation. This subject is unfortunately not specific to Australia, it exists as well for rock art in North Africa and in Europe—in Spain and France in particular for underground prehistoric art sites. Whereas, for an open air site, there is, in principle, more 'freedom' to organise a visit, or a guided tour, it is the opposite for decorated caves where morphology is a constraint and the layout of the galleries is quite incompatible with preserving the aesthetic surrounding of rock art and maintaining stable in situ climatic conditions.

Fay Gale's inquiry, which has been aptly led in co-operation with students from the Geography Department of Adelaide University, rightly stresses the variability of visitor behaviour. In particular, they show how the morphology of sites and the localisation of rock drawings influence visitor behaviour.

In France, whenever it was possible, we undertook to adapt the flow of tourist visits to the morphological and climatic features of the caves, while keeping in mind the needs of conservation. However, a lot remains to be done within this field in agreement with the owners, the managers, and the people in charge of decorated caves.

In France, for caves open to public visits, the problem is to reconcile the requirements of conservation while maintaining access to tourist visits. A solution is required that would take these extremes into consideration and satisfy them both.

We have noticed, while studying caves, that the quality of the guide's comments suffers if the group exceeds twenty-five people. Above that number, members of the group tend to spread over several metres, and it is not possible to constantly keep an eye on larger groups because of cave morphology. No guide who is at the head of a group, no matter how competent he is, can effectively control what happens at the end of the queue. This has been our experience in Dordogne, a region of very well known caves.

For some years a visiting system has been set up at Font de Gaume cave in Eyzies and it gives satisfaction from both points of view, conservation and tourist access. The cave is shaped as an average size corridor no larger than 2.5 metres in the decorated part. The problem has been solved by limiting groups to twenty people plus a guide and a warden. The warden makes sure that different groups do not mix in the cave and that visitors do not touch the decorated surfaces.

For any cave it is desirable to encourage the spread of visits over the whole year, and to balance morning and afternoon visits over the day, even if this distribution goes against the preferences of the tourists. A number of studies have been carried out over several years to determine the thermal variations of air and rock, of air circulation and of the level of carbon dioxide.

At Font de Gaume visitor numbers are kept within these limits at any one time. These visiting conditions have been distributed to local tourist offices, hotels and restaurants; at the entrance of the cave, a sign post explains the reasons which limit the frequency of visits. During the tourist season (holiday, weekend) visits are arranged by appointment, i.e. each morning, visitors can make a 'booking' for the visit of their choice during the day, as far as there is room available. At the time of buying tickets, the cashier divides visitors into different groups (she keeps up-to-date files of group numbers)

and writes down the time of the visit on the back of each ticket. This system is working well and, even if this seems extraordinary, there are practically no cheats!

The opening hours are from 9 a.m. to 12 p.m. and from 2 p.m. to 6 p.m. (in summer). A group leaves every twenty minutes, with a maximum of twenty adults plus a guide and a warden. Each visit lasts for forty minutes. They are evenly spread and leave regularly at a set time, with seven visits in the morning and ten in the afternoon. Four guides plus four wardens ensure the service operates effectively. At the end of the day up to 340 adult tourists and thirty-four other people (guides and wardens) have gone through the cave, and there is a margin of about twenty-six people made up of children under fourteen who cannot be separated from their family during the visit.

In France, observations on visitors' behaviour and 'psychology' have not been carried out as far as those undertaken by Fay Gale on the sites of Kakadu and Uluru. Let us mention, however, the valuable job of explanation done by the guides and wardens in order to make visitors aware of rock art conservation and the actions taken under Michel Lorblanchet, from 1981 to 1985, within the A. Lemozi Museum, associated with Pech Merle cave, for groups of school children.

However, at prehistoric art sites elsewhere in France and Spain, the public is ill-informed of the dangers which can be caused by visitors of open air and underground rock art sites. From now on, an action of public information should be envisaged through the existing structures such as the museums and the media to extend education and develop protection in areas of high visitation.

Jacques Brunet
and Pierre Vidal
Section 'Grottes ornées'
Laboratoire de Recherche des Monuments Historiques
Ministère de La Culture
Château de Champs-sur-Marne
29, Rue de Paris
77420 Champs-sur-Marne
France

By JOSEPHINE FLOOD

This paper complements the paper by Fay Gale on 'Monitoring visitor behaviour at rock art sites' in the November 1985 edition of *Rock Art Research*, so it may be appropriate to comment here on both articles. The first paper deals primarily with the methodology of this study of visitor behaviour and describes the background to its inception. Since the context of the study is largely omitted from the second paper it is advisable that the two articles are read together.

The study was undertaken at the initiative of the Committee for the Protection of Prehistoric Places of the Joint Academies, funded by a National Estate grant. Its basic aim was an analysis of visitor behaviour at sites, which it was hoped would lead to the formulation of some general principles of site management. A pilot study was undertaken in 1982 at Kakadu National Park and further funding was provided by the Australian National Parks and Wildlife Service and the Australian Heritage Commission to continue the work at Kakadu and extend it to Uluru (Ayers Rock).

Substantial results have been produced by this study. Apart from the two papers in *RAR*, a most useful paper on 'The protection of Aboriginal rock art from tourists at Ubirr, Kakadu National Park' was published in 1984 by Gale in a volume on *Visitors to Aboriginal Sites: Access, Control and Management* (ed. H. Sullivan), published by the Australian National Parks and Wildlife Service (PO Box 636, Canberra City, 2601; refer Gunn's review, *RAR* 1: 145-7).

The data presented in these various papers has been collected together by Gale and Jacobs in an overview volume on *Visitor Management for Heritage Sites*, to be published by the Australian Heritage Commission early in 1987. This book deals with the following topics:

- Increasing tourism and the problems of promotion
- The impact of visitors on heritage sites
- Techniques for measuring visitor pressure
- Defining the nature and scale of visitor pressure
- Assessing the behaviour and attitudes of visitors
- An overview and evaluation of visitor management strategies for cultural heritage sites.

In the book's (provisional) title the term 'heritage' rather than 'rock art' sites is used, because although the experimental data are all derived from Aboriginal art sites, many of the issues discussed and solutions proposed have wider application. The basic principles which can be drawn from this work are applicable to all heritage sites, whether Aboriginal, non-Aboriginal or natural features.

This study by Gale and Jacobs will therefore be of immense value to all cultural and natural resource managers. I draw attention to the forthcoming book here to make clear that the various threads of the project have now been drawn together and a synthesis produced. This overview serves two major purposes: to disseminate the methodology developed during the project so that the various agents responsible for managing sites can undertake visitor surveys as they see fit, and to enunciate general principles for the protection of heritage sites in general and rock art in particular.

The fact that such general principles have been developed is important, because this is not made clear in the article in *RAR* on 'Identifying high-risk visitors at Aboriginal art sites in Australia'. Whilst recognising that the article describes a particular case study, I was initially rather disappointed that the authors had not attempted to draw the general from the particular in their conclusions, but instead had strongly emphasised the variations inherent in all factors that have bearing on the management of such sites. Whilst it is true that the visitor population is variable and changes over time, that the vulnerability

of sites to different kinds of visitor pressure is variable, that each site is different and must be viewed as a new management problem, and that regular re-evaluation of visitor patterns is necessary, the existence of certain general principles should perhaps have been mentioned, if only to give a small ray of hope to cultural resource managers . . .

During the last decade there has been a great deal of emphasis in Australia on the difficulties of rock art conservation and the lack of any general technical panaceas or management prescriptions. Whilst this cautious approach has undoubtedly had a good effect in many ways, it has also perhaps led to some negative attitudes towards rock art conservation. Thus three separate applications for a rock art research centre to be established for Australia's Bicentenary have all been unsuccessful. However, possibly the tide is now turning. In 1986 the Minister for Aboriginal Affairs has given a substantial sum to the Australian Institute of Aboriginal Studies for the preservation of rock art, and the Australian Heritage Commission, Department of Aboriginal Affairs, Film Australia and the Northern Territory Government have cofunded a film - *The Land of the Lightning Brothers* - on rock art and its protection. In addition, the Australian National Parks and Wildlife Service has been active in training Aboriginal rangers, and in the conservation of art in Kakadu and Uluru, and various rock art projects are taking place in almost every State. As well as trends such as the increasing interest in rock art and involvement of Aboriginal people in its protection, there is a definite move among site managers towards adopting simple practical protective measures rather than seeking a technical cure-all such as the magic spray-on fixative. It is in this context that the work of Gale and Jacobs is of outstanding value and relevance, not only in Australia but also in the international forum.

Dr Josephine Flood
Australian Heritage Commission
P. O. Box 1567
Canberra City, A.C.T. 2601
Australia

By HOWARD P. McNICKLE

The recent article by Gale and Jacobs (1986) and the reviewers' comments have drawn attention to the issue of whether or not additional rock art sites in Australia should be made accessible to public inspection. The research carried out by Gale and Jacobs is most important in that it has given insight into the behaviour patterns of different categories of visitors under a variety of circumstances. However, another important criterion which did not appear to be mentioned either in the article, nor by the reviewers is the question of the motive of the visitors to the various rock art sites. At any public site, it can be expected that a certain percentage of visitors will be visiting the site specifically to view the art, while a percentage will have been brought to the area for other reasons and to them the art sites are only a minor feature, close to more important attractions. In the case of Uluru it is reasonable to assume that the great majority of visitors will have been attracted by the spectacle of Ayers Rock itself and the desire to climb and photograph it, while the presence of painting sites will be a minor and perhaps unexpected attraction. Many of the visitors will no doubt have been quite unaware of the presence of rock art sites until their arrival at the Rock. Very few of these visitors are likely to have travelled the long distance if the painting sites had been the sole attraction. In the case of Kakadu, however, the rock art is undoubtedly the main attraction for visitors to these sites and this, I believe is the main reason behind the more responsible attitude in visitor behaviour observed at Kakadu.

One of the disturbing aspects of the Gale/Jacobs survey is the number of tour guides, especially at Uluru, who displayed a less than responsible attitude toward the sites to which they led their tour groups. Having spent several years as a tourist guide in New Zealand, I can verify the observation that there is little or no organised system of training, licensing or supervising tourist guides and that tour operators and coach drivers are often selected for reasons other than their ability to lead, control and set an example to the public both in New Zealand and Australia. In such circumstances, the responsibility for visitor control rests with the park rangers. One positive move would be to encourage or even make compulsory, an initial visit to park headquarters by all visitors for an illustrated lecture prior to visits to the various attractions. By this means, visitors can be directed and introduced to sites of particular interest and in the case of rock art sites, the important heritage value can be explained to those previously unaware of their existence or significance. Such previsit introduction sessions have a very strong impact upon visitor behaviour, especially with groups of children (as observed by Gale and Jacobs) and are probably even more effective than warning signs at the sites. In the case of visits to sites by tour parties, in the absence of responsible leadership, this should be carried out by park rangers.

In Australia, when one looks at the overall distribution of rock art sites recorded and the distribution of sites open to the public, some interesting patterns are apparent. Whereas rock art is highly concentrated in the far northern regions, the location of public sites is far greater in the densely populated southeastern regions of the continent and this pattern is also reflected by the various site recording programs undertaken thus far. In New South Wales, Victoria and South Australia, and also at Uluru and in the Alice Springs area, the sites opened to the public are often the most important ones and in some cases the only sites to have been recorded within a large region. Within regions such as the Grampians in Victoria, which are close to large centres of population and subject to unsupervised visits, it has obviously been essential to fence off painted

surfaces for protection. However, other important sites as diverse as the engravings at West Head, north of Sydney and the paintings at Emily Gap near Alice Springs probably receive an even heavier traffic of unsupervised visits with little or no protection and would appear to be at considerable risk. As Walsh has pointed out, many visitors to the southern rock art sites may be disappointed, especially if they have put in considerable efforts to reach a site, expecting to find a gallery of artistic splendour matching that which can be found in the far north, and there is always the possibility that such frustrations may be taken out on the rock art itself, which the disappointed visitor regards as mere graffiti. This is probably a further reason why visitors to Kakadu sites, which present galleries of easily identifiable figures such as fish, animal and human figures, show a greater respect for the rock art than do visitors to Uluru sites, where the paintings are mostly of abstract designs, lines and circles. However, the relative value of any rock art site must be viewed within the context of the region in which it is located and for this reason, the Uluru sites, being very rare examples within a large region, of a particular style, should most certainly be protected. The loss of all the art sites around the base of Ayers Rock would no doubt be a greater loss both to rock art specialists and to the public in general, than would a similar number of sites of highly figurative but far more common motifs in the Pilbara or Arnhem Land regions. However, it would be highly undesirable to erect Grampian type cages in the Uluru shelters as such artificial structures would enormously detract from the natural aspect of the Rock as a whole. Even warning signs look conspicuously out of place, but as these sites are now probably the most frequently visited rock art sites in Australia, visitor behaviour will be a continuing problem.

In spite of having the above reservations about sites already open to the public, I am strongly in favour of making further selected sites in other Australian rock art regions available for public inspection. Having visited all major Australian rock art regions, including virtually all public sites (and having carried out detailed recording in the Pilbara and Victoria River District) I believe that there is much to be gained for rock art research by increasing public awareness of the heritage of rock art within Australia. B. K. Swartz, in his comments, takes a somewhat pessimistic view that public access to rock art sites should not be encouraged as this will invariably lead to vandalism and the probable destruction of rock art sites within a century. Whereas in the United States, where the number of rock art sites is relatively few and the population large, there may be some truth in such a statement, in Australia the situation is quite different. Whereas in the southeast the situation may approximate that in the United States, in the far northern regions of Australia, tens of thousands of important sites are to be found in regions almost completely uninhabited, the great majority of these sites being as yet unrecorded and many are within regions as yet almost unexplored. Since most of these regions lie within the tropical monsoon belt, paintings at these sites are under constant attack from the effects of rain, high humidity, growth of moss and lichen, insect and bird nests, and in many cases they suffer from damage by both native and introduced animals. In the case of painting sites, the quantity of rock art destroyed by human activities within European history throughout Australia would be negligible in comparison to that destroyed by natural processes. Therefore, the major threat to rock art in Australia remains that of natural deterioration rather than vandalism.

I certainly agree with Swartz that extensive and intensive recording programs should be carried out, especially in the badly neglected northern regions, but at the same time I believe that public sites and studies such as that carried out by Gale and Jacobs can also play an important part. To date, rock art recording in Australia has been poorly funded and carried out very much on an *ad hoc* basis and as a result, the southern rock art regions located within reasonable distance of large population centres and institutions have been highly favoured in comparison to the far more prolific but extremely remote northern regions. I believe that one method of making the various funding bodies aware of the need for recording programs to be carried out in these remote regions is to increase public awareness of rock art by opening a number of further sites in these areas. The sites to be made public need not be the most important sites within a region, as has often been the case. In the Victoria River District of the Northern Territory (the area between Katherine and the Western Australian border), sites have recently been opened to the public in the Keep River National Park and are expected to be opened in the newly created Gregory National Park. After having carried out recording in this region during 1984 and 1985, I was asked for suggestions on sites which could be considered for public access (with full consultation with traditional custodians), by Conservation Commission officers. My suggestions were that, if unsupervised visits were to be permitted to sites, then less important sites should be selected for development, whereas if the more significant sites are made public, visitor groups should be strictly supervised. Such supervised tours of rock art sites, run on similar lines to cave tours, would not only protect sites but could also provide in-depth interpretation.

Public sites, with safeguards similar to those outlined above, could provide tourist attractions in remote areas of the north which at present provide only isolated features of interest separated by vast distances. Western Australia contains probably the most varied selection of rock art and also the greatest number of sites (principally in the Pilbara and Kimberley regions) and yet not a single site in this State has been officially created a public site as yet, even though the location of a few sites can generally be obtained by those who make enquiries. The general policy has been to discourage publication of site locations,

even in areas such as national parks. I believe that to create three or four public sites in the Pilbara and Kimberley in full consultation and involvement with Aboriginal custodians can only enhance public awareness and appreciation of Aboriginal culture and benefit the inhabitants of the regions involved. The creation of even half a dozen such sites would amount to only a fraction of one percent of the number of sites known to exist [1]).

The results of studies such as those carried out by Gale and Jacobs could be most profitably used in the selection of the sites that could be most safely opened. Such studies can ascertain the average public reaction to the presence of rock art from different categories of people under different circumstances but can probably do little to predict the most extreme behaviour of those who may visit isolated and unprotected sites. On my first visit to Ubirr in 1981, before barriers and warning signs had been erected, I was surprised to see little evidence of vandalism, at such a well known and frequently visited site. However, such optimism was somewhat dented when on visiting the depths of Koonalda Cave (a site which could hardly be described as easily and frequently visited) to find several quite recent dates and names engraved onto the soft surface over the finger flutings. Could this perhaps be due once again to the differing views of what people consider to be worthwhile rock art? In Western Australia, if one discounts the wholesale destruction that occurred to engraving complexes in the early years of the iron ore industry, instances of vandalism to sites quite close to mining towns have not been particularly severe. A few names and dates are found, more commonly upon unengraved rocks near to the sites than across the art itself. One unfortunate practice, however, has been that of photographers to abrade faded outlines in order to 'touch up' engravings. However, even these habits seem to have diminished in recent years in areas such as Dampier, where warning signs have been erected at sites and mining companies have become increasingly environment conscious.

Perhaps the most unusual example of extreme behaviour I have observed would be on my last visit to Walga Rock in the upper Murchison, Western Australia. Not publicly advertised but well known locally, this site would be the nearest to a public site in Western Australia. The paintings are located in a large but shallow recess at the base of the rock, a granite dome somewhat akin to Uluru. Paintings include superimpositions, geometric forms, simple anthropomorphs, hand stencils and what appears to be a ship, some 350 kilometres from the sea. The rock art, protected only by a warning sign, did not appear to be vandalised, but somebody had gone to the trouble of carrying tins of paint to the summit and had decorated several boulders there with pictures of colourful concentric circles, hand outlines, boats and even a scene with trees!

Some of the instances of vandalism in Australia could almost be tourist attractions in their own right.

[1]) The number of rock art sites in Australia remains unknown but it is believed to be well in excess of 100 000 (ed.).

Howard P. McNickle
P.O. Box 1386
Rotorua
New Zealand

REPLY

By FAY GALE and JANE JACOBS

The three sets of comments provide varying perspectives on our recent articles and certainly keep the debate alive which seems to be very important in a new and emerging area of research.

Brunet and Vidal provide a European perspective showing clearly that problems of visitor management are not new even if they have only recently been taken seriously in Australia. The work of Brunet and Vidal reflects a long term commitment in southern France to the forms of management which not only protect the sites but which also actually encourage controlled tourism and education. These researchers appreciate the fact that superficial generalisations leading to quick, unresearched measures may in the long run be more destructive and certainly more expensive than a slower, scientific approach.

We have found that considerable variation occurs from one art site to the next and that protection against wilful or inadvertent damage by tourists needs to be planned to suit the specific site in question. The research by Brunet and Vidal has shown that the morphology of the sites and the actual location of the art within the particular layout of an area greatly influence how visitors behave at any one site. This exactly concurs with our observations for rock shelters in Australia and illustrates the comparability of our problems in quite different situations. It also illustrates the value of this kind of open debate and sharing of research experiences.

McNickle, speaking from a vantage point of considerable practical experience, makes very useful comments. He clearly understands the situation in relation to tourist guides in Australia and the need to develop appropriate controls and educational programs for these people. The fact that we have found that comparable programs were successful at Kakadu but not at Uluru illustrates the importance of studying the behaviour of tourist guides at particular sites before determining overall guidelines.

It is a well-worn cliché to say that Australia is a vast continent of enormous variety but we do have to come to grips with this fact if we are to allow controlled tourism at rock art sites whilst at the same time endeavouring to protect them. McNickle appreciates this complexity when he points to the diversity of situations and protective processes. He refers to the engravings at West Head, north of Sydney, as an area of very heavy 'traffic of unsupervised visits with little or no protection'. A similar statement could be made of the engraving sites in the Flinders Ranges in South Australia. The areas of heaviest

traffic do seem to be least protected in many cases perhaps partly because the requirements of these sites are very different from those of northern Australia where more resources have been allocated for art protection.

The intention of these particular articles was not to present broad generalisations. As our body of data is so extensive we saw our articles in *Rock Art Research* as an appropriate way of publishing some of our more explicit findings which have been the basis for broader statements made elsewhere and which are clearly enunciated in the forthcoming book to which Flood refers. This book follows on from these and other articles to elucidate the principles we have established in the area of rock art protection.

Flood is concerned that the articles refer only to specific case studies and do not offer what she describes as a 'ray of hope to cultural resource managers'. We anticipate that the forthcoming book will answer some of the key problems. It is clear that there are universals which can be applied but it is also clear that wholesale adoption of methodology or management procedures would be extremely dangerous in a country as rich and diverse as Australia.

This research on visitor management has been extremely difficult because we have been breaking new ground but the need for the work is evident from the enormous support and encouragement we have received from experienced people in Australia and overseas.

Professor Fay Gale
and Jane Jacobs
Department of Geography
University of Adelaide
Box 498, G.P.O.
Adelaide, S.A. 5001
Australia

Rock Art Research 1985, Vol. 2, No. 2, pp. 95-111;
1986, Vol. 3, No. 1, pp. 62-82.

THEORETICAL CONCEPTS THAT LEAD TO NEW ANALYTIC METHODS, MODES OF INQUIRY AND CLASSES OF DATA

Alexander Marshack

FURTHER COMMENT

By JEAN CLOTTES

As A. Marshack has pointed out, evidence of reuse and renewal does exist in cave art. This has long been known, and in particular Count Bégouën, more than half a century ago, several times insisted on the repeated renewal of a feline's head in the cave of Les Trois Frères, and on the number of dart-like markings on its body, which he interpreted as a proof that several ceremonies and magic killings of a dangerous feline had been necessary (Bégouën 1924: 427-9, 1929: 12-3, 1936: 19).

This feline, which Marshack cites in his paper (Marshack's Fig. 16), is not such a good example after all. It was called a lioness by Breuil, probably because of the absence of a mane (even though nobody knows whether Palaeolithic felines possessed a mane or not: Pales 1969: 58-60), and also because of the presence of a smaller feline interpreted as a cub. In fact the animal's sex remains undetermined. Count Bégouën himself used to be more cautious than abbé Breuil and called it *'un grand félin'*. The so-called vulvae were never mentioned by Breuil or Bégouën, probably because with a lioness seen in profile the vulva could not be seen, or even more because the small curved lines which Marshack identifies as such are not sufficiently explicit. It has even been argued that they could represent the scrotum, which would then characterise a *male* lion (Vialou 1981: 287). In fact, Breuil's tracing of the hind part of the lion is not accurate: the upper hook-like 'vulva' is merely a partial doubling of the broad line which outlines the buttocks of the animal, and the three small curved lines underneath are actually much straighter and simply join the leg of the lion to what was supposed to be a second tail; it is just not possible to identify them as an anatomic feature.

More importantly still, the renewal of the tail is a myth, whose foundation is a crude mistake Breuil made when he traced the animal (or which was made when his tracing was redrawn prior to publication, more than twenty years after it had been done). The rendition of the 'second tail' is quite different from the original: it stops at the level of the three small lines mentioned above and does not join the genuine tail; its upper outline consists of three lines and not of a series of small hatchings which, on Breuil's drawing, look like hair; worse, the so-called tail is given a graceful curve which makes it look indeed like a tail, whereas it is a perfectly naturalistic human arm, with a sharp elbow, the hand (drawn in a typically Magdalenian way) extended towards the real tail of the lion.

This was obvious from the excellent photographs published in Breuil's syntheses (1952: Fig. 120; Bégouën and Breuil 1958: Pl. VIII). In the latter, the difference with Breuil's tracing is all the more evident as the two are published side by side. It is rather extraordinary, and a tribute to Breuil's influence, that nobody ever noticed the discrepancy and that Breuil's drawing went unchallenged, except by Count Robert Bégouën who queried it a number of years ago, and who, ever since, has always shown the human arm next to the feline to the numerous prehistorians whom he has led through Les Trois Frères in the past twenty years (Bégouën and Clottes 1984: 405). However, the identification of the 'second tail' as a human arm by Bégouën was mentioned by M. Rousseau, who discarded it in an offhand manner (1967: 102); it was taken up lately in a thesis on Magdalenian art in the

Figure 1.
Detail of the engraved and painted lion figure in the cave of Trois Frères, France; compare the rendition by Bégouën and Breuil, Fig. 16 in the paper by Marshack. The 'second tail' in fact resembles a human arm and hand.

Ariège (Vialou 1981: 287).

As to the renewal of the feline's head, it has always been presented as a fact, when it is no more than an assumption, and a debatable one at that. The lion was published with three heads next to each other. Now, the left one was made up by Breuil with the line of the left front leg which extends to the shoulder; moreover, it has no ear, and what looks like an eye on Breuil's drawing is not engraved but is just one of the numerous marks left by the violent blows the image received around the head and shoulder.

This leaves the lion with *one* realistic head (the middle one on the drawing), under which another head has been engraved. Is the latter a renewal of the first one as has been supposed, or has it a different meaning? We have no ways of knowing for sure, but we may point out that another head was engraved under the belly of the lion, and in this case it indicates obviously not a reuse; therefore, one could argue that next to a realistic feline were engraved separate heads, a human arm, a bird, a lion cub etc., all those elements playing their part in the symbolic composition of the panel.

This comment is not meant as an aspersion on Marshack's paper after so many distinguished specialists have commented upon it and the author has given his response, but it aims to rectify some errors, which, owing to the celebrity of Les Trois Frères and its feline, as well as to the notoriety of Breuil, are repeated again and again, when the indisputable presence of a human arm, the absence of vulvae and any sexual characteristics, and the reasonable doubt one may entertain as to the renewal of the lion's head, ask for a different interpretation altogether.

Dr Jean Clottes
11, rue du Fourcat
09000 Foix
France

REPLY

By ALEXANDER MARSHACK

I wish to thank Clottes for strongly making a point that I have made repeatedly for at least some twenty years: that one cannot use the traditionally published Upper Palaeolithic imagery for analytical or interpretive purposes. I have most recently used this argument in discussing the work of Bahn and Whitney Davis, and the work of Mundkur in his articles in *Current Anthropology*. That I used Breuil's rendition, knowing the dangers, makes the point even more strongly. A large part of my own research has, in fact, involved a careful re-examination of the materials that Breuil and others had published during the twentieth century. I had myself found, and published, voluminous errors of observation or rendition.

Re-examination of the lion panel and correction of Breuil's original rendition represents another instance of the current widespread program of careful study and re-examination of the Upper Palaeolithic materials. These studies are being carried out by a new generation of researchers into the imagery and symbolism of the period: the Dellucs, Lorblanchet, Vialou, Marshack, Bednarik, as well as Bégouën and Clottes. For many years, Jean Vertut, who was documenting many of the images at Trois Frères and Tuc d'Audoubert before he died, would come to the United States where we would spend days discussing the new analytical methodologies we were developing, the new data being discovered in these caves and the corrections being made to the published images. I had myself studied and photographed a few select difficult and complex images in Trois Frères and Tuc d'Audoubert in an effort to verify Breuil's renditions. I therefore stand properly corrected. But the problem is not that simple.

The tendency to use the published illustrations to prove or illustrate a theoretical point has plagued the study of Upper Palaeolithic art as well as the study of 'primitive' art generally. The problem has been compounded by the inadequate, visual surface observations and studies made of these materials. It was my own specialised studies of the Upper Palaeolithic vulva images and the evidence I had for the periodic use of both the vulva images and animal images, that

led me to accept Breuil's illustration. It is therefore interesting that even with Clottes' present description, which has not yet been fully published, the central problem, concerning the diverse and variable use of the lion image, and its association with a range of different images indicating different modes of use, remains. Clottes, for instance, does not mention the darts that 'kill' the lion in Breuil's rendition, an aspect of the image's variable use. The use of the bird, the disassociated human arm, the dart and the 'blows' in relation to a single animal image are significant. The same sort of complex associations occur in other Upper Palaeolithic compositions. This leaves open the possibility that the images were added at different times for different symbolic purposes related to the central image of the lion. That problem was not addressed by Clottes.

It is my feeling that a detailed documentation of the recent reanalysis of the lion panel must be published by Clottes and Bégouën and that following that publication the analysis and the analytical methodology involved must be validated by other researchers using other analytical methods and approaches. If Breuil was wrong in his renditions, and Leroi-Gourhan was simplistic in his renditions, and if each modern researcher is wedded to certain analytical methodologies and theoretical approaches, the validation and confirmation of anything published become vital. The problem was clearly raised in the interchange between Lorblanchet and myself in a recent issue of this journal (Vol. 3, pp. 62).

I think the problem of validation of published materials to be extremely important. I have repeatedly requested re-examination and reconfirmation of my analyses of the Upper Palaeolithic notations at Blanchard, Le Placard and La Marche, and of the macaronis in the homesites and caves. At the end of more than a dozen years this has not been done. My analysis of the Polesini, Kesslerloch and La Marche renewals of animals has not been checked. I could go on endlessly. My analyses have begun to be used in standard texts and they have been both supported and argued against on *a priori* grounds. The images and artefacts have not themselves been subjected to confirming analysis. In science one needs confirmation and validation not only of published data but of the theoretical approaches and analytical methods used.

It will be interesting to have a proper discussion of these problems based on a comparison of analytical results.

Alexander Marshack
4 Washington Square Village
New York, N.Y. 10012
U. S. A.

Rock Art Research 1985, Vol. 2, No. 2, pp. 119-39;
1986, Vol. 3, No. 1, pp. 82-7.

LES PAYSAGES DE L'ART RUPESTRE DE PLEIN AIR: VERS UNE NORMALISATION DES MÉTHODES D'ÉTUDE ET DE CONSERVATION

François Soleilhavoup

FURTHER COMMENTS

By J. BRUNET and J. VOUVÉ

(1) *Normalisation des Approches Méthodologiques*

Dans un excellent document de synthèse, F. Soleilhavoup trace avec vigueur de nouvelles définitions des orientations de recherche sur les méthodes d'études et de conservation de l'art rupestre de plein air. Il exprime avec enthousiasme ce que nous devons attendre d'une normalisation des approches scientifiques de ces questions.

Celles-ci sont depuis ces dernières années à l'ordre du jour à des titres divers, de rencontres et de colloques internationaux. Mentionnons pour mémoire, les recommandations de la consultation internationale de spécialistes sur l'étude, la documentation et la conservation (Val Camonica Septembre 1981), celles du XIème Congrès International des Sciences Ethnologiques et Anthropologiques: Conservation, Enregistrement et Interpretation de l'Art Rupestre (Vancouver Août 1983) et enfin celle de la Réunion Internationale sur la Conservation et la Documentation de l'Art Rupestre de la Région Méditerranéenne à l'initiative du gouvernment de la province de Barcelone pour le compte de l'UNESCO, de l'ICCROM, de l'ICOM et de l'ICOMOS.

Josephine Flood, Whitney Davis, F. L. Virili, Andrée Rosenfeld, ont dans leur commentaires parus dans le n° 2 de Novembre 1985 de *Rock Art Research*, suffisamment abordé le fond et la forme du document de Soleilhavoup; aussi nous parait-il à ce stade souhaitable d'apporter quelques précisions plus ponctuellement concernant la conservation.

La très vaste répartition de l'art rupestre explique la difficulté de rencontre des spécialistes et il faut apprécier les efforts des instances internationales (UNESCO - ICOMOS - ICOM) pour établir les liens et promouvoir des documents et leur diffusion.

La mise en commun de toute la terminologie et de toutes les connaissances, demande une sérieuse réflexion, d'abord sur le plan régional, avant l'extension au plan mondial. Le besoin ressenti d'un lexique international, permettant d'aborder les mêmes problèmes sans équivoque est issu notamment de la réunion de Barcelone et Saragosse de 1983. Et c'est donc dans cet esprit et suite à cette rencontre, que Raj Isar de la Division du Patrimoine culturel de l'UNESCO a demandé à trois participants (P. Vidal, J. Vouvé et J. Brunet) de préparer deux monographies régionales des problèmes de conservation des

oeuvres rupestres dans le bassin méditerranéen et associées à un glossaire. Il s'agit donc d'un essai de synthèse sur deux thèmes particuliers: le milieu souterrain de la grotte de Lascaux et le milieu de plein-air des sites de Tassili-n-Ajjer. 'Ces deux projets de conservation prennent dûment en considération l'interdépendance particulière entre l'oeuvre préhistorique et le milieu naturel.'

L'accueil des lecteurs renseignera les auteurs (J. Vouvé, P. Vidal, J. Brunet) sur l'intérêt et la portée de tels essais monographiques, dans l'optique de la continuation de ce type de document. Ce choix délibéré d'études monographiques prend en compte la singularité de chaque cas d'espèce. Nos collègues australiens, dont Andrée Rosenfeld qui a publié *Rock Art Conservation in Australia*, sous les auspices de l'Australian Heritage Commission, apportent d'ailleurs une très importante contribution dans ce sens à la conservation de l'art rupestre.

La conservation de l'art rupestre de plein-air et souterrain est comme pour les autres oeuvres d'art (peintures murales, vitraux, statues etc.) au point de convergence de diverses disciplines scientifiques et demande l'emploi d'un vocabulaire utilisé tout le long du parcours scientifique et technique des études. Dans un tel regroupement de mots, il n'est pas acquis que le sens général de tous les termes soit connu, ne serait-ce qu'en raison du caractère pluridisciplinaire associé à la sauvegarde de l'art rupestre. Aussi avons nous donné ou adapté une définition en tenant compte de l'origine des termes et de leur nouvelle affectation.

Là aussi nous nous sommes efforcés d'être précis dans notre langue maternelle et dans un contexte régional bien défini; il s'agit d'une première approche qui pourra être ou pourrait être améliorée. De par le monde quand quelques démarches de ce type seront réalisées—si ce n'est déjà fait—une synthèse au niveau supérieur pourra être tentée dans le but d'obtenir un document de référence multilingue. Il reste beaucoup à faire dans cette voie.

Art des cavernes et art de plein-air, s'ils peuvent apparaître aux antipodes l'un de l'autre, n'en sont pas moins reliés par les mêmes problèmes, les mêmes maux qui les affectent. Ces deux formes ont besoin, sur le plan des études de conservation, de la connaissance d'une phase de référence dans l'appréciation de l'état de conservation d'une oeuvre ou d'un ensemble d'oeuvres rupestres, quelle que soit leur ancienneté. La définition de cet 'état' peut être assimilée à un bilan de santé établi à la suite des premières études réalisées sur un ensemble peint ou gravé. Cette notion de bilan de santé a fait l'objet de nombreuses discussions auxquelles Soleilhavoup et nous-mêmes avons participé lors des différentes réunions de Barcelone et de Saragosse en 1983.

Dans le cas précis des oeuvres rupestres des abris du Tassili-n-Ajjer, rappelons que, outre le travail réalisé par Soleilhavoup et synthétisé dans une publication de l'Office du Parc National du Tassili, quatre missions ont été organisées en 1979-1982-1983-1985 avec l'appui d'organisations internationales (UNESCO - ICOMOS) et avec le concours et la participation de l'Office du Parc National du Tassili. Ces missions ont permis d'engager les premières études et d'enregistrer les mesures de référence sur cinq sites majeurs: Tamrit (Timenzouzine), Sefar, Essalam Ouan Amis, Iheren, Abanhiar. Sur chacun d'eux, nous avons pu mieux connaître l'environnement climatique des abris (température, humidité) la nature des constituants des peintures et des dépôts superficiels associés aux parois. Nous avons mis au point une technique de contrôles par photos de 'zones sensibles', fait à plusieurs reprises des essais et tests de nettoyage et d'intervention (ex.: élimination de nids de mouches).

La conservation étant l'ensemble des actes destinés à prolonger la vie d'une oeuvre d'art, elle nécessite la recherche et l'élimination des causes d'altération. Elle est aussi un travail d'équipe qui synthétise le diagnostic de trois disciplines; diagnostic esthétique, diagnostic scientifique, diagnostic technique. Pour effectuer cette synthèse, l'avis du restaurateur praticien devient maintenant nécessaire.

Quant aux missions de prospection proprement dite, il faut les concevoir avec la participation d'un restaurateur associé à l'équipe, ou d'un restaurateur seul, afin de ne pas négliger cet autre volet de la conservation qu'est l'intervention ou la restauration proprement dite.

Des interventions ont été réalisées dans ces sites dès 1961 à Tissoukai par M. J. Taralon, par M. Maranzi en Mai/Juin 1968 dans le cadre de l'UNESCO et enfin par MM. G. Thomson et P. Mora en 1974 toujours pour le compte de l'UNESCO. Il faut dès maintenant définir de nouvelles orientations d'action: nouveaux essais de consolidation, de refixage sur des parois ne présentant aucune trace de peinture et sur des zones témoins avec des pigments identiques à ceux qui ont été employés. Ces nouvelles interventions expérimentales doivent se faire après examen et obtention de documents précis sur l'état des parois. Ces précautions prises, les essais réalisés pourront être interprétés et comparés à des témoins. Ces premières investigations pourront être suivies d'actions de plus grande envergure, d'interventions sur des parois ornées. Ce sera l'ultime phase de la conservation, dans ce thème pilote.

(2) *Extension des Processus et Systémes Analytiques*

Notre structure scientifique repose sur l'association de deux laboratoires spécialisés autour desquels viennent se greffer à la demande d'autres cellules de recherches ou intervenants extérieurs, chargés de missions particulières comme:

- l'investigation souterraine (par des groupements spéléologiques par exemple),
- l'identification de charbons de bois appliqués sur une paroi ornée (par un laboratoire de botanique) etc.

Cette organisation pluridisciplinaire, nous permet d'intervenir depuis plus d'une vingtaine d'années sur n'importe quel site témoin de l'art

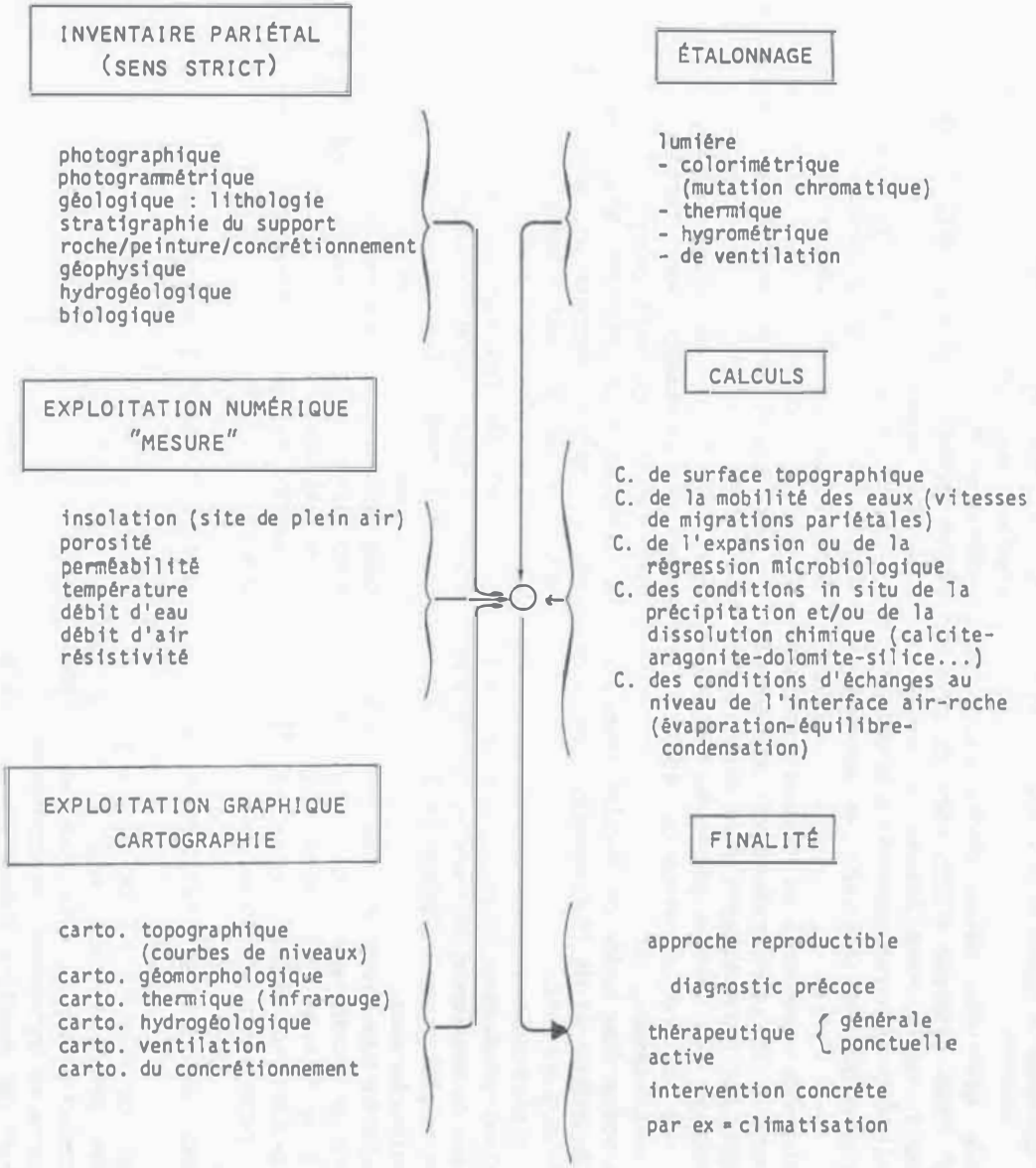

Table 1. Intégration des variables dans le 'fichier-étalon'.

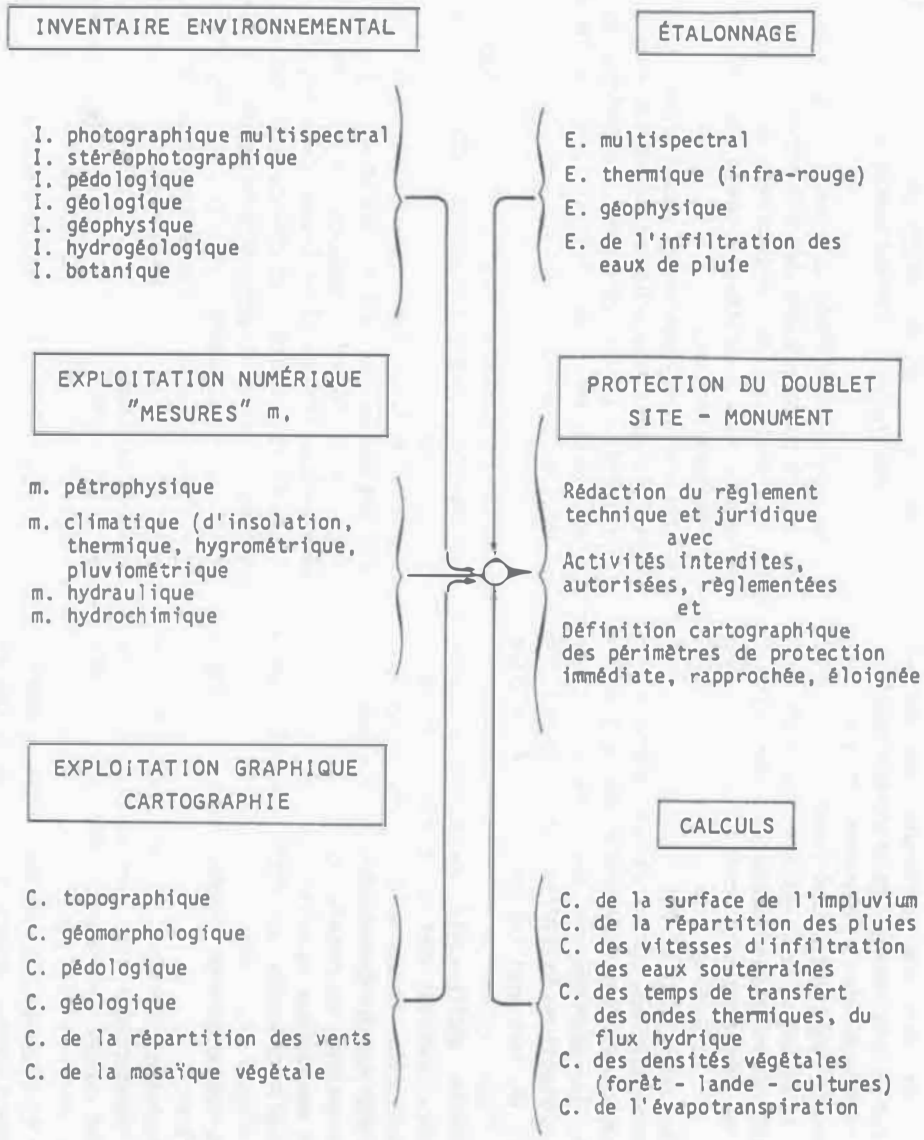

Table 2. Intégration des variables dans le 'fichier évolutif'.

des cavernes, de l'art de plein-air, comme sur n'importe quel monument protohistorique ou historique, recélant des peintures murales et des mosaïques. Il s'agit soit d'une procédure exceptionelle de sauvetage (ex. grotte de Lascaux) soit d'une procédure plus classique, préventive, associée à un diagnostic à court ou à moyen terme.

L'approche scientifique est toujours assortie de l'enregistrement du plus grand nombre de paramètres préalablement à l'énoncé des modalités pratiques de la sauvegarde des oeuvres d'une part, à la protection technique et juridique du monument et du site qui lui est associé, d'autre part.

Notre soucis d'efficacité dans ce domaine où chaque cas est un cas particulier, s'appuie sur une recherche fondamentale et appliquée dont la fiabilité est indépendante de la géométrie de la surface peinte ou gravée, ou des dimensions du monument souterrain ou aérien.

Ainsi nous avons mis au point une approche applicable:

- à des parois ou murs ornés, sans limitation de superficie,
- à des volumes intégrant par exemple des réseaux souterrains de quelques centaines de mètres ou de plusieurs kilomètres de longueur.

'Qui peut le plus, peut le moins' dit le proverbe; dans cet esprit, nous avons affiné la méthode à l'extrême inverse c'est à dire en focalisant l'étude sur *quelques dizaines de décimètres carrés* seulement.

L'exemple que nous avons choisi s'applique à un détail d'une peinture située dans la grotte de Lascaux. Il s'agit d'une écaille rocheuse de 40 x 25 centimètres environ, associée à la peinture la plus énigmatique de la cavité et appelée la 'Licorne'.

La publication associée à ce travail s'intitule 'Contribution de la photogrammétrie et de la thermographie à courte distance à la documentation systématique des oeuvres pariétales préhistoriques en vue de leur sauvegarde. Application à la grotte de Lascaux'.

Elle est parue dans la Revue 'Société Française de Photogrammétrie et de télédétection', Bulletin n° 98 (1985-2), p. 23 à 34.

Le but recherché est celui d'avoir une suite de référentiels photographiques, cartographiques, complétés par le maximum de données numériques afin d'éviter le plus possible l'influence du coefficient d'estimation humain.

Il est important de savoir que sous terre comme en plein air et quelle que soit la latitude des sites, c'est le climat qui impose directement ou indirectement ses effets fastes ou néfastes, au rythme, plus ou moins décalé dans le temps, des saisons.

Cet effet se manifeste rapidement ou à la longue par:

- *un départ de matière* (chute du grain ou des particules de la roche, chute ou migration des pigments colorés ainsi que des dépôts minéraux et/ou organiques, dissolution etc.),
- *un apport de matière* (dépôt de poussières minérales et/ou organiques développement microbiologique, concrétionnement etc.).

En milieu naturel, il apparaît également que les causes primordiales des *détériorations majeures* des oeuvres (s. l.) sont le plus souvent associées:

- soit à une mutation brutale du climat et de l'environnement,
- soit au développement d'une 'crise' climatique à fréquence annuelle ou saisonnière (sècheresse extrême, pluies intenses, incendies, tornades) et quelques fois même des perturbations géologiques (séismes) etc.

Ces phénomènes agissent de façon directe ou retardée dans le temps, chaque cas étant un cas d'espèce.

Par contre, lors d'interventions humaines (aménagement de sites, organisation de visites touristiques, industrialisation etc.) les dégradations sont de tous ordres et de toutes natures.

C'est en tenant compte de cet enchainement de paramètres multiples, que nous avons établi:

- un 'fichier-étalon' ou fichier de référence, reflet d'une microphénoménologie et adaptable à toute circonstance nouvelle et/ou innovation technologique (Tableau n° 1),
- un 'fichier évolutif' adaptable quelles que soient les dimensions du site et l'innovation technologique (Tableau n° 2).

Dans chacun des cas il est important de noter que *ce n'est* pas l'oeuvre par elle même qui est étudiée, mais c'est l'oeuvre intégrée dans son environnement à trois dimensions, c'est cette même oeuvre qui est replacée dans le temps présent et passé.

Cette dernière approche est capitale, car elle permet une analyse rétroactive, c'est à dire avec la possibilité d'un retour en arrière sur trois, six, douze ou dix-huit mois selon les cas.

Elle permet également une analyse prospective dont les retombées peuvent être programmées pour les trois à six prochains mois, en ce qui concerne l'évolution des facteurs thermiques-hydriques et hygrométriques.

Nous sommes conscients que ce qui est réalisable et répétitif lorsque les sites se trouvent à moins de 300 ou 400 kilomètres de distance du laboratoire, devient difficile (voir impossible à suivre) dès l'instant où cette distance passe à 1000 - 2000 ou 3000 kilomètres.

C'est dans cette optique et en fonction des conditions techniques et climatiques que chacun peut adapter les programmes de variables que nous avons résumés ci-après sous la forme de deux tableaux.

Là encore, toute remarque critique et constructive sur cette démarche sera la bienvenue.

Jacques Brunet
Laboratoire de Recherches des Monuments Historiques
77420 Champs-sur-Marne
France

Jean Vouvé
Centre d'Hydrogéologie
Université de Bordeaux I
33405 Talence Cedex
France

By FRANÇOIS RAMBEAU

La communication de F. Soleilhavoup publiée dans *RAR*, de Novembre 1985, apporte un souffle nouveau dans le domaine de la recherche en Art Rupestre de Plein Air. Cette communication est très importante par les démarches proposées pour la conservation et la protection de l'art rupestre en général, et aussi parcequ'elle peut et doit provoquer un resserrement des rangs de tous ceux qui, chercheurs scientifiques, préhistoriens, ou à titres divers se sentent concernés par la défense de ce patrimoine commun 'mémoire de l'humanité et substratum de notre devenir', non encore totalement connu et beaucoup trop souvent négligé.

La méthode de travail proposée est logique, convaincante et sérieuse même si quelques omissions peuvent y être relevées.

La principale à notre avis est le manque de référence ou de recours à un spécialiste de la pierre dans le cas de gravures. Il ne s'agit pas ici du géologue ou du pétrologue pour qu'il est prévu un travail bien déterminé et souvent ingrat, mais du tailleur de pierre, sculpteur ou graveur.

Cette absence parait regrettable dans deux domaines:

(a) Dans le domaine de la terminologie.

(b) Dans le domaine de la technologie.

(a) Dans son exposé Soleilhavoup fait bien ressortir, et nous sommes d'accord avec lui 'que plus les mots sont utilisés, plus ils élargissent leurs acceptations et en définitive, ne veulent plus dire grand chose de précis', et inversement; lorsque l'on utilise plusieurs mots approximatifs, pour désigner la même chose, cette chose perd toute précision: elle devient elle-même approximative. Or nous savons, qu'à défaut d'être poétique le langage technologique est précis; mais qu'importe le manque de poésie à partir de l'instant où ce vocabulaire remplit sa fonction parfaitement.

Il parait presque certain que la pierre a été taillée dans le monde entier et de plus, que selon sa nature, texture, dureté, cohérence, les moyens et les techniques employés ont été, et sont encore actuellement partout similaires. Il ne devrait donc pas y avoir de difficultés majeures à établir une correspondance précise et sûre entre les différentes langues; les techniques étant les mêmes, les opérations de taille successives le sont aussi, donc les mots pour les désigner forcément existants.

L'art rupestre qui ressort d'une technique presque aussi vieille que le monde doit se référer au langage de cette technique et l'utiliser pour normaliser (au sens de norme), préciser et rendre enfin un sens aux mots et aux choses qu'il recouvre.

Si la sémiotique est la branche philosophique étudiant le signifiant et le signifié, il parait souhaitable d'avoir recours à elle, et, qui mieux que le praticien peut connaitre les mots en relation avec sa pratique? Il faut donc rapidement nous rapprocher de l'art architectural et faire participer les appareilleur, tailleur de pierre, sculpteur, graveur à la mise au point de la définition des articles qui nous sont communs. Cette démarche, si elle était entreprise, ne résoudrait pas à elle seule les problèmes de terminologie, mais elle aurait au moins l'avantage d'engager un processus aboutissant tôt ou tard au consensus indispensable. D'autre part, et nous en sommes tous convaincus, il ne peut pas être déraisonnable ni critiquable de vouloir faire appeler un chat . . . un chat.

(b) Dans le domaine de la technique, seul un praticien de la taille de pierre est en mesure de donner des informations sûres concernant les techniques, y compris celles utilisées par nos lointains ancêtres.

Mis à part certains outils relativement nouveaux mûs électriquement ou pneumatiquement (dans le seul but de remplacer la massette), les outils de taille proprement dit n'ont que fort peu évolué; la différence essentielle réside dans le fait que depuis l'âge du fer, nous possédons des outils beaucoup plus durables. Avant cette révolution les graveurs utilisaient la pierre (sens large) contre la pierre. Le rapport de force était loin d'être aussi inégal que nous nous l'imaginons communement.

Ces praticiens pourraient aussi nous fournir de précieuses indications concernant les 'superpositions', tant il s'avère comme de plus en plus probable que la détermination chronologique serait facilitée par l'étude objective des superpositions existantes sur certaines des grandes parois gravées du Néolithique. Enigme énervante et angoissante, car si cette chronologie pouvait être établie avec certitude; celle-ci comparée à d'autres chronologies éclairerait peut-être quelques unes des zones encores inconnues et nous permettrait de faire un pas de plus vers la connaissance de ces hommes du Néolithique.

Nous sommes en droit de nous poser la question suivante: pourquoi faire appel à des praticiens? Nous connaissons les gravures, les étudions depuis de nombreuses années, avons établi des classifications et des typologies. Pour plusieurs raisons, dont la principale est le 'savoir faire'. Entre voir, savoir, et savoir faire il y a des différences non négligeables. Ce qui parait à l'examen évident pour le profane, ne l'est pas systématiquement pour un technicien. Il faut se rendre à l'évidence; tel trait qui apparait comme postérieur à celui qui le recoupe, peut fort bien avoir été éxécuté en premier par un tailleur de pierre habile, techniquement il n'existe pas d'impossibilité absolue.

Les cas de superposition où l'ordre d'éxécution est clairement apparent sont assez rares pour que nous nous posions des questions et remettions en cause notre façon d'appréhender ce problème.

Ce rapprochement entre techniciens et scientifiques peut évidemment avec profit être étendu à toutes les spécialistés concernées.

Dans cette proposition les scientifiques et les préhistoriens ne doivent pas ressentir une diminution ou limitation de leurs prérogatives bien au contraire, mais seulement la volonté de les faire participer à la création des outils dont ils ont besoin. Ces outils, qu'ils soient de nature terminologique ou typologique devraient faciliter une standardisation dans l'établissement

des fiches d'inventaires.

Loin de nous la pensée ou le désir de bloquer toute possibilité objective d'évolution, mais simplement de fixer une délimitation en dehors de laquelle la subjectivité interdirait la comparaison.

Les quelques suggestions énoncées ci-dessus nous invitent à une relecture de la communication de Soleilhavoup. Celle-ci est scindée en deux parties, la première très développée est ce que l'on peut appeler théorique et technique, la seconde basée sur les expériences du Tassili et de l'Atlas saharien est plus succinte mais aussi plus pratique. Par contre la relation entre scientifique conservateur et préhistorien interprétateur est assez floue et demanderait à être éclaircie et précisée, puisqu'en l'état actuel de la rédaction on pourrait penser qu'il y a opposition entre le scientifique préhistorien (ou de sciences humaines) et le scientifique technique (ou de sciences exactes), ce qui ne peut pas être puisque le travail et la méthode proposée sont complémentaires.

Nous pensons plutôt, qu'implicitement il s'agit d'une priorité du scientifique conservateur sur le scientifique interprétateur. En effet, quel pourra être le degré d'interprétation si les mesures conservatoires n'étaient pas prises en temps voulu, c'est à dire maintenant, et qu'il ne reste plus à l'interprétation que ruines et débris? Nous pensons aussi que cette réaction, si elle existe, doit-être contenue et qu'il suffit pour s'en convaincre de replacer les choses dans le contexte voulu par l'auteur: 'Conservation et Protection' du Rupestre; deux étapes qui si elles étaient réussies, permettraient ensuite toutes les interprétations.

Pour conclure, nous sommes convaincu que cette communication n'est pas aussi futuriste qu'elle le parait de prime abord, mais qu'elle arrive en son temps si l'on veut réellement conserver et protéger ce patrimoine que nous ne sommes peut-être pas en mesure d'exploiter correctement; car il est indéniable que le progrès sous toutes ses formes, y compris l'alphabétisation, met en péril cet héritage. C'est sans attendre qu'il faut attaquer de front et sérier les problèmes multiples et très complexes de sa conservation, demain il sera trop tard.

Idéaliste peut-être, il n'en est pas moins vrai que cette communication nous ouvre la voie, et met à notre disposition un plan de travail scientifique et complet. Nous l'espérons développé dans ce 'Guide de Terrain' annoncé.

François Rambeau
'Al Zeribat'
Le Génibaud, par Lévignac de Guyenne
47120 Duras
France

REPLY

By FRANÇOIS SOLEILHAVOUP

L'étude pluridisciplinaire des gisements d'art rupestre, l'étude archéologique et palethnologique, la protection des sites et la conservation des oeuvres sont (ou doivent être) complémentaires et réalisées grâce à des programmes bien structurés, décidés en commissions. Tout cela transparaît bien clairement dans la note de Jacques Brunet et Jean Vouvé que je remercie d'avoir étudié mon texte et apporté d'importants commentaires qui élargissent le champs d'applicatiòn d'étude de l'art sur support rocheux.

Il est certain que la récente publication par l'UNESCO d'un fascicule—le premier du genre—sur la conservation de l'art rupestre, avec deux études de cas et un glossaire illustré, correspond à l'expression d'un besoin unanimement ressenti par les spécialistes de ce patrimoine. Cet ouvrage, réalisé par J. Brunet, P. Vidal et J. Vouvé a le grand intérêt de définir des techniques d'étude des *peintures rupestres* en vue de leur conservation et surtout, il pose des définitions claires, complètes, utilisables par tous, des principaux termes ou expressions utilisés dans tous les domaines de la connaissance de l'art rupestre. A ce titre, on doit considérer ce document comme une base de référence pour les études et les discours ultérieurs.

Bien entendu, il ne s'agit là que du point de départ d'une démarche méthodologique visant à définir avec rigueur le domaine, les contours, les champs d'applications d'une véritable *Science de l'Art Rupestre*, dans laquelle se rassemblent de nombreuses disciplines des sciences de la Terre, de la Vie et de l'Homme.

Les spécialistes de l'étude et de la conservation de l'art rupestre doivent pouvoir à l'avenir, disposer non seulement d'une base terminologique (et conceptuelle), mais aussi de règles de conduite, de façons de faire, de moyens d'action. Quelle que soit la diversité des environnements géoclimatiques de l'art rupestre, un même savoir, une même attitude doivent animer les personnes qui en ont la charge.

Dans le glossaire de MM. Brunet, Vidal et Vouvé, l'accent est mis, presque exclusivement, sur les causes et les effets d'altération des surfaces d'art rupestre souterrain ou subaérien.

Dans beaucoup de cas cependant, la compréhension des processus d'altération nécessite une bonne connaissance des *techniques* propres à la peinture et à la gravure rupestres. Un lexique des termes d'art et de sa technologie s'avère donc indispensable. Par exemple, seul un tailleur de pierre, celui qui sait 'dialoguer' avec la pierre, est réellement habilité à percevoir les techniques utilisées dans la gravure rupestre; d'après l'examen des oeuvres, des traits, des stigmates parfois très ténus, il peut retrouver les gestes, les intentions mêmes des artistes; il connaît les termes spécifiques définissant les moyens, les objets ou les outils, les gestes, les rendus, donc les styles. C'est bien pourquoi, le renfort de tels artisans spécialisés, formés jusque dans leur esprit même à ces métiers d'art, est indispensable. Notre collaboration avec François Rambeau (voir plus loin ses commentaires) va tout-à-fait dans ce sens.

Les travaux de recherche en conservation et de restauration menés par MM. Brunet, Vidal et Vouvé, tant dans les grottes ornées que dans les milieux de plein air, sont exemplaires, et le rapide survol de l'organisation de cette activité

de terrain et de laboratoire illustre parfaitement la fiabilité de la démarche méthodologique et la qualité des résultats obtenus (grottes ornées françaises, Tassili-n-Ajjer etc.).

L'inventaire des détériorations naturelles et anthropiques subies par les surfaces d'art rupestre et leur *contrôle* dans le temps sont résumés dans les tableaux n° 1 et n° 2: 'fichier-étalon' et 'fichier-évolutif'. L'essentiels des informations et des données objectives qu'il est nécessaire de posséder avant toute action conservatoire, est rassemblé dans le 'fichier-étalon'. Cependant je souhaiterai ajouter à cette liste quelques points qui me paraissent utiles et en préciser l'importance.

L'étude des caractéristiques du support rocheux est évidemment le fondement même de la conservation de l'art. C'est l'*étude pétrologique du support* qui doit fournir cette base. Elle inclue la description des structures géologiques et l'interprétation de leur genèse, l'analyse pétrographique, minéralogique et géochimique, aboutissant à la constitution d'une véritable *lithothèque de l'art rupestre*. C'est à partir des données pétrologiques que d'autres investigations doivent êtres menées, et tout particulièrement l'*étude géomicrobiologique* à l'interface roche/atmosphère. Le rôle des associations corticales de microorganismes (champignons, algues, bactéries) s'avère en effet déterminant dans la formation et le développement de cortex, patines, indurations superficielles ou enduits. L'analyse de ces formations doit fournir des renseignements extrèmement précieux sur l'état de l'art rupestre et spécialement sur sa chronologie. C'est une voie actuelle très prometteuse dans la recherche; elle doit donc figurer dans l'inventaire pariétal.

Pour ce qui concerne l'exploitation numérique des données dans les sites de plein air, l'insolation, c'est-à-dire la durée quotidienne moyenne d'exposition à la radiation solaire des surfaces d'art, doit être enregistrée conjointement à la *mesure de l'albédo*, c'est-à-dire le rapport du flux lumineux renvoyé par une surface au flux lumineux total reçu, ou incident. La valeur de l'albédo est utile à connaître car elle renseigne sur le pouvoir absorbant ou réfléchissant d'une surface (rocheuse ou non) et donc sur l'activité photosynthétique et biochimique qui peut en résulter.

Sous la rubrique: 'Exploitation graphique - Cartographie', conjointement aux cartes géomorphologiques des surfaces étudiées, il m'apparaît très utile d'établir des *cartes micromorphologiques* qui permettent de replacer les oeuvres rupestres dans leur contexte pariétal grâce à la représentation graphique des micros-reliefs de surface. J'ai exposé ailleurs les multiples intérêts que présentent ces documents de relevé, tant pour l'archéologue que pour le spécialiste de la conservation.

Quoi qu'il en soit de ces quelques remarques, je suis bien persuadé que la structure de recherche appliquée mise en place par le Laboratoire de Recherche des Monuments Historiques pour l'art rupestre doit être adoptée, étendue rapidement et amplifiée dans le plus grand nombre possible de sites, car elle a déjà largement fait ses preuves en permettant de sauver nombre de chefs-d'oeuvre du passé.

Il est certain que le renfort des spécialistes de la taille de pierre et de la technologie de la gravure permettra de rompre certains cercles dans lesquels beaucoup de personnes intéressées à l'art rupestre se sont enfermées, soit par leur formation propre, soit par confort intellectuel.

C'est bien ce que François Rambeau propose dans les perspectives de ses très utiles commentaires à mon article.

Par l'introduction d'études et surtout d'expérimentations sur la technologie de l'art: outils, supports et résultats (graphismes, styles), François Rambeau va très certainement apporter une contribution importante aux débats sur les méthodes d'études et sur la pertinence des résultats acquis jusqu'à maintenant ou à venir. Il semble bien que dans ce domaine de l'ARPA, aux interfaces multiples, nous nous engagions dans une excellente voie, large, ouverte, dans laquelle le cheminement et l'expérience de chacun pourront éclairer ceux de tous les autres.

Depuis 1982, j'ai eu l'occasion plusieurs fois de travailler sur le terrain avec François Rambeau. La confrontation de nos expériences respectives de naturaliste-géologue et de technicien de la pierre, nos longues conversations devant les parois rupestres nous ont progressivement amenés à envisager la mise au point commune du guide pour l'ARPA dont je discutais des prémisses en 1984. Ce guide, avec le travail de MM. Brunet, Vidal et Vouvé (UNESCO 1985), pourra, nous le pensons, rendre service aux spécialistes et au public éclairé.

La logique et la cohérence de fond de la méthode scientifique que nous proposons pour étudier et interpréter les paysages et les parois rupestres, doivent revenir aux sources conceptuelles des langages appliqués aux différents domaines de l'ARPA. Ce qui importe avant tout, plus encore que l'identité ou la similitude des mots utilisés pour l'établissement d'une terminologie internationale, c'est la concordance des notions sous-jacentes, autrement dit, la communauté de pensée. Cette communauté de pensée est loin d'être acquise dans beaucoup des disciplines connexes de l'ARPA; par contre, de la même façon que le langage des mathématiques ou de la physique est universel, celui de la technique et des racines de l'artisanat ne peut pas prêter à confusion ni à discussion.

Si on veut reproduire actuellement, avec les *mêmes outils* de pierre, les *mêmes gravures* que les hommes préhistoriques ont réalisées sur les parois et sur les blocs, on aura les *mêmes contraintes* matérielles et donc on fera les *mêmes gestes*. Vouloir un même résultat sur une même matière, entraîne nécessairement une même idée et par conséquent un même langage: nous revenons aux sources de la Tradition de l'art et l'Artisanat.

Seuls les praticiens de la gravure et de la peinture rupestres, formés et entraînés à la fois dans leurs conceptions et dans leurs gestes, enracinés dans ces traditions du travail de la pierre, peuvent aider à clarifier les bases mêmes de

nos recherches sur l'ARPA. C'est pourquoi je suis persuadé que Rambeau apportera des éléments neufs et très solides à nos connaissances dans ce domaine. Sous peine de s'éloigner de la réalité objective de nos recherches et de nos travaux, il est indispensable de s'assurer de la collaboration complémentaire des spécialistes de la taille de pierre.

On ne peut que souhaiter un accueil aussi large et enthousiaste que possible par l'ensemble de la Communauté Scientifique Internationale des techniciens de la pierre dont les goûts et les activités les portent à la connaissance de l'ARPA.

Je remercie vivement mon ami Rambeau pour ses commentaires et surtout pour les remarquables perspectives qu'ils ouvrent.

Professor François Soleilhavoup
Laboratoire de Préhistoire du Muséum
National d'Histoire Naturelle UA 184 du
Centre National de la Recherche Scientifique
Institut de Paléontologie Humaine
1, rue René Panhard
75013 Paris
France

8th TRIENNIAL MEETING ICOM COMMITTEE FOR CONSERVATION 6-11 September 1987, Sydney

The Committee for Conservation of the International Council of Museums (ICOM) consists of twenty-six working groups covering all aspects of conservation science. One of these, Working Group No. 24, is concerned with rock art.

The Committee's Eighth Triennial Meeting, the second to be held outside Europe, is to be conducted at the Hilton International Sydney Hotel. AURA members are invited to attend but details are not yet available on cost and program. It has been suggested that the rock art session should be held from 10 to 11 September 1987. It will include a half-day field trip, most probably to the rock art site at Brisbane Waters, Sydney.

Readers wishing to receive the Second Announcement of this event should complete the Expression of Interest Form below, and send it to:

The Secretariat
ICOM 87
Dulcie Stretton Associates
70 Glenmore Road
Paddington, N.S.W. 2021
Australia

ROCK ART SEMINAR AND FIELD TRIP 12-13 September 1987, Sydney

A weekend seminar/workshop will be held following the 8th Triennial ICOM Conservation Meeting in Sydney, and will cover mainly conservation and cultural resource management. It will be followed by a field trip to rock art and other prehistoric sites in New South Wales that will present a variety of conservation problems and site types. The field trip's itinerary will be approximately as follows:

Day 1. Sydney - Milbrodale - Moonbi - Armidale (motel)
Day 2. Armidale via Hunter - Inverell - Gravesend - Nyngen (motel)
Day 3. Nyngen - Mt Grenfell - Broken Hill (motel)
Day 4. Sturts Meadows and Fowlers Gap (stay at research station)
Day 5. Fowlers Gap - Mootwingee - Kinchega (stay at woolshed)
Day 6. Kinchega - Mungo - Balranald (motel)
Day 7. Balranald - Mid West highway to Forbes and Molong (Yuraningh's Grave) - Orange (motel)
Day 8. Orange - Katoomba - Bull Cave - Sydney

The field trip will be supervised by one or two tour guides, and John Clegg is producing a 'guide book' for it. The seminar and field trip, which are expected to attract in the order of forty participants, are organised by Dr Josephine Flood and Dr Andrée Rosenfeld.

Applications for participation will be invited as soon as cost, program and seminar venue are finalised.

ICOM THE INTERNATIONAL COUNCIL OF MUSEUMS, COMMITTEE FOR CONSERVATION, 8th TRIENNIAL MEETING
ICOM CONSEIL INTERNATIONAL DES MUSEES, COMITE DE CONSERVATION, 8EME REUNION TRIENNALE
SYDNEY, AUSTRALIA 6-11 SEPTEMBER, 1987

TITLE/TITRE

FAMILY NAME/NOM DE FAMILLE

GIVEN NAMES/PRENOMS

ADDRESS LINE 1
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ADDRESS LINE 2

CITY/SUBURB
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I EXPECT TO BE ACCOMPANIED BY (NUMBER OF PERSONS)
JE SERAI ACCOMPAGNE(E) DE (NOMBRE DE PERSONNES):

OFFICE USE ONLY/USAGE OFFICIEL UNIQUEMENT

I AM INTERESTED IN ATTENDING THE MEETING YES..... NO.....
JE DESIRE PARTICIPER A LA REUNION YES..... NO.....

PREFERRED LANGUAGE .....
LANGUE PREFEREE .....

SALES AREA

BRIEF REPORTS

Comments on Rock Art Recording

CLEMENT W. MEIGHAN

As director of the Rock Art Archive at the University of California, Los Angeles, I work with extensive documents on rock art collected by many different people over the past century. We concentrate on original field documentation, only a small part of which is published in detail. Hence we can readily observe what people have done, and are doing, in efforts to record rock art sites. These efforts (both past and present) range from the taking of a few snapshots in an hour or so to hundreds of pages of careful element-by-element drawings. The most complete records are accompanied by colour and black-and-white photos, tracings or rubbings, and even such things as casts and pieces of the native rock for geological identification. Such documentation is of course very welcome, but any record is better than no record and even a single photograph has a value in establishing what the site looked like on a certain date.

My feeling is that overly complex recording forms and instructions may inhibit people from providing any information at all, on the feeling that if they do not have the time to do a complete job it is not worthwhile to do anything. Rock art recording, like excavations and most other scientific study, ought to be approached in a series of steps or phases in which each step lays the groundwork for the more detailed work to follow. We have developed some recording forms that are six pages in length and cover every detail we can think of, but I do not know anyone who has ever actually filled out one of these forms. Also, much depends on the particular site, the variety of rock art present, the preservation, and just the amount of man-hours needed to do a proper job of recording. Hence, any form is no more than a check list of things to be considered in making the record, and it has to be remembered that maybe the form does not include some very important things which were overlooked by the designer of the form.

On the matter of standardisation of terminology, the first scholarly meeting I ever attended was marked by a paper telling us that we simply *had* to standardise our terminology (in that case for projectile points). The same refrain has been heard at every meeting since. Now that rock art has gained a certain respectability as a field of scholarly study, there is continual clamour for standard terminology and much indignation from those who find that others are using different names for *their* rock art elements. There is also the ever-present tendency toward jargon and the inventing of cryptic terms understood only by the initiates. Less worry should be devoted to these problems, since standardisation is not going to happen and there are some good reasons why it cannot be done under any universal and rigid classification system. The same element may represent quite different things in different sites, particularly in different parts of the world, and the naming of elements will automatically carry an interpretation which may be well attested in one area but entirely wrong in another. In addition, there will continue to be people who just have to invent new words to refer to old ideas, not to mention the necessary invention of terms for things that nobody has yet described.

On the other hand, there has appeared a very powerful reason for striving for some kind of standard terminology, and that is the possibility for computer analysis of large bodies of data. The computer cannot deal with input which is poorly defined or confuses many discrete elements under the same name. For comparative studies, therefore, it is essential that tabulations be precisely defined and that the units of study be equivalent. Unfortunately, the standardisation of the terminology is the necessary responsibility of the person doing the computer analysis and he cannot assume that everyone is using all the terms in the same way.

The perfect record is no more likely with a rock art site than it is with excavation archaeology. No matter how detailed the record, a revisit to the site will usually yield new information, and sometimes major new information as a later researcher goes to the location with a different set of questions in mind and sees all kinds of things not looked for by the original recorder.

This should not be as discouraging as it sounds. There are some more or less standard kinds of records to be made, essential to anyone doing problem-oriented research. Not all of these are relevant to every site, but the basic records can include:

- A recording form which documents what is present at the site. Very useful is to have also a summary record with a tabulation of the frequency and distribution of whatever elements are defined.
- Photographs, which may be quite varied and include black-and-white, colour, false colour, infrared, or even television recording.
- Drawings, paintings or other artistic representations. Essential for faint and superimposed elements which may not be clearly visible in photographs.

• Tracings or rubbings reproducing the rock art in detail. The main value here is for museum reconstructions where exact size and detail are needed.

• Casts or replicas, again mainly for museum or display use.

To the extent possible, the recorder may employ all of these, but at the least one needs both drawings and photos for a good record.

As for procedures, depending on the time available and the skill of the recorders, I would suggest the following steps or phases of recording:

(1) Document all of the elements present. Selection of just some of the record, particularly selection of the few most spectacular or artistic elements, obviously limits analysis of the site as a whole.

(2) Document all superimpositions.

(3) Document the style changes of individual periods of artistic production (or, if you are lucky enough to have the data to do it, recognise the work of individual artists).

(4) Document what appear to be assemblages of elements done at the same time, groups or 'scenes'.

(5) Look for evidence of retouching, renewing, or modifying an older element.

(6) Record destruction or damage from natural or human causes.

Even if all of these phases are completed carefully there is still no interpretation or explanation of the rock art. On the other hand, this documentation lays the groundwork for rational interpretation and conclusions which would be very speculative unless the data base is complete and thorough. Some attention to all these steps of recording also serves to preserve the data of the site, should it be vandalised or destroyed by contemporary activities of road building and the like.

Professor Clement W. Meighan
Department of Anthropology
University of California, Los Angeles
Los Angeles, CA 90024
U. S. A.

Rock Art and Environment: Observations at Depuch Island

SALLY E. MINCHIN

Archaeological interest in Australian rock art has increased recently, with modern dating methods giving a greater and more accurate time perspective to some of the outstanding rock art galleries in the northern regions. The Pilbara region of Western Australia has been recognised for its magnificent collection of rock engravings—the most durable of rock art exposed to local climatic conditions. Patina studies by Clarke (1977) attribute a Pleistocene antiquity to some Pilbara engravings, and useful dates from Australian Quaternary sites which include rock engravings in the archaeological record come from deposits near limestone markings in Koonalda Cave (Wright 1971), from shell middens in Skew Valley (Lorblanchet 1977), from the occupation floors of Early Man Shelter (Rosenfeld *et al.* 1981) and from several other sites. New dates are forecast in studies of Tertiary limestone caves near Mount Gambier, South Australia, where the Parietal Markings Project (Aslin, Bednarik and Bednarik 1985: 73) has located a stratified sequence of rock art in some sites. Jones (1985: 298) says that 'the way is open for us directly to date some of these skins covering the putative oldest art panels in the region', when referring to art panels in the Kakadu National Park.

The association of coastal reefs and rock art has been little explored. Recent interest in sea level change has accompanied exploration of underwater environmental change occurring along coastal seabeds, reef formation and sunken cultural debris. The importance of our underwater cultural heritage is recognised world wide, as seen in the UNESCO international survey of underwater cultural heritage report (Gifford, Redknap and Fleming 1985). A suggestion made in this report is for ending 'compartmentalisation within archaeology' for 'splitting it up into period or field specialisation, has reduced the level of interchange of ideas, and synthetic works on broad themes are rarely formulated' (Gifford *et al.* 1985: 373). UNESCO has undertaken a two-year program for this survey—the association of Australia's shore line with outstanding art close by could well provide some of the information relevant to such a survey. Inundation of coastal sites has occurred along Australian shore lines, just as it has occurred elsewhere [1]), and among the submerged heritage may well be material significant to our artistic record. Off the coast of France there are stone circles, once on dry land, now 'flooded by the sea' (Scarre 1984: 98). Similarly, changes have occurred to islands, peninsulas and coastal sites in Western Australia, altering the cultural and environmental resource base and providing a need for an overall assessment of our knowledge. These changes have already been used elsewhere in Australia to explain variations in the environmental conditions reflected in rock art, between estuarine and inland conditions. Chaloupka (1984) has used such indicators to assist in the relative dating of rock paintings in Arnhem Land. While there are similarities in coastal studies, there are also individual differences which can make each study unique. Depuch Island presents the opportunity for such a study.

Depuch Island and its Environment

Location, significance and durability have helped to save Depuch Island rock art (Crawford 1964) in its original environment. Located off the Pilbara coast where high tides show a swing of up to 9.3 metres in spring (Hardie 1981; and 1970 hydrographic area map), Depuch has been fortunate to escape inundation, and since 1958 reserve status (McCarthy 1961) has helped protect local fauna and Aboriginal sites (Ride *et al.* 1964). Not far away (Fig. 1) rock art sites have been destroyed on the Burrup Peninsula and at Port

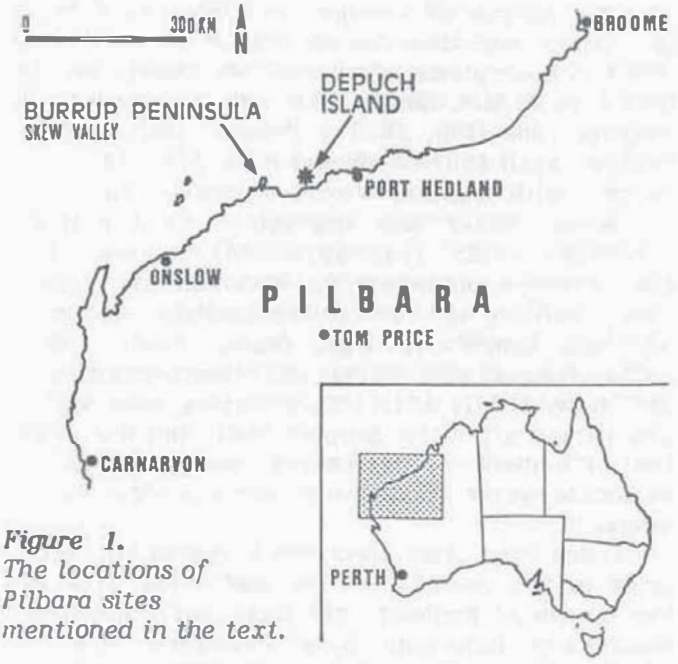

Figure 1.
The locations of Pilbara sites mentioned in the text.

Hedland (McNickle 1985: 64; Minchin 1985: 177). Navigators and explorers during the early nineteenth century recorded sightings of petroglyphs on Depuch (Stokes 1846; Wickham 1842) and the general interest in 'ancient' art and museum collections expressed by explorer Sir George Grey and by Charles Darwin on earlier navigations of H.M.S. *Beagle* may have stimulated this (Grey 1841; Moorehead 1969). This early recognition of Depuch Island may have helped spare sites from destructive impact. Destruction and relocation of some Pilbara rock art since 1958 has included massive change on the nearby Burrup Peninsula, an area with outstanding petroglyphs (Lorblanchet 1977; Virili 1977). After visiting in 1984 sites in the Pilbara that had undergone recent destruction or relocation, it was a relief to find a major rock art complex, Depuch Island, relatively undisturbed.

Depuch Island, a rock-strewn hilltop rising from the waters around, is situated in the Fortescue Botanical District. It is described by Beard (1975: 79) as 'a huge pile of dolerite boulders which are generally unvegetated except for some encrusting lichens but may at times be covered by mats of *Triodia pungens*. There are some beach dunes which are vegetated as on the adjoining mainland. Where there is soil in the valleys, it carries *Acacia pyrifolia-Triodia pungens* shrub steppe. Trees of *Terminalia circumalata* are shown at the foot of rocky slopes'. Depuch Island was named after the Baudin Expedition visited the area in 1801 in the 124-feet long corvette *Géographe*. Louis Depuch was mineralogist of the expedition which anchored offshore an area of Australia described as 'De Witt's Land' in the expedition's map (Baudin 1803). On 27 July 1801 a long boat under the command of sublieutenant Ronsard set off to reconnoitre and eventually to land on a sandy cove (Baudin 1803: 520). Ronsard's report describes the island as

> an accumulation of basalt rocks heaped on top of each other. Frequently, and particularly on top of the peaks, they stand on one of their smallest sides. Some of these rocks are so much taller than the others, that from a distance one would take them for tree trunks. Elsewhere they look like sloping walls. In the dales they are piled up in confusion and appear to have fallen from the neighbouring slopes.

Ronsard's description is both pictorial and archaeologically significant. Standing stones stand today on the ridges of Depuch's tumbled slopes and as elsewhere in Australia are recognised to be Aboriginal topographic indicators.

About seven kilometres long (McCarthy 1961: 121), Depuch Island has several sandy beaches and sites located on beach dunes where artefacts have been exposed by wind erosion. Other sites lie behind the ridges, amongst the rocks, and consist of chipped stone artefact scatters, fossil wood, rock hides or 'pits', and art. Artefacts show clear colour contrasts along broken edges. The same contrast can be seen on chipped edges of large boulders at rock art sites and there are seed grinding patches on several large rock platforms near the shore line. Shell is plentiful, I found several large baler shells and fragments of baler on the adjacent mainland. Bednarik (1977: 70) encountered a well-preserved shell of a baler 'west of Tom Price, 230 km from the nearest coast' and Grey (1841) discovered baler used to hold water at sites inland from the Kimberley coast. Other containers located on Depuch beaches include large sponges which make wonderful containers for transporting shell, birds eggs and marine food. Anadara shells, recognised as a food source, are found live on the mud flats and in shell middens on the mainland (Kriewaldt 1965: 57). Materials useful for artistic activity are among those traded and transported by Aborigines, cf. Jones' (1985: 60) comments on the use of Anadara shell as ochre container.

The Environmental and Historical Significance of the Depuch Island Rock Art

All around life on the island is incorporated in engravings on the rock surfaces. Important to the understanding of this record are the offshore reefs, the mangrove mudflat environment, seasonal climatic change in the region and sea level changes altering relationships between the rocky island, offshore and mainland sites. To the seaward side Depuch Island is partially protected, at the present, by a low limestone reef island which takes the surge of the ocean's waves. Birds roost and nest on the lee of this reef, leaving in flocks to find fresh water on mainland and Depuch Island waterways. Depicted in the rock art these long-legged water birds can still be seen foraging on the island's shore line and on the nearby mudflats.

Sea-related motifs dominate on the rocky headlands of Wickham Beach and Anchor Hill and these rock sites provide excellent vantage points for observing the tidal swings and emergent mud flats and reefs—which provide rich foraging zones at low tide. As turtles struggle up the sandy slopes to lay their eggs, or swim silently past in the waters around, their shapes are engraved on rocks above in an assortment of size and decoration (Fig. 2). Prominent among the marine life depicted are fish of various shapes and some have a narrative component with a reference

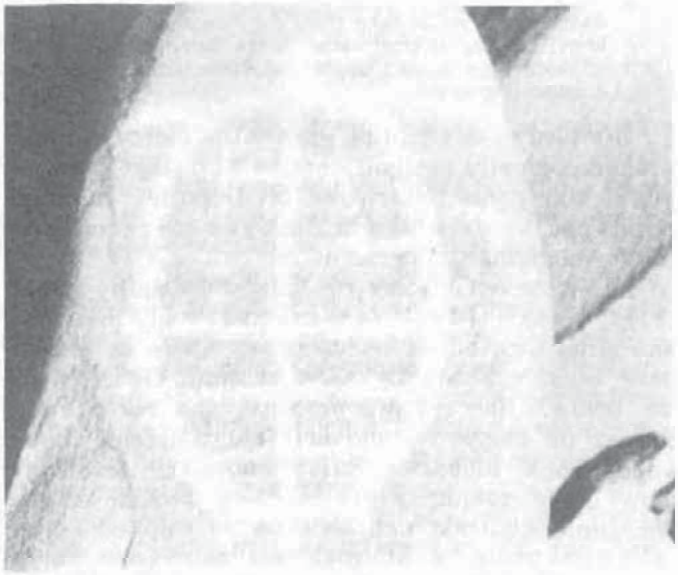

Figure 2. Engraving of large turtle at Anchor Hill, Depuch Island, Western Australia.

to hunting activities (spears etc.). Humans are not the only fishers in the area—among the birds seen searching for fish are reef foragers and sea-eagles. Many of these fishing motifs depicted on Anchor Hill appear very fresh, their outlines contrasting with the rock's patinated surface, yet they were seen by Wickham in 1840. Not all the engravings are of marine life along the coast, and many are fairly faded. Further inland, along Watering Valley, there are fewer marine motifs and many of these are also repatinated. Estimates of the age of surface patination on Depuch Island rocks have been made. Trendall (1964) finds that a weathering skin 0.2 inches (five millimetres) thick could indicate a considerable geological age. Clarke (1977) examined patination of some Pilbara engravings and estimated some to be in excess of 17 000 years old and to have been formed at times of high aridity.

There are rock formations similar to Depuch Island elsewhere in the Pilbara. The Burrup Peninsula rock piles are engraved with rock art in much the same way as seen on Depuch. But Depuch is unique in its own way. Amongst its petroglyphs there are some important inscriptions—important because they have dates in the nineteenth century

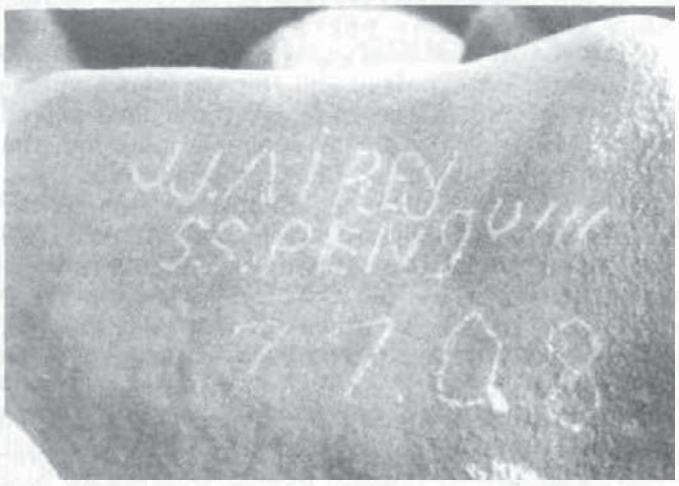

Figure 3. Historic engraving on a rock at Anchor Hill: 'J. J. AIREY S.S. PENGUIN 7.7.08'

and the degree of change associated with these engravings over time can be monitored. On Anchor Hill two inscriptions of interest are clearly visible, that from H.M.S. *Beagle* 1840 and the pearler S.S. *Penguin* 1908 (Fig. 3). The *Penguin* foundered off Onslow, April 1909 (Webb and Webb 1983: 117). The *Beagle*, with Captain Wickham, visited the island to obtain water and engravings were noticed. Davidson (1952: 111, 1936: 65) remarks that the sketches published by Wickham and Stokes over a century ago recorded naturalistic renderings of 'fish, kangaroos, dogs, crabs, insects, birds, human figures, emu tracks' and other compositions. McCarthy (1961: 121, 122) indicates sites visited and named after the *Beagle's* visit, and the valley that members of Wickham's party walked up to locate water, along which are numerous engravings.

There are two protected maritime wreck sites in the locality—of the *Eddystone* 1894 and the *Crown of England* 1912 and I note that beach sands and limestone have cemented artefacts of recent wrecks in shelves of Australian beach rock off arid coasts (Pullan 1983: 14). During my visit I observed a ship's anchor on the reef in front of Anchor Hill, and several other pieces of ship wrecks in the area.

Conclusion

Theoretical studies in art history and of the rock art of Western Australia have given me an appreciation of the difficulties encountered in discussing the meaning of art. Mulvaney (1978: 11) says 'the essential dignity and creativity of Aboriginal society have always been evident for those other Australians who chose to comprehend but, over the past few years, archaeological discoveries have added a historical dimension the great significance of which cannot be overlooked'. My visits to Depuch Island and environs during the spring months of 1984 were the result of detailed previous study, and the realisation that an interdisciplinary approach exemplified in recent Quaternary research provides a useful framework for an archaeologist. Depuch Island is significant in relation to the protection of our cultural and environmental heritage (Minchin, in prep. a, b).

Australia has been colonised by seafarers. Some type of water craft has been essential in visiting Depuch Island since it became separated from the mainland. Wickham noted 'that as no rafts were seen in this area, it was presumed that the natives walked across the sandbanks at low tide to visit the islands of which Depuch appeared to be their favourite resort, to secure turtles and fish. Several huts were seen on the island, but no natives' (McCarthy 1961: 121). Yet a method of water transport was noted by Richardson in 1886 who said the natives of this area sat astride a log of wood and propelled it with their hands (McCarthy 1961: 121, footnote). In 1984, on my visit, it was impossible to walk across to Depuch Island as the mud flats are not easy to walk on and there are deep channels of water to cross. The explorer, adventurer and coastal settler have covered long distances with

a knowledge of tide, wind, seasons and survival technique. Early adventurers, not isolated from a natural environment, may have been less vulnerable than is often suggested and may have possessed the knowledge for long distance voyaging. It is possible to suggest that early colonisers set sail to and from Australia's coasts in two-way voyaging, and that this could have coincided with low sea level periods of the Pleistocene which varied the shore lines and the environmental conditions encountered (Chappell and Thom 1977; Glover 1975; Glover and Lee 1984). My research through journals and documents indicates there is support for a theory of easy movement and settlement of Australia.

Finally I would like to illustrate the significance of Depuch Island by comparing it to a similar small island off the coast of Europe—the Greek island of Delos. I perceive many parallels here. Like Delos in the Aegean Sea, Depuch has a protected heritage; its maritime history and early art are respected and its sites are available for the modern researcher to review and explain. Depuch is uninhabited today, and Delos is so for most of the year. Fresh water is only available at Depuch after rain has filled the rock pools—the water supply of Delos was dependant on only one stream and artificial cisterns and wells. Delos has been the subject of several archaeological surveys (Ling 1976: 47-60) and its art includes mosaics depicting marine and animal life. Homeric works, the *Iliad* and *Odyssey*, were recorded of oral traditions which may go back to the Mycenaen epoch (Kahane 1967: 19). Our oral history is still being recorded. Works such as *Joe Nangan's Dreaming* (Nangan and Edwards 1976) are helping to fill out the picture of our art heritage—just as Homer (c. 800 B.C.) has done, in part, for Delos. The antiquity of rock art in Australia, with dates in the Pleistocene as there are dates in Europe in Palaeolithic cave art, makes Australia's rock art record outstanding. If Depuch Island can be visualised as the 'Australian Delos', its artists, seafarers, and their creativity may be respected and, as Hallam (1972: 465) points out, 'the cognitive capacities' of Aboriginal systems appreciated.

Sally E. Minchin
'Merrifields'
Vasse, W.A. 6282
Australia

[1] The existence of submerged or almost submerged rock art sites on the northwestern coast of North America is significant, yet it has received little attention so far. I have recently brought it to the attention of leading American rock art specialists [ed.].

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REVIEW

Rock Art Conservation in Australia, by ANDRÉE ROSENFELD. 1985. Special Australian Heritage Publication Series No. 2. Australian Government Publishing Service, Canberra. 79 pages, map, colour and b/w plates, extensive bibliography. $A 11.80.

Rock Art Conservation in Australia by Dr Andrée Rosenfeld is the second in a most welcome series being published by the Australian Heritage Commission. It is a well presented text of 79 pages. and contains over twenty-five excellent photographs of all aspects of rock art.

At probably the first international conference on the conservation of rock art, held in Perth in 1977, the overseas delegates commented not only on the wealth of Aboriginal rock art in Australia, but on the significant pioneer work being done to preserve this heritage. It had been anticipated that this would stimulate activity in rock art conservation around Australia to not only maintain this international image, but also to develop it. As regards the variety and number of styles and sites, the immense conservation problems and the fact that rock art in Australia is in some places still a living tradition, must make Australian Aboriginal rock art the most unique in the world. Naturally therefore, we would be expected to be the world leaders in preserving it.

Regrettably, for a number of different reasons, the anticipated development did not take place and in fact there has been an overall decline in rock art conservation studies over the past decade. It was decided at the 1981 Canberra Workshop on Rock Art Deterioration and Conservation (under the auspices of the Joint Academies' Committee for the Protection of Prehistoric Places) that it was time to review what had been happening to date. Although not a conservator in the full sense of the word, it was fortunate that Dr Rosenfeld offered to carry out the work with the support of the Department of Prehistory and Anthropology at the Australian National University. She has not only provided a survey report of the rock art conservation carried out in Australia to date, but has also included details of the current state of knowledge of deterioration processes and conservation and restoration methods. This not only makes the publication a 'state of the art' document, but also provides a most useful handbook for any person—anthropologist, archaeologist, geologist, conservator, and also the general public, including in particular the Aboriginal people, who are interested in the preservation of Australian Aboriginal rock art. Although Dr Rosenfeld expresses concern that some sections of the book may be rather elementary and superfluous for some readers, she can be assured that any book that can be classified as a basic manual will always be of tremendous value—to have all the up-to-date information on the preservation of rock art available (including relevant information from overseas) makes this a most useful publication.

The information is provided under four basic headings. Dr Rosenfeld first discusses rock art and conservation outside Australia to provide a world-wide picture. This is followed by a chapter on rock art in Australia, then detailed chapters on rock art deterioration and conservation.

It is obvious that the problems of preserving rock art around the world are very similar. Although the styles and techniques may be different, and also the location of the art (from deep within caves to open rock surfaces), we are at all times dealing with a painting or engraving on a rock surface. This form of expression of past and living cultures is not really an art as we understand from the Western point of view. It is more an expression of the culture of a race of people. The fact that it was recorded (up to 30 000 years ago) and in many places still exists, and in a few places is still being recorded, makes this 'art form' very tangible. It is extremely popular with tourists and it appears that is where the pressure, and hopefully funds will come from for developing preservation programs for rock art.

Dr Rosenfeld's review of the world-wide situation as regards the deterioration and conservation of rock art confirms what we have more or less assumed for some years. The causes of deterioration, both natural and man-made are very similar, and to quote Ian Wainwright talking about rock art in Canada: 'While solutions may exist to the problems of vandalism and public access in general, there are few methods which can be adopted for the preservation of the natural weathering of rock art sites'. Remember, we are trying to halt the devastating forces of nature and this is never an easy task. Although from work done in Europe, there is now the possibility of controlling the environment inside caves, and hence the rate of deterioration of paintings and engravings, this is virtually impossible with exposed sites which is the Australian situation. Remember, in the former case it was the change in the environment brought about by visitors that necessitated the closing down of the Lascaux cave.

The significance of rock art as a part of the world cultural heritage can be seen by the fact that UNESCO have made this one of their major current heritage activities.

The chapter on rock art in Australia reveals how little work has been done, and by so few people over the past decade. Here and in later chapters one keeps on seeing the same names being referred to, probably no more than a dozen

names in all. This is truly a sad state of affairs. There are also only two major publications that discuss the deterioration and conservation problems. Of course one of the most important factors concerning the study of rock art in Australia is the need for the involvement of the race of people to whom it belongs, the Aborigines. Dr Rosenfeld discusses these aspects, and the various problems that exist. Although only one page of a publication of 79 pages is given to this topic, it is probably due to the fact that there are so many variables as regards Aboriginal involvement that it would need another publication to discuss this subject to the depth it deserves. Whatever the reason, the need for involvement of Aborigines in all aspects of the preservation of their rock art is of paramount importance if any real progress is to be made.

In the section that discusses painting techniques, it is interesting to note that no positive evidence of the use of organic binding media to hold the paint pigments to the rock surface has been found. It is just as significant that most of this data comes from analysis of paint samples and not from the Aboriginal who applied the paint media. It is surprising that so little data is available from the latter source, on the methods and materials used for applying paint to rock surfaces. I suspect that this information is available in unpublished records compiled by field workers and is now in the vaults of National Parks and Wildlife Authorities or at the Australian Institute of Aboriginal Studies. Mind you, Wainwright indicates that in Canada it may be the absence of organic binding media that has helped to preserve the paintings.

The other sections in this chapter look at the dating of rock art—always problematic—and then describe the methods used to analyse and record rock art. The recording of rock art is much more advanced in Australia (although never enough has been done) than conservation, but this will hopefully be the subject of a future publication.

The chapter on rock art deterioration is well written, clear and concise. Dr Rosenfeld is not a specialist in this field but consulted with specialists and has done a good job on this subject. She discusses the major causes of deterioration to the rock ground and to the painting (the engraving being essentially part of the rock ground). These include the influence of moisture in all its forms; silcrete skins, desert varnish and other mineral deposits; physical weathering, including temperature (and in Australia this looks at the problems of bushfires), salt weathering, hydration, and seismic activity and subsidence; biological decay including that caused by insects, bacteria, fungi, algae and lichens. Most of these factors cause some form of damage to rock art although a few, such as silcrete skins, may in fact help preserve it. This publication can obviously only just touch on the processes involved. Ask any geomorphologist about the weathering of rocks—this is a discipline in its own right and will reveal that rock weathering is a very complex process. This complexity will be compounded by all of the other agents mentioned above.

The last chapter attempts to provide some of the answers to the deterioration of rock art, but as is quite obvious from the work carried out to date there is not a great deal that can be done with current knowledge. Gross factors of deterioration, such as water flow over a surface, or animals rubbing on a painting site, can be resolved relatively easily, but for example nothing yet can be done to stop an engraving or painting on granite exfoliating due to natural weathering of the rock.

It is pointed out by Dr Rosenfeld that prior to any form of intervention, the ethical aspects of conservation must be fully understood, as must again be the involvement of Aboriginal people.

Dr Rosenfeld's account of the conservation work carried out to date is again very clear. She has given enough detail to indicate the complexity of the problem and showed yet again that we are a long way from satisfactory solutions. The use of synthetic consolidants for flaking paintings, for example, is a process developed from the conservation of building stone. However, what might work on a deteriorated cut stone surface on a city building may not work when applied to the surface of a massive sandstone rock in the desert. Although some small progress has been made there is still a long way to go. The important point that does emerge is the necessity to draw on related expertise, such as stone conservation scientists' and biochemists' to help to provide answers to some of the problems.

The control of people and vandalism is one area where some success has been noted, but this is probably just as well handled by better education of the public visiting rock art sites.

In her conclusions Dr Rosenfeld states that 'This survey of rock art conservation issues has identified more problems than solutions' and 'Further research is urgently needed on two fronts: a more precise understanding of the process of rock art decay and stability; and means of halting or mitigating the effects of the processes of decay'. This is very true, but at least this publication lays the ground work for future studies and is therefore highly recommended reading for all concerned with the preservation of rock art. It is hoped that this study will contribute to reactivating research activities so that Australia can justly and proudly claim to be the world leader in this field of rock art studies.

Dr Colin Pearson
Principal Lecturer
Conservation of Cultural Materials
Canberra College of Advanced Education

Due to the volume of valuable contributions we have not been able to realise the announced expansion of the reviews section in the present issue. As the backlog of important rock art literature continues to grow we will endeavour to include an expanded section for reviews and abstracts in the May 1987 issue.

ORIENTATION

DARWIN 1988

THE FIRST AURA CONGRESS
Darwin, 29 August - 2 September 1988
EXTENDED FIRST ANNOUNCEMENT

To be held under the auspices of the Northern Territory Museum of Arts and Sciences, and with the support of numerous organisations in Australia and abroad.

The events and considerations leading to the selection of venue and time of the First AURA Congress have been described in *AURA Newsletter* No. 4/2, in August 1986. As reported on the following pages, the Steering Committee for this event has since been established and we have begun to form the required subcommittees. Great care has been taken to make these committees as representative as possible: they include some of the world's most distinguished rock art scholars, traditional rock art owners/custodians, freelance researchers, academics, cultural resource managers and senior representatives of relevant government agencies. The members of already established committees are listed on page 195.

As the site for this important international congress, Darwin is the supreme choice. Not only does this exciting city provide a central base from which to reach the world's largest rock art concentrations, we have found Darwin's government authorities singularly helpful, enthusiastic and supportive, and we take this opportunity to thank the officers concerned. Public agencies are not always noted for their responsiveness in such matters, so let it be said that those of the Northern Territory are exceptional. Already, the Congress enjoys the support and assistance of the following organisations in the Northern Territory alone:

Northern Territory Museum of Arts and Sciences;
Conservation Commission of the Northern Territory;
Northern Land Council;
Australian National Parks and Wildlife Service, N.T. Division;
Northern Territory Tourist Commission;
Aboriginal Sacred Sites Authority;
Northern Territory Convention Bureau.

Numerous other organisations, in Australia and overseas, have also registered their support already, which will be acknowledged in due course.

Located in the tropics, Darwin lies in one of the few remaining great wilderness areas, close to the Kakadu National Park which is of World Heritage listing. Darwin is surrounded by immense rock art concentrations, including those of Arnhem Land, Kimberley, Victoria River and, further south, Uluru (Ayers Rock). Climatically, Darwin's best time of the year is August/September, when the temperature ranges from 21°C to 32°C and the sun shines for an average of ten hours per day. For the information of our American visitors: Texas is half the size of Northern Territory, and its population density is 420 times greater than the Territory's. The vastness of this 'Land of the Dreamtime' defies description. From The Red Centre to the Top End, Northern Territory offers an infinite variety of landscapes: the seemingly endless flood plains with their teeming wildlife and flora in the north, the tranquillity of Katherine Gorge, the dramatic sandstone country of the Arnhem Land plateau, the vibrancy of The Centre's canyons, or the stunning majesty of its granite domes.

Despite its geographical remoteness, Darwin is most accessible. There are two weekly flights by each of three international airlines (Qantas, Garuda, Royal Brunei), as well as many flights by several pan-Australian airlines.

PROGRAM OF THE CONGRESS

While the actual Congress will be from 29 August to 2 September 1988, pre- and postcongress field trips will both occupy up to three weeks. At least one brief field trip will be conducted during the congress week, for those delegates who are unable to stay longer than one week.

The Congress will comprise a substantial academic program (about ten subject-based symposia of papers, which will be arranged by consolidating the responses to the initially proposed seventeen symposium subjects); slide and poster sessions; panel debates; a rock art film and video fair; a rock art publications display; a variety of exhibitions and demonstrations; and various official functions, especially the First General Meeting of AURA. Other events will be announced later.

While the various 'ancillary' events will no doubt be of the greatest interest to most participants, the Presentation of Scientific Papers will certainly form the core of the Congress. It will cover all aspects of rock art research but certain subjects are expected to emerge as central or dominating themes. Particularly the session on psychoarchaeology and interpretational synthesis holds the promise of a scientific watershed event of historical significance. Two other subject areas are likely to emerge as similarly important: the dating of rock art world wide, and of course the area in which Australia is now the world leader: the conservation, protection and management of rock art.

At this early stage we estimate that about 150 papers are likely to be presented. We will try to accommodate all papers offered and, in contrast to most scientific conventions, a rigid time limit will not be enforced. Presenters wishing to take longer than twenty minutes will be required to demonstrate the necessity to do so. Presenters are requested to keep their papers concise, nevertheless, and they are encouraged to use visual material such as slides, maps and leaflets. All presenters must be prepared to respond to questions from the audience. Although the congress language is English, brief presentations in other major languages will be admitted in some circumstances and will be followed by a translater's synopsis. AURA will have first publication rights on all papers presented. We anticipate the publication of all suitable papers, if not in *RAR* then in an AURA Special Publication of the congress proceedings, through an affiliated organisation or by other means. Intending presenters of papers or other contributions must submit their application on the form provided. To meet the requirement of responding to audience questions all papers must be presented by the author, but in exceptional circumstances the symposium chairperson may decide to relax this rule. All applications to present a paper or other contribution (slides, film, video, posters) must be sent to AURA's editor initially, who will liaise with the symposium chairpersons to determine the final symposium subjects and to assign each contribution. All scientific papers proposed for presentation must relate to at least one of the following provisional symposium subjects:

A - Rock art studies in the Old World
B - Rock art studies in the Americas
C - Regional studies in Australia and Oceania
D - The rock art of northern Australia
E - Recording methods and computerisation
F - Dating methods and theory
G - Rock art and prehistory
H - Rock art and ethnography
J - Rock art and art history
K - Psychoarchaeology, interpretational synthesis
L - Pictures and human behaviour
M - Site management and visitor control
N - Conservation and geomorphology
O - Retouch: an option to conservation?
P - International aspects, standardisation
Q - Aspects of promotion and publication
R - Specialist working papers

Panel debates will be encouraged, particularly those addressing intellectual or theoretical subjects. They will be modelled on the system of the *RAR Debates*: following the presentation of a precirculated debate paper, a selected panel of specialists will comment in the presence of an audience. A moderator, who may be the author of the debate paper or another suitably experienced person, will guide and promote the debate, and the audience will have the opportunity of contributing to the

same by asking questions or adding relevant information.

Details of the First General Meeting of AURA will be available later, and a draft constitution will be circulated prior to the Congress. Attendance of this event will of course be limited to paid-up Full Members of AURA.

Similarly, details of other events and functions, such as the publications display, bark painting exhibition, demonstrations by traditional rock artists and by tribal bark painters, Aboriginal dance performances, opening dinner in a ballroom, barbecues and so forth, will be announced later.

PRECONGRESS PUBLICATIONS

These will be received by all registered participants, and will include the following:

- Complete program of the Congress;
- Abstracts of all papers scheduled to be given;
- Precirculated papers;
- Details of panel debates;
- Comprehensive details of all field trips, including practical information (duration, itinerary, arduousness, accommodation and travel arrangements, recommendations on equipment, etc.);
- List of all congress participants;
- Information package on Darwin and its environs;
- Information on package tours (additional to field trips);
- Information on other events in Australia which roughly coincide with the Congress, and which may be of interest to participants;
- Any other information the Precongress Publications Subcommittee considers relevant.

FIELD TRIPS

Pre- and postcongress field trips will range in length from a few days to about three weeks. Field trip parties will be escorted by experienced rock art specialists who will conduct impromptu debate sessions and consultations at the sites where appropriate. Participants will be provided with prepared notes, extracts of publications and other background literature prior to the commencement of field trips, and will be encouraged to contribute to field debates. These field trips are not expected to be simple 'show and tell' sessions, or excursions designed for tourists; they are intended to be learning experiences for all, and participating specialists in specific fields will be expected to be available for consultation and discussion, for the benefit of other participants, and also for that of the local rock art researchers, conservators or site managers.

Precongress field trips will be timed to conclude simultaneously just before the Congress Opening, and they will be repeated after the Congress, providing delegates with the opportunity to participate in a trip they had to miss prior to the Congress because they had attended another. The staggered commencement and completion dates of the field trips will allow participants the greatest flexibility in planning their schedule, while also facilitating our logistic planning. Individual itineraries can be created, ranging in duration from five days (Congress only) to about seven weeks (by including the two longest field trips) or even more (by adding package tours).

As mentioned above, at least one brief field trip will be conducted during the congress week, for delegates unable to stay beyond this week. Field trip participants are reminded that the proposed Symposium D ('The rock art of northern Australia') will be an introduction to the excursion destinations. Specifically planned to complement the field trips, it will be conducted by the researchers actually engaged in exploring and studying these rock art regions.

FURTHER INFORMATION

It is a policy of this Congress to encourage Aboriginal involvement. Traditional site custodians have already been included in the committees responsible for planning, and Aboriginal participation will be conspicuous throughout the Congress.

An information desk will be set up by the Northern Territory Tourist Commission during the Congress.

Ansett Airlines of Australia, who have been appointed as the Official Domestic Airline of the Congress, have made the following discounts available to congress participants:

- Participants travelling to Darwin for the Congress only (seven nights) are eligible for a 21.5% discounted normal economy class fare;
- For groups of ten or more adults travelling together on forward and return flights, fares will be at the normal economy class less 15%.

All overseas participants will be required to clear Australian Customs at their arrival point. They will be required to possess a return ticket, a passport, and in certain cases an entry visa. In our Second Announcement we will provide a list of the countries whose citizens require a visa to enter Australia.

An official international air carrier still has to be chosen and we expect to negotiate drastically reduced air fares for overseas participants.

In addition to the congress field trips, a large selection of tour packages will be available to participants, to various destinations in northern Australia, to EXPO '88 (in Brisbane), and to the islands north of Darwin (including Bali, Java, New Guinea).

REGISTRATION

To register for participation, simply complete the Registration Form provided with the First Announcement (a copy has been enclosed, but we will gladly provide any number of additional copies upon request). You may either enclose payment of the registration fee, or defer payment until later, bearing in mind that 'early registrations' (reduced registration fees) will close by 31 March 1988.

'Early registration fees' are $A 80.00 for AURA members, half fee ($A 40.00) for AURA members who will be bona-fide students in August 1988, and $A 130.00 for others (US$ 52.00, 26.00 and 84.00 respectively).

After March 1988, these fees will increase to $A 100.00, 50.00 and 150.00 respectively.

Registration fees will cover the following:

- Cost of venues and facilities;
- Participation in all academic sessions;
- Attendance of all functions, exhibitions, demonstrations and debates;
- Attendance of film and video fair;
- Participation in any field trip at reduced cost;
- Substantial information and stationery package upon arrival;
- Field trips literature;
- All precongress publications;
- Light refreshments during academic sessions;
- Reduced travel fares and excursion packages;
- Special rates for postcongress publications and cassette recordings of academic sessions;
- Extensive activity programs for accompanying persons;
- Passes for attendees and accompanying persons, for various free or reduced-cost services (lunch, creche, transport etc.).

It is important to note that no cancellation charges will apply to REGISTRATION FEES paid and that such fees WILL BE REFUNDED IN FULL in case of nonattendance.

The Congress will have limited funds for subsidising or waiving registration fees for a number of attendees—refer comments below.

All completed Registration Forms, except those from African countries, are to be posted to AURA, P.O. Box 216, Caulfield South, Vic. 3162, Australia.

Registration Forms from Africa must be directed to Dr O. Odak, Kenya Archaeological and Ethnographic Research Agency, P.O. Box 10614, Nairobi, Kenya.

ACCOMMODATION

A complete range of accommodation and restaurants is available within easy walking distance of the congress venues. Accommodation will range from student quarters, YMCA and other low budget lodging, to luxury hotels of international standard. In order to effect block bookings we will require a reasonably accurate indication of requirements, and for this purpose the Registration Form divides the available accommodation into four categories based on price. For the information of overseas delegates, hotel charges in Australia do not differ greatly from those prevailing in North America or western Europe. However, due to the recent massive devaluation of the Australian currency OVERSEAS VISITORS WILL FIND COSTS EXTREMELY LOW in Australia.

Intending participants are urged to book their accommodation early. Darwin is a popular tourist destination, and another anthropological convention will coincide with our Congress. The Registration Form offers the option of booking accommodation now, and participants choosing to do so should include a payment equal to the cost of two nights' accommodation, at the intermediate rate of their choice. In the case of nonattendance this payment would be refunded provided that the cancellation is received in time to permit transfer of the booking.

When making payment for registration or accommodation booking please note the following: cheques, traveller's cheques and international money orders are acceptable. Please make all cheques payable to 'FIRST AURA CONGRESS'. Overseas participants should note that Australian banks reject cheques which are printed for use in another currency, but which have been completed in Australian currency; thus cheques must be either printed in Australian dollars, or completed in the currency of the country where they are issued.

Receipts will be provided for all payments received and

accommodation reservations will be confirmed.

CONGRESS SUBSIDISATION

Congress subsidisation of travel costs and fees will be limited. It is necessary that participants initially make application to their own organisations, governments or private funding sources. Applications for congress subsidisation can only be considered if they are accompanied by evidence that other sources have been approached, that alternative funding avenues have been fully explored and exhausted.

As is the practice at other international conventions, subsidisation will be on the basis of a priority rating scheme, which will favour committee members and chair persons of symposia; participants from Third World countries where currency restrictions apply; and other persons whose presence at the Congress would be highly desirable for scientific reasons. The costs that are most likely to be subsidised from congress funds will be those of registration, field trips, accommodation and travel within Australia. Overseas air fares are the least likely area to attract subsidisation.

SUMMARY

Any queries or suggestions concerning the Congress should be directed to one of the two congress chairmen. If they concern field trips, accommodation or the congress venues, please write to:

George Chaloupka
AURA Congress
c/- Northern Territory Museum
G.P.O. Box 4646
Darwin, N.T. 5794
Australia

Concerning symposia, presentations or subsidisation please contact:

Robert G. Bednarik
AURA Congress
P.O. Box 216
Caulfield South, Vic. 3162
Australia

This First Announcement will be followed by a Second Announcement during 1987, providing updated and expanded information including: final list of symposium titles, preliminary list of the papers to be presented, comprehensive details for overseas participants.

Early in 1988, a Final Announcement will appear which will provide full and final particulars of the Congress.

Robert G. Bednarik

ORGANISING THE 1988 AURA CONGRESS

Preparations for the First Congress of the Australian Rock Art Research Association are proceeding as planned. The organisational network shown below was prepared within days of deciding time and location of the Congress, and an 'Advance Announcement' has been published in the August 1986 issue of the *AURA Newsletter*. Since then, the organisers have been busy establishing the committees that are required to conduct the preparations for such a major international convention, and that will eventually conduct the Congress itself. Literally hundreds of letters have been written to various public and nonpublic organisations and to individual researchers and administrators, and numerous meetings addressing specific aspects of the Congress have been held in Darwin, Canberra and Melbourne. The principal developments so far are briefly related here.

Executive authority and responsibility for the scientific standard of the Congress are vested in the Steering Committee, which is to become the Executive Committee eventually. This body, consisting of eight Australian and eight overseas members, will provide several of the symposium chairpersons, as well as the chairpersons of the various subcommittees. The composition of the Steering Committee is almost finalised. Its Internati-

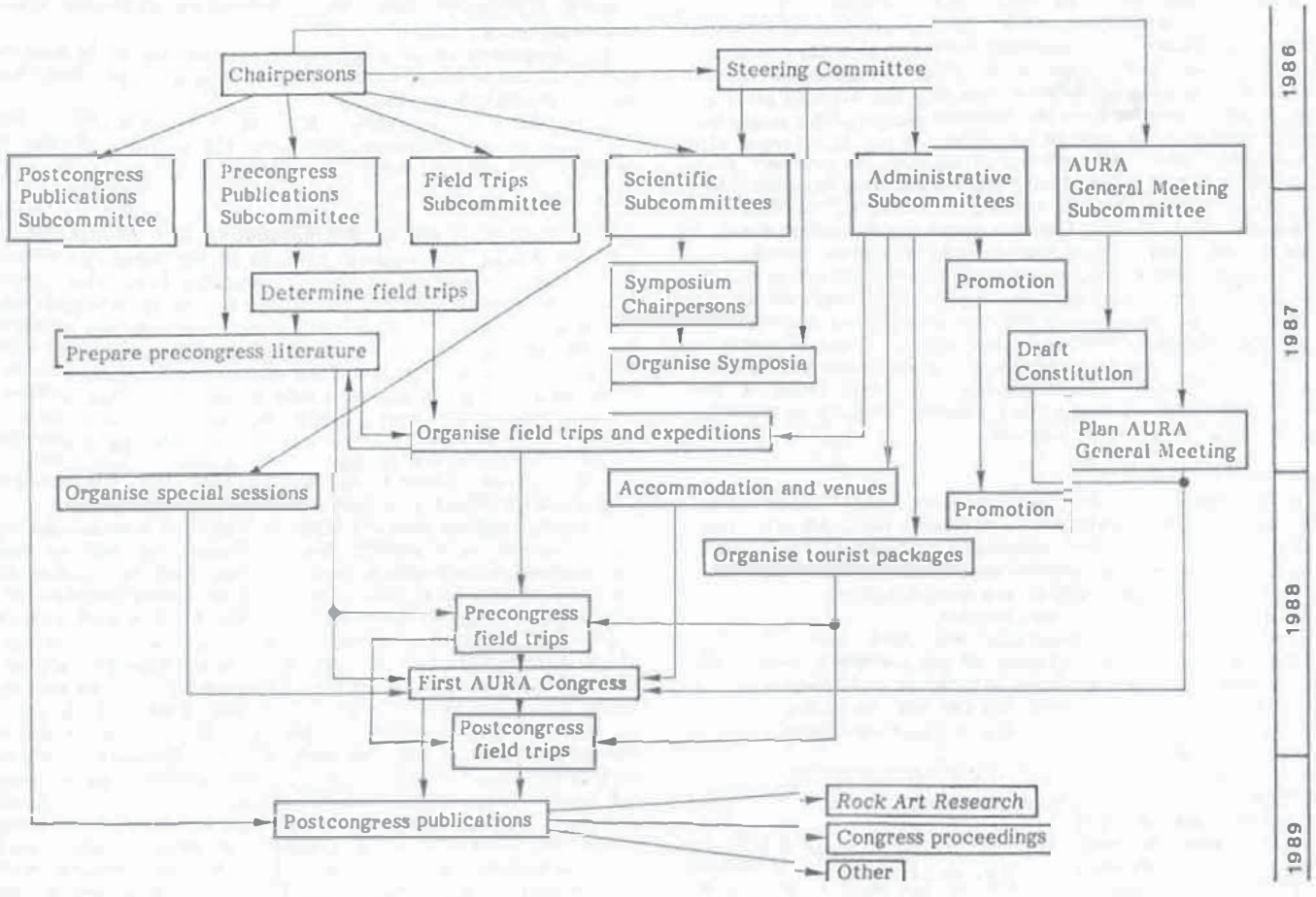

Table 1. Organisational network of the 1988 AURA Congress.

onal Chapter will include three distinguished rock art scholars from Europe, three more from North America, and one each from Africa and South America. They are:

Professor Emmanuel ANATI, Director of the Centro Camuno di Studi Preistorici, Chairman of the International Committee on Rock Art (Italy);
Dr Whitney DAVIS, University of California, Berkeley (U.S.A.);
Dr Michel LORBLANCHET, Centre de Préhistoire du Pech Merle (France);
Professor Clement W. MEIGHAN, Director of the Rock Art Archive, University of California, Los Angeles (U.S.A.);
Dr Osaga ODAK, Director of the Division of Ethnology and Ethnoarchaeology, KAERA (Kenya);
Professor Eduardo RIPOLL-PERELLO, President of the Spanish National Committee for Rock Art, Director of the National Archaeological Museum in Madrid (Spain);
Professor Jack STEINBRING, University of Winnipeg (Canada).
(The South American delegate has yet to be announced.)

The Australian Chapter of the Steering Committee consists of:

Robert G. BEDNARIK, founder and editor of AURA;
George CHALOUPKA, Northern Territory Museum of Arts and Sciences;
John CLEGG, University of Sydney;
Warwick DIX, Principal of the Australian Institute of Aboriginal Studies;
Dr Josephine FLOOD, Director of the Australian Heritage Commission;
David MOWALJARLAI, traditional site custodian, Western Australia;
Dr Andrée ROSENFELD, Australian National University;
Grahame L. WALSH, Takarakka Rock Art Research Centre and NPWS of Queensland.

Two of the subcommittees are already operational and meet in Darwin as required. They are:

The Darwin Administrative Subcommittee, consisting initially of:

Kim ACKERMAN, anthropologist;
George CHALOUPKA, chair;
Yvonne FORREST, Northern Land Council;
Pina GIULIANI, Northern Territory Museum;
Dr Peter MURRAY, Head of Division of Human Sciences;
Vivienne SOBEK, teacher;
Margie WEST, Curator of Australian Ethnography.

This subcommittee will be augmented in July 1988 to cope with its workload.

The Field Trips Subcommittee will also be expanded as required; it comprises initially:

Kim ACKERMAN, anthropologist;
George CHALOUPKA, chair;
Billy HARNEY, traditional site custodian;
Ivan HASKOVEC, Kakadu National Park;
Dick KIMBER, anthropologist;
Hilary SULLIVAN, Kakadu National Park;
Paul TAÇON, prehistorian;
One representative of the Conservation Commission of Northern Territory.

rgb

News in Brief

AURA's success has encouraged endeavours to form further rock art organisations. From 13 to 16 November 1986, a meeting of South American rock art researchers has been held in Cochabamba, Bolivia, with the purpose of founding a rock art society. The convener is AURA member Dr Roy Querejazu Lewis.

*

Shirley-Ann Pager intends to shortly launch SARARA (Southern African Rock Art Research Association), another organisation modelled on AURA. Hopefully, the countries of the southern African subcontinent can all be involved.

*

AURA member Dr Niéde Guidon, whose French-led team is responsible for the recent sensational discoveries in Piaui, Brazil, invites interested AURA members, especially students, to participate in the continuation of her work. Students would be entirely provided for by her team.

*

Early in 1986, Dr Josephine Flood agreed to serve as an Editorial Adviser for this journal. We omitted to report this in our May edition.

Continued on page 196

Letter to the Editor

Dear Mr Bednarik,

I am somewhat alarmed at the amount of misinformation presented in David Moore's synopsis of rock art papers presented at the 1985 A.A.A. Conference (*RAR* May 1986, pp. 93). Many of the papers, as described by Moore, sound very different from what I remember was presented at Valla Park but I can only clarify and comment on the inaccuracies and errors rife in the description of my own paper.

(1) It is true that I consider the methods and theories of archaeology and art history to be fundamental to an understanding of prehistoric art but my basic approach is multidisciplinary and I also utilise appropriate ethnographic, psychological, biological, geomorphological and other data and constructs as is evident in all of my previous work including the A.A.A. paper.

(2) Rigid analyses of patterns found in bodies of prehistoric art and detailed descriptions of the stylistic attributes of individual works within larger bodies are necessary in any study but they are not the *only* things that will throw light on prehistoric thought and social relations—they are *only* the starting point.

(3) The people of Cape Dorset are very different from the prehistoric Dorset Eskimo known to the archaeological record of northern Canada. In 1929 Diamond Jenness analysed a collection of artefacts from Cape Dorset and recognised them to be the products of a group of people culturally distinct from the prehistoric Thule Eskimo or the contemporary Inuit. He named the people 'Dorset' after the area from which the collection was drawn but the prehistoric Dorset lived throughout the Canadian Arctic and beyond and were not confined to one small area. The Dorset are thought to have been an indigenous group that developed from pre-Dorset peoples and flourished for a little over 2000 years between approximately 500 B.C. to A.D. 1500. The Cape Dorset Inuit are descendants of an historic Eskimo group which, in turn, had its origins in Thule peoples that migrated into the area from Alaska after A.D. 1000. It is possible that some Thule and Dorset intermarried but, archaeologically, the prehistoric Dorset became extinct around A.D. 1500. My study focused on prehistoric Dorset Eskimo art and in no way did I suggest links with the contemporary people of Cape Dorset as it would be absurd to do so.

(4) The climatic and other changes (i.e. inward movement of the Thule with consequent displacement) occurred in the final 500-700 years of Dorset existence or between A.D. 800-1500, and not over the past 2000 years.

(5) A.D. 600 should read A.D. 800.

(6) I did not use the term 'Mimi' and the recent, excellent work of George Chaloupka and Darrell Lewis has shown that this term is confusing, confounding and more or less useless in describing the various forms and styles of rock art produced in western Arnhem Land over the past 18 000 or more years. It is antiquated and should be discarded.

(7) A.D. 1450 should read 1500 *B.P.* and I did not say there was a 'great efflorescence of X-ray art' but that X-ray features were increasingly added to more paintings after the beginning of the Freshwater Period or 1500 B.P. There were likely many 'efflorescences' in western Arnhem Land art and the exact nature of changes in X-ray art production is only partially understood at this time. As well, as I pointed out in my paper, X-ray paintings are only part of a larger recent style complex which includes various forms of stick figures, stencils, solid human and faunal representations and so forth.

(8) I do not interpret the changes in art production 'as an attempt to increase spiritual control over the environment in the face of climatic change' although some of the art objects may be involved with this. Indeed, I pointed out that many previous investigations into the art of both areas have concluded this but that this interpretation is insufficient and inaccurate. My conclusions as to 'why the art was produced' are very different from this and I arrived at different conclusions for each area and group studied.

At the moment I am engaged in nine months of fieldwork in Kakadu National Park and adjacent portions of Arnhem Land. After I have finished in February of 1987 I would be in a better position to submit a paper or report to *RAR* should the editor or others deem it desirable. Please forgive me for taking up so much space but I felt it necessary to reply to Moore's summary. After all, accuracy in interpretation and reporting is what all of us strive for in rock art research!

Paul S. C. Taçon

A computerised data base of the known Victorian rock art motifs has been compiled by the Victoria Archaeological Survey. Its use by rock art researchers is welcomed. For further information contact the Site Registrar, Victoria Archaeological Survey, 29-31 Victoria Avenue, Albert Park, Vic. 3206, Australia.

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A discount of 20% is available to AURA members on all publications of the Institute of Archaeology, University of California, Los Angeles. Refer to p. 158 for available rock art volumes.

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Forthcoming Major Rock Art Events

14th ANNUAL SYMPOSIUM OF ARARA, St George, Utah, 23-25 May 1987. This will include a panel debate on rock art conservation and protection, and be attended by a few Australian AURA members, in addition to about forty of our American members.

8th INTERNATIONAL SYMPOSIUM OF AMERICAN ROCK ART, Santo Domingo, Dominican Republic, 8-14 June 1987. American, European and Australian AURA members will present papers.

11th CONGRESS OF THE INTERNATIONAL UNION OF PREHISTORIC AND PROTOHISTORIC SCIENCES, Mainz/ Frankfurt, first week September 1987.

ROCK ART SEMINAR AND FIELD TRIP, Sydney, 12-21 September 1987. Refer to p. 184.

THE WORLD CULTURES OF ANCIENT AMERICA: an international conference on epigraphic and other evidence. Northern spring of 1988, San Francisco. Details to be advised.

FIRST AURA CONGRESS, Darwin, August/September 1988. Refer preceding pages.

New AURA Members

We welcome the following new members who have joined the Australian Rock Art Research Association during the twelve months to November 1986:

Australian National Gallery, Canberra, A.C.T.
University of Adelaide, Adelaide, South Australia
Dr Michel Lorblanchet, Cajarc, France
Mr K. C. DenDooven, Las Vegas, U.S.A.
Mr LaVan Martineau, Globe, U.S.A.
Mr D. G. Blizzard, La Crescenta, U.S.A.
Mr William T. Biskamp, Dallas, U.S.A.
Professor George F. Carter, Bryan, U.S.A.
Ms Sylvia Fein, Pleasant Hill, U.S.A.
Professor M. Jane Young, Austin, U.S.A.
Mr Adrian G. Davey, Canberra, A.C.T.
Dr James L. Swauger, Pittsburgh, U.S.A.
Ms Keo Boreson, Spokane, U.S.A.
Koorie Information Centre, Fitzroy, Victoria
Mr Douglas Mazonowicz, New York, U.S.A.
Mr Rolf Hangartner, Harare, Zimbabwe
Mrs L. M. Taylor, Leigh, New Zealand
Mr Warwick Dix, Campbell, A.C.T.
Library of the Canadian Conservation Institute, Ottawa, Canada
Mr Denis Gojak, Ingleside, New South Wales
Mr Robert S. Brown, Norwalk, U.S.A.
Professor Wang Ningsheng, Kunming, People's Republic of China
Dr Donald E. Weaver, Jr, Flagstaff, U.S.A.
Mrs Angela Moir, Raymond Terrace, New South Wales
Mr David Abrams, Sacramento, U.S.A.
Mr Richard Cosgrove, Hobart, Tasmania
Ms Fran Pease, Burnie, Tasmania
Professor Lawrence L. Loendorf, Grand Forks, U.S.A.
Dr Carl William Clewlow, Jr, Santa Monica, U.S.A.
Mr Paul Clark, Sandy Bay, Tasmania
Mrs Ellen Young, La Canada, U.S.A.
Mr Len A. Unger, Alectown, New South Wales
Mr David Lindeblad, Omak, U.S.A.
Dr William J. Jobling, Sydney, New South Wales
Mrs Helen K. Crotty, Sandia Park, U.S.A.
Mr Trevor Kennedy, Warrnambool, Victoria
National Library of Australia, Canberra, A.C.T.
Mr Sidsel Millerstrom, Berkeley, U.S.A.
Dr William D. Andrus, Claremont, U.S.A.
Professor William J. Burke, Tempe, U.S.A.
Dr Niéde Guldon, Campinas, Brazil
Professor David Gebhard, Santa Barbara, U.S.A.
Mr John W. Green, El Paso, U.S.A.
Professor Pedro A. Mentz Ribeiro, Santa Cruz do Sul, Brazil
Mr William R. McGlone, Albuquerque, U.S.A.
Ms Kristal Buckley, Sandy Bay, Tasmania
Dr Michael Underdown, Kew, Victoria
Mr Campbell Grant, Carpinteria, U.S.A.
Centre de Recherche Interdisciplinaire d'Archéologie Analytique, Bordeaux, France
Dr G. S. Tyagi, Sagar, India
Dr W. Breen Murray, Monterrey, Mexico
Dr Paul G. Bahn, Hull, England
ICCROM Library, Rome, Italy
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Professor Helmuth Fuchs, Toronto, Canada
Tozzer Library, Cambridge, U.S.A.
Mrs Miriam A. Lowrance, Alpine, U.S.A.
Mr Lee J. Thompson, Macquarie, A.C.T.
Col. James G. Bain, Albuquerque, U.S.A.
Dr Eloy Linares Malaga, Arequipa, Peru
Ms Mary Dallas, Balmain, New South Wales
Mr James R. Miller, Prescott, U.S.A.
Mr Patrick F. Carey, Watsonville, U.S.A.
Mr Tom Zell, Prescott, U.S.A.
Mrs Barbara A. Wisemantel, Inverell, New South Wales
Mr James Thomson, Parramatta, New South Wales
University of Pennsylvania, Philadelphia, U.S.A.
Dr E. Breck Parkman, Santa Rosa, U.S.A.
Mrs Rosalie Pearson, Chandler, U.S.A.
Mrs Cheryl Szpak, Bundeena, New South Wales
Mr Colin S. Reid, Kenora, Canada
Dr Bernardo Vega, Santo Domingo, Dominican Republic
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Ms Angelika Geischlaeger, Vienna, Austria
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Mr Robert Neal, Helidon, Queensland

The membership of AURA therefore stands at 311, and it continues to grow steadily.

BACK COVER: Lightning Brothers and their Children. Section of large polychrome panel (red, white, black, brown) totalling about eighty 'children'. Deep carvings densely cover the boulders below. Wardaman Rock Art Complex, Northern Territory, Australia.

NOTES FOR CONTRIBUTORS

Manuscripts of research papers should preferably be from 2000 to 5000 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 five centimetre 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 key words (e.g., 'Petroglyphs - patination - style - Pilbara') placed above the title.

Footnotes ought to be avoided where possible. The bibliography and references in the text should follow the conventions established in most Australian archaeological and anthropological journals, following the style indicated in this issue.

If line drawings are included, they must be larger than the intended published size (by a factor of about 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 formal deadlines, but material intended for a particular issue ought to be available about three months before publication. Galley proofs will not be issued. Each author, or group of co-authors, will receive thirty free copies of their article; additional copies are available at cost.

All correspondence should be directed to:

The Editor
Rock Art Research
P.O. Box 216
Caulfield South, Vic. 3162
Australia

Typesetting by Elfriede K. Bednarik, Archaeological Publications
Printed by Complete Print Shop, Melbourne

DARWIN 1988

FIRST AURA CONGRESS