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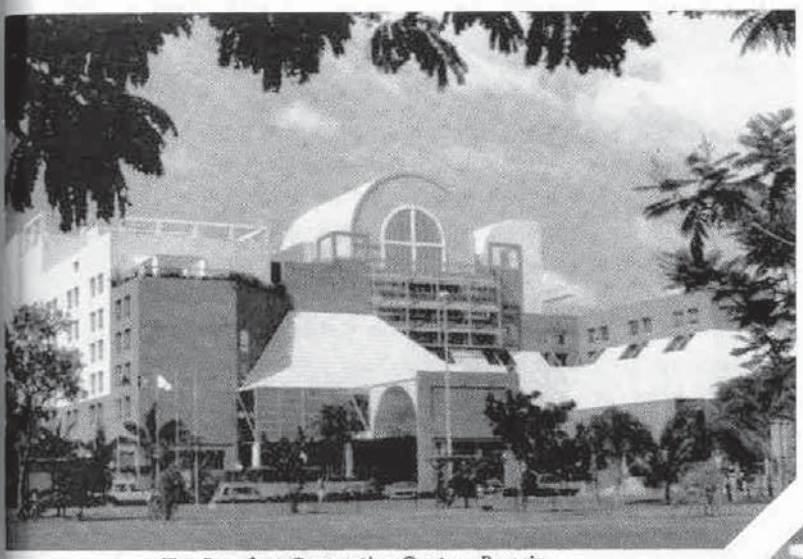
THE AUSTRALIAN ROCK ART RESEARCH ASSOCIATION (AURA)

ROCK ART RESEARCH

Volume 5, Number 2 NOVEMBER 1988



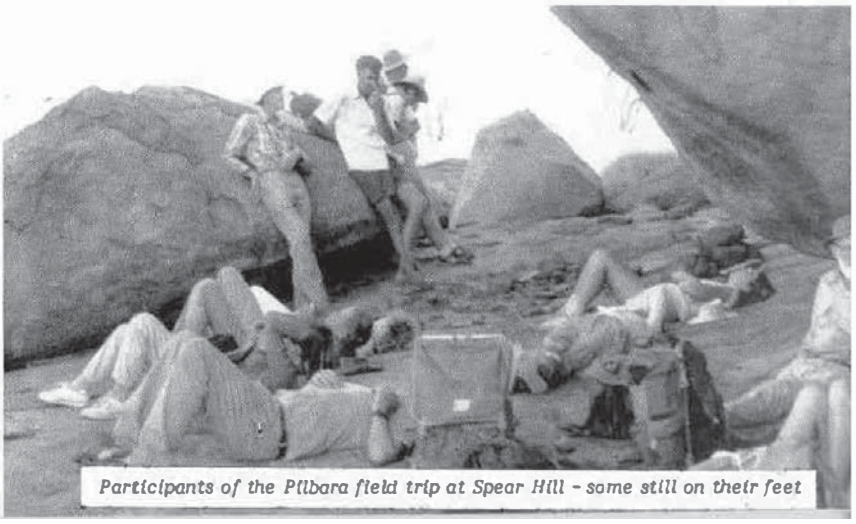
The Beaufort Convention Centre, Darwin

David Mowaljarlai and Warwick Dix (Australia)

DARWIN '88

Three Americans sharing strong common interests: Alexander Marshack, Prof. Whitney Davis and Paul Faulstich

Prof. Chen Zhao Fu (China) in discussion with George Chaloupka (Co-chairman of congress)



Participants of the Pilbara field trip at Spear Hill - some still on their feet

FIRST AURA CONGRESS

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.

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

Archaeological Publications, Melbourne

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KEYWORDS: Middle Pleistocene - Hominids - Engravings - Mark-producing behaviour - GDR

DELIBERATE ENGRAVINGS ON BONE ARTEFACTS OF HOMO ERECTUS

DIETRICH MANIA and URSULA MANIA

Abstract. An occupation floor at the Middle Pleistocene hominid site of Bilzingsleben near Halle, German Democratic Republic, has been dated to the penultimate interglacial. Four bone artefacts found among remains of *Homo erectus* bear indisputable intentional engravings. The markings document the production of systematic patterns, and they provide the first unequivocal evidence that *Homo erectus* produced incipient art—thousands of centuries before the advent of Upper Palaeolithic art. The paper demonstrates the need for reassessing existing concepts about the cognitive faculties of early hominids, and it provides the first tangible evidence for hypothesising about the early human intellect.

The discovery in 1969 of a Lower Palaeolithic archaeological horizon, located in a travertine layer near Bilzingsleben on the northern border of the Thuringian basin (district of Halle, German Democratic Republic; Fig. 1), led to a research excavation by the Landesmuseum für Vorgeschichte Halle/Saale, which is still being continued (Mania et al. 1980; Mai et al. 1983; Mania and Weber 1986).

The archaeological horizon comprises an open occupation site from the Middle Pleistocene and its age is about 300 000 to 350 000 years BP. It dates from the Holstein Complex, which represents the Mindel/Riss interglacial. The finds of ten human skull fragments and seven human molars have been shown to belong to a late representative of *Homo erectus* (Vlček 1978). In the present context, only a brief overview of the archaeological situation is provided, and readers are referred to Mania and Vlček (1987) for further information.

The open air site was situated at the shore of a small lake in the vicinity of the mouth of a brooklet. To date, an area of about 900 m^2 of this living floor has been exposed (Fig. 2; Plate 1). Discrete zones of find associations and special structures could be shown to exist at this open air site. These structures are the remains of three simple dwellings of oval or circular form, each possessing a diameter of 3-4 m. Their outlines were recognised by peripheral accumulations of large bones and stones. A hearth was located to the south-east and in front of them, consisting of charcoal and stones bearing traces of having been exposed to fire. To the south-east, beside each dwelling, were two workshops, indicated by centrally placed anvils. More workshops, manifested by the distribution of stone, bone and wood remains bearing evidence of working, appeared in the large area in

Figure 1.
The location of Bilzingsleben, in the south-west of the German Democratic Republic.

The plates mentioned in this article are on pages 127 to 132 inclusive.

MS received 26 May 1988

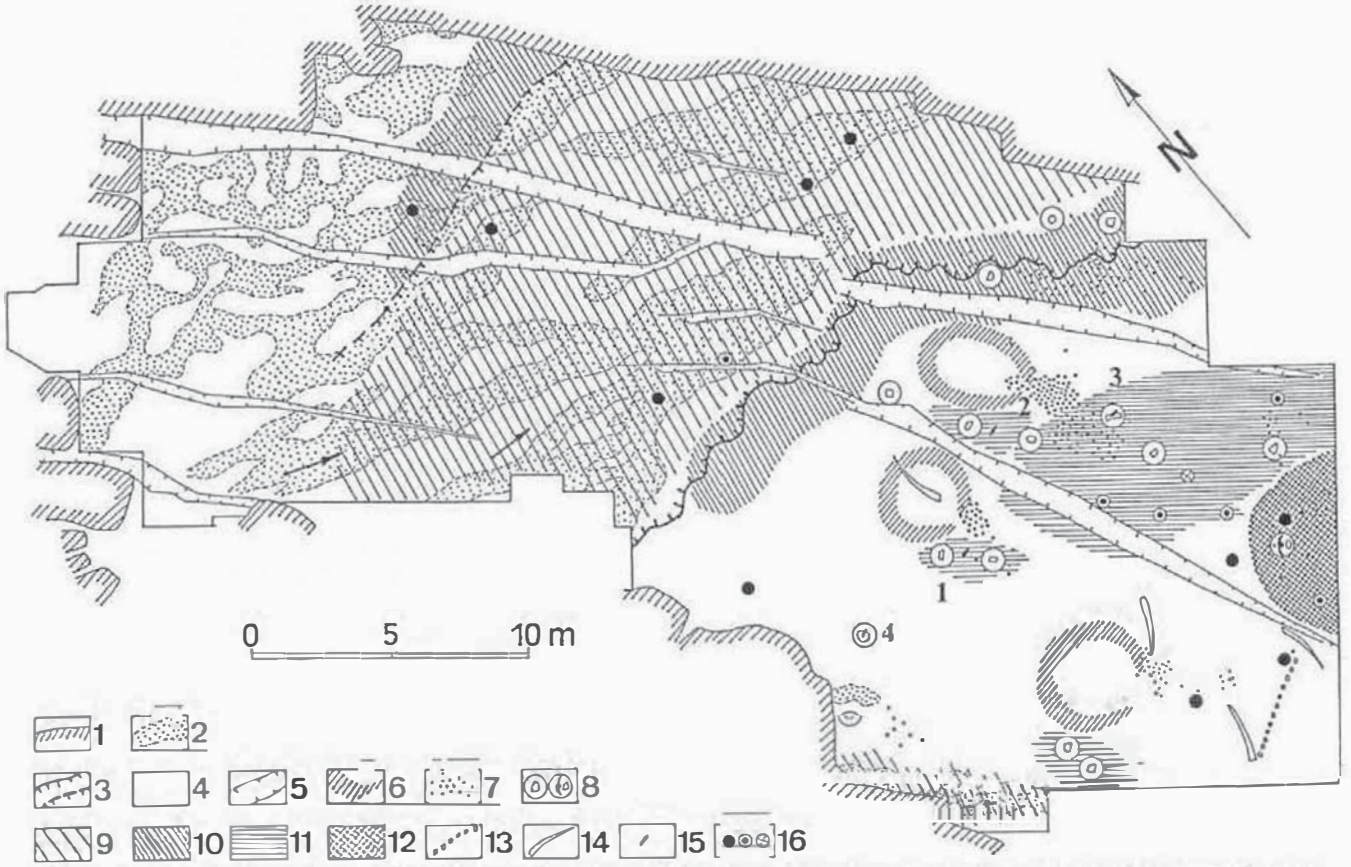

Figure 2. *Bilzingsleben. Plan of Lower Palaeolithic open-air site situated in the Middle Pleistocene travertine layer; the locations of find categories are indicated.*

LEGEND:
1 - Boundary of the quarry situated in travertine;
2 - Creek troughs (arrows indicate direction of flow);
3 - Lake shore in the area of the mouth of the brooklet (proved and assumed);
4 - Shore terrace;
5 - Crevices in the travertine sequence;
6 - Outlines of dwelling structures;
7 - Hearths;
8 - Workshops with anvil erected in the centre (in one case a bison skull was found beside the anvil);
9 - Rubbish heap in the lake basin;
10 - Activity zone at the shore;
11 - Large workshop zone;
12 - Area of pebbles, centrally situated;
13 - Alignment of stones;
14 - Tusks of the forest elephant;
15 - Bone artefacts 1-4, bearing deliberate engravings;
16 - Hominid remains (cranial fragments, tooth, milk molar).

front of the dwelling structures. Around it extended a zone of manufacturing activity where blanks, half-finished pieces, finished tools and manufacturing debris of stone, bone, antler and wood were found.

North-east and north of the dwelling structures, the shore line of the lake could be detected. Here, another activity zone was demonstrated, in the form of a 3 m-wide strip. It is assumed that this area was selected for special activities which necessitated the presence of water (Plate 2).

A rubbish heap of the open air site extended into the adjoining lake. Here, large quantities of fractured animal bones and a great number of artefacts were found. As food refuse, the fractured animal bones occurred less frequently on the occupied shore terrace.

Almost in the centre of the area in front of the dwelling structures, about 5-8 m away from them, a roughly circular floor differed from the archaeological horizon by being free of waste material, half-finished pieces and tools. It consisted of a pavement of small pebbles and bone debris and it appears that the area was marked by this pavement in order to keep it clear for special purposes. Its former function can only be clarified satisfactorily following its complete excavation in the years to come.

The human cranial remains were found partly on the rubbish heap (Individual 1), partly on top of and beside the centrally situated paved area (Individual 2). The tool types are in each case distinguishable by the raw material used for their manufacture. Thus there are large cobble tools, small tools of *silex* [chert] retouched on edges and surfaces, other tools from the thick *compacta* of elephant

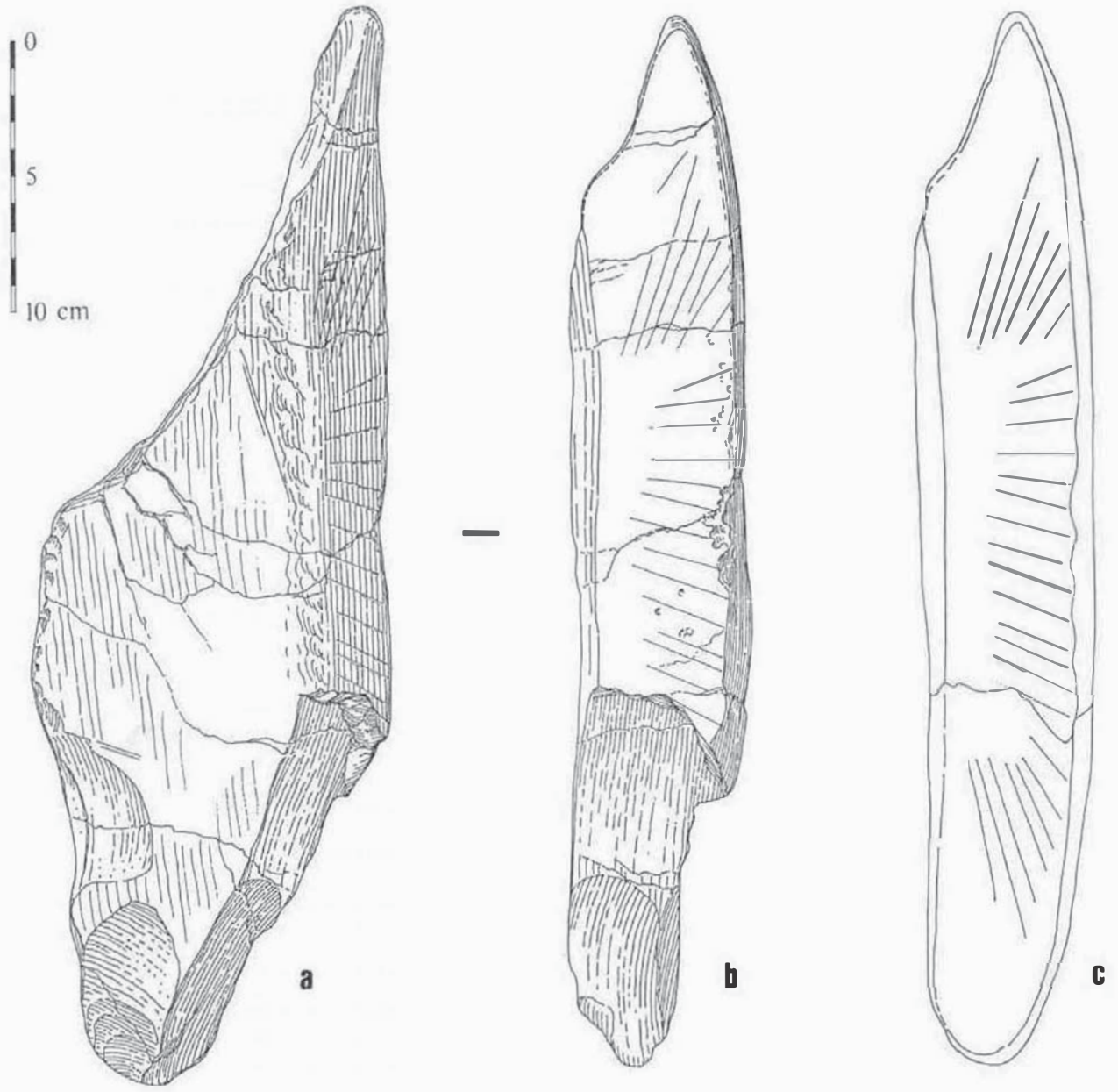

Figure 3.
Bilzingsleben. Artefact 1 with sequence of engraved lines: plan view (a), longitudinal side view (b) and reconstruction attempt (c).

bones determined according to their functions, percussion implements from antlers and remnants of wooden tools. Specific techniques for percussion, splitting and retouching are discernible. Of great interest are some bone artefacts whose surfaces are covered with engraved lines which cannot be explained as incidental or use wear. These engraved lines are arranged in groups or in sequences of single lines. They are the oldest intentional marks known to us at the present time. Significantly, these artefacts always occurred in connection with the workshops. Their preliminary description follows but it is emphasised that microscopic studies of these finds have not been completed yet.

Artefact 1 (Fig. 3; Plates 3-8).

This specimen was found between the workshops of the central dwelling structure. Its length is 395 mm, width 120 mm, thickness 65 mm. It is a tool which was manufactured from the spall of an elephant tibia. Fractures on one longitudinal edge and one end demonstrate its former use as a percussion tool. The second end is pointed and its tip has been markedly rounded by use. The plane, 50-60 mm wide longitudinal surface displays a sequence of straight, single lines engraved into it. This sequence begins at the pointed end with a group of seven divergent lines, which adjoins a central sequence consisting of fourteen single straight lines engraved at regular distances, forming a fan-like arrangement. The first end shows fractures at the edge that seem to enable us to reconstruct the manner in which the sequence of lines continued over the missing part of the artefact. Another group of lines seems to have been arranged adjacent to those preserved and placed symmetrically to the first group (refer to Fig. 3c for a reconstruction attempt). The lines on this specimen are single, straight and regular engravings. Microscopic analysis shows them to be of identical cross-section and groove diameter, which allows the assumption that they were all engraved with the same tool. The sequence of lines appears to have been fashioned in the course of one single process.

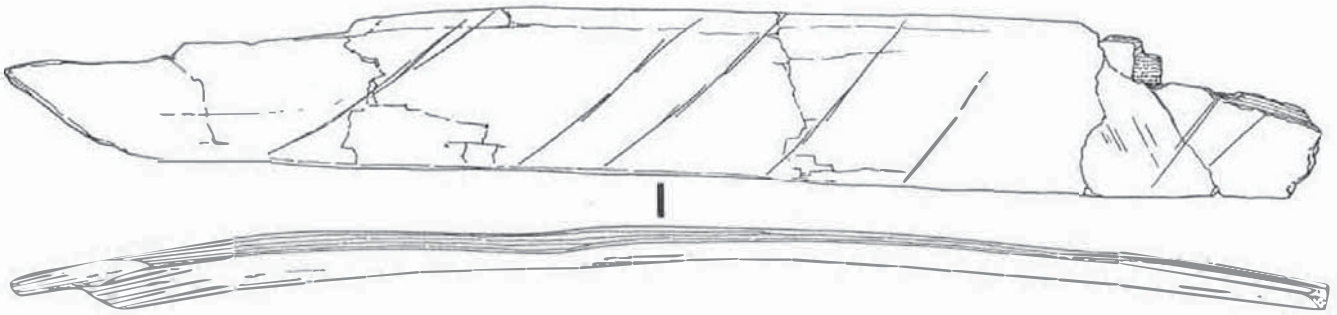

Figure 4. Bilzingsleben. Artefact 2 with diagonal lines.

Artefact 2 (Fig. 4; Plates 9-11).

This artefact was found between the workshops of the northern, oval-shaped dwelling structure. Its dimensions are: length 286 mm, width 36 mm, thickness 5-9 mm. It is the distal part of a flat, slightly curved rib of a larger mammal. Its ends were chopped off, and its surface was partly smoothed and polished. The flat-convex outer surface displays four parallel, 50-60 mm long, oblique lines arranged at different distances. Each marking consists of three single lines of an overlapping order at the ends, forming together one straight mark. Parallel to these four lines, two other but incomplete oblique lines are positioned in keeping with the relative direction of the four. Beginning with the most pronounced of the marks, the distances between the six lines are 60-20-40-40-60 mm, and can be expressed in the ratio 3 : 1 : 2 : 2 : 3. All these lines are morphologically uniform and we infer that a single engraving implement was used in their production. The first line is strongest and was engraved by repeated application of the tool.

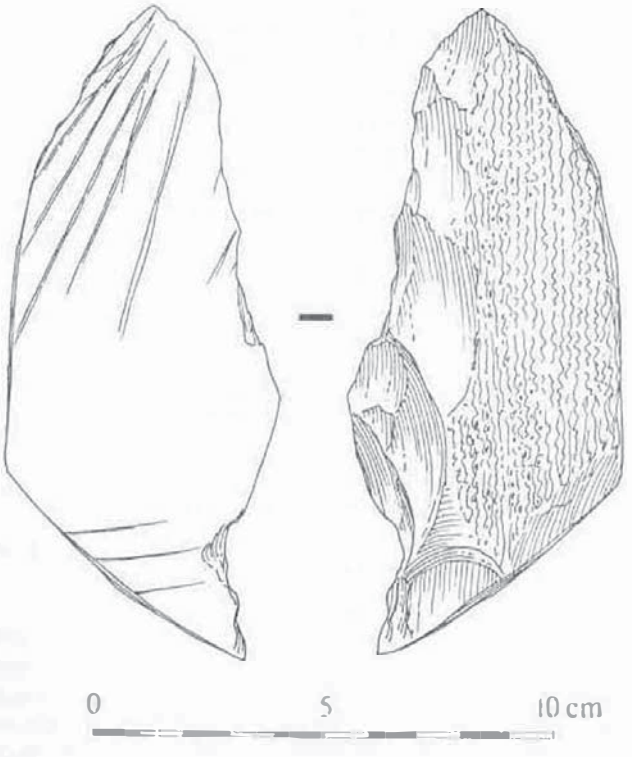

Figure 5.
Bilzingsleben. Artefact 3 with group of lines.

Artefact 3 (Fig. 5; Plate 12).

The third engraved fragment was found lying close to an anvil made from the tibia of an elephant and was used for the working of wood. Its dimensions are: length 141 mm, width 61 mm, thickness 20 mm. The artefact consists of a triangular, flat *compacta* fragment from an elephantine extremity bone. One longitudinal edge bears extensive retouch. Beginning from one end, a group of five straight, up to 70 mm long, divergent lines is engraved on the flat upper surface. Some of the marks consist of double lines, probably due to double engraving in order to achieve stronger marks. All lines display the same cross-section and groove diameter.

Artefact 4 (Fig. 6; Plate 13).

This artefact was found lying in close contact with the upper surface of an anvil made from a travertine block that served for the working of wood and bones. The artefact's length is 114 mm, its width 55 mm, its thickness 9 mm. On the flat-convex upper surface of the thin, flat bone object, seven parallel lines were engraved at regular intervals of 3 mm. The first and fifth element in this series are of a length more than twice (35 mm) that of the other lines (14 mm), and they are both composed of two individual lines, in each case one crossing the other at an acute angle. Microscopic analysis proved all lines to be uniform; they were engraved by the same tool.

These four sequences of simple lines engraved on bone artefacts are among the only marks observed at the site that can be regarded as deliberate, with the greatest probability. 1) Only two more artefacts from Bilzingsleben bear probable intentional marks, consisting also of simple lines. In one case, an arc composed of several lines appears on a quartzite slab. The mark is limited at its base by a transversal line. Secondly, a tarsal bone from the forest elephant (*Palaeoloxodon antiquus*) bears what appears to be an engraved double rectangle on the concave joint surface. These specimens are still to be investigated more thoroughly under the microscope before they will be discussed by us.

Conclusions

The four bone artefacts bearing the rhythmic sequences of lines described above are most important for our understanding of the culture of the Middle Pleistocene *Homo erectus*. It is not easy to

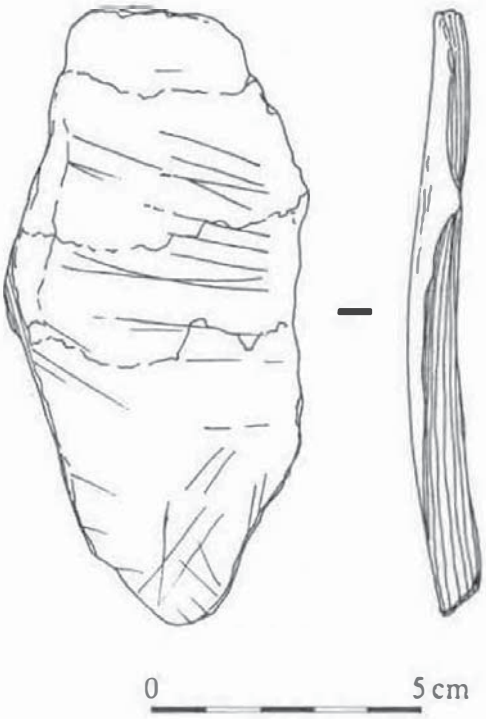

Figure 6.
Bilzingsleben. Artefact 4 with sequence of lines.

infer their meaning and function without being speculative. However, we can observe that they must have had some significance, be it of a communicative, mnemonic or other form. The results of our investigations and observations of other cultural evidence at the site of Bilzingsleben (including artefacts, technologies, dwelling structures, use of fire, hunting operations, pavement situated centrally etc.) permit a number of inferences concerning the culture of *Homo erectus* at this site:

(1) There was an artificially established environment, consisting of a home base with dwelling structures, fire places, workshop areas and specific activity zones.
(2) Artefact manufacture and utilisation of artefacts, indications of anticipatory and preconceived behavioural patterns. The following aspects should be considered:
- Artefact-specific selection of raw material is evident.
- There is differentiation in the functions of different tools.
- Specific concepts and notions of form seem to have been generally and gradually accepted.
- There are specific manufacturing techniques, taking into account the different qualities of the raw materials.
- There is considerable complexity and variety among the technologies applied.
- There are aspects of planned behaviour in the subsistence extraction strategy: organised big-game hunting.

(3) Evidence of specific cultural practices:
- Engraving of rhythmic sequences of linear marks.
- Relationship of these engraved artefacts to workshops.
- Deposition of antler cudgels in pairs within the occupation zone as well as on the lake floor.

(4) Incipient ritual behaviour. It is possible that the pavement situated centrally on the occupation site should be considered in this context:
- Human skulls are smashed (encephalophagy?).

(5) Spiritual world:
It can be inferred from the deliberate engravings on bone artefacts that *Homo erectus* had gained the faculty of abstract thinking. In connection with the above points 1 to 4 it can be argued that a simple concept of the world did already exist in man's mind at the time the Bilzingsleben site was occupied.

(6) Language:
Even the simple culture of *Homo erectus* is not imaginable without the existence of language as a means of communication. Verbal communication is a prerequisite for the creation of deliberate markings which must have conveyed information of some form. Thus its existence is proved by the deliberate marks engraved on the bone artefacts of Bilzingsleben.

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1) G. Behm-Blancke (1987) thinks he recognises angles, angle sequences, circular and curved motifs, grid-crosses (*Gitterkreuze*), double crosses, even the primitive representation of an animal, on some bone artefacts from the site of Bilzingsleben.

Mostly, however, these lines are heterogeneous use wear of a type covering surfaces of all bone artefacts, together with numerous other wear traces, and which consequently must be considered in connection with them. In other cases they are abrasion marks accidentally produced by humans and animals walking on and over the artefacts (Plate 14).

In Behm-Blancke (1987), our four objects bearing engravings correspond to Objects II, 1-4. All the other objects or motifs (I, III-XII and XIV) and the 'representation' of the animal ('*Tierbild*' motif C) have to be regarded with greatest caution. They seem to represent accidental traces. Behm-Blancke (1987: 43) writes that Object II, 2 (which corresponds to our Artefact 1) was altered by a 'recent fracture on one end'. This is incorrect. The fracture was produced by *Homo erectus* when utilising this artefact.

Behm-Blancke also discusses a bear cult which he believes to have been practised at Bilzingsleben. The excavation campaigns have proved satisfactorily that there are no accumulations of particular bear bones on the site of Bilzingsleben but that these remains were found almost uniformly distributed over the whole occupation area. Such explanations should therefore neither be given nor considered before the completion of our excavations.

COMMENTS

By PAUL G. BAHN

I consider this preliminary summary of some of the Bilzingsleben markings to be of crucial importance, but since the reasons for this are self-evident I shall restrict myself to three observations.

(1) More and more pre-Upper Palaeolithic finds of this type are turning up, and they are by no means limited to the brief list circulated by Davidson and Noble at the 1988 AURA Congress, and about to be published by them in *Current Anthropology* (1989). This list omits not only Bilzings-

leben (although these markings have been known for some time) but also Mousterian and Acheulian markings from Italy (Riparo Tagliente and perhaps Alto Cave—see Leonardi 1983; 1987: 118, 122), West Germany (Bocksteinschmiede—see Marshack 1987, 1988a), the U.S.S.R. (Tsonskaïa—see Frolov 1981: 63, 77) and France (La Chaise de Vouthon and at least three other Charente sites—see Debénath 1988: 16; Bouvier 1987: 15).

I think it is clear that far more of this kind of thing must exist but has never been published, either because the excavator did not observe the marks (nonfigurative marks have never attracted much attention among scholars with eyes only for nice animal figures), or because they were dismissed as natural or accidental as there was not supposed to *be* any marking before the Upper Palaeolithic. Careful study of existing Lower and Middle Palaeolithic collections would probably reveal many more specimens, and it is to be hoped that the Bilzingsleben finds will cause more people to be on the lookout for this type of thing in their excavations.

(2) The authors state that Behm-Blancke believes he sees the primitive representation of an animal on a bone artefact from Bilzingsleben, a specimen which they treat with the greatest caution and think is probably accidental. It is worth noting that Feustel (1987: 60) has also mentioned marks on a Bilzingsleben bone that may be the depiction of a large animal (Bahn and Vertut 1988: 208). If the authors' view is correct, it is important that this 'animal figure' be definitively disproved by them as soon as possible. On the other hand, if Behm-Blancke and Feustel were correct, it would make Bilzingsleben even more remarkable than it already is.

(3) I am a little uneasy about the reconstruction in Figure 3c: what is the evidence for assuming that there is another set of seven lines at the broken end, which therefore makes the object's decoration perfectly symmetrical? I would have thought it just as likely that there were no further marks on the broken area, or that the long series continued down. The authors may be correct, and their reconstruction is certainly a possibility, but I would welcome clarification on the reasons for their choice (they mention 'fractures at the edge', but in their reconstruction the seven lines do not reach the edge).

This point is important because the existing marks form an already impressively regular pattern, but if there was the complete symmetry they propose, this artefact would be altogether remarkable for its time.

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By ROBERT G. BEDNARIK

Dating and Authenticity

Two aspects of the Bilzingsleben objects need to be verified before we can consider the profound heuristic effects of this discovery: their dating, and their authenticity.

The Bilzingsleben site has yielded many hominid remains belonging to a *Homo erectus* that is craniomorphometrically most closely related to Asian forms and to one of the Olduvai specimens (Mania and Vlček 1987). Over 100 000 chert artefacts of an evolved Lower Palaeolithic industry have been excavated. Like some of their contemporaries, the Bilzingsleben hominids were specialised big game hunters, 60% of their food remains found are megafaunal. Paleontological, botanical and climatic information renders the chronological placement in Mindel/Riss eminently sensible, and radiometric dating, while remaining vague, also favours the penultimate interglacial (Schwarcz et al. 1988). Geological dating ought to be reliable in the Saale region, after all, this is where the Scandinavian Pleistocene sequence was identified, and two of its glacials were named after rivers close to Bilzingsleben.

Archaeologically it is no doubt desirable to provide more precise dating, but for our purposes the information already available is quite adequate: for exploring cognitive evolution it seems to matter little at this stage whether the engravings are 250 000 or 350 000 years old. It is far more important to consider the repercussions of this evidence.

Whenever a find of such significance is made, it is unfortunately necessary to effectively preclude the possibility that the presenters have become the victims of a hoax or false alarm. This has happened to some of the most judicious researchers in archaeology and prehistoric art studies. In the present case that possibility can be excluded with complete confidence, I believe. The objects have experienced extensive surface modification which clearly postdates the engravings. The markings were manifestly incised with stone tools. An artificial ageing process would result in surfaces that may not quite match those of the many bone fragments from the site, and it would seem impossible that all 24 principal researchers participating in this huge project (many of whom are among the foremost students of their respective discipline) could have all been deceived. Moreover, the objects themselves offer several clues. Some of the surface alterations, such as the minute accretionary specks, would be difficult to reproduce. Many of the engraved lines (on Artefacts 3 and 4) have been intersected by desiccation cracks, which have caused misalignment of the otherwise almost straight lines. This is especially pronounced on Artefact 3, where these lines cross the cracks at several different angles. I have checked the degree of misalignment in all cases and found that they are all consistent with the crack widths they relate to (Fig. 1). I do not believe that the tension cracks could be modern, which means that a forger would have needed to correctly misalign each line. This is possible in theory, but hardly feasible in practice. Also, there are two or three extraneous fragments lodged in the drying cracks.

Finally, a forger would carefully select the motifs to achieve maximum 'impact'. If not very conversant with the types of markings to be expected in a Lower Palaeolithic context, he or she might select some obvious and 'popular' motifs—most li-

kely animal outlines. On the other hand, a well informed hoaxer would probably provide a series of carefully planned, simple patterns that in some way imply symbolic meaning, or contain some other 'message' intended for the researchers.

The markings in fact do convey some common theme, but in a rather stochastic fashion that appears strikingly unmeditated. Indeed, they may not even have all been produced by the same person, especially Artefact 3 illustrates a marking strategy fundamentally different from that of 1 and 4. Conversely, all the marking schemes evident on these objects can be observed among those of archaic cave markings in Australia—which is unlikely to be known among East German hoaxers.

Next, we need to examine the possibility that the Bilzingsleben markings could be the result of some natural or technological process. Natural processes producing marks or patterns on bone surfaces include chemical solution (the patterns of which are determined by the bone fabric), abrasion by clasts and sand grains (which produce indiscriminate striations and erratic lines, patterns I have examined on numerous bone and stone objects) or gnawing by carnivores (this results in distinctive marks that are very different from the Bilzingsleben engravings, and there should be marks on opposite surfaces).

There are basically three potential types of 'technological markings': those caused by testing of stone tools, and incidental marks where a hard surface has been used either as support for some utilitarian process, or where a bone fragment has been used as a tool and rubbed against some material marking it. An explanation as tool testing marks, as once proposed by Clegg and White (1972) in another context, would prompt me to ask: what would such tools have been used for if not for engraving bone? Besides, the mark sequences from Bilzingsleben appear to be more extensive than would be required for testing burin points, and they are systematically arranged.

While Artefacts 1, 3 and 4 do have flat surfaces that could have been used as cutting boards, that possibility can be disregarded in respect of Artefact 1. Not only are its markings arranged in a systematic order, they are on the narrow, bevelled, longitudinal facet, while the flat upper surface bears no markings. The incisions on No. 3 clearly avoid the central part of the available surface, and they do not resemble incidental cutting marks, most in fact run over the edge. This leaves only Artefact 4, a small and thin fragment that is hardly suitable as a cutting board.

Internal Analysis

If we consider the alternative utilitarian hypothesis, Artefact 3 seems the most likely to have been used as a tool rubbed against some pointed stone object. But this explanation becomes less persuasive so closer one examines the lines. They are amazingly straight for their lengths, and five of them were unequivocally made by the same tool. I disagree with Mania and Mania when they suggest the double lines on this specimen could be due to double engraving. It is absolutely impossible to produce such a perfectly spaced second line freehand.

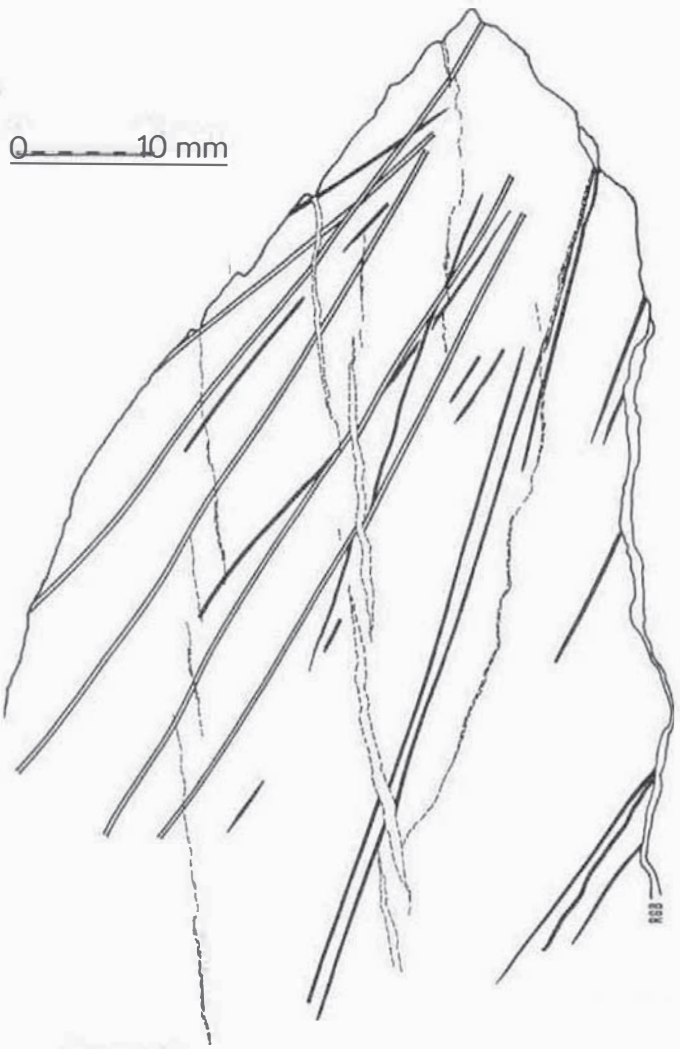

Figure 1.
Marshack-style internal analysis of Artefact 3 from Bilzingsleben. Note the five double lines made by a single tool; the superimposition sequence; and the misalignment of lines where they are intersected by desiccation cracks.

Moreover, the second line is frequently interrupted due to minor unevenness of the surface, varying groove depth, or change in the angle of tool relative to medium surface. The burin point had two minute projections, which were 0.3-0.4 mm apart, measured at right angle to the direction of movement. From the observation that the angle of the tool was altered occasionally and from the straightness of the grooves we can deduce that they were incised with a slow and deliberate movement. A number of other such empirical observations are possible, e.g. that the heavy dorsal retouch on the edge of this bone tool was produced later than the engravings. Some of these impressions, however, need to be confirmed by examination of the actual objects, for which even the excellent photographs are no substitute.

On the subject of identifying striations in engraved grooves I should clarify that while I have succeeded in reliably identifying the type of material used to make engraved lines in Australian limestone caves (Bednarik 1986a, b), and even in confidently identifying the markings made with a single

tool through striation 'fingerprinting', this work related to quite different conditions of preservation. Although the Bilzingsleben bone objects are remarkably well preserved (no doubt due to their location under many metres of travertine, providing, I assume, an unusually high pH), their surface is so corroded that I find it even doubtful that groove widths and sections can be effectively compared to conclusively determine whether the same tool has been used.

Only one of the four objects bears some evidence of repeated tool application in producing individual marks. One mark particularly, on Artefact 2, brings to mind the 'multiple stroking' Marshack (1977: 291) reports on the Acheulian bone from Pech de l'Azé. Interestingly enough it also occurs on a rib fragment of a large mammal. Arcuate elements such as those on Bordes's find are lacking on the Bilzingsleben objects, but Mania and Mania mention 'an arc composed of several lines on a quartzite slab'. I await their description of this with utmost interest.

Evidence of Nonutilitarian Behaviour

Very recently, Chase and Dibble (1987) have reviewed the evidence of a Middle Palaeolithic 'symbolism' at some length, arriving at the conclusion that no such evidence is available: 'there may be isolated and unique occurrences of symbolic behaviour', 'but there is no repeated patterning'. Can there have been isolated occurrences of symbolic behaviour? If there were, does this mean that some prodigy Neanderthals invented and practised symbolic behaviour in total isolation—watched by their bewildered but uncomprehending contemporaries? Yet on the same page, Chase and Dibble (1987: 284) attribute a 'rudimentary capacity for symbolic expression' even to chimpanzees! Is symbolic behaviour intended to be synonymous with Edwards' (1978) 'nonutilitarian activity', or does it refer to the more specific activity of performing an act that has no actual relationship to an intended purpose?

The most frequent evidence cited in favour of nonutilitarian activities in the Lower Palaeolithic seems to be the occurrence of ochre: an apparently deliberately shaped slab of ochre from Ambrona in Spain (Howell 1966), several facetted fragments from Terra Amata in France (de Lumley 1966), lumps in Olduvai Gorge (Leakey 1971) and possibly Zimbabwe (Klein 1978), and evidence of ochre use at Beçov, Czechoslovakia (Marshack 1988b) and Hunsgi, India (Sankalia 1976). But even the bodily application of ochre does not effectively refute a utilitarian function of the substance. Ethnographically, a mixture of fat and ground ochre has been reportedly applied as a protection against sunburn, and the Tasmanians, for instance, protected themselves against the biting cold of their environment by rubbing their bodies with a mixture of muttonbird fat and red ochre (Flood 1983: 171).

The occurrence of fossil casts (Oakley 1981), crystal prisms (Pei 1931) and such curios at Lower Palaeolithic sites has also been cited as evidence of an incipient 'aesthetic sense' in *Homo erectus*—a rather simplistic pronouncement. Nevertheless, this evidence of an awareness of novel phenomena is well demonstrated in the Acheulian, in fact I can add some relevant, as yet unpublished information. I recovered clear quartz crystal fragments from the sole Acheulian site of Austria (Gudenus Cave), which include an artefact made from a large crystal, with several intact crystal facets. It brings to mind the twenty crystal pieces from Chou-k'ou—tien. However, this does not necessarily indicate an aesthetic sense in the makers, it merely suggests an appreciation of the novel and unusual. More importantly, it implies the existence of a taxonomising mind, by indicating that differences were perceived between common and unusual phenomena.

The direct evidence of nonutilitarian activities prior to the Mousterian is thus presently limited to the engraved fragment of an ox rib from Pech de l'Azé (c. 300 000 years old; Bordes 1969); the apparently modified scoria pebble from an Acheulian horizon at Berekhat Ram, Israel, resembling a female figurine (Goren-Inbar 1986); the artificially grooved and pecked phonolite cobble from Olduvai (Leakey 1971: 269); and the elephantine vertebra from Stránská skála, Czechoslovakia (Valoch 1987). The last-mentioned find is of particular interest here: not only is the site less than 500 km from Bilzingsleben, and has also produced remains of *Homo erectus*, the engraved lines are very similar to those on Artefacts 1 and 4 from Bilzingsleben, and even occur on an elephant bone, as do at least two of the four Bilzingsleben engravings. Conversely, the markings described by Leonardi (cited in Bahn's Comment above) are unlikely to be intentional, they include several features typical of natural markings.

It emerges that the finds described by Mania and Mania represent the main body, and almost the sole unequivocal examples, of marked objects predating the Mousterian. They amply vindicate Marshack's and Bordes's courageous pronouncements regarding the Pech de l'Azé object, and they suggest that it may be time to seriously consider the existence of pre-Aurignacian rock art as proposed by Bahn (1984) and myself (Bednarik 1984a). The belief apparently held by most researchers, that art suddenly began with a big bang at the dawn of the Upper Palaeolithic, has no logical or factual basis. The figurines contemporary with the first incipient iconic art of Europe are so extraordinarily sophisticated that they can only be the result of a long tradition (Marshack 1976, 1988), the Berekhat Ram figurine is over 233 000 years old, and the idea that a cognitively unsophisticated neophyte (cf. Frost 1987) suddenly began producing art in western Europe may appeal to French jingoism, but is as absurd as the strange and illogical belief that Franco-Cantabrian art was a cave art (Bednarik 1986c: 41).

Discussion

I believe that the authors have overreached themselves with some of their conclusions, particularly the last three:

(1) If the Bilzingsleben skulls have been smashed (which presumably has been demonstrated) it does not necessarily indicate incipient ritual behaviour. The lack of postcranial remains (Mania and Vlček

1987: 9) is conspicuous. How does it statistically compare with the remains of other species?

(2) Deliberate engravings do not indicate the faculty of 'abstract thinking'. Admittedly, the widespread idea that nonhuman primates produce intentional markings is questionable (except where an environment of prolonged exposure to humans is involved. Has anyone ever observed marking behaviour among apes living in a natural environment? Has anyone reported pongid rock art?). But it is true that nonprimate mammals not only produce rock engravings intentionally, they even do so to convey a clear message to other members of their species; they produce signs (for an example, refer to Bednarik 1986c: 163). If I shared the predilection of many colleagues for positivist language and argument I would say that this proves conclusively that not only can engravings of consistent patterns be deliberately produced in the absence of a human intellect, they can even possess a clear semantic content. By suggesting that the deliberate marks indicate a faculty of abstract thought, the authors may in fact be trivialising their find. Its scientific significance is perhaps primarily that it does not *indicate*, but *foreshadow* such a faculty.

(3) The linguistic ability of *Homo erectus* has been the subject of much conjecture—which remains inconclusive. I have argued that cognitive perception and the use of an image system require no language, that 'the latter may be a considerably more recent development' (Bednarik 1986c: 37, also 44-5) which was preceded by nonverbal communication systems. Certainly, verbal communication is not a prerequisite for creating deliberate markings, it is far more likely a result of such activities.

Each of the four objects Mania and Mania describe exhibits a different marking scheme, but there is a conspicuous common theme which identifies them as examples of a common 'tradition' (using the word without implying more than established, uniform and nonutilitarian behaviour pattern). I see them as unequivocal responses to physical aspects of the artefacts. Psychologically they are responses to the shape of surfaces, perhaps to their edges. The narrow, slightly convex surface of the rib fragment invited a bold design of prominent, oblique lines. The configuration of the convergent lines on Artefact 3 reflects the outline of the implement and clearly focuses on its upper end. The trapezial form of the longitudinal surface on Artefact 1 is mirrored in the perfectly balanced arrangement of the markings. The seven lines near the pointed end of the object are about parallel to the trapezium's oblique side, and the lines near the centre of the decorated facet are roughly perpendicular to its longitudinal edges. *These marks document a response to geometric aspects of the object*, and once a large enough sample of such markings is available it should be possible to draw valid psychological inferences concerning the basis of these responses. Moreover, precisely the same marking scheme is found on the elephantine vertebra from Stránská skála (Valoch 1987: Figs 13:2, 14-5): again, a series of convergent lines is arranged along the edge of the bone, and is in perfect symmetry with the object's overall shape. This observation demands that those of us who have found it difficult to accept that Lower and Middle Palaeolithic people possessed a marking tradition reassess their ideas forthwith, especially as another find has just been made elsewhere (**RAR**, forthcoming).

This does not explain, however, the 'cultural' context of the marks. Without first-hand examination I reserve my judgment on the question of multiple tool use in individual marking sets, but I doubt very much that different people or tools were involved, except as already noted for Artefact 3. Was each object marked by a different person, and were its marks completed in a single process? My answer is a guarded 'Probably yes', to both questions, but this is little more than an opinion.

Having arrived at the speculative part of my Comment, I offer the explanation that *Homo erectus* decorated 'interesting' aspects of the immediate physical environment. Evidence of such a tendency is found in other archaic marking traditions, as I have proposed at the Darwin congress, and it is consistent with the stimulus-arousal theory (Berlyne 1960; Hamilton-Smith 1986) which holds, among other things, that optimal level of arousal is not only neurophysiologically of survival value, but also of hedonic benefit. I have argued that traces of this 'marking impulse' still survive in us today (Bednarik 1988). This interpretation denies the earliest known markings any meaning (or semantic dimension). The initial communication of meaning cannot have been by language, because language production presupposes meaning. But marks could only gain a semantic dimension if the meaning could be conveyed. I resolved this profound impasse by proposing that meaning ('abstract communication', Bednarik 1984b) was first 'conveyed' when *nonfunctional experiences common to all humans* were externalised. This probably occurred unintentionally at first, as perhaps with the Bilzingsleben objects, but once the communicative value of certain arrangements was recognised, the emergence of meaning and various communication forms became possible. Quite reasonably the individuals capable of partaking in these 'games' had an evolutionary advantage and this led to rapid natural selection based on intellect. That, in a nutshell, was the prelude to the Upper Palaeolithic although the details (especially of the correlations between various contributing factors) must have been rather more complex.

According to this model (which has the benefit of massive evidential support, not only of consistent neuropsychological, neurophysiological and logical reasoning) the serious business of intellectual evolution began with 'play', in Halverson's (1987) sense (and human culture is not man's innovation—nor woman's, as it has recently become fashionable to claim; young people, even children, were the probable culprits—cf. Bednarik 1986c: 49; it is inappropriate to cite in this context Upper Palaeolithic evidence, because by that time 'art and culture' had in all probability become 'establishment property' and already served to reinforce social structures, as is the fate of all innovations). It is ironic that Halverson explicitly excludes pre-Upper Palaeolithic marks from consideration in

his art for art's sake hypothesis, because that model is almost certainly valid when applied to the marks of the Lower Palaeolithic. Were they art? I permit myself only one comment: those who cannot give a satisfactory and scientific definition of art (without resorting to that word 'aesthetics') should not attempt to define what art is not. And that seems to include most, if not all of us (refer call for definitions of 'art' at the 1987 Valcamonica Symposium after debate proved inconclusive; cf. *B.C. Notizie* IV, No. 4, p. 9).

The crux of interpreting the Bilzingsleben marks seems to be the purpose, 'adaptive function' (Stenhouse 1987) or evolutionary role of nonutilitarian activities (Edwards 1978) during the 'prereflective phase' (Davidson and Noble 1989) of hominid evolution. Since they relate to developments that are directly responsible for 'humanness' (art, language, consciousness, world views) it seems that we can agree on one point: whatever the precise status of the Bilzingsleben objects in these developments may be, they undeniably document a crucial phase in them, and they may well be more important for our understanding of these developments than any other evidence ever recovered.

Robert G. Bednarik
Editor

By IAIN DAVIDSON

A full assessment of the importance of these remarkable objects would require a critique of the whole archaeological work at Bilzingsleben, which is beyond the space available, and I also do not have to hand the relevant publications. I cannot, therefore, question the dating, although I would have preferred that the authors had made reference to the Th/U and ESR dates (see Harmon et al. 1980; Schwarcz et al. 1988) rather than only the suggested stratigraphic position. Surely there are not many people who will commit themselves to an unambiguous understanding of the attribution 'Mindel/Riss interglacial' any more. After all the site was once said to be dated to the Eemian interglacial and comparable in age to Ehringsdorf. The discussion by Cook et al. (1982: 33) suggests that the chronology is by no means clear, except that it is older than 228 000 years. Schwarcz and his collaborators put the lower limit as 414 ± 45 ka, but concede that the date could be from the interglacial described as either oxygen isotope stage 9 or stage 11 (the Eemian interglacial is stage 5) and the oldest deposits at Ehringsdorf are now dated to stage 7.

It is also possible to question the interpretations of some of the archaeological features, even in the absence of detailed descriptions. These are important issues because the language used about these finds influences our willingness to accept the attributions of significance to the bone artefacts at the end of the paper. Thus we are told a story about a 'living floor', 'discrete zones of find associations', 'simple dwellings . . . recognised by peripheral accumulations of large bones and stones', 'two workshops, indicated by centrally placed anvils' and other such imaginative reconstructions. No doubt there are detailed justifications for these evocative descriptions in the primary sources. Nevertheless, Binford (e.g. 1983) has devoted much effort to demonstrating the fragility of such myths. I simply ask: would it be possible for scientists operating in this school of writing to observe any arrangement of discarded material which could not be given an attribution of such deliberate planning? Supposing we found a chimpanzee archaeological site, how would we describe it? As I say, I cannot go further here, and would require detailed analysis of the original reports to do so. The important point is that by this way of talking about the archaeological evidence we are already softened up for the possibility that these were humans, not hominids. Maybe they were, although I doubt it, but we cannot accept this evidence as argument for the proposition.

Part of the problem is that the bone artefacts themselves are important. The number of objects claimed to be modified in ways unrelated to utilitarian function which can be reliably dated earlier than the Upper Pleistocene is still vanishingly small. Before this discovery they were all single objects from sites widely dispersed in space and time, and with no similarity between the objects. It is hard to imagine that there was any shared meaning for such objects. In the present case, though, the fact that lines were scratched on at least four objects; that all of them have subparallel lines; and that two of them also have groups of radial lines is indeed suggestive that the creatures which made the lines were doing something more than accidental. It is further important that several of the other modified objects predating the European Upper Palaeolithic were also bones scratched with subparallel lines (e.g. Klasies River Mouth, La Ferrassie, Cueva Morín). We have no way of knowing why this was so. It would be comforting, in our problem of confronting the recentness of our non-human ancestry, to think that such objects are the result of actions by humans using a symbolic code, or even counting something, but Noble and I have recently argued that this is unlikely (e.g. 1988, 1989). Such consciousness, in our argument, must have followed the discovery of symbolism through the creation of depictions which resulted from gestures involving mimicry. This is why the language used to describe the discoveries is so important.

Thus I welcome this important publication of the four bone objects from Bilzingsleben, but I reject the interpretations in the conclusions. There is no warrant for the attribution of 'home base', 'dwelling structures', 'workshop areas' and 'specific activity zones'. It is unremarkable, on this evidence, that different raw materials were used for different tools and that there was a 'differentiation in the functions of different tools'. How could it be otherwise? But these claims are all part of the atmosphere which allows the attribution of planning. And where is there any evidence for 'planned behaviour in the subsistence extraction strategy'? There are animal bones, but were the animals hunted, scavenged or neither?

When we turn to the grander claims, the lines

on the bones are in 'rhythmic sequences'. What are rhythmic sequences? Can you produce more than one line in anything other than a sequence? And what makes it rhythmic? We are told of a relationship between the objects and the 'workshops'. Can there be anything other than a relationship?

And this is important because all the time we are being prepared for the big claims. What is the evidence that the hominid (not human) skulls were smashed? Could the taphonomic history of the site lead to the preservation of hominid skulls in a state other than broken? What is the evidence that they were eaten? Surely that sort of claim has been disposed of before and it does nothing to enhance our scientific understanding of the behaviour of our ancestors to throw in such unsupported and unwarranted guesses. But these become part of the claim about the 'faculty of abstract thinking' of *Homo erectus*. I remain unconvinced, as I do about the final claim about language.

As Noble and I have argued, the archaeological evidence does not speak for itself on this matter. Any claim about language must involve a development of theory about the relationship between language and other aspects of communication and perception, and above all about the relationship between these and archaeology. I agree that verbal communication and the 'creation of . . . markings which conveyed information of some form' are inextricably linked, but did *these* markings convey any information? My original interest in the early marked objects was originally excited by the scarcity of them. By what criteria can we accept that information was conveyed through objects which were (because they are) so rare? How could we know that the Tata tooth was an object with symbolic meaning if it was (because it is) unique? My initial answer was that there was no sharing of meaning, as if the social mechanism by which meaning could be shared had not come into existence. It was through my collaboration with Noble that I came to realise that the medium itself, by which meaning can be shared, had not come into existence. Yet any evolutionary argument must posit that selection operates on characters which already exist. In the process by which meaning came to be attributable to marks, there must have been marks to which meaning was attributed. Why, in the absence of meaning, such marks were made we cannot tell. I would suggest that it is likely that marking was a frequent sporadic occurrence, and that most of the several times it occurred in the last 300 000 years no meaning was attributed, because *meaning* did not exist. As Noble and I have argued, only when such marking became depictive, probably through the freezing of imitative gesture, did it become possible for hominids to recognise meaning in marks. The Bilzingsleben bones present a challenge to this argument because there are so many of them, but it is not, I think, fatal. It might be if there proves to be a large corpus of such marked bones from a continuous sequence of other sites from the late Middle Pleistocene. That is rather comforting, as it suggests that the argument that Noble and I have presented is capable of being falsified.

A final quibble. Simply to assert that something is 'not imaginable without the existence of language' is to say nothing. Noble and I have imagined it! If we are going to allow assertions about *our* imaginations as evidence in archaeological discussion, then I will assert that I cannot imagine a creature with language doing so little for so long. I remain unconvinced that *Homo erectus* had language. The finds from Bilzingsleben are too important to be saddled with these unsubstantiated claims.

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By WHITNEY DAVIS

Presenting the earliest known markings by *Homo*, this paper is extremely significant. It is especially important that we are dealing with a *group* of artefacts rather than a single example.

In 1976, Alexander Marshack presented some evidence for, and a number of views about, the existence of a 'symboling tradition' in the Mousterian. All of the marked or otherwise worked artefacts he presented were isolated cases; furthermore, there were no strong similarities among them. Insofar as the existence of a 'tradition' seems to call for repetitions of a morphology, or, at least, for perspicuous relations among the separate artefacts thought to belong to it, Marshack's evidence was not, I believe, particularly forceful—although extremely suggestive. Indeed, recently Davidson and Noble (writing before the appearance of this report) have suggested that no 'traditions' of symbolic mark making—in any strong sense of 'tradition'—existed before the Upper Palaeolithic (Davidson and Noble 1988, 1989; and for a recent review of the range of evidence, Chase and Dibble 1987). The four marked bones from Bilzingsleben tend, I think, to lend support to Marshack's hypotheses. Although further study is required, and I would like to reserve judgment, it strikes me that there are substantial morphological and structural similarities among the marking and sequencing procedures used on the four bones. We may be watching the reproduction of a tradition here.

Nevertheless, we must be cautious. I cannot tell from the report itself, nor from the photographs, whether the authors would wish to see *all* four bones marked by the same tool (= person?), although it is explicitly stated that all markings on *each* bone were produced by the same tool (= person?). (This fact would presumably tell against Marshack's frequently repeated claim that many marking sequences, at least of the Upper Palaeolithic, are 'periodic' and cumulative, that is, produced by several or many people over a long period of time; these mark sequences were evidently produced by a single person in one sitting—but this does not tell against their 'traditional' status, for the mark maker or other mark makers might go on, in another single sitting, to reproduce a similar sequence.) If tradition by definition involves the

participation of more than one individual—the individual's reproduction of a procedure from the past—then this whole matter must be decided before broader conclusions can be reached.

Even if further inspection suggests that the marks on all four bones were made in or belong to a 'tradition', I see no reason to jump immediately to the conclusion that this was a tradition of making *meanings*, that is, that the markings (as the authors claim) have 'significance, be it communicative, mnemonic or other . . .'. We must distinguish what might be called 'mere tradition', the replication of morphologies through learning and imitation, from 'cultural practice', traditions of replicating *signs* and *meanings* (see Davis 1988a, b, n.d.). These terms are used in a special way here, of course, for in ordinary usage 'tradition' involves the reproduction of meanings and values as well as forms and motifs; but I want to point to an important difference. When a typist types a page in a script for a language unknown to him, he is replicating a traditional morphology without partaking of its meaning. Similarly, I might choose to wear a costume I observe others wearing because it suits some unique ulterior purpose of mine, and knowing nothing of its emblematic significance for other wearers. In these instances, tradition has been carried on but meaning has not. Furthermore, tradition may be carried on with a *changed* meaning. Finally, not all artefacts or marks, which might be traditionally replicated, are signs at all; as I have stressed elsewhere, not all style is symbolic (Davis 1988b). There may be no meaning in the mark *to be* replicated.

Unless a convincing decipherment of the individual marks or of the sequences can be put forward, all talk of their 'significance'—that is, signifying dimension—is totally premature. In my opinion, the marks should not be thought of as anything more than 'self-sufficient', to use a term I once applied to some chimpanzee scribbles and to what at that time was the known body of Mousterian scratchings (Davis 1986).

Although made deliberately and for their own sake, self-sufficient marks are still *just marks*. They are interesting to the mark maker just in virtue of their appearance *as* marks (mere morphologies, not 'transparent to' a meaning), or perhaps in virtue of elementary motor-muscular pleasures to be had in the act of making them ('play'). It is a major leap from self-sufficient to what I called 'semantic' marks, that is, marks with a denotational dimension (for example, as notation or depiction). The problem of this leap is the problem of what I would like to call the 'Gricean hump' in hominid evolution—the emergence of intentions and meanings in representation and communication from a pre-intentional or what Davidson and Noble have recently called a 'prereflective' state, the evolution from 'natural' to 'non-natural' meaning (see Grice 1957, 1982; Bennett 1976; Tennant 1984; Davis 1989).

In Marshack's well-known decipherment of Upper Palaeolithic mark sequences (e.g. 1972), what had appeared to many other investigators as merely self-sufficient marks were plausibly shown to denote, functioning as notations of lunar phases. I do not deny that the mark sequences from Bilzingsleben might be open to such decipherment. The morphology of the mark sequence on Artefact 2, for example, seems highly structured. However, in principle self-sufficient marks may possess a great deal of structure—as a function of a particular rhythm of making—without any meaning. (The typist typing an unknown script is not for that reason alone necessarily typing in a simple, undifferentiated or unpatterned manner.) For both empirical and theoretical reasons, then, I would resist the temptation to construe these objects semantically.

I am not entirely certain what the authors have in mind in claiming that the marks document a 'faculty for abstract thinking' in *Homo erectus*. Such faculties should be precisely *at issue in* a genuinely evolutionary analysis—not imported into such analyses from off-stage as an explanation for the data. What is the empirical justification for this claim? The drawings of great apes exhibit a considerable degree of morphological structure (see references in Davis 1986). For example, apes rhythmically repeat lines spreading out from a central node in a 'fan' pattern, or circle around and around the initial trace of the first gesture to create vortex formations. In itself such a structure is not evidence for 'abstract thinking'. It can be economically explained in terms of the ape's simple sensitivity to the location of the centre and edges of the surface to be marked and in terms of the rhythm of manufacture itself; in other analytic vocabularies, we can think of the ape's perceptual *Gestalten* as governing its production (see an excellent discussion in Whiten 1976). In either case, there is no need to invoke some 'faculty for abstract thinking', like planning ahead, to account for these morphologies. (I am not, of course, denying that *some* ape drawings might have a semantic dimension, for the ape; but this has to be established on other grounds than the structure of the morphology produced.) Even among young children, as Norman Freeman (1980) has elegantly demonstrated, 'planning' of drawing develops relatively late in what is otherwise a process regulated by what Christopher Alexander (1962) has identified as intrinsic psychological effects like 'levelling and sharpening' of form; as Diana Korzenik's (1977) experiments suggest, the more mature child 'plans' in response, often, to particular communicational tasks—that is, when he or she must *use* drawings, for which he or she has already developed strong and resistant schemata, to put across highly specific messages not well served by the mere reproduction of the stereotype.

If the authors mean that the mark makers of Bilzingsleben exhibit preferences for orderly pattern—for 'abstract' qualities of 'design'—I would perhaps agree; but these kinds of preferences are well documented among the great apes, and have rarely, among primatologists, been taken as any evidence, in themselves, of 'abstract thought' (see again Whiten 1976).

In the light of these comments it will be no surprise that I am not persuaded, at least on the evidence of the marks alone, that the mark makers of Bilzingsleben possessed 'language as a means

of communication'. As I have already noted, in and of themselves the marks do not prove that they informed their makers *about* anything, that is, were semantic. Moreover, that a mark might inform its maker about something is still not in itself proof that this information was communicated or communicable. As John Searle (1986) has recently reminded us, the representation of information to and for a particular maker precedes the communication of that meaning to others and is not in itself a guarantee that communication is intended or will be successful. In their vigorous recent polemic, Davidson and Noble (1988, 1989) have gone so far as to suggest that the tracing of gestures in marks, and the production of depictions, must be the basis *for* a language of shared meanings. From their vantage point, then, the marks of Bilzingsleben are precisely part of the evolutionary prehistory of language. Although I disagree with their account of the origins of depiction as such (Davis 1989), they are right to avoid assuming that the making of meaningful marks is either a product of or evidence for language.

To my mind, claims about the marks as 'signifying', as documenting a 'faculty for abstract thought', and as evidence for 'verbal communication' are unnecessary. For myself, I am interested in the possibility that it is precisely in the making of marks, or replication of other activities, that signification and abstract thought were evolved. It would therefore be mystifying or tautological to account for the properties of the marks by invoking some background of signification or abstraction. Even if the direction of analysis I am advocating does not completely pan out, the marks of Bilzingsleben, as described, do not justify the authors' stronger claims—as I have tried briefly to suggest in this Comment. By all of this I do not mean to gainsay the importance of the evidence presented in this report. The marked bones are, I suspect, strong evidence for the nature and evolution of replicatory activities in hominid evolution. As such they should have a crucial role to play in a nontautological account of the evolution of human cognition.

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By ALEXANDER GALLUS

In reading this paper several subjects immediately impose themselves for reconsideration, made the more urgent in the light of recent discoveries in Pakistan.

(1) *The position of* Homo erectus *in space and time.* Dennel, Rendell and Hailwood (1988), after reporting Late Pliocene in situ stone artefacts, c. two million years old, mention several implications. I quote:

(a) '*Homo habilis* was distributed as far east as Pakistan'. Note: This is hardly credible.

(b) '*H. erectus* is an Asian lineage at least as old as *H. habilis*'. Note: This is recommendable. Leakey, Tobias and Napier denied a 'lineal connection' between *Homo habilis* and *Homo erectus* (quoted in Howells 1973: 63); see also the useful list of the oldest known artefacts and of the oldest known hominid remains, in Dennel et al. (1988: 98).

(c) 'Another as yet unidentified toolmaker is responsible'. Note: This is hardly the case. Moreover it is an argument *ex silentio*.

(2) *The nature of the association between* Homo erectus, *toolmaking and 'deliberate engravings'.* The association between toolmaking and 'deliberate engravings' links the German finds with *Homo erectus* and Australia. If one regards this as ethnospecific then one has an argument for the presence of *Homo erectus* in Australia. In that case the noted similarities of behavioural symptoms in Europe and Australia may still become explained by a diffusion model—not of behavioural elements, but as the symptoms of a centrifugal expansion of *Homo erectus* lineages westward and southward, reaching peripheral positions from an Asian centre.

In Europe the Acheulian and Mousterian have provided 'missing links' for the engraved and finger-shaped 'art' of *Homo sapiens sapiens* (e.g. Marshack 1976: Figs 5-7, 9-12).

The engraving on the 'nonutilitarian' rib bone from the Acheulian level of Pech de l'Azé (dated c. 300 000 BP) is attributed to a late *Homo erectus* (Marshack 1976: 278, Fig. 12). The 'nonutilitarian' Mousterian bone from La Ferrassie was deposited in a Neanderthal grave (op. cit.: 276, Fig. 6). It can be approximated to Figure 3 in the article under comment. The engraved Mousterian 'plaque' from Tata in Hungary was shaped from a mammoth molar and ochred red (op. cit.: 277-8, Figs 9-11). The 'nonutilitarian' bone from Bulgaria with an engraved zigzag motif is also Mousterian (op. cit.: 277, Fig. 7). See also remarks on the Mousterian of La Quina (op. cit.: 276, Fig. 5). The smaller pendant is engraved with small convergent lines towards its point, like Figure 5 in the article under comment.

In Australia an archaeological time depth linking *Homo sapiens* to *Homo erectus* remains elusive, but this hiatus may be self-inflicted by researchers. Field work in the relevant geological and stratigraphic levels (the terrace systems of rivers including the oldest terraces, and excavations in deep cave deposits) has been strangely neglected.

Engraved and 'finger-fluted' parietal 'art' in Australia certainly predates the same behavioural phenomenon in Europe, and the presence of Java man so near to Australia gains new significance in the light of the present debate.

(3) *Palaeopsychological interpretation.* I agree with Bednarik (1986b) that the question of 'derivation' has still preference over questions about 'meaning' and that the quest for 'meaning' is actually the quest for 'the emergence of human cognitive perception and thus human intellect', i.e. it is an evolutionary question of the highest order.

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By JOHN HALVERSON

The discovery that *Homo erectus* deliberately made engraved marks is important and interesting. What are we to make of these marks? More specifically, what are the cognitive implications, concerning which the authors come to such far-reaching conclusions? The marks do imply some sense of rhythm and symmetry, but I do not see how we can infer much more than that. The claim that they indicate abstract thinking and language seems to me less inference than a leap of faith based on the gratuitous assumption that the markings '*must* have conveyed information of some form', that 'they *must* have had some significance, be it of communicative, mnemonic or other form' (italics added). Where does this 'must' come from? I hope the authors will address this fundamental question, for I see no reason to suppose that the markings need have had any purpose or effect beyond the mild satisfaction of 'circular reactions'. Rhythmic activity and repetitive behaviour are virtually universal animal characteristics: they need not be mindful, purposive or even conscious. *Homo erectus* may well have had the rudiments of operational intelligence, as Wynn (1979) has persuasively shown from an analysis of tool making. But in the case of tools we can be certain of a purpose and goal, presupposing both a 'mental template of "form" ' and a 'procedural template directing processes through time' (Gowlett 1986: 251), that is, active cognition. The case of markings is quite different. We have no way of knowing if the final results seen on the Bilzingsleben objects were envisioned or planned from the beginning, that is, whether any cognitive templates were involved, since the results are not recognisable representations, such as pictures, or recognisable artefacts, such as tools. The marks may be simply repetitions of an essentially meaningless action having no symbolic intent or function, but giving a 'protoaesthetic' pleasure based on motor rhythm and visual symmetry. In an evolutionary perspective, the latter seems to me the more plausible interpretation.

If the markings did not, and were not meant to, 'convey information', then verbal communication is not a prerequisite. To me (and I am certainly not alone), 'the simple culture of *Homo erectus*' is quite readily imaginable without verbal language. No part of that culture as it is now known, including tool making, requires any communication that could not have been easily managed by gesture and imitation, the power of which seems often greatly underestimated. Even in our own extremely verbal culture it is remarkable how much we use communicative gesture and indeed rely on it. Refined and elaborated in a nonverbal culture, gestures could almost certainly convey as much information, at a concrete level of reference (abstract concepts are much more difficult), as words. Even if the markings did have symbolic value (which, I repeat, there is no reason to suppose), it could probably have been adequately conveyed or explained by gesture, depending on the complexity of meaning or reference involved. And a 'simple culture' of the Middle Pleistocene hardly presupposes much symbolic complexity.

'Incipient ritual behaviour' is suggested as a possibility, though the suggestion is not based on the markings but rather on site characteristics. No doubt there is always that possibility, but there are other possibilities on equal footing, or lack of it. Ritual behaviour arises and derives from repetitious behaviour, and in some contexts (e.g. ethology and psychopathology) ritual behaviour may still refer to 'meaningless' repetition, but in an anthropological context ritual implies an ideational content, as is clearly implied here by the notion of a 'simple concept of the world' attributed to *Homo erectus*. But attributed on what grounds? Even the extremely thin evidence of Neanderthal burials is completely absent from Acheulian culture. Perhaps it *can* be argued from points (1) to (4) that some idea or concept of 'the world' existed, but it is not argued here, and it is not easy to discern how it might be.

In short, the first three sets of inferences seem reasonable, but the last three seem as far-fetched as they are far-reaching. There seems to me nothing in the markings even to suggest, let alone require, abstract thought, language or a world concept.

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REPLY

By DIETRICH MANIA and URSULA MANIA

Es ist für uns wichtig, feststellen zu können, dasz in fast allen Kommentaren nicht bezweifelt wird, dasz es sich bei den Ritzlinien und Strichfolgen der beschriebenen vier Objekte um etwas anderes handelt als lediglich um zufällige Arbeitsspuren (Fälschung ist ausgeschlossen, wie R. G. Bednarik selbst allein mit Hilfe der Fotografien der Objekte feststellen konnte). In der Frage nach der Bedeutung dieser Ritzungen gehen unsere Meinungen auseinander. Hier sind wir wohl an jenem Punkt angelangt, wo die Beweisfähigkeit des Materials aufhört. Die Kommentatoren sehen lediglich die vier beschriebenen Objekte, losgelöst aus ihrem Fundzusammenhang und dem gesamten Fundkomplex. Wir aber sehen sie nach einer 15-jährigen Zeit der Ausgrabung und Materialbearbeitung in diesem Kontext. Allein aus verschiedenen Befunden, wie den Behausungsstrukturen, der Feuernutzung, dem Nachweis verschiedener Aktivitätszonen und spezieller Arbeitsplätze, aus den verschiedenen Arbeitsprozessen und Handlungsweisen, läszt sich für den geologisch (Ende des Mittelpleistozäns) wie phylogenetisch späten Vertreter des *Homo erectus* von Bilzingsleben ein vergleichsweise hoher Entwicklungsgrad bewuszter Handlungen erschlieszen.

Einerseits waren diese Menschen in der Lage, mit Überlegung ganz gezielt den Schaft eines

Langknochens vom Elefanten längs zu spalten, indem sie in einer geradlinigen Reihenfolge einen Keil aufsetzten und einschlugen, andererseits sollen sie bei dem Einritzen regelmäsziger Strichfolgen auf so harter Unterlage, wie Knochen es sind, sich nichts gedacht haben. Als gedankenlose Spielerei könnte man ein Verhalten ansehen, wenn beispielsweise nebenbei spielerisch Linien und Muster mit dem Finger oder einem Stock in eine Sandfläche oder auf eine andere glatte Bodenfläche gemalt werden, aber nicht, wenn dazu ein Silexgerät und ein kräftiges Eindrücken in die harte Oberfläche nötig sind, damit die Spuren sichtbar werden, und das in regelmäsziger Abfolge und Anordnung. Hier musz auf einen ganz wichtigen Zusammenhang hingewiesen werden: Kleinkinder und Menschenaffen—letztere unter dem Einflusz der Umgebung in menschlicher Gefangenschaft—sind in der Lage, in gedankenloser Spielerei Linien und Linienmuster zu erzeugen. Aber sie haben keine Kultur, die sie selbst verfertigen, und sei es auch nur in dem mässigen Umfange, wie sie zu verfertigen der *Homo erectus* von Bilzingsleben bereits imstande war. Diese Kultur fand ihren Ausdruck in den einfachen Wohnbauten, in der Nutzung des Feuers, der gezielten Werkzeugherstellung und Bearbeitung anderer Rohstoffe, in gewissen Formvorstellungen. Deshalb müssen die Ritzmuster, die er auf den vier Knochenartefakten hinterlassen hat, auch anders bewertet werden als jene von Kleinkindern und Affen.

Besondere Bedeutung hinsichtlich ihrer Intentionalität messen wir den Linien des Objekts 2 zu. Die Oberfläche der Rippe, in die sie eingeritzt waren, ist leicht überschliffen und poliert. Die Linien bestehen jeweils aus drei, hintereinander angesetzten Teillinien! Das ist allein bei vier dieser diagonal-parallel zueinander verlaufenden Linien zu beobachten. Ohne eine Absichtlichkeit läszt sich das nicht erklären. Reine, unbewuszte Spielerei ist es wohl auch nicht, wenn auf dem Objekt 1 einmal das Strichbündel am Ende des Artefaktes, zum anderen die aufgefächerte Strichfolge auf der anschlieszenden flachen Längsseite eingeritzt wurden. Es waren besondere Lageveränderungen des Stückes und Griffhaltungen der Hand, vielleicht auch Arretierungen notwendig, damit die Striche derartig regelmäszig und tief eingeritzt werden konnten. Diese Handlung ist unseres Erachtens nur durch Absichtlichkeit erklärbar. Dasselbe gilt auch bei den anderen Stücken, besonders dem kleineren Objekt Nr. 4: Um die Ritzungen mit einem Silexgerät auszuführen, muszten die Knochenartefakte jeweils bewuszt in eine besondere Lage gebracht und absichtlich festgehalten werden.

Auszerdem handelt es sich bei allen vier Objekten um besonders zugerichtete Knochenartefakte und nicht um willkürlich gebrochene Knochensplitter aus dem Abfallhaufen. Es ist ausgeschlossen, dasz die Strichfolgen und Linienmuster auf diesen Artefakten, nach den besonderen Vorbereitungen, die zu ihrer Einritzung nötig waren, absichtslos und unbewuszt ausgeführt worden sind. Auf ihre besondere Bedeutung kann auch die Lage der vier Objekte hinweisen: Sie befanden sich immer in Verbindung mit eindeutig erkennbaren Arbeitsplätzen: Objekt 1 und 2 lagen jeweils zwischen zwei dicht nebeneinander liegenden Arbeitsplätzen mit Ambossen aus Muschelkalkblöcken, wie sie vor zwei Wohnbauten gefunden wurden. Auf diesen wurden besonders Knochen zertrümmert, aber auch bearbeitet. Objekt 3 lag dicht neben einem Ambosz aus der Tibia vom Waldelefanten. Diese war mit Knochen und Steinen abgestützt und unterfüttert. Sie diente als Unterlage zur Holzbearbeitung, nach Aussage zahlreicher Abfälle und stangenförmiger Artefakte aus Holz, sowie von Steingeräten, die zu ihrer Bearbeitung verwendet wurden. Objekt 4 lag, mit seiner geritzten Oberseite nach unten, im Kontakt auf einer groszen Travertinplatte, die nach den Arbeitsspuren auf ihrer Oberfläche und den Geräten und Abfällen dicht neben ihr als Ambosz zur Bearbeitung von Knochen und Holz gedient hatte.

In bezug auf das Objekt 3 sind wir Bednarik's Meinung. Es ist einleuchtend, die Doppellinien dieses Strichbündels auf die gezahnte Schneide eines Werkzeugs zurückzuführen. Bei geringer Drehung des Werkzeugs veränderte sich jeweils der Verlauf der Doppellinie, sodasz wir—bei genauer mikroskopischer Betrachtung—auch die äuszere, in ihrer Form etwas abweichende Doppellinie zum Strichbündel zählen müssen und nicht zu einer anderen Ritzungsphase, wie es Bednarik auf einer Skizze darzustellen versucht.

Wir hoffen, dasz wir mit diesen Ergänzungen zu unserer vorläufigen Mitteilung über die Knochenartefakte mit Ritzungen von Bilzingsleben—sie sollen in naher Zukunft eine eingehende systematische Untersuchung erfahren—zeigen konnten, dasz sie nur im Zusammenhang mit dem gesamten archäologischen Befund und dessen ökonomischen, ökologischen, biologischen, soziologischen und kulturellen Aspekten untersucht und gedeutet werden können.

Drawings and photographs:
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English Synopsis

In their Reply, Dietrich and Ursula Mania acknowledge first that the commentators unanimously accept the Bilzingsleben markings as deliberate engravings, rather than seeing them as incidental traces of some utilitarian work process. Concerning the meaning of the markings they note that the objects themselves probably allow no reliable deductions. However, the commentators could only consider these four objects in isolation, removed from their archaeological context, while the authors view them with the insight they have gained during their 15-year excavation and research project at Bilzingsleben.

Mania and Mania reiterate that various strands of evidence at Bilzingsleben tend to credit the site's geologically and phylogenetically late representative of *Homo erectus* with a comparatively developed ability to act consciously. For instance, the method of splitting elephant longbones by repeated application of a wedge along a straight line implies planned behaviour. The meticulous production of regular engraved patterns on the hard surface of bone tools cannot be equalled to the playful patterns infants and nonhuman primates might produce, and which postulate no culture.

The intentionality of the Bilzingsleben engravings is well exemplified by Artefact 2. Each of its four diagonal lines consists of three separate grooves. Simple unconscious play can hardly account for the structured line sequence on Artefact 1. The arrangements on all four bone artefacts indicate that the positioning, of either the engraved object or the burin used to

incise the lines, must have been changed during the engraving process. This seems to postulate deliberate action. Moreover, the four objects are not merely discarded bone fragments, each one is an especially modified bone artefact. They were all found in clearly identifiable workshop contexts: Artefacts 1 and 2 were among workplaces with stone anvils on which bones were smashed and worked; Artefact 3 was adjacent to an anvil of an elephant tibia which was underpinned by bones and stones, and used for the production of wooden implements, as shown by wooden debris and stone tools; and Artefact 4 lay, engraved side down, on a travertine plate which had also served as an anvil, as indicated by use wear traces on its surface as well as by the artefacts and debris of bone and wood found adjacent to it.

Concerning the lines on Artefact 3, Mania and Mania agree with Bednarik's interpretation of the double lines as the result of a notched burin point. They conclude their Reply by announcing their intention to present a more exhaustive and systematic analysis of the Bilzingsleben engravings in the near future. rgb

Résumé. *La couche d'habitation au gisement du Pleistocène moyen de Bilzingsleben, près de Halle, G.D.R., a pu être datée à l'avant dernier Interglaciaire. Quatre objets en os trouvés parmi les ossements de l'Homo erectus portent des traits gravés intentionels indiscutobles. Ces traits gravés témoignent la production de tracés systématiques, et nous apportent pour la première fois une preuve sans équivoque que des milliers de siècles avant le Paléolithique supérieur, l'Homo erectus a pratiqué l'art. Les données présentées dans cet article manifestent la nécessité de réexaminer les idées courrantes sur les facultés cognitives de ces anciens hominiens, et elles nous apportent les premières données solides qui nous permettent de construire des hypothèses sur les débuts de l'intelligence humaine.*

Zusammenfassung. *Eine Bewohungsschichte der mittelpleistozänen Hominiden-Fundstelle von Bilzingsleben, nahe Halle, Deutsche Demokratische Republik, ist der penultimaten Zwischeneiszeit zugewiesen worden. Vier unter den Überresten des Homo erectus gefundene Knochenartefakte tragen unbestreitbare intentionelle Gravierungen. Die Zeichen weisen die Herstellung systematischer Muster nach, und sie liefern die ersten unzweideutigen Beweise, dasz Homo erectus, Jahrhunderttausende vor dem Beginn der Kunst des Oberen Paläolithikums, Proto-Kunst schuf. Der Artikel veranschaulicht die Notwendigkeit, bestehende Konzepte über die kognitiven Fähigkeiten früherer Hominiden zu revidieren, und er bietet das erste überzeugende Beweismaterial, mit dem auf den frühen menschlichen Intellekt geschlossen werden kann.*

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- Publication of an annual *Boletín* with international news items, and including articles on the rock art of Bolivia and other South American countries, and a current bibliography. All publications include a detailed English summary.
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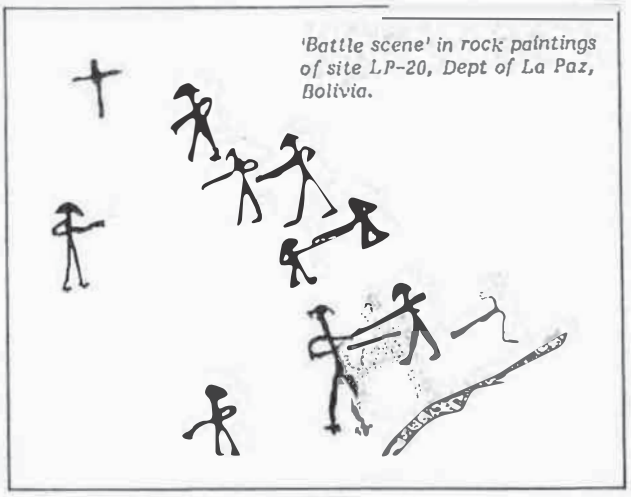

'Battle scene' in rock paintings of site LP-20, Dept of La Paz, Bolivia.

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KEYWORDS: Petroglyphs - Rock varnish - Cation-ratio dating - South Australia

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

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Abstract. The (K+Ca)/Ti cation-ratio (CR) of rock varnish provides a calibrated-age when CRs of varnishes are correlated with radiometric ages of varnishes in a region. In the Karolta area of arid South Australia 24 petroglyphs are assigned chronometric ages based on a semilog least-squares regression (a cation-leaching curve) of varnish CRs and accelerator mass spectrometry radiocarbon ages. The details of the CR method of dating petroglyphs are outlined. Mean CR ages of 24 selected petroglyphs range from c. 1400 to c. 31 500 years BP. The selection of petroglyphs for analysis was random, within the limits of selecting an appropriate suite of motifs. The results of radiocarbon-calibrated cation-ratio dating indicate that the style of the petroglyphs at Karolta has remained largely unchanged for about 30 000 years.

Introduction

Surface remains comprise a substantial part of the archaeological record (Bowler 1971; Maynard 1979; Binford 1980; Bettinger 1982; Rosenfeld 1982; Smith 1987). They are especially noticeable in arid regions, consisting of rock art, stone tools and flakes, and geoglyphs. These cultural remains have long been excluded from serious consideration by archaeologists, due in part to the lack of chronological control and hence the inability to relate these remains to an established cultural sequence. The requirement of a good 'stratigraphy' is important in that the sequence of cultural horizons can be clearly understood, but subsurface stratigraphy is not required when age control can be obtained for surface artefacts.

A method of dating surface artefacts, cation-ratio dating, has been developed for use in arid and semi-arid regions, based on the microchemistry of rock varnish. Rock varnish is a thin coating of manganese and iron oxyhydroxides, clay minerals and trace elements that forms a stable coating in arid-alkaline regions (Potter and Rossman 1977; Dorn and Oberlander 1982). Its development on artefacts and petroglyphs has long been used in a qualitative, visual way (e.g. Basedow 1914; Heizer and Baumhoff 1962; Grant 1967). While the visual parameters of colour and apparent thickness are not strictly related to time, two changes in the chemistry of rock varnish have been tested under controlled circumstances: radiocarbon content and changes in the cation-ratio (CR) of (calcium+potassium)/titanium.

Both accelerator mass spectrometry (AMS) radiocarbon and cation-ratio dating of rock varnish are needed to estimate ages of petroglyphs. AMS radiocarbon dating provides numerical-age control (dating terminology from Colman et al. 1987) for the exposure of a geomorphic surface. The exposure of a geomorphic surface could, alternatively, be provided by conventional radiocarbon, uranium-series, K-Ar or other methods, if available. The key is that an age for the subaerial exposure of a surface is needed in the local region where the petroglyphs are to be dated. AMS radiocarbon dating of rock varnish frequently provides the only way of obtaining ages for geomorphic surfaces near the petroglyphs of interest. Unfortunately, AMS radiocarbon dating of rock varnish is not appropriate for dating petroglyphs directly, because the varnish formed on most petroglyphs has too little carbon even for an AMS analysis at present. However, radiocarbon dating of varnish could be an appropriate method for dating geoglyphs.

CR dating is used to estimate petroglyph ages, as only micrograms of material are needed for the determination of varnish CRs with proton-induced x-ray emission (PIXE; Cahill 1980; Cahill et al. 1984). The CR of (K+Ca)/Ti in varnishes on surfaces of known numerical age are compared to the

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The plates mentioned in this article are on pages 133 to 137 inclusive.

CRs of petroglyphs. This calibration is a semilog least-squares regression called a cation leaching curve (CLC) where the CR is on a linear scale and the age is on a log (base 10) scale. A CR date is, therefore, a calibrated date based on the accuracy and precision of the relationship between the varnish CR and the calibration points.

A short introduction should be made to rock varnish, since so much is dependent on an understanding of its formation and development. Rock varnish is a <2 micron to >500 micron coating that forms on stable rock substrates. It does not form by the weathering of the underlying rock (Potter and Rossman 1977; Allen 1978; Perry and Adams 1978; Elvidge and Collet 1981; Krumbein and Jens 1981; Dorn and Oberlander 1982). Although it is most common in deserts, hence its usual name of desert varnish, it forms in non-arid environments and should be called rock varnish (Dorn and Oberlander 1981b; Krumbein and Jens 1981). A typical dark-brown to black varnish is composed of up to two thirds clay minerals, up to one third manganese and iron oxides (Potter and Rossman 1977, 1979), and over 30 trace elements (Dorn and Oberlander 1982). In a surface position, where petroglyphs are found, the constituents are derived from atmospheric fallout. The manganese and iron oxides form the cement that binds the clay minerals and trace elements to the rock surface. Considerable research by three different groups has demonstrated that the manganese (and likely much of the iron) is concentrated by manganese-oxidising micro-organisms (Krumbein 1969; Krumbein and Jens 1981; Dorn and Oberlander 1981a; Taylor-George et al. 1983; Palmer et al. 1985). Dorn and Oberlander (1982) present a review of the origin and characteristics of rock varnish.

One critical assumption for petroglyph dating that must be met is that varnish is in a continuous state of growth. Any periodic erosion of varnish would invalidate the use of varnish to date the petroglyph, in that the varnish age would not reflect the timing of petroglyph manufacture, but would only provide an estimate of when the varnish was last eroded. Erosional episodes can happen by aeolian abrasion, by biogeochemical erosion by agents like lichens or microcolonial fungi, or by acidity imposed by microenvironmental variables such as acidic leaf litter. Varnishes in arid, alkaline regions appear to have experienced the least biogeochemical erosion but the most aeolian abrasion. By thoroughly understanding the varnish system, such erosional episodes can be detected by thin-section or scanning electron microscope (SEM) analyses of varnish cross-sections.

The Karolta Site

The petroglyph site from which samples were taken for the present study, Karolta, is one of the major sites in the Olary region of South Australia (Fig. 1). Located on the southern fringe of the arid zone of Australia, the area experiences a mean annual evaporation rate of about 2000 mm; this greatly exceeds the mean annual precipitation of 200 mm. The area is continental and experiences extremes of temperature, ranging from -6°C in winter to over 45°C in summer.

The Karolta site is located on a low ridge of dolomitic siltstone (Pepuarta Tillite; Campana and King 1958), the preferred surface for petroglyphs in the region (Nobbs 1984). The ridge is a small feature on a broad undulating plain of low relief near Olary, a township on the highway from Adelaide to Broken Hill. The outcrops of Pepuarta Tillite are not higher than 2 m, about the same height as the dominant nitre bush (*Nitraria schoberi*). Some 300 m north of the petroglyph site is an outcrop of massive-quartz, with a large scatter of quartz fragments downslope. This could have been the source of material for tools used in making the petroglyphs, as the quartz is relatively hard compared to the dolomitic siltstone. It would be interesting to locate these tools and determine if the varnish ages on them reflect the range of ages, discussed later, for the petroglyphs. Excavations might also reveal datable material in association with tools.

The low ridge on which the site is situated is bounded on three sides by an intermittent stream that drains the hills to the west and north. The bedrock ridge apparently controls the drainage of this stream. The bed of the channel is currently 10 m below the present level of the ridge and about 700 m west of the petroglyph site. The bank above the stream near the site is approximately 1.6 m high. A bed of boulders sits at 0.4 m above the present channel. In 1983, in a bank about 50 km to the north (at Cons Bore on the Plumbago Historic Reserve), a sample of ash from a subsurface hearth (0.4 m below surface) produced a radiocarbon date of 3930 ± 120 years BP (GAK 9886). Also at this site, a date of 11 400 ± 200 years BP (GAK 10314)

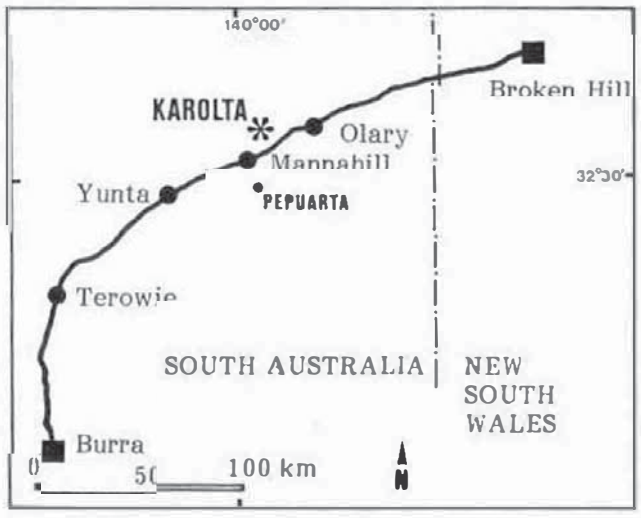


Figure 1. Location of the Karolta petroglyph site.

was obtained from a thin layer of ash 1.2 m below the surface, directly overlying a layer of boulders. This ash layer, about 3 cm thick in 1983, has yielded no hearth stones or occupational material as of 1988. With ongoing stream erosion, however, the ash layer has thickened to 15 cm and is assuming a lens shape. The position of the material and radiocarbon dates provide data for fluvial sedimentation in the region (Nobbs 1983). Significantly, the latest-Pleistocene radiocarbon dates directly on the boulder bed could possibly indicate a greater influence of these streams in the period immediately prior to c. 11 400 years ago. More study of the sediments would be required to be sure.

METHOD OF DATING PETROGLYPHS WITH ROCK VARNISH

Overview of Cation-Ratio Dating

Two general steps are involved in the CR dating of petroglyphs. First, a calibration with numerical ages is established for varnish CRs. Second, varnish CRs of petroglyphs are compared to this calibration to assign chronometric ages to the rock engravings. Previous applications of this method to petroglyphs in western North America are presented by Dorn and Whitley (1984) for the Coso Range and Whitley and Dorn (1987) for the Mojave Desert, both in eastern California.

In establishing a calibration, numerical ages are first obtained for the exposure of geomorphic surfaces. The assumption is that the numerical age represents the time when the varnish started to form; this is a reasonable assumption, since varnish starts to form on subaerial rock surfaces in arid regions about 100 years after exposure. The numerical age may be obtained by such means as K-Ar dating of volcanic flows, but this method's range is often beyond the period of archaeological interest, at least in Australia and in the New World. A more useful timescale is that of radiocarbon, and AMS radiocarbon dating of varnish does provide an estimate of when the varnish started to form.

CRs are also obtained for the same varnishes on the same geomorphic surfaces where the numerical ages are obtained. Great care is needed in sampling to reduce the likelihood of biogeochemical or aeolian erosional problems. It is also important in the sampling process to select microenvironments for the calibration sites that are similar to the sites where petroglyphs are to be dated. The idea is to have the same leaching environment for the calibration samples as for the petroglyphs. Once collected in the field, and mechanically removed from the underlying rock in the laboratory, the varnish is analysed by proton-induced x-ray emission (PIXE) at Crocker Nuclear Laboratory, University of California at Davis (Cahill 1980; Cahill et al. 1984).

Other tests are conducted in the development of a calibration. Dust is collected from natural dust traps (crevices in rocks, depressions on rock surfaces, vesicular horizon of desert pavement soils) and a CR is established for the <2 micron fraction of dust collected from each of the calibration sites (and petroglyph sites). This is to estimate the 'initial ratio' and to make sure that no anomalies of K, Ca or Ti are present at the calibration sites or petroglyph sites. For example, some local environments have a high level of ambient titanium, and this would result in a non-comparable CR for the varnish. It is also important to find the older historical surfaces in the region to assess how long varnish takes to start to form.

After the numerical ages are obtained and the varnish CRs are determined, three cation-leaching curves (CLC) are constructed. A central curve is a least-squares regression of the mean CRs and mean numerical ages for the different geomorphic surfaces and the initial ratio (ratio estimated by CR of <2 micron fraction of dust fallout; age estimated by the age of the youngest historic surface where varnish is first detected). Upper and lower error curves are determined by using the plus 2 standard errors and minus 2 standard errors of the varnish CRs and the numerical-age estimates. An empirical approach is used for constructing the upper and lower error curves, rather than a theoretical diffusion approach, because the variables that affect how varnish CRs change are not completely understood. Further, it seems inconsistent to use an empirical approach to determine a central calibration and a theoretical approach to determine the error CLCs. It is best at this point in the development of the method to estimate errors on a purely empirical basis.

Calibrated ages for petroglyphs are obtained by using these three CLCs and by using the CR of the varnish on the petroglyph. Since the varnish formed on the petroglyph after manufacture, the varnish date is a minimum-limiting estimate for the age of the petroglyph. The varnish is removed mechanically from the petroglyph in the field. Enough is collected for three PIXE analyses; in the case of Karolta, the area of removal was not noticeable. Dust is also collected from each petroglyph site; this is to compare the CR of the <2 micron fraction with the ratios obtained at the calibration sites. If there is a substantial difference in initial ratio, it is possible that a new set of calibration sites would have to be sampled, although this has not yet occurred. It is critical to have a similar leaching environment at the calibration sites and at the petroglyphs.

Petroglyph ages are obtained by comparing the varnish CR with these CLCs. Two approaches are used to obtain a calibrated varnish age. One is to use the 'age' of each separate PIXE analysis (by the central CLC) to obtain an average and standard error. The other approach is to combine all three PIXE analyses together. In this method, the mean CR of three PIXE analyses of the varnish on a petroglyph, compared to the central CLC, gives a mean CR age for the varnish on the petroglyph. The lower 2 standard error CR of the petroglyph, when compared with the upper 2 sigma CLC, provides the maximum older error possible. The upper 2 standard error CR of the petroglyph, when compared with the lower 2 sigma CLC, provides the minimum younger error possible.

Establishment of Calibration at Karolta

Building a calibration starts with estimating the initial ratio of cations in the varnish. The rock varnish on petroglyphs is composed of airborne fallout that is less than 2 microns in size, reworked, and fractionated by microbial and chemical processes (Dorn 1983). The CR of the initial ratio (IR) can be approximated by the CR of the <2 micron fraction of airborne fallout. Dust is collected from natural traps of aeolian material (rock surface depressions, Av horizons, rock crevices) from the calibration and petroglyph sites. The <2 micron size is fractionated by nucleopore filters. Human-placed dust traps are not used, because modern airborne fallout is often affected by anthropogenic changes in the landscape. Roads, diverting of surface waters, subsurface water extraction, and changes in fire fighting procedures all affect the modern composition of aerosols (Baronne et al. 1981; Eldred et al. 1984; Dorn, unpubl. data). It is felt that these natural aeolian traps provide an integration of dust fallout over a longer period of time than human-placed dust traps, and hence would better reflect the natural airborne fallout.

The IR at the Karolta site in South Australia is assigned an age of c. 100 years. This is supported by observations of historical inscriptions at the site. SEM observations of chips taken from a 1930 inscription did not indicate the presence of varnish. SEM observations of chips from a 1884 inscription at Karolta, however, did indicate the presence of incipient varnish (Plate 1). Thus a lag of between 50 and 100 years appears to exist between the making of an inscription and the onset of varnishing. It is interesting that observations of historically faced rock in the south-western United States also suggest a lag of about 60 to 100 years (Dorn and Whitley 1984; Dorn, unpubl. data).

Field Procedures

The field procedures used for collecting varnishes for a calibration point, for a CR point and its corresponding AMS radiocarbon date on varnish are as follows. Enough varnish is collected for both an AMS radiocarbon analysis and a series of CR analyses from the same site. Only black (manganese-rich) subaerial varnish is collected, and only from the tops of larger clasts or exposed rock outcrops at a site. The area of varnish needed for one AMS radiocarbon date depends on how much organic matter is in the varnish, something difficult to judge in the field. Up to a square metre of surface area may be needed, although the amount is usually much less. (Keep in mind that this is from a geomorphic surface, used for a calibration point, and not from a petroglyph. The area of varnish required for a CR date at Karolta is less than a few square centimetres.)

Certain microenvironments were avoided in sampling because they are known to alter a cation-ratio (Dorn 1983; Dorn, in prep.). At Karolta, for example, the 'small fan' calibration site listed in Table 2 had abundant lichen growth. The varnish adjacent to the lichens was heavily decayed and the varnish CRs near the lichens were very different from varnish CRs in more xeric sites. The varnish CR in Table 1 from this calibration site was collected only from these xeric boulders. This is because these microsites have conditions more similar to the petroglyph site. Another example of a microenvironmental factor avoided at Karolta was microcolonial fungi (MCF) (Staley et al. 1982). MCF are common in the Karolta area, as in much of Australia (Staley et al. 1983; Dorn, unpubl. observations). The MCF growing at Karolta are largely destructive of the varnish (Plate 2). They were, therefore, avoided in field sampling and carefully excluded in the laboratory sample preparation.

The general strategy in sampling for AMS radiocarbon dating is to collect samples from geomorphic surfaces that would cover the spread of petroglyph CRs. If time and resources permit, more geomorphic surfaces should be collected than would be processed for AMS radiocarbon dating to ensure that some of the calibration dates reflect the spread of petroglyph CRs. To save analytical funds, the petroglyph CRs and the CRs of calibration samples should be determined first. The CRs of the calibration samples that reflected the spread of the CRs of the petroglyphs could then be processed for AMS radiocarbon dating. At Karolta, four calibrations were originally collected at Pepuarta Bluff, at the Oulnina Homestead (Plate 3). One turned out to be radiocarbon 'dead' and could not be used as a calibration point. The three points used in the calibration are identified by the lettering 's' in Plate 3.

One concern about a radiocarbon date on rock

Materials	Corrected date	1 S.E. for date	Cation ratio	1 S.E. for CR	AMS radiocarbon
Initial ratio	100	0	10.54	0.77	-
Small fan	2330	150	7.43	0.18	2120 (ETH 2804)
Bluff slope	23 700	240	4.93	0.11	21 550 (ETH 2941)
Bluff slope	38 000	560	4.51	0.12	34 590 (ETH 2940)

Table 1.

Calibration data. The date for the initial ratio is from the historical 1884 inscription at Karolta. The corresponding cation ratio is from the < 2 micron fraction of dust from the calibration and the petroglyph sites. The radiocarbon calibration sites are identified on Plate 3 (see p. 133). Uncorrected accelerator mass spectrometry (AMS) radiocarbon dates are in the last column. The corrected date (as discussed in the text) is provided with the 1 standard error (S.E.) of the AMS analyses. The cation ratios are from an average (and standard error) of 5 PIXE analyses.

SAMPLE	CATION RATIO	1 STANDARD ERROR OF CATION RATIO	MEAN DATE (Based on Cation Ratio)	2 SIGMA EXTREME ERRORS (Based on PIXE Analyses)	AVERAGE OF MEAN DATES (with 1 standard error)
K-13					
PIXE Analysis a	7.8	0.97	1507		
PIXE Analysis b	8.14	0.86	1080		
PIXE Analysis c	7.74	0.9	1598		
Ave a,b,c	7.89	0.22	1375	1,400 (350 - 3,300)	1,400 ± 280
K- 6					
PIXE Analysis a	7.16	0.88	2818		
PIXE Analysis b	7.57	0.82	1887		
PIXE Analysis c	7.48	0.74	2061		
Ave a,b,c	7.4	0.22	2221	2,200 (700 - 4,800)	2,250 ± 500
K- 5					
PIXE Analysis a	6.5	0.45	5377		
PIXE Analysis b	6.76	0.77	4169		
c (Ca anomaly)	9.8	1.05	213		
Ave a,b	6.63	0.18	4734	4,750 (1,900 - 8,700)	4,775 ± 850
K-17					
PIXE Analysis a	6.38	0.76	6047		
PIXE Analysis b	6.37	0.71	6106		
PIXE Analysis c	6.65	0.66	4643		
Ave a,b,c	6.47	0.16	5555	5,550(2,300 - 9,800)	5,600 ±825
K- 8					
PIXE Analysis a	6.31	0.69	6476		
PIXE Analysis b	6.43	0.66	5758		
PIXE Analysis c	6.62	0.59	4781		
Ave a,b,c	6.45	0.16	5628	5,650(2,400 - 10,000)	5,675 ± 850
K-15					
PIXE Analysis a	5.97	0.71	9032		
PIXE Analysis b	6.28	0.76	6669		
PIXE Analysis c	6.3	0.68	6539		
Ave a,b,c	6.18	0.19	7330	7,350(3,200 - 12,700)	7,400 ± 1,400
K-14					
PIXE Analysis a	5.86	0.61	10059		
PIXE Analysis b	6.07	0.7	8190		
c (MCF anomaly)	4.2	1.05	51060		
Ave a,b	5.97	0.15	9061	9,050(4,400 - 14,500)	9,100 ± 1,300
K-16					
PIXE Analysis a	5.75	0.71	11202		
PIXE Analysis b	5.8	0.76	10667		
PIXE Analysis c	6.03	0.68	8517		
Ave a,b,c	5.86	0.15	10079	10,100(5,050 - 15,800)	10,100 ± 1,400
K- 9					
PIXE Analysis a	5.79	0.67	10772		
PIXE Analysis b	5.67	0.6	12114		
PIXE Analysis c	6.01	0.55	8685		
Ave a,b,c	5.82	0.17	10476	10,500(5,200 - 16,700)	10,500 ± 1,700
K-24					
PIXE Analysis a	5.9	0.66	9673		
PIXE Analysis b	5.74	0.64	11312		
PIXE Analysis c	5.56	0.66	13491		
Ave a,b,c	5.73	0.17	11386	11,400(5,800 - 17,900)	11,500 ± 1,900
K- 2					
PIXE Analysis a	5.52	0.72	14030		
PIXE Analysis b	5.84	0.7	10258		
PIXE Analysis c	5.78	0.68	10878		
Ave a,b,c	5.71	0.17	11641	11,650(5,900 - 18,200)	11,700 ± 2,000
K- 4					
PIXE Analysis a	5.6	0.54	12973		
PIXE Analysis b	5.37	0.51	16248		
PIXE Analysis c	5.36	0.51	16408		
Ave a,b,c	5.44	0.14	15183	15,200(8,600 - 22,100)	15,200 ± 1,950
K-20					
PIXE Analysis a	5.21	0.51	19002		
PIXE Analysis b	5.44	0.5	15172		
PIXE Analysis c	5.49	0.55	14448		
Ave a,b,c	5.38	0.15	16090	16,100(9,200 - 23,400)	16,200 ± 2,450

SAMPLE	CATION RATIO	1 STANDARD ERROR OF CATION RATIO	MEAN DATE (Based on Cation Ratio)	2 SIGMA EXTREME ERRORS (Based on PIXE Analyses)	AVERAGE OF MEAN DATES (with 1 standard error)
K-11					
PIXE Analysis a	5.21	0.48	19002		
PIXE Analysis b	5.06	0.46	22007		
PIXE Analysis c	5.39	0.42	15933		
Ave a,b,c	5.22	0.17	18817	18,800(10,100 - 27,000)	19,000 ± 3,050
K-19					
PIXE Analysis a	5.11	0.59	20956		
PIXE Analysis b	5.09	0.62	21370		
PIXE Analysis c	5.36	0.57	16408		
Ave a,b,c	5.19	0.15	19421	19,400(11,700 - 27,200)	19,600 ± 2,750
K-18					
PIXE Analysis a	5.08	0.44	21581		
PIXE Analysis b	5.28	0.44	17744		
c (Ca anomaly)	8.63	0.97	669		
Ave a,b	5.18	0.14	19579	19,600(12,000 - 27,200)	19,700 ± 2,700
K-10					
PIXE Analysis a	5.21	0.62	19002		
PIXE Analysis b	5.21	0.64	19002		
PIXE Analysis c	4.91	0.69	25487		
Ave a,b,c	5.11	0.17	20981	21,000(12,600 - 29,500)	21,200 ± 3,750
K-12					
PIXE Analysis a	4.9	0.73	25738		
PIXE Analysis b	4.91	0.55	25487		
PIXE Analysis c	5.13	0.51	20550		
Ave a,b,c	4.98	0.13	23799	23,800(15,500 - 31,700)	23,900 ± 2,900
K-22					
PIXE Analysis a	4.99	0.46	23568		
PIXE Analysis b	5.06	0.46	22007		
PIXE Analysis c	4.78	0.49	28945		
Ave a,b,c	4.94	0.15	24669	24,700(15,900 - 33,300)	24,800 ± 3,650
K- 3					
PIXE Analysis a	5.03	0.56	22663		
PIXE Analysis b	4.94	0.51	24749		
PIXE Analysis c	4.76	0.57	29517		
Ave a,b,c	4.91	0.14	25487	25,500(16,700 - 33,900)	25,600 ± 3,500
K-21					
PIXE Analysis a	4.8	0.62	28384		
PIXE Analysis b	5	0.59	23338		
c (Ca anomaly)	7.53	1	1962		
Ave a,b	4.9	0.14	25728	25,700(16,900 - 34,100)	25,900 ± 3,600
K- 1					
PIXE Analysis a	4.94	0.48	24749		
PIXE Analysis b	4.66	0.52	32552		
PIXE Analysis c	4.72	0.45	30695		
Ave a,b,c	4.77	0.15	29184	29,200(19,700 - 38,200)	29,300 ± 4,100
K- 7					
PIXE Analysis a	4.69	0.37	31610		
PIXE Analysis b	4.62	0.5	33851		
PIXE Analysis c	4.88	0.48	26246		
Ave a,b,c	4.73	0.13	30396	30,400(21,200 - 38,800)	30,600 ± 3,900
K-23					
PIXE Analysis a	4.73	0.63	30396		
PIXE Analysis b	4.78	0.66	28945		
PIXE Analysis c	4.56	0.49	35898		
Ave a,b,c	4.69	0.12	31610	31,600(22,500 - 39,800)	31,700 ± 3,700

Table 2.

Cation-ratio age-estimates for varnish on petroglyphs at Karolta, South Australia. These dates are minimum-limiting ages for the manufacture of the underlying petroglyphs. The cation ratios are given for each PIXE analysis and an average of the three analyses. Calcium carbonate and microcolonial fungi anomalies are indicated (these anomalous samples were not used in generating the age-estimates). The different ways of determining the cation-ratio dates are illustrated in Figure 3. The extreme estimate is the most conservative. The mean date estimate deals only with the central tendency of the cation ratios and the calibration curve.

varnish is the possible time-transgressive development of varnish on different substrates and different microenvironments. It is preferable to sample the same lithology for the calibration and the petroglyphs. This is not because the lithology affects the varnish CR; great effort is made to ensure little contamination exists. It is because each lithology will accept varnish colonisation at a different rate. For example, quartz on a desert surface accepts varnish much more slowly than basalt, because the smooth quartz surface inhibits clay accumulation and microbial colonisation (Dorn and Oberlander 1982). It is important that the rate of colonisation and spread be as similar as possible between the petroglyphs and the calibration sites. This helps cancel out a possible uncertainty of how time-transgressive varnish growth is. Tests of the CRs of varnishes on clasts of different lithologies on a landform with the same age (river terrace, lake shoreline) have shown this effect not to be statistically significant if samples are collected and prepared in the laboratory carefully (Dorn, unpubl. data), but if it is possible to rule this factor out it would be best.

Another important factor to stress is the importance of microenvironment. Since CR dating is affected by biogeochemical factors, it is important that the microenvironments of calibration sites and petroglyphs be as similar as possible. For example, if petroglyphs are on talus boulders, the calibration points would ideally come from talus. The petroglyphs at Karolta and the calibration sites are both from bedrock. Also, the underlying surface from the calibration microsites had a micro-undulating appearance roughly similar to the percussion marks of the petroglyphs.

Laboratory processing of the samples collected at Pepuarta Bluff (Plate 3) for AMS radiocarbon dating involves several steps.

(1) The samples are washed in distilled water and scrubbed lightly with a soft toothbrush. The samples are sorted into 10 groups of equal surface area. A random chip is broken off a random sample from each of the groups for examination by SEM-EDAX. The depth of the unscraped varnish in the different micropositions (ridge, depression) is characterised by SEM. In addition, the occurrences of anomalous concentrations of palaeo-MCF, palaeo or current bioetchpits, or filamentous fungi are checked (cf. Dorn and Oberlander 1982; Dorn 1986). If found in great abundance in this initial random sampling, random chips are taken from each rock in the sample set. Rejection of a rock piece for further processing occurs if the abundance of bioetchpits is greater than 1 in a 100 square micron area, or if MCF are eroding into the varnish and have a concentration of greater than 5% on the surface, or if the filamentous fungi have a concentration greater than 5% of the surface area. If more than 25% of the rock samples are rejected, the site is resampled, if it is to be dated successfully at all.

(2) Using 10-45x magnification under a stereomicroscope, all but the lowest 10% of the varnish is removed by mechanical scraping with a tungsten-carbide needle. Thickness of the remaining varnish is established by SEM analysis for randomly selected pieces from each of the rocks sampled. If the thickness of the remaining varnish is less than 10% of the depth of the unscraped varnish, it is ready for the next step. If not, the particular rock is re-scraped. A black varnish scraped to such a thin layer has a distinctive milky-brown appearance under 45x magnification.

(3) This basal layer is then completely scraped off, and random samples from each rock are again checked by SEM to determine complete removal of a varnish.

(4) A sample of similar volume is collected from the underlying rock. This is to assess the potential addition of carbon from the underlying rock that is incorporated into the sample.

(5) The basal layer of the varnish (and the rock control sample) is processed to concentrate the stable organic matter deposited when this basal layer formed. The procedure to concentrate the stable organic matter is as follows:

(a) The sample is placed in centrifuge tubes. All processing is conducted in the fume hood.

(b) The sample is treated with 35-38% HCl and digested for 3-45 minutes, centrifuged, and decanted. The samples are washed in distilled water and centrifuged 6 to 10 times.

(c) The sample is equilibrated with concentrated HF (52-55%) for 1 to 36 hours, depending on the length of time needed for digestion. Longer times are usually required for the control sample of the underlying rock. The samples are centrifuged, and washed in distilled water 6 to 10 times.

(d) The varnish sample is dispersed in distilled water and 4-5% sodium dithionite is added. The mixture is digested for 30 to 50 minutes in 40°C water bath, centrifuged and decanted. The procedure is repeated once or twice. The sample is then washed/centrifuged 3 times in distilled water. This is similar to the process described by Deb (1950).

(e) The sample is then equilibrated with 0.3 M hydroxylamine hydrochloride in 0.1 M nitric acid (pH 2) for 15 to 90 minutes. The supernatent is decanted and the sample is washed in distilled water and centrifuged 3 times. This step may be repeated. It is similar to the procedure used by Chao (1972).

(f) It is unusual to find detectable levels of organic matter in the control samples of the underlying rock. However, if organic matter is found, the varnish sample from the site is not suitable for radiocarbon dating. This is because contamination from the underlying rock may affect the AMS radiocarbon analysis. A more suitable lithology is selected when the site is resampled. In addition, a procedure is used where only the varnish, and none of the rock is sampled, as in cation-ratio dating. This is much more labour intensive and hence is not used unless detectable carbon is found in the underlying rock.

(7) The sample is then analysed by accelerator mass spectrometry (AMS). A detailed description of AMS radiocarbon dating is presented in Suter et al. (1984) and Jull et al. (1986).

The procedure for the preparation of a varnish from a calibration site (steps 1-9) and petroglyphs

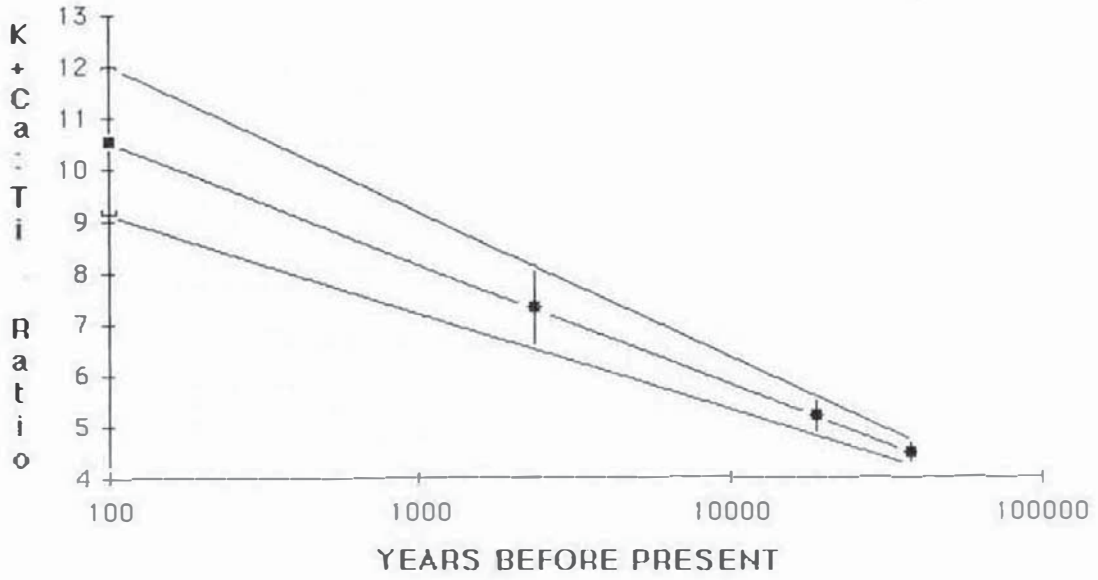


Figure 2.
Cation-leaching curve for the Karolta petroglyph site and vicinity. The derivation of the central curve and the error curves is discussed in the text. The c. 100-year 'initial ratio' is derived from the (K+Ca)/Ti ratio of the < 2 micron fraction of aeolian fallout collected at the petroglyph outcrops and the calibration sites. Other calibration points were collected from sites designated on Plate 3. Horizontal errors are 2 sigma age-uncertainties; vertical bars are 2 sigma cation-ratio errors.

(steps 4-9) for cation-ratio dating is as follows.

(1) The samples for CR dating are collected in the field from at least 15 different rock surfaces. This is to obtain the natural variations for CRs. If these rock surfaces are grouped by lithology and size into 5 different classes, previous tests have determined the least variation among similar lithologies and sizes; hence, to be most cost efficient the goal is 5 composite PIXE analyses to summarise the CRs from multiple surfaces.

(2) After breaking up the samples into 5 different groups, the samples are scrubbed lightly with deionised water to remove loose detritus, and air dried for 5 to 24 hours.

(3) Under 10 to 45x magnification, a tungsten-carbide needle is used to remove mechanically the entire varnish down to the bottom layer.

(4) The scrapings are passed through a filter system with nucleopore filters of 45, 15, 2 and 0.2 microns. This allows rock contaminants to be easily separated from the varnish. Also, varnish pieces that are >2 microns can be reduced in size to avoid matrix effects during the PIXE analysis.

(5) Contamination from the underlying rock is estimated for volume and chemistry by SEM examination of randomly selected scrapings. The criteria for distinguishing varnish from rock fragments is morphological (Dorn 1986) and mineralogical (Potter and Rossman 1979).

(6) The scrapings of each of the 5 groups (for the calibration site) and 3 groups (from a petroglyph) are then mounted on Kapton film with a lead-free spray adhesive.

(7) Analysis for a varnish CR is by proton-induced x-ray emission (PIXE) by Crocker Nuclear Laboratory at the University of California at Davis (Cahill 1980; Cahill et al. 1984).

(8) At each calibration site and the two outcrops at Karolta, the <2 micron fraction of the dust that has been accumulated in the natural dust traps is analysed by PIXE.

(9) A PIXE analysis of varnish is not used in computing a CR from a site if it is anomalously high or low, and a specific cause can be found. Typical causes of anomalies are local abundance of Ti or Ca, or biological anomalies that got past the initial examination. One of the tremendous advantages of the PIXE method of CR determination is that a wide variety of elemental analyses is completed. If a CR is anomalous, it is easy to determine if the other trace elements (or major elements) are similarly anomalous. If they are, it lends faith to the rejection of a PIXE analysis. If a PIXE analysis is not used for a calibration site (or petroglyphs), the reported CR represents only one less PIXE analysis. All of the calibration points in Table 1 are from 5 PIXE analyses; some of the petroglyph PIXE analyses were rejected for anomalies (Table 2).

Cation-Ratio Dating with Karolta Cation/Leaching Curve (CLC)

A CLC is a semilog, least-squares regression where the CR of (K+Ca)/Ti in varnish is calibrated by numerical dating methods. In the case of Karolta, AMS radiocarbon ages on rock varnishes collected from the nearby Pepuarta Bluff form the basis of calibrating varnish CRs (Figure 1; Plate 3). The central CLC in Figure 2 is constructed by correlating mean calibration age with mean CR for the same site. 10% is added to the analytical AMS radiocarbon age (listed in last column of calibration data in Table 2) in order to correct for a systematic bias. The AMS radiocarbon date on varnish is of the *lowest layer* in the varnish, not the timing of the onset of varnishing. What is of interest in petroglyph dating is when the varnishes started to form on the petroglyph, hence we need to get

as close to a minimum-limiting age as possible for dating the actual manufacturing of the motifs. Collaborations between R. I. Dorn and the University of Arizona Accelerator Research Group are in progress to determine the relationship between surface exposure and the AMS radiocarbon age on varnish. Tests of varnish AMS radiocarbon dating at sites that already have conventional radiocarbon dates for surface exposure have shown that the varnish age is about 10% younger than the conventional date. This topic will be elaborated in greater detail in a later section.

The central CLC in Figure 2 is represented by equation (1):

$$\text{Varnish CR} = 15.27 - 2.35 \times \log_{10}(\text{age}) \qquad (1)$$ Central Curve

Two different types of errors are listed in Table 2. The first is based on the central regression only. The assumption is that the regression represents a best fit to the calibration data. Each separate PIXE analysis yields a CR (and analytical error) listed in Table 2. When the CR for each PIXE analysis is compared with equation (1), the central curve, a mean date is obtained. The average of these 3 separate CR age estimates gives a best estimate, and a standard error for these 3 calibrated ages provides an error estimate. Figure 3A illustrates this method.

If one requires an extremely conservative approach to the estimation of errors, the extreme 2 sigma errors in Table 2 are provided. They use the 2 sigma errors in the calibration curve and the 2 sigma errors for the petroglyph CRs. The upper CLC in Figure 2 represents an empirical semilog regression of the upper 2 S.E. errors of the calibration dates and the calibration CRs (and lower 2 S.E. errors for lower CLC). These CLCs are represented by equations (2) and (3):

$$\text{Varnish CR} = 17.71 - 2.86 \times \log_{10}(\text{age}) \qquad (2)$$ Upper Curve

$$\text{Varnish CR} = 12.85 - 1.85 \times \log_{10}(\text{age}) \qquad (3)$$ Lower Curve

The extreme error range listed in Table 2 incorporates the errors at a 95% confidence level for all variables. Figure 3B illustrates this method.

DISCUSSION OF THE DATING METHODS

Tests Conducted

Rock varnish CRs are calibrated by varnish AMS radiocarbon dates. It is, therefore, imperative to understand how accurate and precise these age estimates are. Theoretically, the varnish AMS radiocarbon ages should underestimate the exposure of a surface to subaerial varnishing for several reasons: (1) there is a c. 100-year time lag between the exposure of a surface and the onset of varnishing; (2) there is some time transgression as it takes some time for these patches to grow laterally. The rate of this growth varies with microenvironmental factors, including lithology; (3) the organic matter dated is from a *bottom layer*, some of which accreted after the surface was exposed.

Tests between R. I. Dorn and the University of Arizona Accelerator Research Group are in progress to assess these effects. Varnishes have been collected from sites with independent age control, such as pluvial lake shorelines, where charcoal exists below lava flows and where palaeomagnetic data exist for basalt flows. About a third of these tests have been completed, and preliminary results indicate that the varnish AMS radiocarbon age is about 10% younger than corresponding conventional radiocarbon ages (Dorn, Jull, Donahue and Linick, in prep.). Given this net effect, the AMS radiocarbon ages for varnishes have been adjusted by 10% in Table 1.

In an effort to evaluate the precision and accuracy of CR dating, several tests have been conducted. Dorn (1983); Dorn, Tanner et al. (1987); and Dorn, Turrin et al. (1987) have compared varnish CRs with K-Ar dates in the Coso, Cima and Big Pine volcanic fields of eastern California. Harrington and Whitney (1987) have similarly compared varnish CRs with K-Ar, uranium-trend and uranium-series ages in southern Nevada and central New Mexico, U.S.A. The fit of the least-squares regression is excellent, where more than 80% of the variance is explained.

Some archaeological tests of CR dating have been completed. Dorn et al. (1986) analysed CRs of varnishes on sequences of flakes and cores in the Mojave Desert. They found that the mean CR dates had standard errors of about 10% for a given sequence. It is a reasonable assumption that a given sequence of refitted artefacts was flaked at the same time: hence, CR dating appears to have an archaeological precision of about ± 10%. Dorn, Proctor et al. (in prep.) compared the CR dates of surface point types in west Texas (such as Plainview) with stratigraphic ages obtained from 'classic' Palaeoindian sites in the area. Preliminary results indicate a reasonably close fit, but with a slight tendency for the varnish CR ages to underestimate the established age of the typology.

In both of the above studies, varnish on unworked surface 'cortex' yielded CRs lower (older) than the varnishes on the worked surfaces. This was also the case at Karolta. Varnishes collected from the unengraved surfaces yielded CRs beyond the range of radiocarbon (c. 40 000 years). If the calibration in Figure 2 is extended in a linear fashion, an uncertain procedure but useful for comparative purposes here, an age of about 60 000 years BP is approximated for varnish from an unworked exposure. This could represent the time when the outcrops were exposed or exhumed by erosion of the unconsolidated sediment surrounding the outcrops.

The unworked varnish at Karolta outcrops was studied in detail as a part of this investigation, with SEM, microprobe, PIXE and other analytical methods. Its development is largely irregular and patchy. It is most noticeable in depressions and cracks and not as well developed on the smoother surfaces. This is probably due to growth of varnish in favourable microsites where varnish can grow, such as pits, depressions and surface irregularities. This is one reason why the varnish dating on petro-

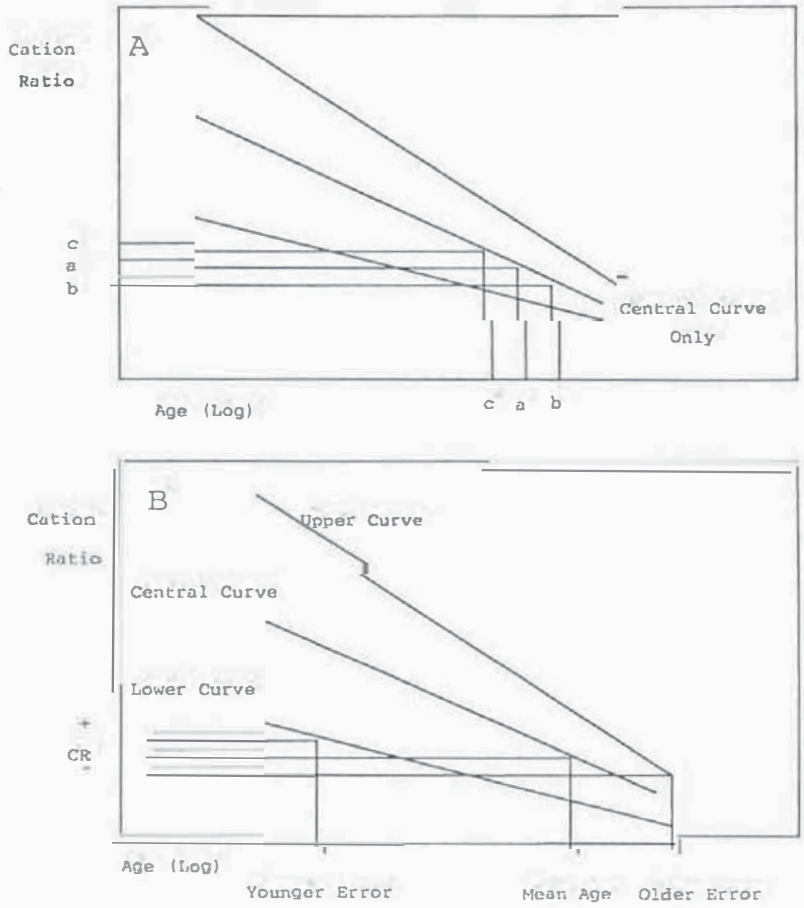


Figure 3.
How varnish ages in Table 2 are estimated.

METHOD A: AVERAGE OF THE MEAN DATES. The cation ratios of three separate PIXE analyses (a, b, c) of varnish on a single petroglyph are compared with the central curve (equation 1) in Figure 2. A 'mean date' is obtained for each cation ratio. These are averaged in the last column of Table 2 and the standard error provides the 1 sigma uncertainty.

METHOD B: AVERAGE OF THE PIXE ANALYSES. The three separate PIXE determinations (a, b, c) are averaged with a ± provided by the standard error. The average cation ratio provides the best age-estimate from the central curve. The upper 2 sigma cation ratio provides an extreme 2 sigma younger error when the lower curve is used. The lower 2 sigma cation ratio provides an extreme 2 sigma older error when the upper curve is used.

glyphs should work well: the engravings provide irregularities for varnish to start quickly. The calibration surfaces at Pepuarta Bluff (Plate 3), therefore, were also selected for these irregularities to make varnish development and cation-ratio changes most comparable.

Why Cation-Ratio Ages are Minimum-Limiting Estimates

Virtually every confounding factor that might cause an error would make the CR age estimates for petroglyphs too young. Please note that the following effects have been reduced at Karolta by using correct procedures for sampling, sample preparation and calibration development. We do not consider the following problems to be serious at Karolta, but we list them numerically in order to present all our concerns to the reader:

(1) If there is a substantial time lag between petroglyph manufacture and varnish development, as Dragovich (1986) notes is possible, the ages would be too young. We consider this to be unlikely at Karolta because of observations of historical engravings (Plate 1).

(2) Any erosional episodes of varnishing on the petroglyphs would make the ages too young. Their occurrence is unlikely for two reasons. First, thin section analyses of very small flakes removed from the petroglyphs do not show episodes of palaeoerosional activity. Second, there is a great scatter in the petroglyph ages: if an erosional event occurred, it should have 'reset' the varnish CR clock.

(3) Any contamination of material from the rock underlying the varnishes in the petroglyphs would include more K and Ca than Ti, given its dolomitic composition. This would result in too young an age of prediction. This contamination is probably less than 2%, as evaluated by SEM analysis of random petroglyph scrapings.

Possible Questions

There are a few commonly arising questions that could be answered here.

Is the method destructive? No. It only removes a small amount of varnish. The effect is only noticeable after the specific location of varnish removal is pointed out and upon close examination at a distance of a few tens of centimetres.

Can another method be used to evaluate CRs besides scraping of the varnish? Yes. Harrington and Whitney (1987) have proposed a purely SEM method of CR dating. We do not favour it for several reasons. (1) It is immensely destructive of petroglyphs, requiring coring of the petroglyph. (2) The level of chemical contamination from the underlying rock as it affects varnish CRs has not yet been tested. (3) There are theoretical objections as to why the CRs obtained by the SEM method would make the varnish age too old. (4) Scraping is the only way to obtain AMS varnish radiocarbon dates for calibration purposes; and AMS varnish radiocarbon dating is often the only way to obtain calibration ages for varnish CRs.

How expensive is CR dating? The PIXE analysis of varnish on an individual petroglyph is less expensive than radiocarbon dating. At Karolta, with all the laboratory preparation and assessment procedures used, the combined cost is about US$150 for each petroglyph date. A separate and sizeable analytical expense is the development of a calibration,

in the range of US$10 000.

Results of Cation-Ratio Dating at Karolta

Table 2 lists the raw varnish CRs, raw CR errors, and age estimates for the varnish on 24 petroglyphs at the Karolta site in South Australia (Fig. 1). As noted earlier, there are two types of error estimates. One treats each PIXE analysis as a separate estimate of age, and the other combines the 3 PIXE analyses into a composite signal. The standard error of the mean dates (1 sigma in Table 2) often provides a lower error because the mean CRs cluster together. The extreme error range in Table 2 incorporates all known 2 standard errors from the petroglyph CRs and the calibration curves. The errors are much larger, because the 2 sigma error curves (equations 2 and 3) are used, instead of only the central curve (equation 1). Both error estimates are listed so the reader can gain an understanding of the limitations of the method.

Antiquity of Human Presence in Australia

Our results indicating an occupation of the Olary region as early as 31 000 years BP are consistent with the accumulating data for an early arrival of humans into Australia. Evidence for occupation of Australasia by 38 000 years BP has been found on an uplifted seashore terrace on the Huon Peninsula in New Guinea. Thermoluminescence dating of volcanic ash that incorporated an axehead has established a minimum age of 38 000 years (Groube et al. 1986).

Evidence of early human occupation of the Australian continent proper comes from an archaeological site at Upper Swan near Perth in Western Australia. Preliminary investigations of small areas in an extensive open air site on the flood plain of the Swan River have yielded stone artefacts associated with charcoal. A series of radiocarbon dates indicates that the extreme south-west of the continent was populated by about 38 000 years BP. The extreme south-east was probably also occupied by that time (Bowler 1976).

Evidence exists for occupation prior to our earliest petroglyph date in the Willandra Lakes system, about 200 km to the south-east of Olary, by about 33-35 000 years BP. A Core and Scraper Industry associated with fires, burnt bone and unionid shells was located in beach deposits of the Mungo stratigraphic unit with an associated age of 32 750 ± 1250 years BP (ANU 331; Bowler 1971). Subsequently, charred plant remains within dark sand sediments of the same unit gave an age of 35 300 (ANU 687; Barbetti and Polach 1973).

Smith (1987) found a piece of red pigment associated with a late Pleistocene site in central Australia. A radiocarbon date from this site indicates that what is now the arid core of the continent was settled by 22 000 years BP. This piece of red pigment could represent material used in painting (Jones 1987).

It has been assumed for some time that petroglyphs in the Olary region have great antiquity. Basedow (1914) had thought the petroglyphs in south-eastern South Australia to be very old, and made during a fairly short time. He based his assumptions on visual observations of rock varnish on the petroglyphs; he thought their varnish cover resembled that of Egyptian petroglyphs. In addition, Basedow (1914) thought that some motifs seemed to represent tracks of extinct megafauna; for example, a large four-toed track found at Yunta Springs resembled a track made by the extinct wombat-like creature diprotodon. Observations of extensive weathering further supported his assertion of a great age for the Olary petroglyphs.

Robert Edwards in 1966, when reporting on petroglyphs in central and South Australia, observed that rock varnish was well developed in petroglyphs and that weathering conditions and minor earth movements had damaged petroglyph surfaces. These processes do not occur rapidly, and hence further support was presented for the notion of great antiquity for this human activity. Edwards (1966) also observed that the major petroglyph sites in the arid zone of Australia were placed where the rock was suitable for making petroglyphs *and* close to supplies of permanent or semi-permanent water.

The first direct analytical data on the antiquity of rock art in Australia was presented by Robert Bednarik who in 1981 obtained radiocarbon dates from reprecipitated limestone layers in a cave near Mt Gambier, also in South Australia. These speleothem formations had covered petroglyphs on the ceiling of the cave, and their radiocarbon content provided highly conservative minimum dating of 5500 years for the second-youngest rock art tradition at the site (Bednarik 1984, 1985a). D. Dragovich used the same method to establish minimum ages for rock varnish near Broken Hill, by analysing a superimposed dense carbonate rind. She found that the main varnish is over 10 000 years old (Dragovich 1986).

An interesting feature of the petroglyphs in the Olary region is the absence of dingo tracks. The dingo is thought to have been introduced into Australia about 4000 years ago (Flood 1982).

Our results of radiocarbon-calibrated cation-ratio dates for rock varnish within petroglyphs at Karolta suggest an early occupation for the southern fringes of what is now the arid zone of Australia. Moreover, the dates show that people had been conducting a non-oral method of communicating ideas to one another as early as 31 000 years BP. These analyses, however, are in basic agreement with other evidence for early human occupation in Australia.

DISCUSSION OF THE KAROLTA SITE

Reasons for the Choice of Karolta for Cation-Ratio Dating

The aim of the project is to establish a chronology for the style at one site, rather than to attempt to sample motifs from several sites. Of the 30 or so known petroglyph locales recorded in the region, Karolta 1 was selected for the following reasons:

(1) The motifs and petroglyph surfaces have been well documented and an analysis table and

Tracks						Lines		Circles				Dots	Abr. grooves	Designs	Total
165	68	251	9	23	3	77	43	591	30	6	10	297	227	26	1826
519=28%						120=7%		637=35%				297=16%	227=13%	26=1%	=100%

Table 3. Motif analysis table for Karolta 1 petroglyph site, South Australia.

drawings of the few unusual designs (Table 3; Fig. 4) are elaborated in Nobbs (1984). The motifs are characteristic of petroglyphs in the Olary region and conform to the regional style. There are no recognisable animal or human figures present at this site.

(2) Historically, Karolta 1 was the first petroglyph site reported in South Australia (Waterhouse 1902). It was known formerly as the Mannahill Engraving Site.

(3) Although there is very little vandalism at present, one very important inscription—'A 21 H.M. 1884' (Plate 20)—afforded, along with the type section of Pepuarta Till (12 km to the south), material suitable for the calibration of varnish cation-ratios. In addition, it is certain that the motifs have not been outlined in chalk to facilitate photography. The calcium of the chalk could pose a contamination problem for the (K+Ca)/Ti ratio.

(4) Excellent subaerial, manganese-rich rock varnish is present on exposed rock surfaces.

(5) The site is seldom visited at the present time, as it is exposed to the elements and is no place for a picnic.

(6) A further consideration in the choice of this site was the character of the motifs present. The style is homogeneous and uniform across the site.

Techniques for Manufacturing Karolta Petroglyphs

The percussion techniques of pounding and pecking were used to make the petroglyphs at Karolta, with the exception of the abraded grooves. McCarthy defines percussion as

> fashioning the outline, part or whole of the surface of a figure [motif] with pits made with a pointed stone or other implement. The nature of the percussion and of the tools employed and the texture of the rock produce considerable variations in the size of the pits (McCarthy 1979).

This definition applies to the technique used in the arid zone of Australia, and at Karolta in particular. Percussion can involve either pounding or pecking (see Flood 1987: 120 for definitions).

There is an abundant scatter of white quartz fragments surrounding the outcrops that bear petroglyphs. About 300 m north of the site there is a large outcrop of white massive-quartz surrounded by quartz fragments which have eroded to an area downslope from the outcrop. The petroglyph site is on a low rise so that the fragments surrounding it could not have been transported there naturally from the massive-quartz outcrop. The quartz fragments surrounding the art site were probably used to make the petroglyphs. The dolomitic siltstone at this place is relatively softer than quartz.

At Karolta, series of small marks form the basic outlines of the motifs as well as the interiors. Small marks make up the infilling of solid motifs such as tracks, infilled circles and dots, motifs which are sometimes called intaglios.

The technique used in making abraded grooves is different. They have the appearance of having been cut into the rock. These 'cuts' can be up to 9 mm deep. It is suggested that they were formed by scratching a primary line with a pointed hard stone and then deepening and widening the scratch by repeatedly drawing a hard, sharp stone along the primary line, turning the stone slightly with each stroke to make the groove wider.

The Style of Motifs at Karolta

In order to discuss possible changes in style or differing phases, it is necessary to define the categories into which the motifs can be placed. The style is simple and the motifs can be placed in six categories.

(1) *Tracks.* The words that refer to specific tracks are placed in quotation marks, thus 'bird' track, 'macropod' track, 'human' track, in order to indicate the use of a word as a name rather than a label (cf. Flood 1987: 99). The category of tracks includes all marks that can be recognised as depictions of marks left by the movement of the animal or human over the landscape. Tracks account for 28.4% of the total motif count at Karolta. They form a significant component—up to 60% of the motif content of the style in this region. While tracks or *empeintes animales* do occur in rock art elsewhere, notably Tassili (F. Soleilhavoup, pers. comm.) and in parts of southern Africa (the spoor petroglyphs in Zimbabwe; Walker 1987), they are there usually comparatively recent; they occur in isolation or together with the depicted animal or associated with other motifs, such as dots and meandering lines. The great profusion of tracks in the so-called 'Panaramitee style' in the Olary region appears to be a unique characteristic of this style of Australian rock art. J. McDonald, when studying this style near Broken Hill in western New South Wales, was able to identify specific differences between red kangaroo/euro, grey kangaroo and megafauna tracks. She also identifies trails of tracks (McDonald 1983). 'Bird' tracks, 'macropod' tracks and 'human' tracks arranged in trails occur at Karolta and one of a set of six 'bird' tracks was sampled for CR dating. Two examples of emu tracks, left in the soil close to the petroglyph site can be seen in Plate 4.

(2) All straight or curved lines that do not form closed motifs are classified as *lines.* They very seldom consist of a single row of impact marks, usually consisting of rows three or four marks wide. Meandering lines are similarly constructed and are

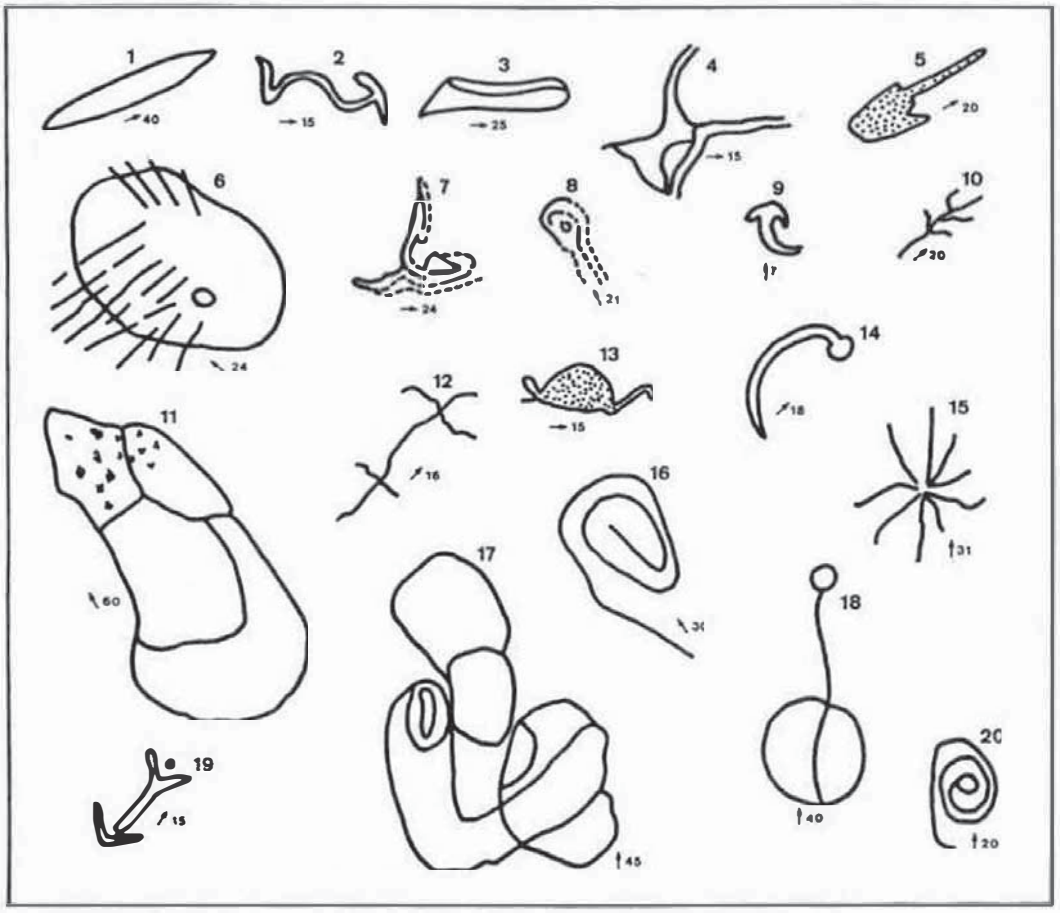


Figure 4.
'Designs' at Karolta I petroglyph site. Dimensions in cm, measured in the direction of the arrow (after Nobbs 1984).

included in this category.

(3) The category of *circles* includes all closed circular motifs such as joined circles, barred circles, concentric circles, spirals and ovals. The rings of marks outlining natural holes in the rock surface are here called decorated pits and are classified in the final general analysis as circles. Very occasionally, circles consist of a single line of percussion marks.

(4) The term *dot* is applied to a particularly common motif of this style. For the purposes of this paper, a dot is defined as a motif consisting of many small marks, clustered together to form a roughly circular shape, seldom more than 2.5 cm in diameter (Plates 5, 6). Dots have the appearance of very small infilled circles and are equivalent to 'scats' from western New South Wales (Clegg, pers. comm.). They occur occasionally in rows, but more often in clusters of up to 20 units. Although dots are really very small infilled circles, they are placed in a separate category because possible behavioural significance can be attributed to them, and because they are so numerous. It has been suggested that in some cases they represent emus' eggs.

(5) The *abraded groove* is the only motif in the style that exhibits a technique that is different from percussion. The proper term for this motif is a straight line, but the term abraded groove specifies a certain type of straight line that is readily recognised as being different in technique from the pounded or pecked straight line; so for the purposes of this paper, the term is given motif status and placed in a separate category (Plate 11). Abraded grooves near the Lightning Brothers painting site in the Northern Territory were called 'rain cuts' by an informant.

> The rain-making ceremony involved singing and dancing and each man present cut a groove in the rock to make the Old Man bleed and bring rain . . . In an indirect way the grooves represent tally marks (Arndt 1962).

It has been suggested that these marks were made in the process of sharpening tools, but the dolomitic siltstone is much softer than stone tools that were usually sharpened by secondary trimming. Axe grinding grooves beside a creek west of Armidale, New South Wales, have a rounded profile instead of the V-shaped profile of the abraded groove (Mulvaney 1969: Plate 64).

(6) The last category—*designs*—includes motifs that are either uncommon, complex or distinctive for one or two particular sites. When Karolta was recorded in 1982 it was found that there was a total of 1826 motifs, of which only 1.4% can be classified as designs. There are no recognisable figures such as 'lizards' at this site (Table 3; Fig. 4). Plate 8 shows the one example of this category sampled for CR dating.

Choice of Motifs for Cation-Ratio Dating

The site at Karolta consists of 25 outcrops of dolomitic siltstone. Most of the preferred outcrops have surfaces that have no petroglyphs on them. The outcrops are roughly aligned in a north-south corridor with the most northerly group of them separated from the remainder by 1 km of bare ground. Two widely separated outcrops containing petroglyphs were chosen for CR dating, and motifs on these outcrops were chosen at random. One outcrop, containing motifs K1 to K14, was 90% cove-

red with petroglyphs in a sparse distribution. Here the motifs were chosen from five different surface exposures (Plate 9; Plan 1). The other set of samples, K15 to K24, was from an outcrop that was 45% covered with petroglyphs that were densely arranged on the surface, with very little apparent superimposition. Here the motifs were chosen from the width of the petroglyph surface (Plate 10; Plan 2). Plates 5-8 and 11-19 illustrate the other motifs CR dated at Karolta.

The possibility of damage to motifs during sampling was a source of great concern to M. Nobbs prior to collection. However, the CR method, as practised by R. I. Dorn, poses no threat to the integrity of the motifs sampled. Before and after photographs were taken at the time of sampling, and six months later. It is very difficult to detect any damage. This makes the CR dating especially valuable, in that it is almost a nondestructive way of obtaining minimum-limiting ages for petroglyph manufacture.

DISCUSSION OF CATION-RATIO CALIBRATED AGE-ESTIMATES

Change of Style

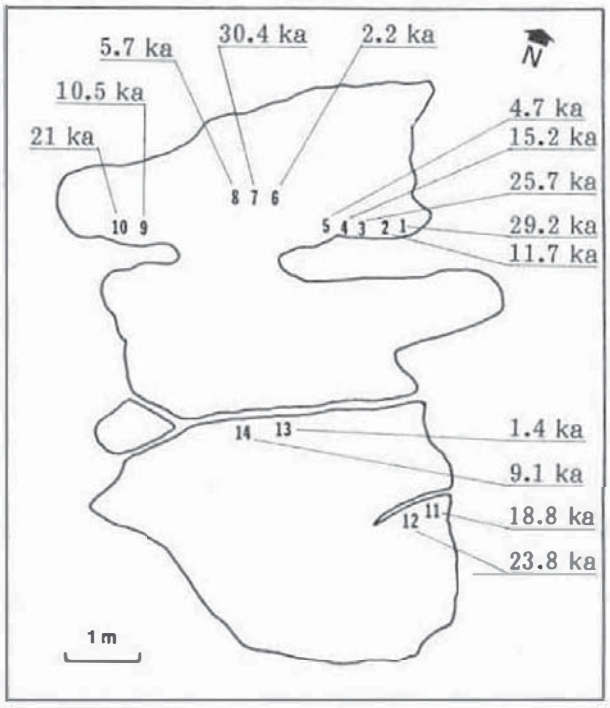
The range of dates for the 24 motifs sampled at Karolta may be seen in Figure 5. It is well to be cautious of inferring from these dates at one site a continuity of style for the whole region that contains probably upwards of 40 000 motifs. It is evident, however, that the components of the style at Karolta may have remained unchanged for more than 25 000 years, with the oldest motif dated at about 31 600 radiocarbon-calibrated years BP (Table 2). This shows most clearly when tracks are considered (Fig. 6). The earliest 'bird' track is dated at c. 25.8 ka (Plate 19) and the youngest is at c. 1.4 ka (Plate 16). The earliest 'macropod' track is dated at c. 23.8 ka (Plate 15), and the youngest is at c. 2.2 ka (Plate 13). In both cases, it can be seen that the tracks are recognisable as marks representing the movement of the animal in the landscape. Thus the style of a large component of the petroglyphs at Karolta has remained unchanged.

The style of the circles, dated between c. 30.4 and c. 19.6 ka remains unchanged within the range of dates. The interior of the two circle motifs K8 (Plate 13) and K14 (Plate 17) (oval at c. 9 ka and circle at c. 5.6 ka) are both infilled. Dots appear very early in the record—about 29.3 ka and as late as 11.6 ka. The two abraded grooves sampled came from separate outcrops, and the CR calibrated ages are very close, K20 at c. 16.1 ka (Plate 19) and K4 at c. 15.2 ka (Plate 12).

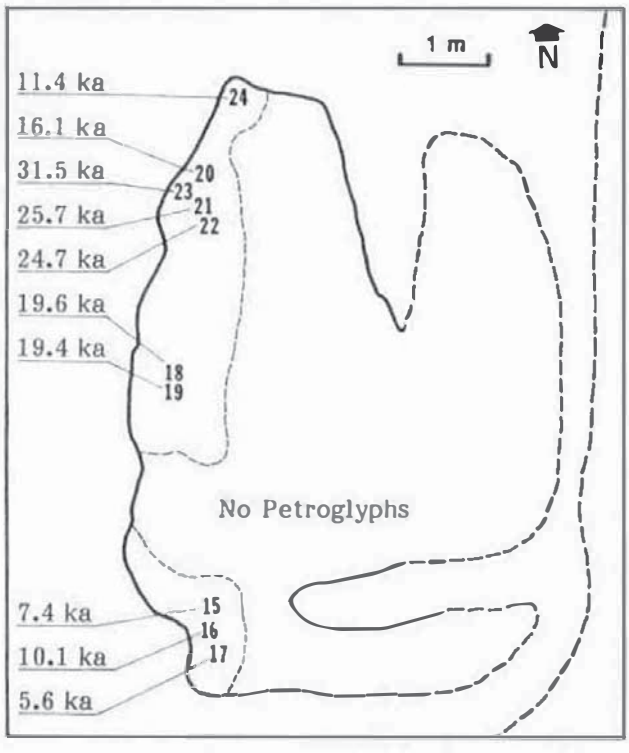
While it is often impossible to prove superimposition by visual inspection, one example of superimposition has been demonstrated by CR dating. K4 is an abraded groove dated at about 15.2 ka, with a 'macropod' track dated at c. 4.7 ka superimposed upon it (Plate 11).

Repeated Use of Special Surfaces over Time

Two groups of sampled motifs exhibit a great range of dates in a small area, with apparently negligible superimposition. At one outcrop the motifs K1 to K5 occur on a surface measuring 1 m^2 (Plate 9) and the date range is from c. 29.3 to 4.7 ka. On the other outcrop (Plate 10) sampled, motifs K20 to K23 occur on a surface measuring 60 by



Plan 1.
Plan of outcrop 25, Karolta I. 90% of the total surface bears petroglyphs. Cation-ratio dates for varnish are shown with sample numbers and locations of sampled motifs (see Plate 9).



Plan 2.
Plan of outcrop 16, Karolta I. 26% of the total surface bears petroglyphs (see Plate 10).

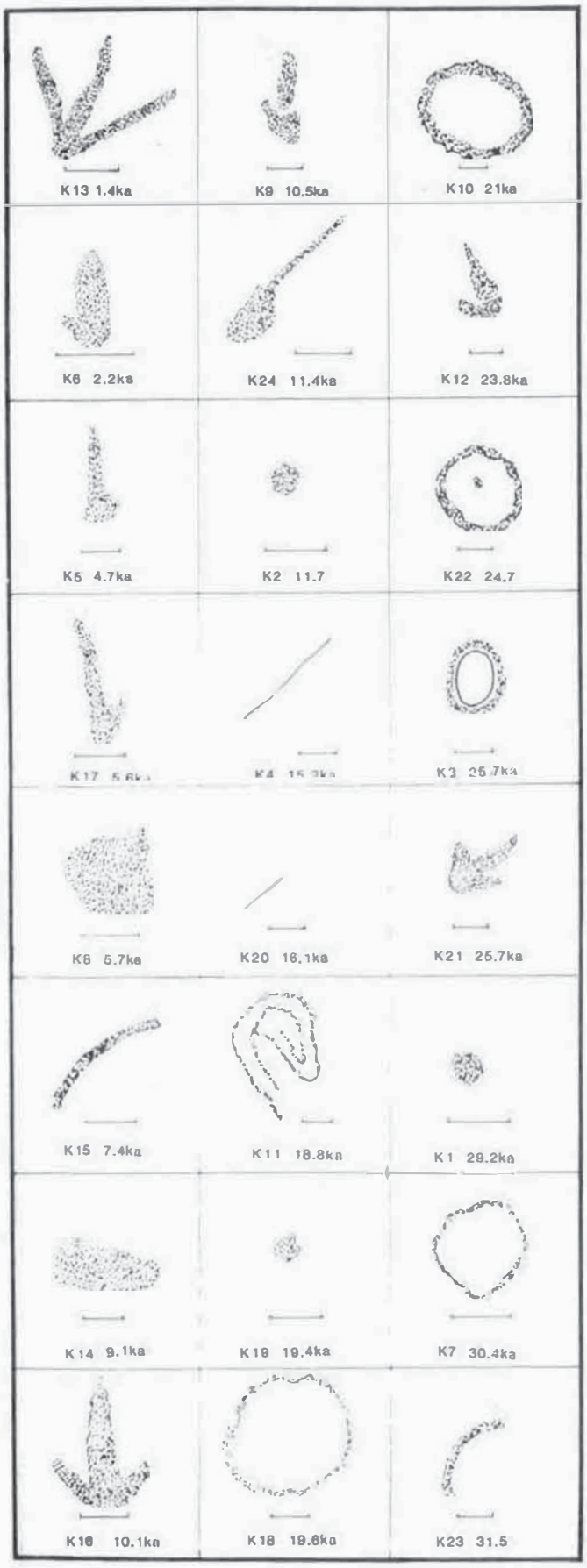


Figure 5.
Tracings of Karolta petroglyphs dated by CR analysis of varnish, arranged according to average cation-ratio calibrated age-estimates. Scale bars 5 cm.

40 cm, with a range of 20 100 years (Plate 19). The petroglyph surface on this latter outcrop is roughly 7 m long by 1 m wide. Motifs K15 to K24 came from this surface and have a range of between c. 31.5 and 5.5 ka, or about 25 000 years. This indicates that humans returned to the same surfaces again and again, and ignored apparently suitable surfaces adjacent to their preference. It also suggests that the outcrops at the site have remained exposed for a very long time, at least c. 31 500 years. The site has not been buried by a cover of aeolian or fluvial material during this time.

Technique

The same skilful and well controlled technique for making petroglyphs was used at this site over the full span of 31 500 years. The petroglyphs were made with a conscious effort to avoid other motifs on special surfaces. With the exception of the abraded groove K4, the motifs do not appear to have been reworked and are seldom superimposed. This is unlike the paintings in the region, where it is possible to see superimposition of layers of motifs, but not their calibrated- or numerical-chronology (dating terminology from Colman et al. 1987).

Patterns

On many of the petroglyph surfaces at Karolta, indeed at most sites in the region, groups of motifs are arranged in distinct patterns or compositions. This feature of the style has been described from western New South Wales (McDonald 1983). The oldest sampled 'bird' track, c. 25.8 ka, is one of a line of six identical 'bird' tracks crossed at nearly right angles by another line of five identical 'bird' tracks (Plate 19). Both sets point away from groups of dots (emu eggs, possibly), perhaps suggesting emus leaving their nest. There is, of course, a possibility that the 'bird' tracks were added gradually over time, but this can be tested by further CR dating and it appears that the scribe or scribes could internalise.

The range of dates from Karolta poses some problems for researchers of this style. Some questions could be answered if more of the 1826 motifs at Karolta could be dated. For example, do the close dates for abraded grooves mean that these were made during a limited time span? 127 samples of abraded grooves are available to examine this question. Also, were the 'bird' tracks, discussed in the prior paragraph, all made at the same time? Why did the later scribes place motifs so close to the earlier motifs on the same 'special' surface? Did it become necessary over time to add to the information on the surfaces? Were the stories that probably motivated the manufacturing of the petroglyphs part of the same tradition? It is interesting to note that the painters at Papunya and elsewhere in arid northern Australia, at the present time, use the same array of motifs that is present in the Olary region. 'In Aboriginal Australia there is a great variety of alternative forms of maps, making constructions for land navigation. In some cases these constructions take the form of paintings' (Gale 1986), or petroglyphs. Would it be possible, by extrapolation and with a sound chronology, to find clues to the meanings of the petroglyphs

in the Olary region? A paper given at the First AURA Congress in Darwin by Des Coulthard—a ranger with the South Australian Aboriginal Heritage Branch, from Neppabunna in the Flinders Ranges and a member of the Adnyamathana tribe—gave a list of motifs and their meanings used in painting sites in the Ranges in living memory. All of the motifs given in Coulthard's paper occur in petroglyph and painting sites in the Olary province.

> Although many of the engraving symbols are unusual and their meanings are not apparent to everyone, the Adnyamathana people of today compare them to their more recent paintings and believe that most of the symbols relate closely to them (Coulthard 1988).

If it were feasible to date all 1826 motifs at Karolta, this would probably answer questions about Karolta and possibly the region. Such an extensive effort, however, would not answer questions about the many other places in Australia where this style is found. Some motifs at Karolta are quite old, but it cannot be said that this range of dates applies to other regions in Australia. There is a suspicion that this may be so, for example, at Ingaladdi and the Cleland Hills in the Northern Territory (Mulvaney 1969: Plates 45 and 76), but we have seen that the Papunya painters use this style at the present time. Could it not have been picked up and used at any time or place during 30 000 years or more?

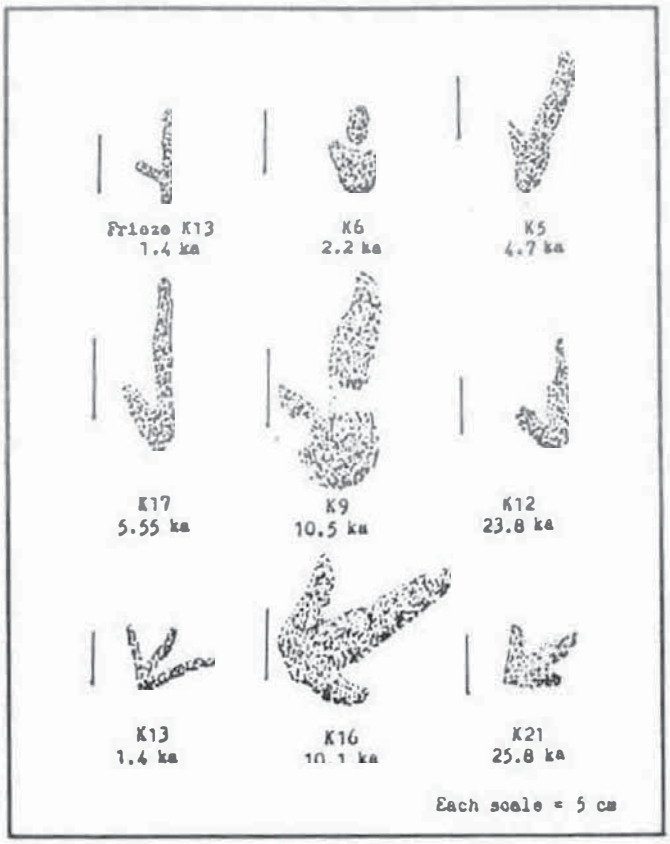


Figure 6.
Range of cation-ratio dates for 'macropod' and 'bird tracks', Karolta.

Conclusion

Cation-ratio dating provides a radiocarbon-calibrated chronology for assigning minimum-limiting ages for petroglyphs in the Olary region. The range and temporal distribution of dates from about 31 600 to 1400 years is fairly constant (Figure 7). This range is based on 24 age estimates. It can be stated with confidence that the basic categories of style were present, with the exception of the abraded groove, during the 10 000 years preceding 21 000 years BP (8 motifs). The basic categories were present between 21 000 and 10 000 years BP (8 motifs), and again (excepting the abraded groove and dots) between 10 000 and 1000 years BP. Very slight evidence of any change in the style of the motifs is to be found in the form of the 'bird' track, K13 (Plate 16), where the percussion marks are much shallower than in the older 'bird' track motifs.

The range of dates shows that the stylistic tradition at Karolta has remained unchanged for at least 30 000 years. This lack of change is similar to other locations in the world, for example as noted by Lewis-Williams (1984) in southern Africa and Whitley and Dorn (1987; unpubl. data) in eastern California. 'Style' does not necessarily change with time. If the cultural environment were stable there might not be a need for change in the style of rock art.

The spread of ages, surprising to some, may allow these individuals to question the validity of the dating technique. We would respond in two ways. First, the age estimates are internally consistent, as the single case of superimposition discussed earlier shows. Also, the varnish on two parts of one design gave essentially the same age, dot

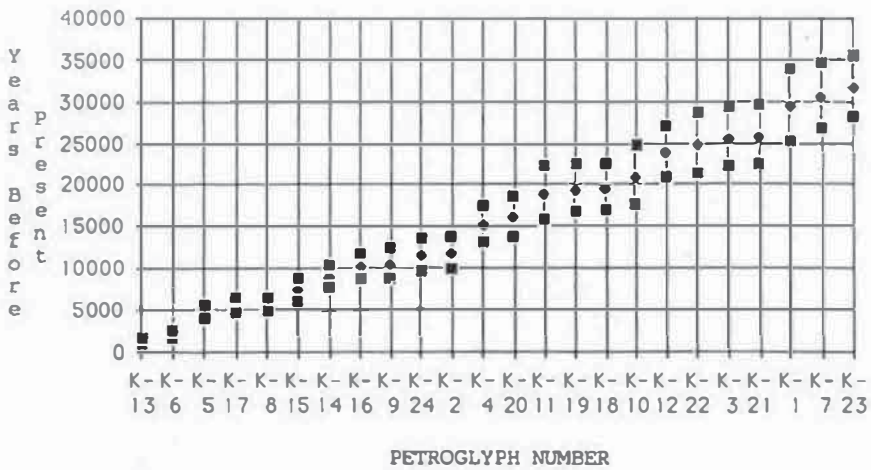


Figure 7.
Petroglyph cation-ratio dates arranged according to mean date (with 1 standard error), as presented in the last column in Table 2.

(K18 at c. 19.4 ka) and circle (K19 at c. 19.6 ka). Second, the most likely cause for a systematic error in the dating method would tend to make all the ages the same. If the rock varnish had experienced an erosional episode (or episodes), it would have affected all the varnishes. If this were the case, the dates would all be similar, they would mark the onset of varnishing after the last erosional episode. The range of cation-ratio ages portrayed in Figure 7 indicates this is not the case.

The cation-ratio dates at Karolta should be viewed as preliminary, for several reasons. More calibration points should be obtained to increase the confidence in the calibration. The 10% correction for the radiocarbon calibrations may change some, but probably not significantly. Lastly, the method is less than a decade old and is under a constant process of growth and development. We consider it a healthy sign that others have found success with the cation-ratio method (Glazovskiy 1985; Harrington and Whitney 1987; Pineda et al. 1988).

Dating rock varnish formed on petroglyphs places a minimum age on the rock art. This has been demonstrated to be significant at Karolta, because it places rock art in arid Australia to before 31 000 radiocarbon-calibrated years BP. The geographical understanding of the people had remained secure and stable for a great length of time, and (perhaps) without a written record, their knowledge of landscape and their dependence on it is reflected in the petroglyphs as an exquisite continuity of skills. It is unlikely that the style and technique developed suddenly, and it is probable that even older motifs occur in this region.

The older cation-ratio dates obtained for this style point up the fact that people living in the arid zone of Australia, some 200 km from the Willandra Lakes system to the south-east, and the same distance from the better watered coastal areas to the west, were returning again and again to make petroglyphs on special rock surfaces. This coincides with a time when people were living on the shores of Lake Mungo. Our results also establish that the people that occupied what is now the arid zone of Australia were making petroglyphs, undoubtedly for a purpose, prior to 31 000 years BP. It is possible that settlement of this area could have been well developed by this time since a non-oral method of communication had developed.

Archaeologists in Australia, as well as in North America, have demonstrated a disinclination to treat Aboriginal and Native American rock art as little more than a side issue to the serious consideration of the history of these peoples. The fact that art is a part of human culture and human experience is often overlooked by many of these archaeologists. It is to be hoped that with the advent of a reliable method of assigning calibrated ages to rock varnish within petroglyphs, archaeologists will learn to incorporate this rich data base into their research methodologies.

Acknowledgments

We wish to acknowledge the part that *Rock Art Research* has played in this project. It is the only Australian publication dealing solely with the study of prehistoric art and much of the work being carried out in this field would remain unavailable without this journal.

We thank John Clegg, Jo Flood and Natalie Franklin for their helpful criticisms, Jack Nobbs for photography, Denise Dorn for field and laboratory assistance, T. Cahill and K. Kusko for PIXE analyses; and T. Tamers, J. Stipp of Beta Analytic, and W. Wolfli of Zurich, for AMS radiocarbon dates. Funded by NSF Grants EAR 87-57014 and SES 86-57014.

COMMENTS

By ROBERT G. BEDNARIK

Before commenting on this important paper it is only right that I should admit to possibly being somewhat biased in favour of Nobbs and Dorn's results. Not only have I been involved in organising their project (for example by teaming them up) and thus have a keen interest in its success, I have on various occasions expressed my preference for 'direct dating' of rock art over 'archaeological dating', because the latter relies on inferences about relationships that often do not stand up to scrutiny. Moreover, the results of Nobbs and Dorn provide considerable support for claims I have made about Panaramitee-type petroglyphs for many years. For instance, I have proposed that the abraded grooves at Panaramitee North are chronologically sandwiched between two superficially similar artistic traditions of percussion petroglyphs (Bednarik 1985b: 81, 1988a: Fig. 1). Dorn's dates suggest the same for Karolta 1. More importantly I have postulated that the oldest Panaramitee motifs 'are certainly several times as old as the youngest', at a time when various other authors conceded that they could not even discriminate between the earlier and later elements in individual superimpositions. I could therefore be accused of being biased in this debate and readers will have to decide themselves whether this is reflected in my Comment.

Having reviewed the CR method some years ago I arrived already then at the conclusion that it tends to provide conservative (i.e. too young), but not very accurate dates. Since then, Dorn has devised ingenious means of minimising errors and limiting their sources, but several problems remain:

(1) The factor of 'time transgression', i.e. the time it takes for incipient varnish patches to grow laterally and to form a complete coating, remains unknown. My observations indicate that it can vary quite significantly. This may always remain the one major error source of the method.

(2) The time incipient varnish patches take to establish can range from about 20 years to a few millennia, and even if Nobbs and Dorn's estimate of 100 years were correct for Karolta 1 this would not necessarily apply elsewhere. I have microscopically examined numerous historical, dated petroglyphs (about 40 at one site alone, at Spear Hill Complex, Western Australia) and I believe that there are great variations in the time span required for varnish formation. While Engel and Sharp (1958: 515) report an instance in California where

a 'good varnish coating' had developed on the stones of a graded road within 25 years, I have observed a lack of incipient deposit on suitable surfaces that are centuries old. Hunt's (1954) and Blackwelder's (1948, 1954) comments on this subject remain eminently sensible. They need to be considered by Dorn, as well as the findings of archaeologists such as Anati (1963: 189; Anati's key statement has been misquoted by two Australian authors), Rhotert (1952) and Mori (1965). It is also well known that various factors (e.g. roughness of surface, type of rock, relative location of surface, chemical environment, perhaps climate) influence the rate of initial growth, and I feel that especially the role of moisture remains inadequately understood. The varnish has been subjected to research for over a century, but for most of that time it was thought to be a phenomenon of arid environments, when in fact these merely facilitate its survival with their high pH and low precipitation. (The preference of Nobbs and Dorn of the term 'rock varnish' over 'desert varnish' is to be applauded.)

(3) As Nobbs and Dorn observe, any episode of varnish erosion during the history of a coating would invalidate the CR date, rendering it again too young. How confidently can this possibility be excluded in practice?

(4) How confidently can the investigator detect the past activities of lichens and microcolonial fungi, which could jeopardise the reliability of CR dates?

(5) Could other micro-organisms, such as bacteria and algae themselves, have a similar effect?

Significantly all these potential error sources would result in younger rather then older dates, which could only amplify inherent trends of the method itself. I therefore accept the results of this project with some reservations, seeing them as a set of probable, rough minimum dates, and as a persuasive argument favouring the idea that the people who produced the region's early archaic petroglyphs belonged to the population of the nearby Willandra Lakes system, which included the Lake Mungo people. This would only confirm what some of us had suspected for a long time.

A few observations and some minor captious reflections: Nobbs and Dorn's remark about the contamination of varnish through the application of chalk (which distorts the ratio of the crucial cations) adds considerable emphasis to my severe criticism of 'chalkaholics' (Bednarik 1979: 30, 1987)—the people addicted to the chalking of petroglyphs.

While this comprehensive paper provides detailed descriptions of the analytical methods involved in the CR method, it does not, I believe, give how-to-do-it-yourself instructions. Crucial decisions concerning acceptance or rejection of samples have to be made in the field, and they require a level of experience and competence that, quite possibly, very few researchers besides Dorn himself possess at the present time. While the samples for other types of physico-chemical dating methods can often be processed by technicians on a routine basis, the CR method demands an extremely high level of experience with the dating medium.

The authors mention three groups of researchers who demonstrated that the Mn/Fe in rock varnish is concentrated by micro-organisms. This was in fact established earlier by Scheffer et al. (1963) who proposed in much detail that the heavy metal ions were mobilised by members of an edaphon composed chiefly of cyanophycae. Moreover, as I pointed out a decade ago, it was actually an Australian (Francis 1920) who first recognised the involvement of algae in the formation of varnish, but this remained ignored for almost half a century because it was incompatible with the dominant model.

The word style is used liberally in this paper, in contexts such as 'change of style', or 'the style of circles'. Can circles have a style? How would one distinguish between a Gothic circle and a baroque circle? Can track depictions have a style? If they are truly 'naturalistic' they require no style, and if they are not their identification as tracks may be questionable. If style is 'the nonfunctional variation in art' (Sackett 1977), and if it codifies 'emblemic' (Wiessner 1983) visual information, characteristics such as technique (related much more to rock type and tools used, at least in rock art), size (related to nonstylistic constraints and considerations in rock art) and motif (in a style, any motif can carry some emblemic information) do not define a style. I am well aware that even the Panaramitee-type traditions exhibit some stylistic traits, but in a very subtle form that has generally not been recognised in the literature so far.

In my view, the concluding paragraphs fail to do justice to this important and very well argued paper. They include a few clichés and obscure assumptions. For instance, the evidence of a non-oral method of communication is seen as indicating that settlement of the region could have been well established. If the first colonisers of Australia already carried with them a tradition of mark production (Bednarik 1988b), the occurrence of archaic rock art would provide no indication of settlement duration.

Also, I do not share the authors' concern for convincing archaeologists to include rock art data in their methods. It is no more relevant what they think of Nobbs and Dorn's results than it is relevant what rock art researchers think of archaeological data. It is far more important that rock art researchers are made aware of both the potentials and the limitations of Dorn's method, and that they understand the basis and implications of Nobbs and Dorn's results. These results are no doubt also of marginal interest to many others, among them microbiologists, geochemists and archaeologists, none of whom make a great effort to convince us of the relevance of their data. The CR method does have a major archaeological application, it can date stone implements and lithic debris on surface or deflated sites, but this application is not considered here.

I would like to applaud the authors' restraint when they emphasise that their dates must be viewed as preliminary. Also they avoid the trap of pronouncing the continuity suggested by their dates as evidence of a continuous, unchanging single tradition. This greatly enhances the paper's credibility.

The logical next step in Dorn's work would be to obtain CR dates from the type of multifacetted detritus I have described in the Dampier Archipelago (Bednarik 1979). The facets of these boulders are of greatly differing ages and they often bear petroglyphs. The dating of a sequence of facets would provide a series of related minimum and maximum ages (in a region of great stylistic variation in rock art) and could result in an absolute chronological framework for the Pilbara petroglyph sequence. This would therefore overcome the current limitation of the CR method, of still only providing minimum ages.

Another potential research involvement would be offered by what I have labelled the Connick Project: during the recent Pilbara field trip of AURA, chemist Prof. Robert Connick mooted the idea that a long-term international project be set up to study repatination processes. This would entail centuries of monitoring test markings in various parts of the world, and should involve the participation of a CR specialist.

I perceive only one fundamental problem with the actual results of Nobbs and Dorn: the postulated early appearance of track motifs. The Pleistocene traditions of Early Man Shelter (Queensland; Rosenfeld 1981) and the Mount Gambier cave sites (South Australia; Bednarik 1986) are free of tracks, and they share many characteristics. Indeed, the Mt Gambier corpus provides a perfect stylistic link between Tasmania and Early Man. There are basically four possibilities to account for the proposed occurrence of early tracks in the Olary region:

(1) The dates are wrong;
(2) The motifs in question do not depict tracks;
(3) There were two art traditions in use at some time between 12 000 and 26 000 BP, one with and one without tracks;
(4) The non-track tradition predates 26 000 BP.

Unfortunately there seem to be some apparently valid objections to each of these alternatives. The problem can be resolved by further research, and it would be useful to reflect that tracks are often considered 'figurative', but that in the final analysis most, if not all, 'geometric' motifs are also figurative. The crucial separation is therefore not between apparently figurative and nonfigurative motifs, but between those involving the abstraction of three-dimensional objects, and those based on two-dimensional visual experiences, including tracks (Bednarik 1985b: 82).

Why did the Emu walk away?

The entire subject of track depiction in early Australian petroglyphs is a complex one, and it is most appropriate here to reiterate the profound importance of Clegg's dictum that we should not pretend to know what something depicts in prehistoric art. Clegg has provided us with some fascinating hints about how our perception of art is determined by our cultural, and indeed cognitive, conditioning (e.g. Clegg 1984: 116). To illustrate this, let us consider a 'composition' Nobbs and Dorn mention—fortunately without attributing unequivocal meaning to it: a group of dots, associated with a series of trident motifs that resemble bird tracks leading away from the dots. Whenever this motif

association finds mention in the literature, we are told that the arrangement represents an emu walking away from a clutch of eggs. This sounds eminently reasonable (as do most other interpretations in rock art), so why consider the matter any further? Needless to say, it 'proves' that isolated tridents also depict emu tracks.

This is the stuff ethnocentric and supposedly iconographic interpretation of rock art is generally made of. Nobody questions why the emu always seems to walk *away* from, never *towards* the nest; or whether this interpretation has been tested; or whether alternative ones have been discredited. Yet it is patently obvious that the tridents could be a row of arrows pointing to the dot cluster, in the fashion of a treasure map. The objection against this interpretation would presumably be that the convention of indicating direction with an arrow is a European one. In other words, people who drive cars and use street signs and computers are far more likely to employ projectile barbs as a metaphor than hunters and gatherers.

I am not trying to devise some mock argument or be facetious, I merely wish to illustrate the complexity of symbol systems, and the inadequacy of crude attempts to break their codes (I have recently related some highly pertinent ideas on this; Bednarik 1988c). There are of course many other interpretations possible: for instance the tridents might depict vulvae arranged in a row. Rather than engaging in some (no doubt mind-boggling) speculation about the possible symbolism of this interpretation (let us bear in mind that in European Upper Palaeolithic art, tridents are inevitably interpreted either as vulvae, or as arrows, and the respective preferences seem to tell us much about their exponents), I wish to point out that a series of aligned tridents may simply be a broken-up herringbone pattern, a very common motif among early Australian petroglyphs. This is a somewhat more convincing explanation, in view of the many known examples where two tridents are joined (e.g. in Early Man). Readers are encouraged to think of other potential interpretations; to do so is a useful exercise to cultivate objectivity in one's thinking.

Of course a large proportion of trident motifs shows pronounced characteristics of bird tracks, which is what prompted me (Bednarik 1985b: 81) to suggest that perhaps prehistoric visitors of art sites, like us, perceived a bird track when in fact they looked at a trident motif (the latter has no immediately apparent meaning in the iconographic system of modern or recent humans), and their own copies of this motif incorporate their interpretation: they are indeed tracks. Hence the apparent stylistic continuity at the Olary sites reflects not cultural continuity, *it reflects the adaptation of the same motifs by successive users of the sites.*

Continued on page 139

DELIBERATE ENGRAVINGS ON BONE ARTEFACTS OF HOMO ERECTUS. *By Dietrich Mania and Ursula Mania*

***Plate 1** (above left).*
Bilzingsleben. View of the excavation area; state in 1978.

***Plate 2** (above right).*
Bilzingsleben. View of the former lake shore, with the adjoining shore terrace on the left, covered with numerous finds. Viewed towards north-west.

***Plate 3** (below).*
Bilzingsleben. Workshop. On one slab of stone (anvil), the fragment of an elephant pelvis; to the right, close by, are some choppers made of limestone and quartzite. Behind the anvil, seen in profile, engraved bone artefact 1 can be recognised.

***Plate 4** (right).*
Bilzingsleben, bone artefact 1. Middle portion of the sequence of lines.

Plate 5.
Bilzingsleben, bone artefact 1. Group of single lines engraved at end of artefact.

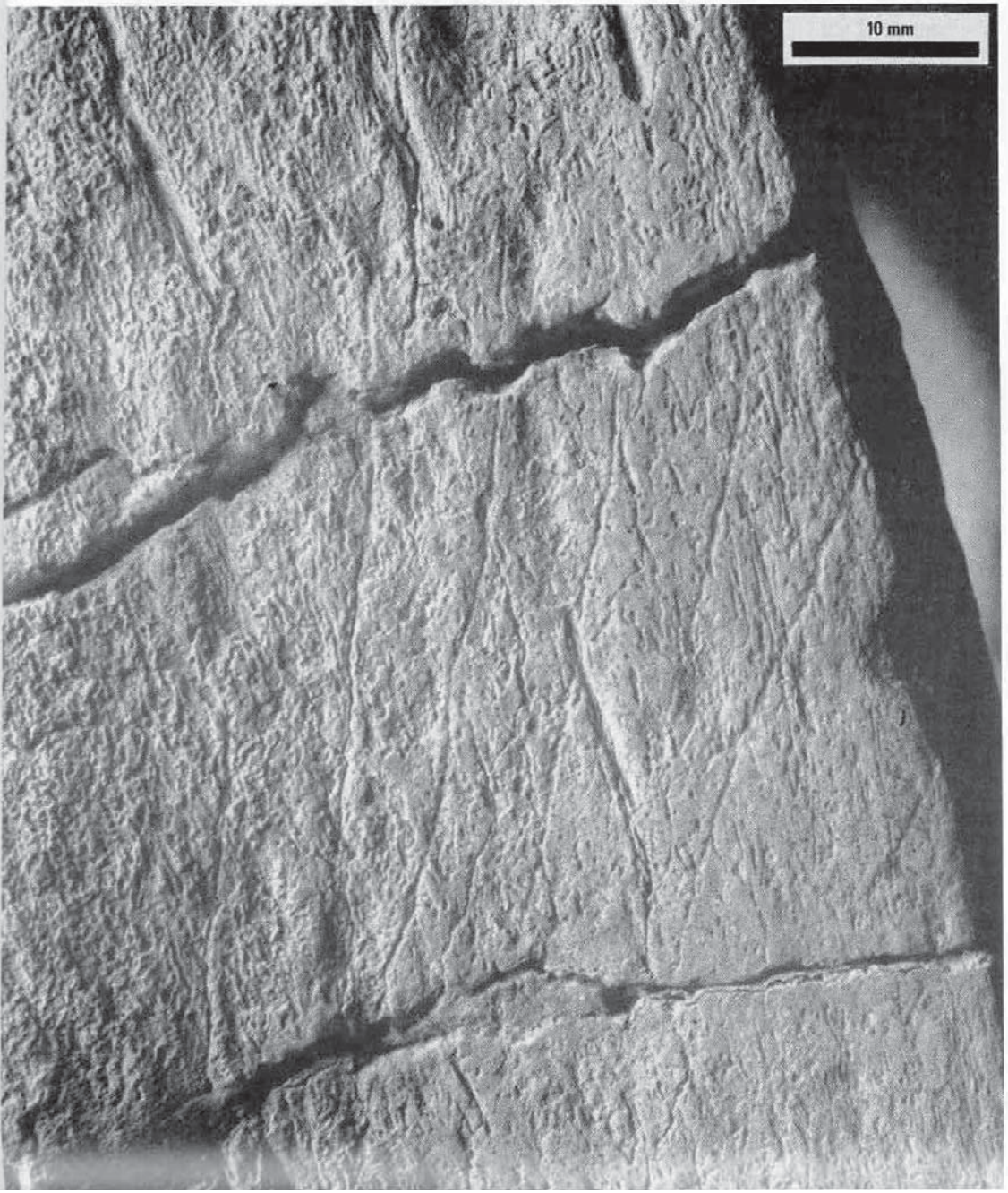


Plate 6.
Bilzingsleben, bone artefact 1. Middle portion of the sequence of lines, left part.

Plate 7.
Bilzingsleben, bone artefact 1. Middle portion of the sequence of lines, middle part.

Plate 8.
Bilzingsleben, bone artefact 1. Middle portion of the sequence of lines, right part.

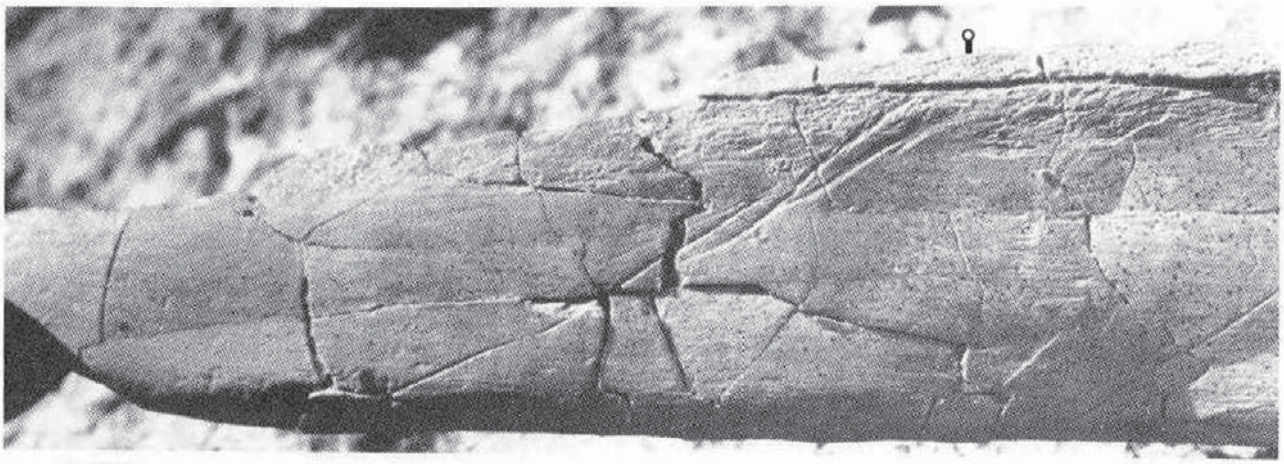


Plate 9.
Bilzingsleben. Bone artefact 2, diagonal line near one end.

Plate 10.
Bilzingsleben. Bone artefact 2, detail of area adjacent to that shown on Plate 9.

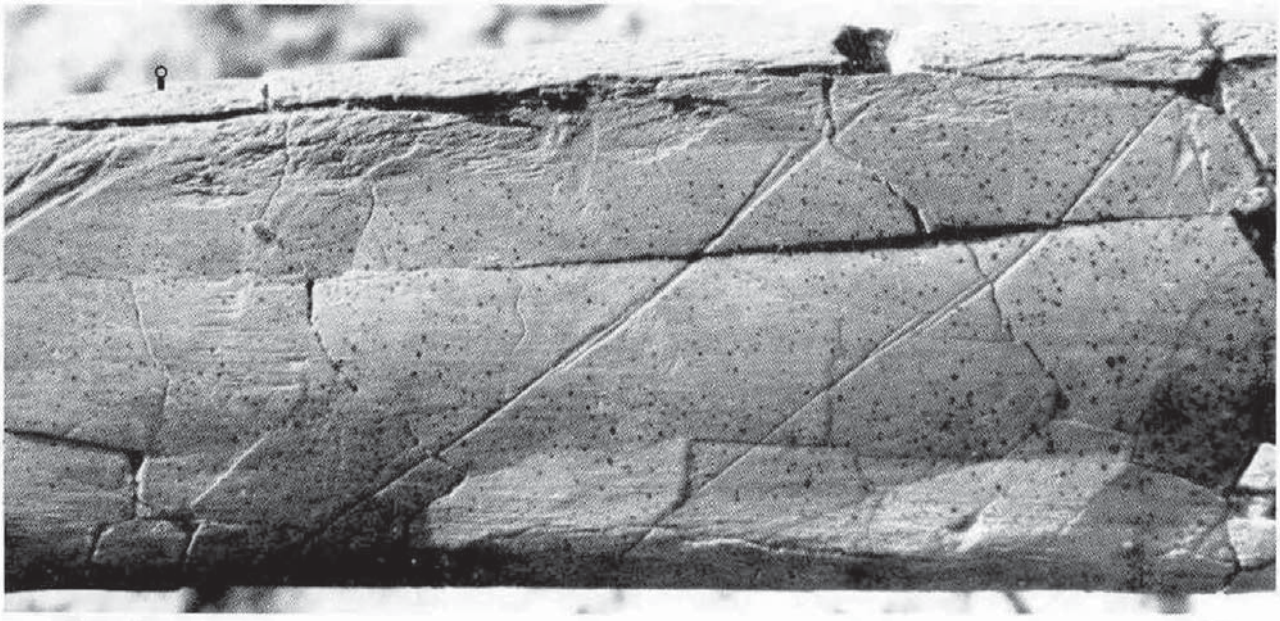


Plate 11.
Bilzingsleben. Bone artefact 2, diagonal lines in the middle of the specimen.

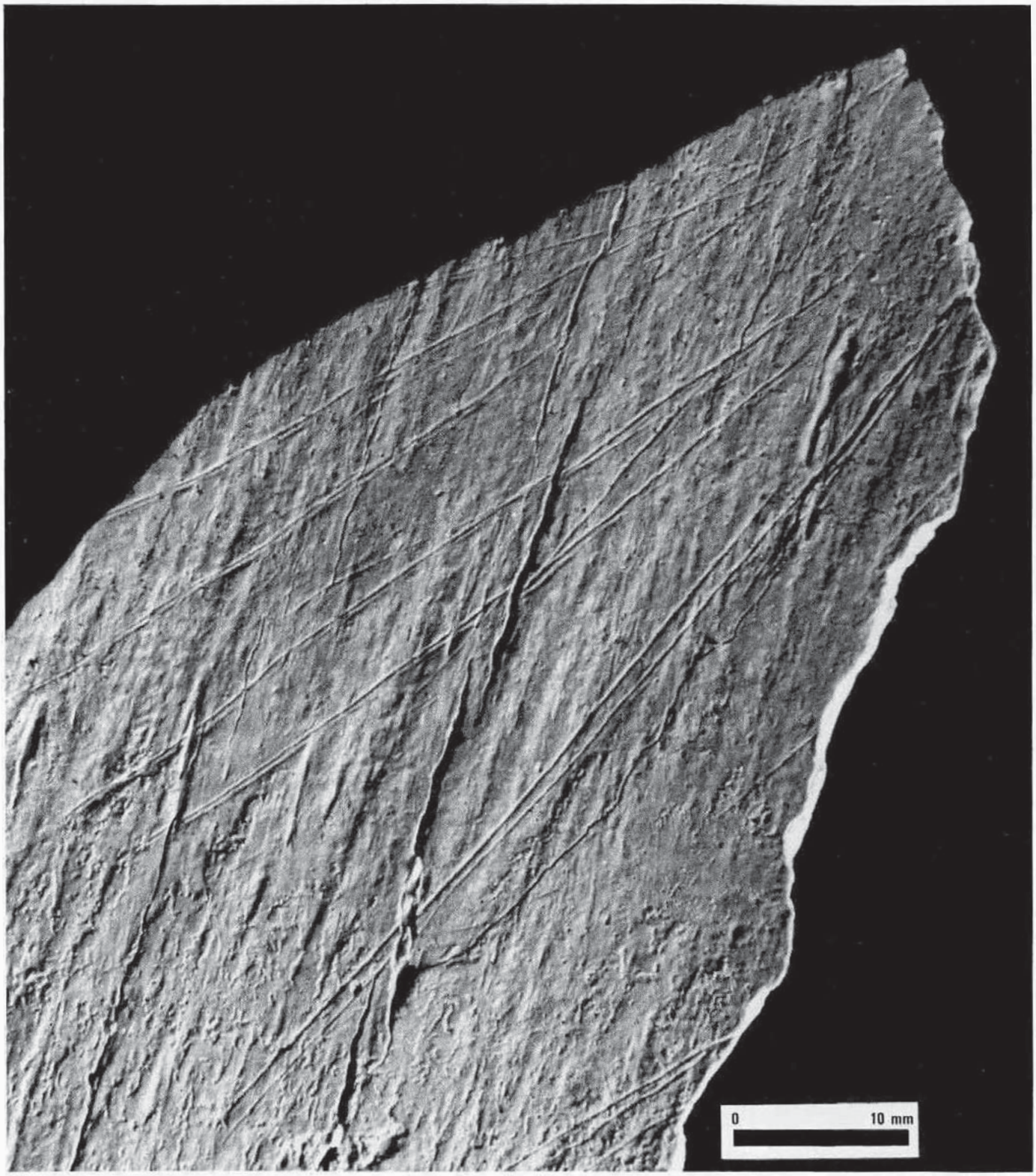


Plate 12. Bilzingsleben. Bone artefact 3.

DELIBERATE ENGRAVINGS ON BONE ARTEFACTS OF HOMO ERECTUS
By Dietrich Mania and Ursula Mania

Plate 13. *Bilzingsleben. Bone artefact 4.*

Plate 14. *Bilzingsleben. Use wear traces on bone artefact, for comparison.*

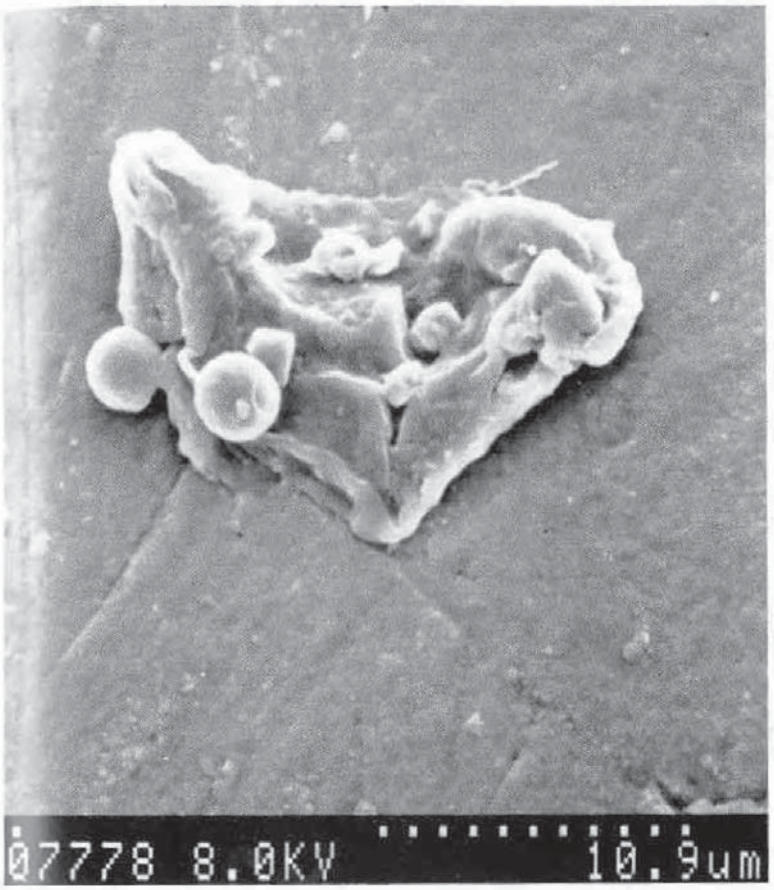


Plate 1.
Scanning electron micrograph (SEM) of incipient varnish developed on the c. 1884 inscription at Karolta. Since no varnish was observed on a 1930 inscription at the site, varnish probably starts to form after about 50 to 100 years at Karolta. This is similar to rates of varnish onset in south-western North America.

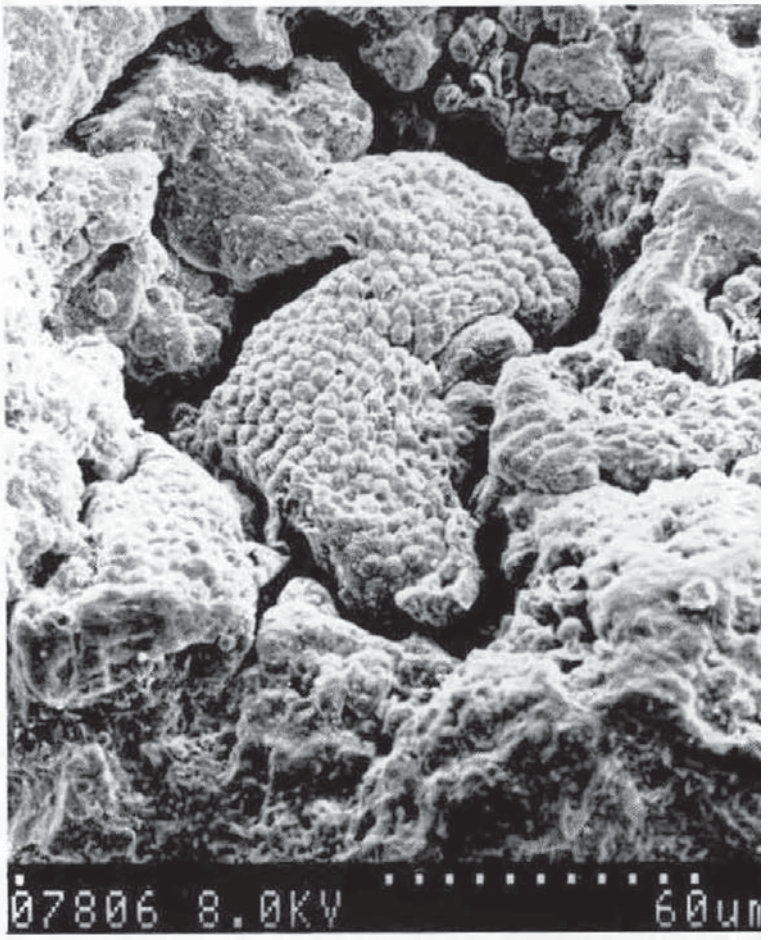


Plate 2.
SEM of microcolonial fungi on an unpecked surface at Karolta. Microcolonial fungi were largely avoided in sampling, due to their effect of changing a cation ratio.

Plate 3.
Pepuarta Bluff on Oulnina Homestead near Mannahill and near the Karolta Site. The cuesta of dolomitic siltstone provides geomorphic exposures of different ages. Varnish samples from three hillslope exposures and a small alluvial fan were collected for radiocarbon dating. The oldest exposure was beyond the c. 45 000-year limit of the technique and was not useful as a calibration point for this study. The locations of the three younger AMS dates, in Table 1, are identified by the letter 's'.

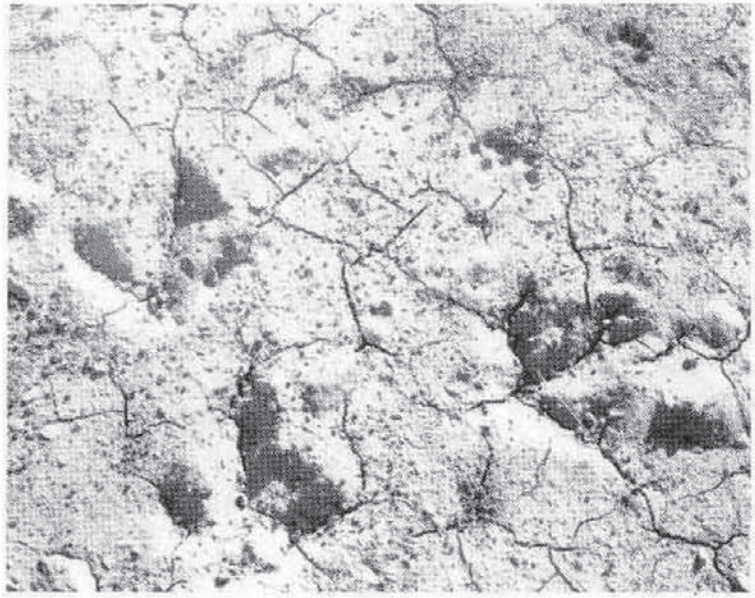


Plate 4.
Emu footprints left in the soil near the Karolta site.

Plate 5.
Petroglyph K1, a dot, about 29 ka. One of a group of 18 dots, within a group of 8 dots, similar to K12 and K19. Diameter of dot about 20 mm.

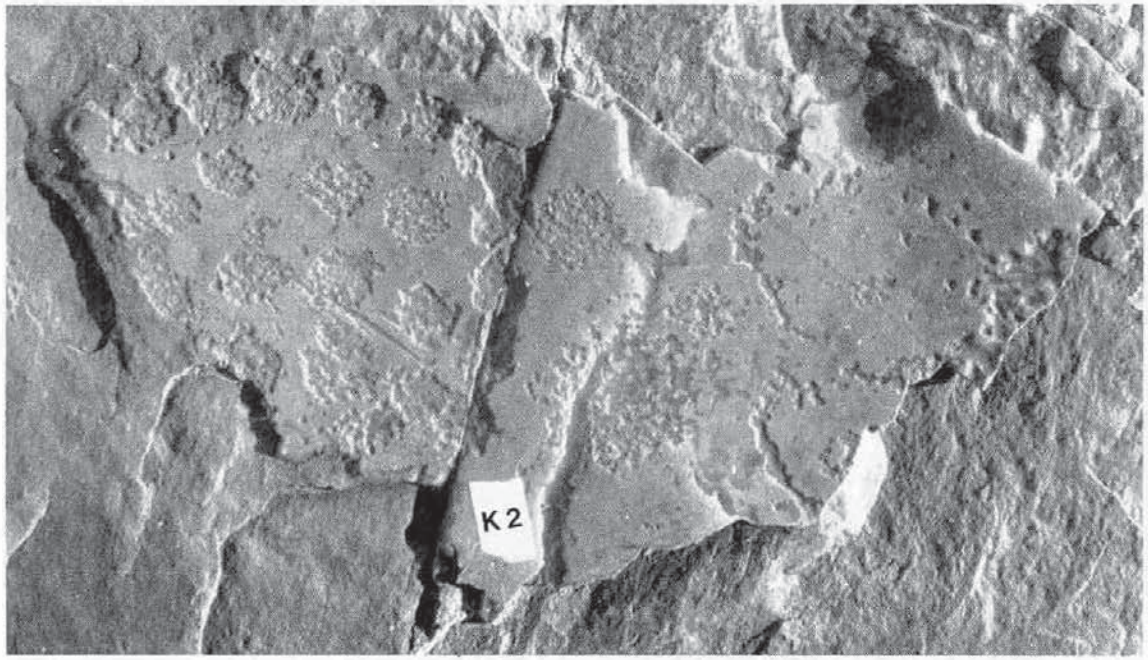


Plate 6.
Petroglyph K2, a dot, about 12 ka. This dot, about 20 mm in diameter, is less than 30 cm from K1, with a difference in age of about 17 000 years. Spalling has removed a smooth surface that could have hosted petroglyphs between K1 and K2.

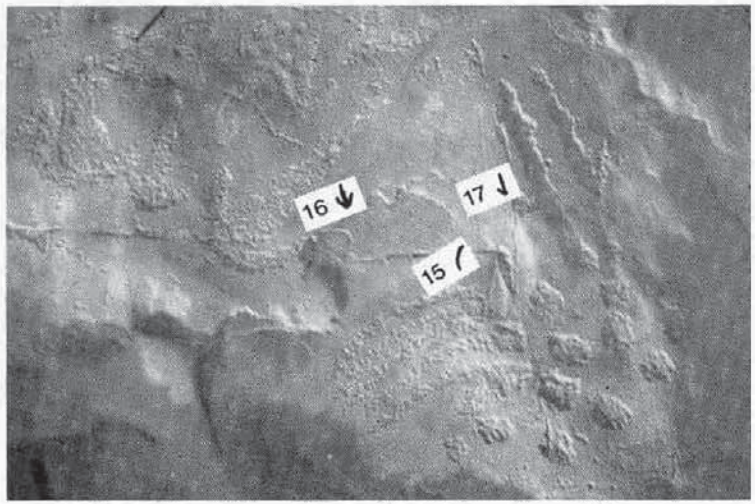


Plate 7.
Petroglyph K15, a curved line, about 7 ka; K16, a 'bird track', about 10 ka; K17, a 'macropod track', about 5.6 ka. All motifs are younger than 10 000 years. Surface measures 35 x 50 cm.

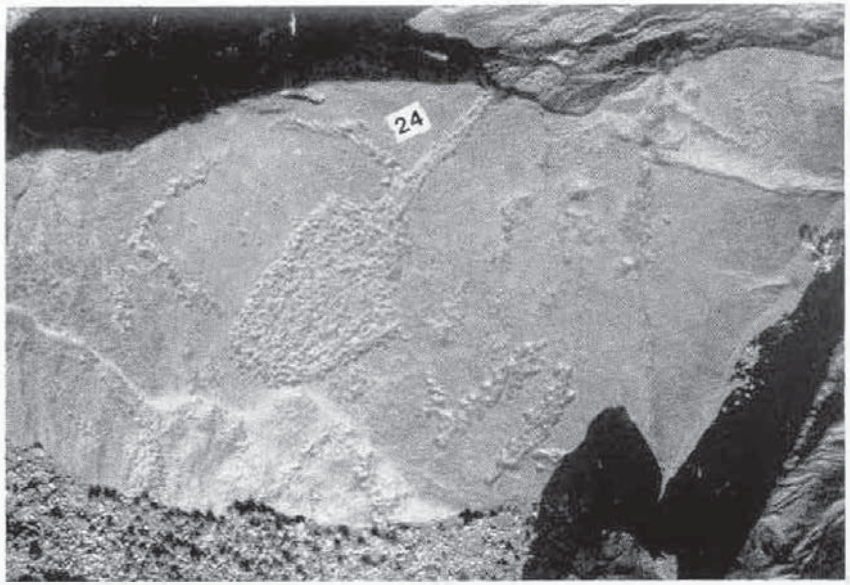


Plate 8.
Petroglyph K24, an 'abstract' motif, about 11.4 ka. The extension leading away from it is about 15 cm long.

Plate 9.
Karolta petroglyph site, outcrop 25. See Plan 1.

Plate 10.
Korolta petroglyph site, outcrop 16. See Plan 2.

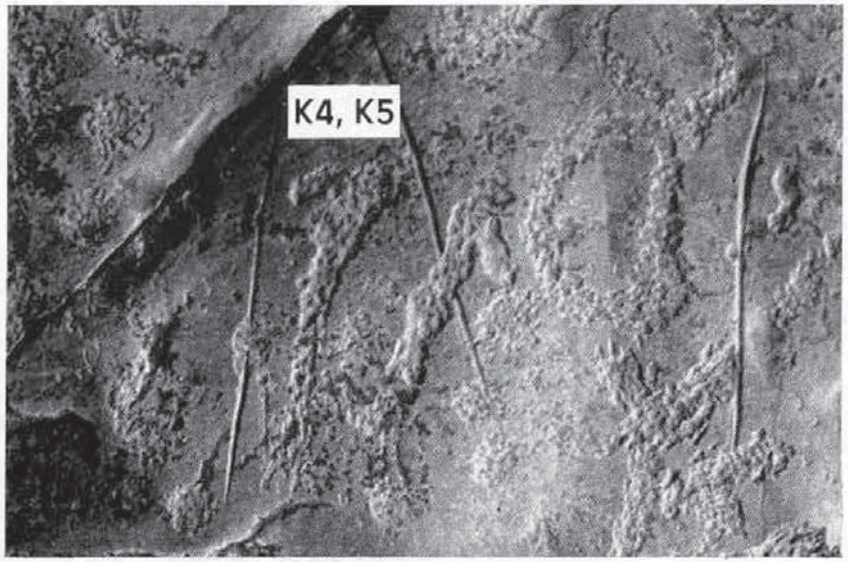


Plate 11.
Petroglyph K4, an abraded groove, about 15 ka; K5, a 'macropod track', about 4.7 ka.

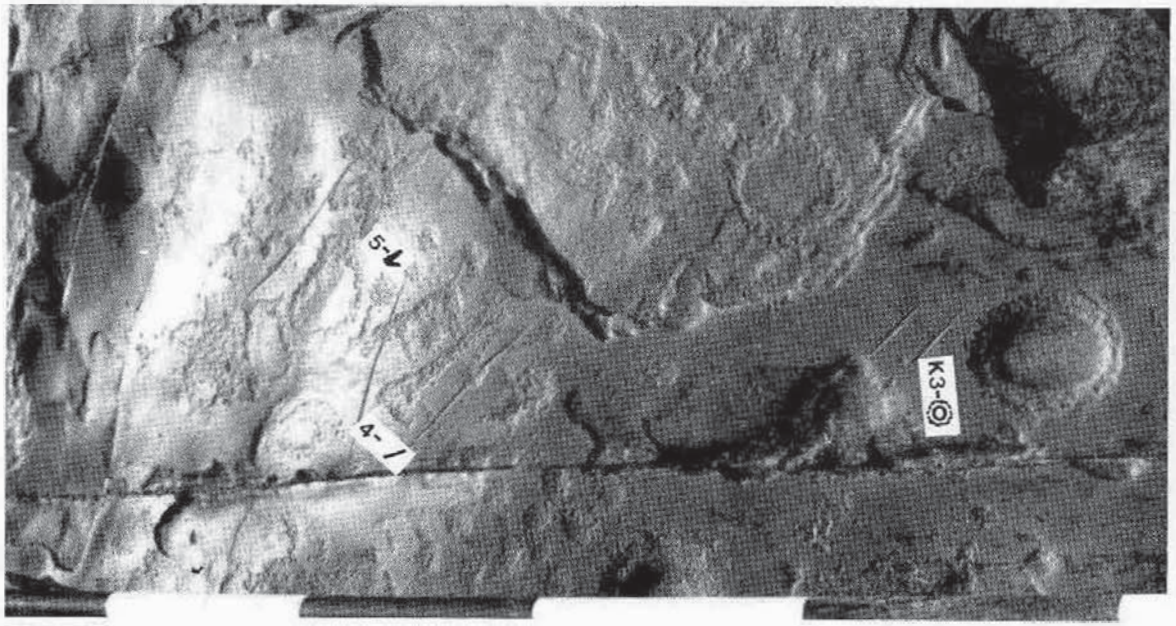


Plate 12.
Petroglyph K3, a decorated pit, about 25.5 ka; K4, an abraded groove, c. 15 ka; K5, a 'macropod track', about 4.7 ka. The use of decoration to emphasise a natural feature is evident early on, as shown by K3. The abraded grooves K4 and K20 are very close in age. The divisions on the survey staff are 20 cm.

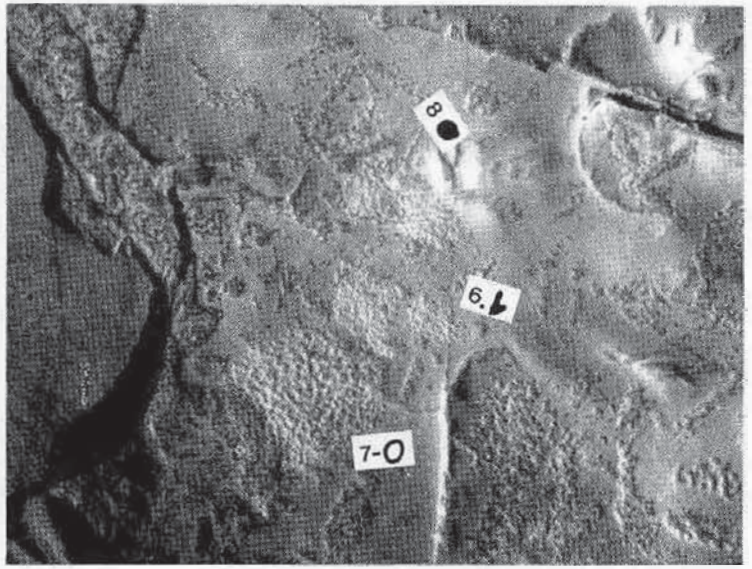


Plate 13.
Petroglyph K6 ('macropod track'), c. 2.2 ka; superimposed over K7, a circle of c. 30.5 ka; K8, an infilled circle, is 5.7 ka. K6 is of lighter colour than other tracks sampled, and more finely pecked. The panel comprising K6, K7 and K8 measures 30 x 40 cm and includes one of the oldest and one of the youngest motifs dated.

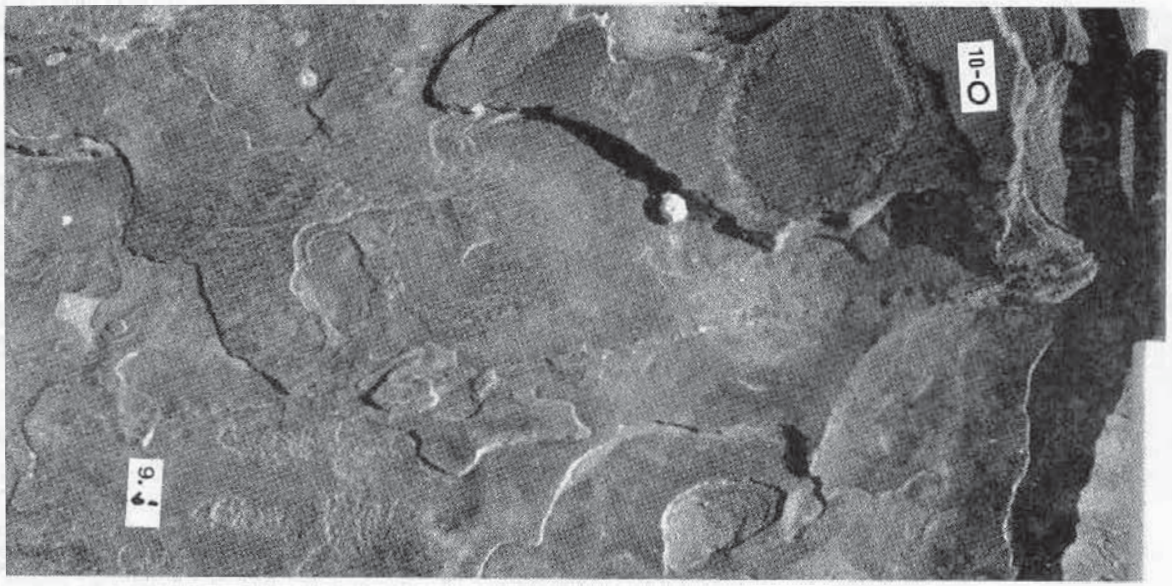


Plate 14.
Petroglyph K9 ('macropod track'), c. 10.4 ka; K10, a circle, c. 21 ka. 'Pad' of K9 is distinctly separate from the 'toes'. K10 was made after spalling of the rock surface. Sampled motifs are about 40 cm apart.

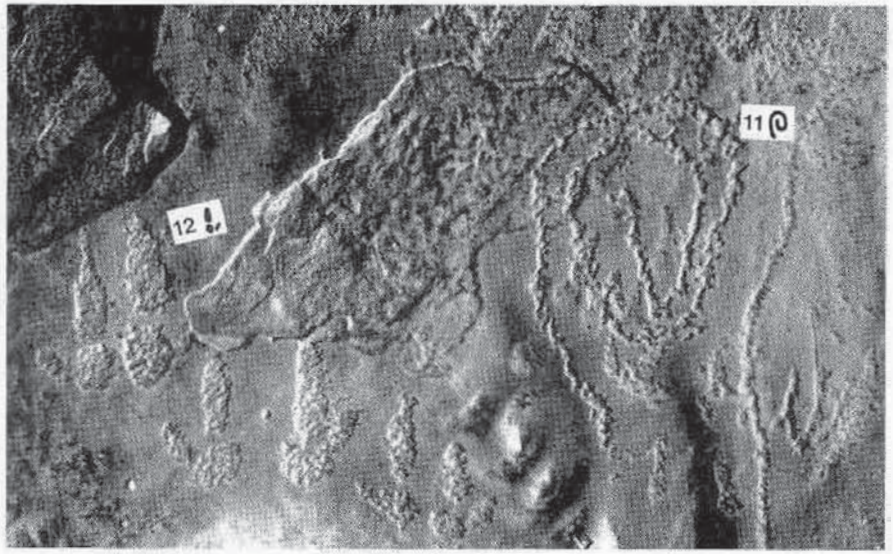


Plate 15.
Petroglyph K11 (spiral), about 19 ka; K12 ('macropod track'), c. 24 ka. K12 is deeply pecked and has a gap between 'toes' and 'pads'. The surface measures 60 x 80 cm and K11 overlaps a spalled area.

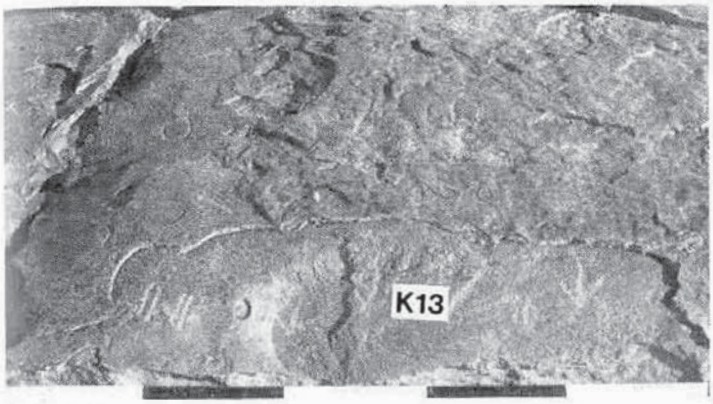


Plate 16.
Petroglyph K13, a 'bird track', c. 1.4 ka. Youngest motif sampled.

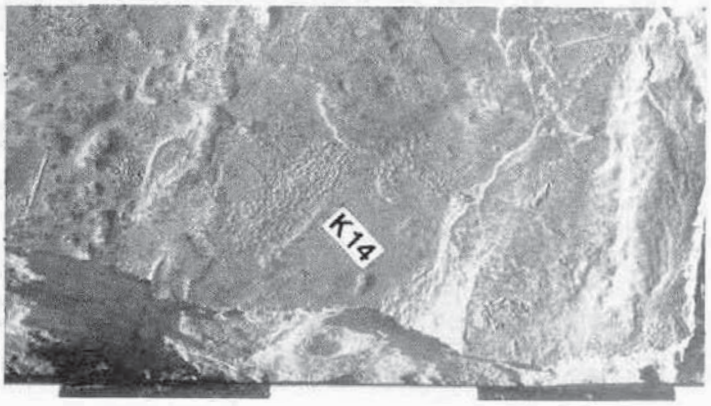


Plate 17.
Petroglyph K14, an infilled circle, about 9 ka.

Plate 18.
Petroglyph K18, a circle, c. 19.6 ka; K19, a dot, c. 19.4 ka. The ages of the varnishes on these motifs cannot be differentiated by the CR dating method. Since they are so close in age, these motifs are considered to form a single arrangement.

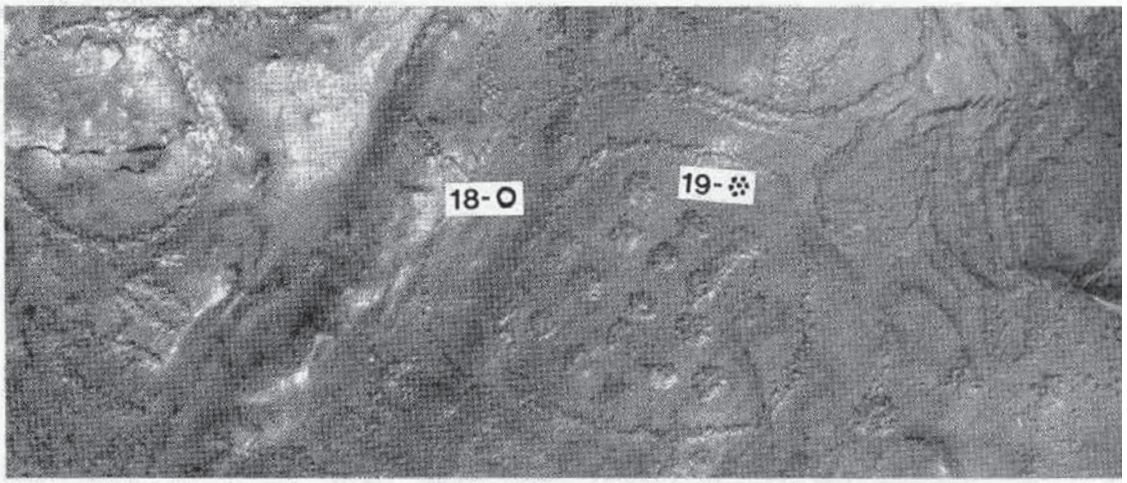


Plate 19.
Petroglyph K20, an abraded groove, c. 16 ka; K21, a 'bird track', c. 26 ka; K22, a circle, c. 25 ka; K23, a curved line, c. 31.5 ka. The surface measures 30 x 60 cm. K21 is from a line of 6 'bird tracks', crossed at nearly right angle by a line of a similar set of 7 'bird tracks'. Both lines come from a group of dots, suggesting emus leaving nests. K23 is the oldest motif sampled.

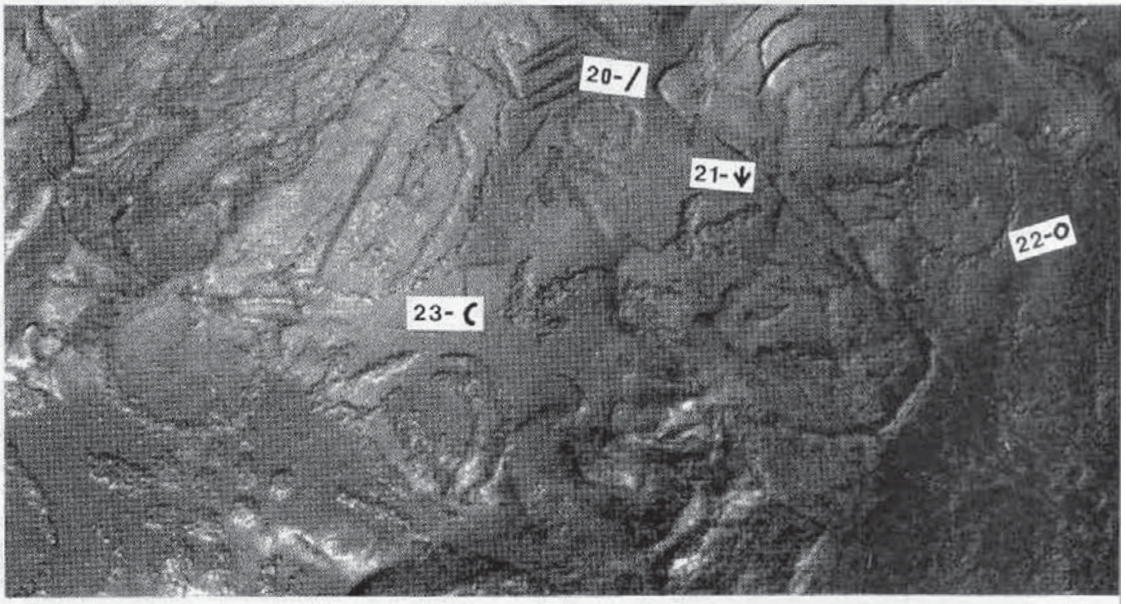


Plate 20.
Historical inscription on outcrop 16, Karolta.

*

AGE DETERMINATIONS FOR ROCK VARNISH FORMATION WITHIN PETROGLYPHS: CATION-RATIO DATING OF 24 MOTIFS FROM THE OLARY REGION, S. A.
M. F. Nobbs and R. I. Dorn

Plate 1.
Limestone tower, Chillagoe-Mungana region, north Queensland.

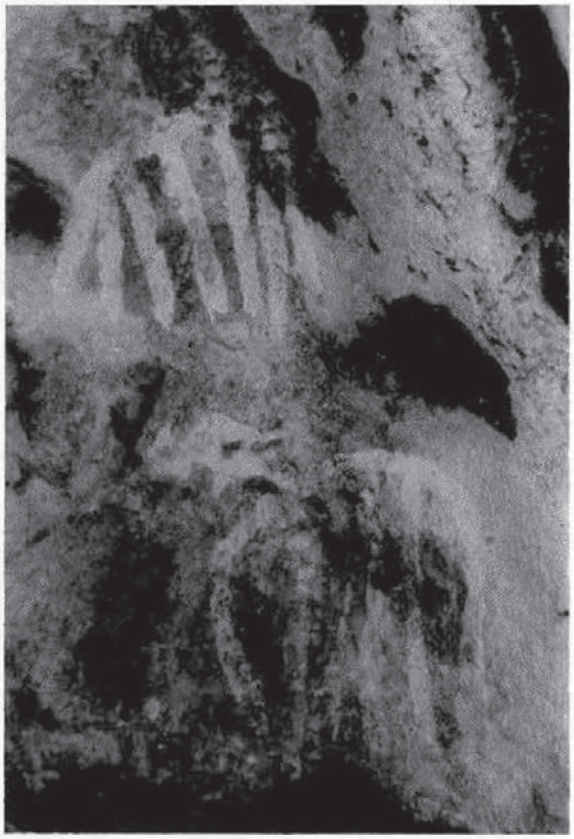


Plate 3.
Type E1 motifs, Chillagoe-Mungana region.

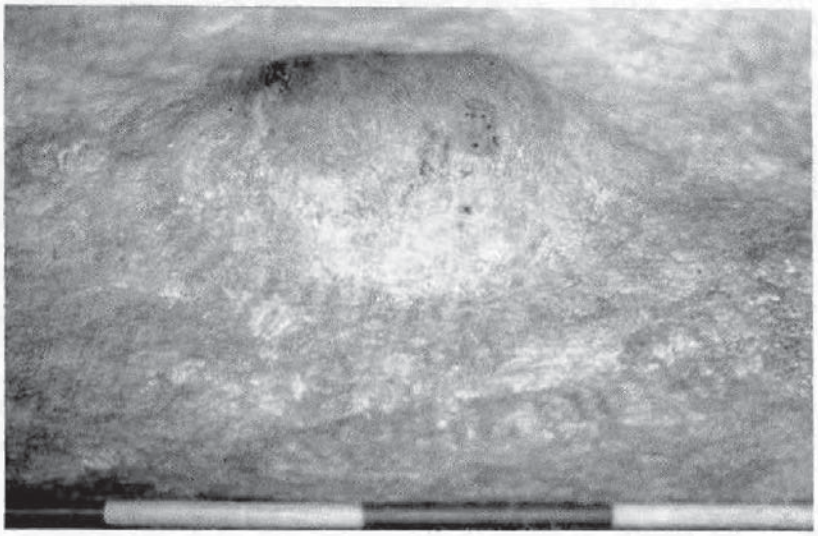


Plate 2.
Rock incisons, Chillagoe-Mungana region.

Plate 4 (above).
Type E2 motif.

Plate 5 (left).
Large natural depression in rock overhang, surrounded by lines of red ochre.

ROCK PICTURES OF THE CHILLAGOE-MUNGANA LIMESTONE BELT, NORTH QUEENSLAND. By Bruno David and Maree David

The ethnographic evidence is unequivocal on that point: according to the Aboriginal consultants, petroglyphs were made when the sites were created, in the very distant past (e.g. Edwards 1966: 36), and their designs were incorporated into other Aboriginal art systems, including paintings. Thus the concept of a single continuous style and culture is not only illogical, it is contradicted by both archaeological observation (the considerable changes over the 30 000 years in question) and ethnographic information.

The importance of this paper is selfevident: the project by Dorn and Nobbs has resulted in the earliest absolute (minimum) dates for rock art in the world. This does not imply that Australia has the oldest rock art—merely the oldest dated rock art. Moreover, the paper provides substantial evidence that 'direct dating' of rock art is feasible. While there are notable examples of convincing archaeological dating (especially Rosenfeld 1981), the cumulative errors it can involve often render any method more reliable that is based on some indisputable direct physical relationship between rock art and some geomorphological phenomenon (Bednarik 1979), even if such a method is itself less accurate than radiocarbon dating.

I congratulate Margaret Nobbs and Dr Ronald I. Dorn on this outstanding achievement, which represents a milestone in the study of Australian rock art.

Robert G. Bednarik
Editor

By JOHN CLEGG

This is a valuable and interesting paper. It raises many points, which, in conjunction with the recent Darwin conference, I found very stimulating.

How Wonderful if it were True!

Hypothesis: The availability of dates for pictures will make other prehistorians accept pictures as an important source of information (last paragraph).

Fifteen years or so ago an (even then) eminent prehistorian explained to me that the reason 'rock art' was not taken seriously enough for research to be fashionable and adequately funded, is that the methods available to study it are not rigorous enough. So Lesley Maynard (McMah 1964; Maynard 1977) and I (Clegg 1977a) spent ten years and more (with the aid and support of many allies, and against the negative vibes of most colleagues) developing and exemplifying suitable rigorous methods (Clegg 1977b, 1981). After these achievements, I hoped that the eminent prehistorian would be prepared to take 'rock art' studies seriously: not so; he shifted his ground and demanded that he be allowed to judge the results of non-trivial work before reconsidering.

As I should have known in the first place, some of us are willing to tackle the challenges posed by pictures; others are not: the differences may be rational, but are somewhere deeply built in, and mere achievements are unlikely to make anti-picture prehistorians change their spots or stances. The 'rigour-lacking argument' is merely one of several rationalisations, or excuses to ignore prehistoric pictures. The most popular is certainly the lack of a reliable dating method, but the removal of that excuse for ignoring us will not suddenly make us the subject of prehistorians' engrossed attention.

Hierarchy of Knowledge

Some other prehistorians believe that one must proceed systematically. There is an hierarchy of credibility and degree of certainty which relates to archaeological data and interpretations. One must not EVEN THINK ABOUT a next step until the first step has been thoroughly established. For instance: dating is fundamental to prehistory; pictures should not be studied unless they are firmly dated.

This is fine if one is searching for TRUTH. But such procedure dictates unacceptable limits on investigation, exploration, research and education. We may, after all, be studying or finding out about prehistory for the light it sheds on ourselves, or the mind-stretching or insighting qualities of the exercise, rather than a bland search for (always changing, and mostly trivial or irretrievable) Truth.

Researchers must be permitted to start anywhere (while stating to the best of their capacity where that is, and what assumptions are being made) and pursue enquiry in any direction, by whatever methods, just as far as one may choose—always being aware that some methods are more reliable and satisfactory and convincing than others.

By Murphy's Law, the most interesting and worthwhile questions are those which are least tractable and for which data is least reliable. Conversely, establishable facts tend to be trivial or truisms (people were adapted to their environments; their behaviour was traditional).

One of the most important and most obvious lessons of the First AURA Congress in Darwin (Clegg 1988), is that prehistorians do not have a monopoly of prehistoric pictures; plenty of other people from other disciplines and paradigms, with different rights, ethics and imperatives have as much or more right to study or use the pictures as do prehistorians.

Dorn's dating may remove the most common *excuse* for many prehistorians to ignore rock art research; it will not affect the behaviour. Something much more significant has happened: after the Darwin congress, rock art research is an entity in its own right; we have contributions to many disciplines, but need be dependent on none. In so far as the relevant disciplines ignore our contribution, they are incomplete.

If the Dorn Method Works . . .

By the time these words are available in print, we should know, for the comments of specialists with knowledge I lack—like Dragovich and Soleilhavoup—will doubtless inform us whether the collecting methods Dorn describes are adequate to select a true sample from the eroded remains of a tangled

mess of disorderly, microscopic filaments—or whatever the material which becomes rock varnish looks like during production.

Independent Checks

There are two interlinking pieces of evidence that I miss in this paper; one is evidence of concordance between Dorn's and independent dates; the other is discussion of Australian pictures' changes which the dates investigate.

I know only one Panaramitee site (Sturts Meadows), but it has plenty of evidence for sequences of varnish (Clegg, forthcoming). I imagine that other sites have comparable phenomena which provide an arguable sequence to check Dorn-dates.

SEQUENCES ARGUED FROM EROSION, CARBON DATING AND EXTINCT FAUNA

I. Erosional Events

Some parts of Old Rock (Rock VIII, South Saddle, at Sturts Meadows) are much more finely split than the smooth, newer-looking surfaces. Although it is not easy to make out the engravings on the more-split rock, it clearly has a high incidence of abraded grooves per total counted engravings.

> % abraded grooves on 3 'old' (more split) surfaces: 30.78, S.D. 19.19
> % abraded grooves on 8 nearby 'non-old' (less split) surfaces: 0.24, S.D. 0.60

Even if the diagnoses of 'old' and 'new' were erroneous, abraded grooves are more abundant on old-looking rocks than elsewhere. There are signs of differential distribution over the whole site—abraded grooves are more abundant in Saddle than elsewhere. This could be because Saddle was engraved at times when abraded grooves were fashionable, or that Saddle was a favoured place for them.

It has not been possible to isolate enough 'definitely old' and 'new' surfaces to take this line of investigation further.

II. Exfoliation

Several 'whale-backs' at Saddle have one surface evidently younger than the remainder, because a skin of rock has peeled off it. There are engravings on both newer and older surfaces, but far more on the older surfaces, suggesting the exfoliation happened during the engraving period (but before the rock varnish formed, for both new and old surfaces are varnished).

The numbers of different types of engravings were counted at each of 11 areas no more than 100 m apart. The areas were classed as 'old' or 'non-old', the latter having evidence of exfoliation post-dating the former. Three situations are compared:

Old Rock old surfaces;

Old Rock non-old (exfoliated) surfaces;

5 other (non-old) surfaces in Saddle.

The old surfaces' differences from the non-old samples are:

(1) high proportion of abraded grooves;

(2) equal numbers of !bird tracks and !macropod tracks.

Non-old sites have more !macropod than !bird tracks, and few if any abraded grooves.

Pearson correlations between incidences of motif types on six areas on Old Rock were calculated, and are shown below. There are three *'old'* areas, and *rough, new* (smooth) and *intermediate* (between rough and smooth) areas.

	Old 2	Old 3	Old 1	Rough	Int	New
Old 2	1.000	**.907**	**.892**	**.845**	**.850**	.698
Old 3		1.000	**.869**	.632	.668	.702
Old 1			1.000	**.881**	**.911**	.379
Rough				1.000	**.989**	.275
Int					1.000	.285
New						1.000

Correlations of 0.798 or more are significant at the 0.01 level (1%) and are boldface.

The NEW area is not much like any of the others; in fact there seems to be a real difference between the smooth (NEW) and rough/intermediate areas. The old, rough and intermediate areas' samples are highly correlated. There are no significant correlations between these areas and the new area.

III. Carbon Dates

Deirdre Dragovich obtained Pleistocene dates from a carbonate sample at Sturts Meadows. Perhaps Dorn could date some varnish from the same rock?

> Manganese-rich varnish occurs on outcrop surfaces and within Aboriginal engravings in an area to the north of Broken Hill. Radiocarbon dating of a 3 mm thick layer of calcium carbonate overlying varnish yielded an age of 7090 ± 310 years bp (SUA 2011). The compact inner layer of carbonate was later separated into two subsamples for dating by accelerator mass spectrometry: the ages obtained were 10 250 ± 170 years bp (Beta-13803) and 10 410 ± 170 years bp (Beta-13804). The main varnish, and possibly the engravings, thus have a minimum age of at least 10 000 years. Patches of incipient younger varnish have developed on the surface of the carbonate layer (Dragovich 1986: 149).

IV. Engravings apparently of Extinct Fauna

It has been clear that the Panaramitee overlapped with extinct fauna, and that therefore possible depictions of extinct fauna must be taken seriously, since 1981, when Rosenfeld established a Pleistocene date at Early Man Shelter in Cape York. But not until 1982, when McDonald established that it is possible to diagnose track representations' species, has there been any point in examining tracks for that purpose.

Mountford (1929: 245-8) claimed that the head of a sea-going crocodile is depicted at Panaramitee [North] itself, many hundreds of kilometres from the nearest living crocodile. He argued that crocodiles were in the area in Pleistocene times, and claims such a date is possible for the Panaramitee engraving. This crocodile depiction is questionable on the grounds that the engravings look like an (for the Panaramitee) ordinary set of overlapping circles, or like a dilly-bag; they look more like the skull of a crocodile than the scaled head. [Readers

are referred to the recent paper by Berndt, abstracted in *RAR* 5: 79.]

Mountford and Edwards (1962: 97-9) claim depictions of marine fish and sea-going turtle at Panaramitee. In the same article they state that there are no dingo tracks in the Panaramitee style. Their absence argues for considerable age, as dingo was introduced 7000 years ago.

Later Mountford and Edwards report dingo tracks (1964: 855, Fig. 5A, M). To me the figures seem undiagnostic of dingo: the hind (4-toed) foot of a dasyurid or phascogale could be indicated. At Sturts Meadows there are several such engravings, a few of which trails have 5-toed and 4-toed tracks. This is characteristic of, among others, Tasmanian devil (*Sarcophilus*) and Tasmanian tiger (*Thylacine*), both of which are extinct in the area, but not of dingo, which has only four toes at ground level on all four feet.

The dingo interpretation depends on scale; the engravings could represent enlarged tracks of a marsupial mouse. The argument that tracks were depicted to the correct size is remade by Tindale (1951: 381-2) on behalf of some big !arrows which are possible *Genyornis* footprint depictions, and Edwards (1965: 229) on behalf of some large, *Procoptodon*-like kangaroo tracks.

The only tracks whose scale is easily determined are those of human feet. At Sturts Meadows there are many engravings which resemble the tracks of tiny human feet—1 or 2 cm long. So I regretfully conclude that not all the !track engravings at Sturts Meadows are life-size representations.

*

All the above examples indicate areas where sequences of Dorn-dates could be compared with argued relative dates. Should the Dorn sequences fit with any of the independently argued sequences, both methods would be reinforced. The arguments for non-Dorn sequences are open to investigation by others.

This paper is an exciting and valuable contribution. I wish merely that we had been told how to take a sample of varnish, so that we could go about trying the method in interesting circumstances. As it is, we seem to be still dependent on Dorn himself. Between reading the paper and writing my Comment on it, I applied for research money to collect suitable samples. Worse luck, the fund to which I was applying has an upper financial limit, which could offer Dorn only half the price of a return ticket to Australia. Let us hope that I get the money, and Dorn is able to come and take more samples, which will eventually be processed to confirm the viability of the dating process and contribute to our understanding of the prehistory of Australian pictures.

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

By D. DRAGOVICH

The paper by Nobbs and Dorn is a 'landmark' event in the quest to provide an absolute age framework for rock engravings in Australia. For the first time, ages of individual engravings have been estimated without recourse to relative chronologies based on evaluations of varnish thickness/colour or interpretations of engraving style or possible superimpositions.

The results are of considerable interest not only because they provide an estimated age of more than 30 000 years for the oldest engraving sampled, but also because it seems that stylistic variations over this extended time period were minimal. One implication of this finding is that the chances of establishing a meaningful absolute age range of engravings in this area without subjecting each individual engraving to cation-ratio dating techniques appear remote.

In addition to setting up absolute ages for 24 engravings, the paper provides a detailed description of the field and laboratory methodology used for cation-ratio dating, which hopefully will encourage replication of the technique in other locations.

The removal of varnish from within engravings can be a sensitive issue and although the actual varnish area needed was not indicated, visible alteration to the varnish surface was apparently minimal. If establishing engraving ages is considered by researchers to be of sufficiently high priority, the samples needed for cation-ratio dating will have to be carefully planned so that the maximum amount of useful information can be obtained. The good fortune in obtaining a random sample of engravings spanning around 30 000 years may not be repeated elsewhere, owing to the lack of stylistic or visible varnishing variation as noted earlier.

It is pleasing to note that Ron Dorn is continuing with his work aimed at establishing the relationships between conventional radiocarbon dates and AMS dates of varnish, and examining the likely variation in cation ratios caused by microenvironmental variation. Such endeavours will add further precision to the use of both radiocarbon and cation-ratio dating for varnishes, and hence varnished engravings.

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

By STEVEN L. RENEAU and CHARLES D. HARRINGTON

Geochemical changes in rock varnish over time provide a potentially valuable technique for estimating the ages of many features in arid and semi-arid lands that previously eluded attempts at dating. Systematic changes occur in the ratio of the cations (K+Ca)/Ti in varnish over time that can be utilised in estimating ages, as first shown by

Dorn and Oberlander (1981b) and later independently verified by other researchers (e.g. Harrington and Whitney 1987). However, we have reservations concerning the technique as described in Nobbs and Dorn, some of which will be discussed in this Comment.

As stated by Nobbs and Dorn, the accuracy of a cation-ratio (CR) date of a feature of unknown age is 'based on the accuracy and precision of the relationship between the varnish CR and the calibration points'. In the study of Nobbs and Dorn, a high level of confidence could be inferred from the remarkable internal consistency in the calibration curve presented in Figure 2. However, the accuracy of both the age assignments and the CR analyses in this study seem to be systematically over-represented, and the actual precision of the technique is thus probably much less than reported, as discussed below.

(1) The '2 sigma cation-ratio errors' used in constructing Figure 2 and in calculating calibrated ages are much less than the reported uncertainty of the laboratory analyses. In the third column of Table 2, standard errors for the analyses are typically 10-12%, yet the reported 'standard error' of the average CR for each sample (actually the standard deviation of the values in the second column) is typically 2-3%. The latter is subsequently used in estimating uncertainties and the reported laboratory errors are ignored.

It should also be noted that the vertical error bars in Figure 2, showing a perfect semilog decrease in magnitude with increasing age, are idealised and do not correspond to the data in Table 1.

(2) The age assignment of the 'initial ratio' (IR) is shown as having no uncertainty, although this is clearly unrealistic. Dorn places the IR at 100 years on his CR curves, based on the observation that 'incipient varnish' has been detected on historic surfaces of this age, while it is apparently absent on younger surfaces. This 'incipient varnish' is not a uniform layer that is instantaneously formed, but instead starts in patches and spreads (e.g. Dorn and Oberlander 1982). The time necessary to develop a complete varnish layer is not known, although Dorn and Oberlander (1982) have estimated that it requires greater than 1000 years in parts of the western United States. It seems that the age of an IR should more appropriately reflect the time necessary to develop a complete coating and as such must be greater than 100 years. The appropriate age is obviously imprecise and this should be reflected in a significant uncertainty for purposes of calibrating a CR curve. Because of the semilog nature of CR curves, relatively minor age differences of several hundred years for an IR would substantially change its position and thus alter a curve.

(3) An additional uncertainty concerning the IR is the assumption that samples collected from natural dust traps, presumably reflecting an average of latest Holocene fallout, are chemically representative of Pleistocene dust flux. It seems that this assumption needs testing due to the major changes in climate, vegetation and associated geomorphic processes that have occurred over the last glacial/interglacial cycle. Temporal changes in the source and chemistry of dust are probably common, and Dorn (1984) has proposed that these are the cause of microchemical laminations in varnish. The representativeness of recent dustfall is thus questionable. Because of the uncertainties associated with an 'initial ratio', we suggest that it would be best to delete this calibration point.

(4) Based on unpublished data on radiocarbon dates from the lowest layer of varnish from surfaces of known age, Dorn has reported that the radiocarbon dates are generally about 10% too young. He thus applies a 10% correction to the radiocarbon dates in developing a CR curve. As presented in Table 1 and used in developing the CR curve, uncertainties of as little as 1% are reported on these corrected dates. Under the best of circumstances, if accreted carbon has an age of 'zero', his correction must be imprecise due to inherent variabilities in the actual thickness of scraped varnish and variable accretion rates of varnish. A simple, uniform 10% correction thus seems unrealistic. A plus-or-minus on the correction needs to be estimated, and this uncertainty needs to be included when calibrating a CR curve.

(5) The age of the accreted carbon is of critical importance in verifying the accuracy of radiocarbon dates from rock varnish. It has been assumed, but not proven, that the carbon has a 'zero' age when accreted, and is derived from local vegetation. However, a lack of understanding as to the nature of the organic material being dated limits confidence in this assumption. It is not presently understood to what extent the organic matter in rock varnish is microbial in origin or consists of plant material. If plant matter is a significant component, as seems likely, then it seems imperative to determine whether these organics are entirely derived from plants growing on the surface in close proximity both in time and space to the newly formed varnish, or whether much of the organics may be introduced as part of the regional dust flux. This dust is largely derived from erosion of surfaces in the region which contain components of older carbon. It is well known that organic carbon in modern soils does not have a 'zero' radiocarbon age, but instead has an 'age' reflecting the recycling rate of carbon in the soil (e.g. Birkeland 1984). The occurrence of extensive playas [claypans] in many arid regions adds further uncertainty as to the source and age of the organic carbon. Such playas are an important source of dust in many areas (e.g. Dorn 1984), and organic matter contained in these sediments should date to pluvial periods. Incorporation of old carbon into varnish would provide erroneously old radiocarbon dates, affecting the calibration curve and yielding calibrated dates that are too old.

Cation-ratio dating of rock varnish is a potentially valuable technique for estimating ages of features in arid and semi-arid lands where material dateable by more conventional methods is not available. However, more caution seems appropriate to avoid overstating the precision of this technique.

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REPLY

By M. F. NOBBS and R. I. DORN

When a relatively new method is applied, there should be caution, and hard questions asked by those interested in its application. From our perspective, we hope these comments are constructive in that they suggest new tests, new insights, and tell us where their authors think we are going right or wrong. We are greatly pleased by the innovative and interesting comments. As this issue goes to press shortly we cannot hope to discuss all of them adequately. Only a few select points will be chosen.

John Clegg and R. G. Bednarik provide excellent suggestions for independent tests of varnish dating. R. Dorn is anxious and willing to examine situations where the methods of dating rock varnish can be tested. He has already communicated to Clegg his willingness to participate in the project mentioned. Alternatively this site could be examined by others interested in using varnish dating techniques. This brings up the comment by Bednarik about the need for experience in dating varnish. Bednarik is correct in his view that considerable experience is required for proper sampling and sample preparation. Cation-ratio dating has been tried by Glazovskiy (1985), Harrington and Whitney (1987) and Pineda et al. (1988). Other researchers, like Dragovich (1988), are examining some of the harder issues such as micron-scale variations in cation ratios. Dorn's students have learned correct sample selection and sample preparation. The learning curve was steep for some, taking months. Others, like D. L. Tanner, were more proficient in laboratory sample preparation than Dorn within a few weeks. Varnish dating is not like radiocarbon dating; one cannot simply collect a charcoal sample and submit it to a laboratory for a fee-based analysis. When Dorn asked for a head count of individuals interested in a short course in cation-ratio dating at a recent U.S. Geological Society of America meeting, one person out of over 100 declared an interest. We hope that the relative difficulty of this method does not discourage rock art researchers from being willing to learn the method.

Bednarik's discussion about 'style' is well taken. M. Nobbs will further evaluate this issue in the near future in a subsequent paper. Bednarik also questions the early appearance of 'track motifs'. We anticipate that extremely close scrutiny of individual motifs may be called for. For example, Nobbs' subjective evaluation of motif K13 is that it was made with a lesser degree of skill. Ongoing work by Nobbs may assist in the resolution of this issue.

Bednarik questions the way varnish develops and correctly concludes that the implication is that a calibrated date on varnish is only a minimum age estimate for the manufacture of a petroglyph. Still, there is a good reason why the dates would be close constraints. Older varnish is usually adjacent to the engravings, serving as a ready source of varnish-forming bacteria for relatively rapid colonisation. Dorn and Oberlander's (1982) biogeochemical theory of varnish growth, indeed influenced by the wisdom of years of observations by Charlie Hunt and Elliot Blackwelder, is that the rate of varnish formation is limited by the rate of manganese concentration by bacteria. In deserts this rate is extremely slow, taking thousands of years to form a complete coating. (Dorn and Oberlander [1982] have rejected Engel and Sharp's [1958] 25-year-old varnish as just re-formed desert pavement.) If Bednarik's site of no incipient varnish was from a smooth substrate like broken chert or quartzite, this rate can be even slower. A partial answer at this point is to continue to gather data by collecting varnish from sites of known independent age (such as historical features, conventional radiocarbon-dated features) and continue to make observations of varnish development at these sites. In summary, this issue simply highlights the view that varnish dates are minimum-limiting constraints on the age of the underlying petroglyph.

Reneau and Harrington raise many excellent concerns about the accuracy and precision of the CR age estimates. We agree with the essence of their points 2 and 3, but not some of the details. The bottom line is that the rate of varnish growth is being tested in as many situations as is possible. It is always best to conduct these tests on varnishes on the same lithology where the petroglyphs are engraved and in a microenvironment that is similar, such as right next to previously varnished rock. This was possible at Karolta, 100 years appears to be an appropriate estimate for the first onset of varnish formation, based on observations from an engraving carved in 1884.

The confusion in comment No. 1 probably derives from a lack of explanation for Table 2, and we thank Reneau and Harrington. Each PIXE measurement (a, b, c) is assigned an analytical error that is often over 10%. This is listed in the third column. When the PIXE measurements are averaged, the standard error of this average is often much lower. This is also listed in the third column next to the average for the three different measurements. The error curves are indeed generated using the 2 sigma uncertainties associated with the CRs and the radiocarbon dates. The 2 sigma CR error bar in Figure 2 for the 'small fan' should actually be much smaller; it is portrayed too large. (I have to manually add the error bars into the chart with the graphics program I use. I made that one too large. Thanks for catching this. RID)

Comments 4 and 5 by Reneau and Harrington concern the radiocarbon method of dating rock varnish. Given the information in our paper, their concerns are quite valid. However, in the time between submission of the paper and this Reply, Dorn has received the results of dozens of tests on radiocarbon dating of varnish conducted in collaboration with the University of Arizona accelerator research group. At semi-arid sites, roughly similar in nature to Karolta, the radiocarbon age of the varnish is just about 10% younger than the conventional radiocarbon age at control sites. Only the laboratory analytical errors are reported in our paper, because we did not know how to properly

add the error associated with this correction. We now have a preliminary estimate of ± 5% (1 standard error). In other words, the best estimate for the *error* associated with a varnish radiocarbon date should be the analytical laboratory error plus 5% of the date. This is based on extensive tests conducted on varnish at sites where the conventional radiocarbon age is known. These results and other concerns regarding the nature and source of the organic matter being dated are discussed in detail in a paper by Dorn, Jull et al. that is under review at the present time (Dorn et al. in prep.).

This is an appropriate place to urge support for new and innovative dating methods. Varnish dating is only the tip of the iceberg. The coming years will see the trial and implementation of a host of new methods. We only hope that the rock art research community will be as helpful and kind, yet appropriately sceptical, to the individuals developing these methods as you have been to us.

Once again, we thank the reviewers, commentators and the editor for their time, interest, suggestions and ideas. We hope that the addition of this new dating technique will support the growing rock art movement—greatly accelerated as it is by higher levels of innovation and scholarship, and by events like the Darwin conference. Rock art research is increasingly a multidisciplinary endeavour. Whether or not this will force the sceptics to take rock art research seriously, as Clegg also hopes, remains to be seen. We hope that the adage, of a new perspective truly starting when the old ideologies die, is incorrect in this case.

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Résumé. La proportion des cations (K+Ca)/Ti (CR) dans les vernis sur roches peut donner une date calibrée quand une corrélation peut être faite entre les valeurs CR et des déterminations d'age radiométriques sur les vernis d'une région. Dans la région aride de Karolta dans le South Australia des dates chronométriques ont pu être déterminées pour 24 pétroglyphes d'après une courbe de lessivage des cations pour des valeurs CR de vernis et des déterminations radiocarbonne par la technique d'accélération des particules et des mesures de masses atomiques. Les détails de la méthode CR pour dater les pétroglyphes sont présentés. Des dates moyennes CR pour 24 pétroglyphes varient de 1400 à 31 500 BP. Ces pétroglyphes ont été choisis au hasard, tout en assurant qu'il y ait représentation suffisante des motifs divers. Les résultats des datations CR calibrées au radiocarbonne indiquent que le style de pétroglyphe à Karolta n'a pour ainsi dire pas changé pendant 30 000 ans.

Zusammenfassung. Das (K+Ca)/Ti Kationen-Verhältnis (CR) von Wüstenlack bietet ein kalibriertes Alter, wenn die CRs der Lacke mit radiometrischen Alter von Lacken eines Gebietes in Wechselbeziehung gebracht werden. Im Raum von Karolta, im ariden Teil Südaustraliens liegend, werden 24 Petroglyphen chronometrische Alter zugewiesen, die sich auf eine Kationen-Auslaugungskurve und Accelerator Masse Spektrometrie Radiokarbon-Alter berufen. Die Einzelheiten der CR Methode zur Datierung von Petroglyphen werden erörtert. Durchschnittliche CR-Alter von 24 bestimmten Petroglyphen reichen von etwa 1400 bis 31 500 Jahren BP. Die untersuchten Petroglyphen wurden willkürlich ausgewählt, innerhalb der Grenzen, eine passende Auswahl von Motiven zu erfassen. Die Ergebnisse der Radiokarbon-kalibrierten Kationen-Verhältnis-Datierung deuten an, dasz der Stil der Petroglyphen von Karolta weitgehend unverändert blieb über 30 000 Jahre.

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VISHNU SRIDHAR WAKANKAR
4 May 1919 – 3 April 1988

Padmashri V. S. Wakankar was born at Neemach (M.P., India) in a traditional Brahmin Hindu family. This environment inculcated in him a thirst for Oriental studies and fine arts. His Sanskrit learnings diverted him towards the studies of epigraphy and palaeography, and he was attracted to numismatics and art history. His collection, comprising thousands of coins and a large number of stone sculptures, is displayed in the Vikram Kiriti Mandir Ujjain. His school of fine arts is expressive of his academic zeal and unstinted dedication to the subject. He made concerted efforts to raise public consciousness of the national heritage. It was for his distinguished services that the Indian government conferred upon him the honour of Padmashri.

After his graduation Wakankar became a full-time worker of the Rastriya Swayam Sewak Sangh (R.S.S.), a voluntary social organisation. The organisational work provided him with the opportunity of traversing the entire Malwa region in Madhya Pradesh and of acquainting himself with the land and the people of the region. This training of R.S.S. made him idealistic, self-reliant, devoted and hard-working. With his modest life style he represented the essence of being Indian. All these qualities made him a worthy explorer.

In 1951 he joined Professor H. D. Sankalia of Deccan College, Pune, who was then excavating Maheshwar in the Malwa region. Wakankar acquainted himself with the discipline of archaeology and exploration techniques. This training provided him with the opportunity of assisting Dr H. V. Trivedi, the then M.P. State Director of Archaeology, in the survey of the area which was likely to be submerged under the Chambal dam project. During this assignment he excavated Manoti, Awara and Modi, where he was introduced to rock art.

After his Chambal assignment Wakankar started his own institute of the fine arts, named Bharati Kala Bhawan. He also began combing the Malwa region which brought to light a large number of Stone Age and protohistoric sites. A number of rock art centres in central India were for the first time explored by him. The exploration of Bhim-Betaka was his finest achievement. From 1960 onwards the author joined him in his expeditions as a member of this team.

Wakankar left for France in 1962, with a view to learning new techniques in archaeology and rock art research. After his return he joined Vikram University, Ujjain, as Director of Excavation and Museums. He undertook the excavation at Kaytha, a protohistoric site near Ujjain. His explorations during this period added the new painting areas of Shivapuri, Gwalior, Narsinghgarh, Katni and Rajasthan in the central part of India.

While working at Putalikarar, the author evolved a method of dating Indian rock art in 1966. So as to verify the results, we conducted primary excavations in the rockshelters of Putalikarar, Raisen, Kharwai, Shymla Hills, Shahad-Karar, Bhim-Betaka and Baniya-Beri in Madhya Pradesh. A large-scale excavation was undertaken by us at Bhim-Betaka in 1970. Here a continuous sequence from Palaeolithic to historical times was recovered. This also provided some evidence for dating of Indian rock art.

After gaining his Ph.D. in 1973, Wakankar concentrated on his explorations and excavations. To collect first-hand information and to document the rockshelters he visited a large number of painting areas, with minimal facilities at his disposal. He also excavated Mandsour, Dongwada, Azad Nagar, and Runija, which proved to be the landmark in the protohistory of the Malwa region.

Gradually he gained international recognition and was called upon to deliver lectures in India and abroad, on Indian culture and rock art. He also organised certain exhibitions on rock art in foreign countries.

Dr Wakankar remained busy with research work till his last days. At the age of 67 he led an expedition for the search of the Saraswati River in the desert of Rajasthan. He collected a large quantity of archaeological and cultural material from the banks of this river. His effort received wide public acclaim due to the sentiments attached to this holy river. He was publicly honoured by several organisations in the country.

He founded the Research Institute on Rock Art and donated all his property to it. Though his health was fast deteriorating, his enthusiasm kept him busy with the task. While he was in Singapore in connection with a conference of the Vishwa Hindu Parishad, he suffered a heart attack and expired on 3 April 1988. He was to have led the Indian delegation to the First AURA Congress, his travel arrangements for this purpose had been completed. His final contribution to international rock art studies was read in Darwin *in absentia*.

Dr Wakankar's passing away at this juncture has been a great loss to Indian archaeology, and particularly to rock art research. He was an institution in himself and it will be very difficult to replace him. He will be remembered forever, particularly for his contribution to the promotion of the studies in the field of Indian rock art.

Dr S. K. Pandey
Chairman, Indian Rock Art Research Association (IRA)

KEYWORDS: Rock pictures - Cultural conventions - North Queensland

ROCK PICTURES OF THE CHILLAGOE-MUNGANA LIMESTONE BELT, NORTH QUEENSLAND

BRUNO DAVID and MAREE DAVID

Abstract. The Chillagoe-Mungana limestone zone of northern Queensland has recently been extensively surveyed for rock art sites. 41 sites have been systematically recorded, and their art differs remarkably from that of the Laura and Koolburra Plateau sites, to the immediate north. On the other hand, the rock pictures of the semi-arid and arid zones to the west and south-west appear to be more akin to those of Chillagoe. The authors believe this reflects past social relations and that statistical work on the basis of motif frequencies could facilitate the reconstruction of past cultural units and boundaries.

About 120 km west of Cairns (north Queensland), along the more gentle slopes west of the Great Dividing Range, lies a narrow belt of limestone karst formations. This Upper Silurian/Lower Devonian sedimentary facies formed under relatively shallow off-shore reef conditions about 430 million years ago. The section relevant to this study, known as the Chillagoe Formation, contains numerous bluffs (or 'towers') of massive fossiliferous limestone, some of which exceed 500 m in length and width, and 100 m in height (Plate 1).

Much of the limestone of the Chillagoe Formation is buried beneath the landscape. The exposures (bluffs) extend in a SE-NW direction for about 65 km, beginning near Almaden (30 km south-east of Chillagoe) and terminating just south of the Walsh River. The maximum width of the exposed Chillagoe Formation is approximately 11 km, about 5 km south of Chillagoe.

The limestone bluffs around Chillagoe are riddled with hundreds of deep caves and rockshelters, of varying sizes. Whilst no rock pictures[1] have been located in the dark areas of any of the caves, many of the rockshelters and cave entrances contain occupational deposits, rock pictures, or both. During an extensive study from May to August 1987, 41 sites containing rock pictures were located and recorded. Of these, 36 were located in the limestone formation, and five in the adjacent granite country.

This paper reports the results of our work in the region. The spatial distribution of sites in the landscape will be reported elsewhere (David and David in prep.). In this report we investigate (1) motif types; (2) techniques employed; and (3) relative chronology, through superimposition.

MS received 25 November 1987

Survey Methods and Results

To locate sites, a large proportion (25.2%) of the limestone outcrops from the Chillagoe-Mungana-Rookwood region was systematically surveyed for picture sites, sampling bluffs from all the major outcrops of the Chillagoe Formation. In addition, all sites known by local residents were also recorded. Consequently, 41 picture sites were recorded. At all times there were at least two people involved in site surveying. Survey was undertaken with the help of 1:45 000 aerial photographs and 1:50 000 topographic maps.

All sites were recorded on standard Archaeology Branch (D.C.S., Brisbane) recording sheets, and each rock picture was sketched onto a sheet of graph paper at an approximate scale of 1:10. Most pictures were photographed on both colour slide and monochrome film, and a proportion was traced on acetate film. The colour of each motif was noted, along with observations on deterioration, damage, superimposition, proportion and area of rock wall decorated, and type of occupational evidence. The location of pictures within the site, site aspect, and location of site on the tower were also noted (cf. David and David in prep.).

Following David (1987), the research area was divided into four land units (plain, hill, riverine and karst units), and it was shown that the density of open sites was particularly high in the riverine and plain units (16.0 and 5.7 sites/km² respectively). This research shows that there is a much lower density of shelter sites than of open sites, with a mean of 1.6 rock picture sites per km² of limestone outcrop. Indeed the sparse distribution of picture

The plates mentioned in this article are on page 138.

sites in the area was fully realised in the field, given that several days of survey would often yield no shelter sites whatsoever. This, of course, may purely reflect the continued reuse of a small number of shelter sites by the region's prehistoric occupants.

The number of rock pictures per site in the region falls within the range for other regions in northern Queensland. There is a mean of 22.9 pictures per site, compared with a mean of 27.6 pictures for the Mitchell-Palmer sites (David in prep.), 18.9 pictures for the Koolburra Plateau sites (calculated from Flood 1987), and 75 paintings and stencils for the Laura sites (Flood 1987). The Chillagoe picture sites hence stand in marked contrast to the Laura sites, but show a density of pictures per site similar to that of the Koolburra Plateau and Mitchell-Palmer regions.

Techniques and Motifs

Three techniques of illustration were employed by the prehistoric rock artists of the Chillagoe region. They are:

(1) Painting, at times with a brush-like implement, as 'brush' marks can be seen on numerous paintings. Paintings account for 87.8% of Chillagoe's rock pictures.

(2) Stencilling, of both hands and items of material culture (5.1% of Chillagoe's rock pictures).

(3) Engraving. All engravings in this analysis are peckings. A large number of thin, V-grooved, vertical incisions were recorded, but have not been included in this analysis as it is not known to the authors whether they formed a part of the picturing behaviour which we are attempting to analyse, or whether they are the by-products of the sharpening of stone tools. Ground points have been seen from the region by one of us (B.D.), and it is possible that these incisions were no more than the results of tool-making/maintainance (see Plate 2). Peckings contribute 7.1% of Chillagoe's rock pictures.

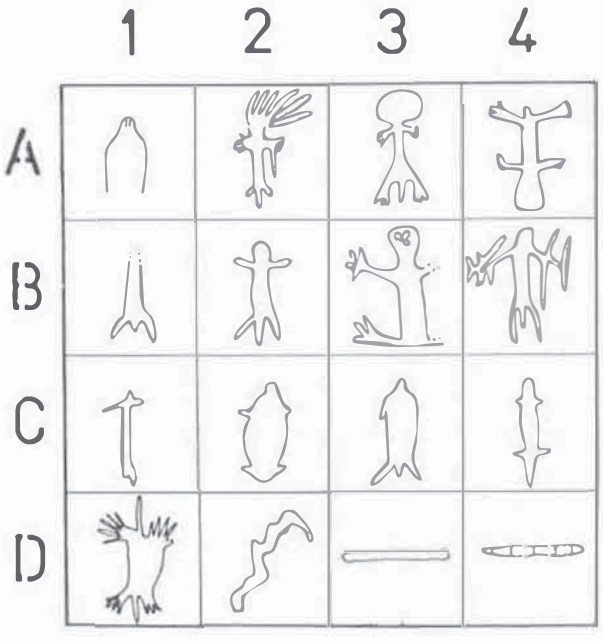


Figure 1. Motif Type A, different picture forms.

Motif Distribution

In the process of field recording, every identifiable picture was sketched onto graph paper. The sheets of graph paper were subsequently photocopied, each sketched picture cut out, and from these our categories were identified. In a previous, preliminary analysis undertaken after only 24 sites had been recorded, categories were identified independently by each author (David and David 1987), with a very high degree of correlation between both systems. Upon completing the full recording of the 41 sites, the categories were entirely redone by one of us (B.D.), with a resultant minor modification of the original categories (the main difference being that the original Motif Type E was split into Types E1 and E2). Although we find the categories satisfactory, we will mention the points of overlap in the system which, like in most taxonomic systems, we feel are needed to qualify the classifications employed.

(1) Tridents. We have included all tridents in one category (Motif Type G), principally for consistency's sake. Rosenfeld (1981) has previously argued that bird tracks can be differentiated from other types of tridents by the nature of the meeting point of the three prongs. We have found, however, that intermediary forms exist among the Chillagoe tridents, and therefore have chosen to include all within the one category. As will be seen, however, whether tridents are included in the track category (Motif Type G) or the 'simple' geometric motif category (Motif Type F) makes no significant difference to the results.

(2) Figures L6 and L7 from Motif Type E2 (see Figure 8) can, arguably, be included in Motif Type A (figurative anthropomorphous and zoomorphous designs). We have not included them here, however, because these two motifs differ markedly from the depictions of anthropomorphs and zoomorphs recorded from the Chillagoe region, and seemed more in line with the extended linear designs of Type E motifs than those of Type A motifs. In any case, forms L6 and L7 are represented by only one picture each and, therefore, do not affect the results of our statistical analysis in any significant manner.

(3) Figure G6 of Motif Type E2 (see Figure 8) differs from Figure K9 of Motif Type F (see Figure 10) in size only. Whereas the former are large (often exceeding 1 m in length), the latter are always very small, usually less than 10 cm in length.

These, we believe, are the major qualifications applicable to our taxonomic system. Like in other such systems, they are problems of overlap. Given

	1	2	3	4
A	1	1	1	1
B	1	1	1	1
C	1	1	1	4
D	1	4	7	1

Table 1. *Distribution of paintings of Motif Type A, Chillagoe, north Queensland.*

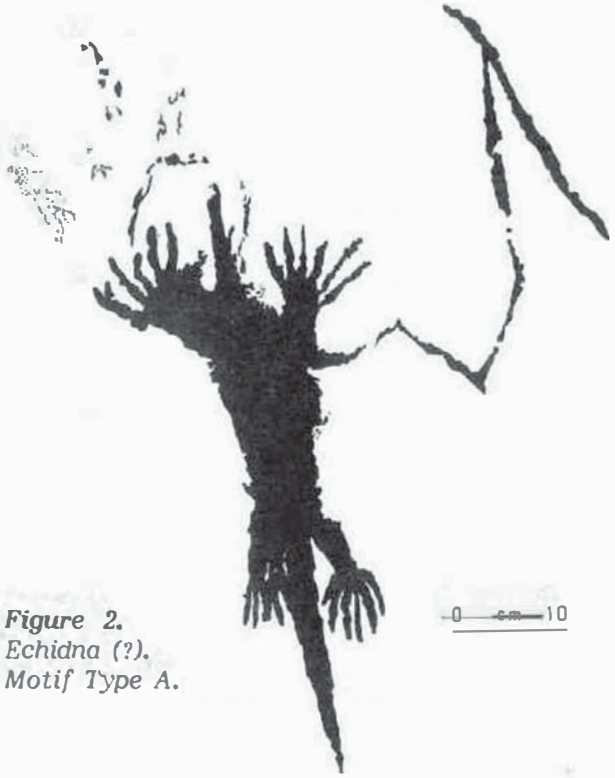


Figure 2.
Echidna (?).
Motif Type A.

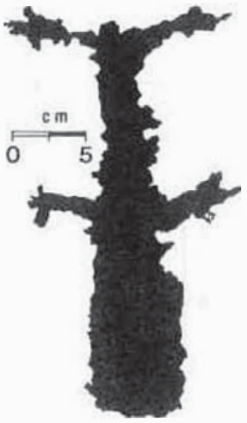


Figure 3.
Anthropomorph.
Motif Type A.

that the problem areas are represented, in one case, by only two individual pictures, and in the other would not alter in any way our general conclusions (because the cases in question involve our three categories which, together, characterise Chillagoe's rock pictures; see below), we are satisfied with the classification's ability to represent the pictures observed from the region, as well as to represent the analysis' reproducibility by independent observers. There is a general high degree of internal integrity within each category and, notwithstanding points 1 to 3 above, a high degree of discreteness between categories. A description of the motif types identified from the region follows.

Paintings and Stencils

Motif Type A: here are included all the figurative pictures of anthropomorphs and zoomorphs (Figures 1-3). They include representations of echidnas (?) (Figure 2), lizards, turtles and snakes (items C2-D4, Figure 1), as well as anthropomorphous designs (Figure 3, and Figure 1, items A1-C1). These are usually in red or mauve monochrome, with only 28 individual pictures recorded. 19 of these are representations of animals, including 12 variations of the snake motif. As will be seen later, this paucity of 'figurative' representations is a striking characteristic of the region's rock pictures and will be important in the comparative analysis of various north Queensland picture assemblages. All Motif Type A pictures are painted (i.e. none are engraved). The distribution of motifs of this type is tabulated in Table 1.

Motif Type B: all but one of the pictures of this motif type are painted, the other is stencilled (Figure 4). These are figurative (naturalistic) depictions of items of material culture, and all identifiable recorded designs are of boomerangs. Only three individual pictures were identified. These are all in red or mauve, whilst the stencilled boomerang was stencilled in red and subsequently infilled with white pigment. The distribution of pictures of Motif Type B is shown in Table 2 and Figure 4. They are rare amongst the pictures of the Chillagoe region.

Motif Type C: motifs are complex, nonfigurative designs, and are usually (but not always) bichrome. These differ from Motif Type D to G pictures in that Type C pictures are complex infilled-area paintings, whereas the latter are line paintings and engravings. Figure 5 and Table 3 show the vari-

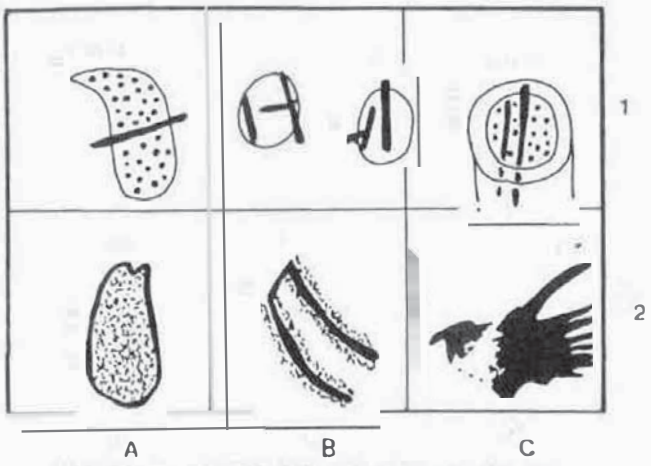


Figure 5. Motif Type C, picture forms.

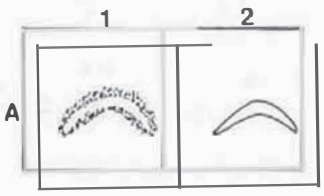


Figure 4.
Motif Type B, picture forms.

	1	2
A	1	2

Table 2.
Distribution of paintings and stencils of Motif Type B.

1	1	1	1
1	1	1	2
A	B	C	

Table 3.
Distribution of paintings of Motif Type C, Chillagoe, north Queensland.

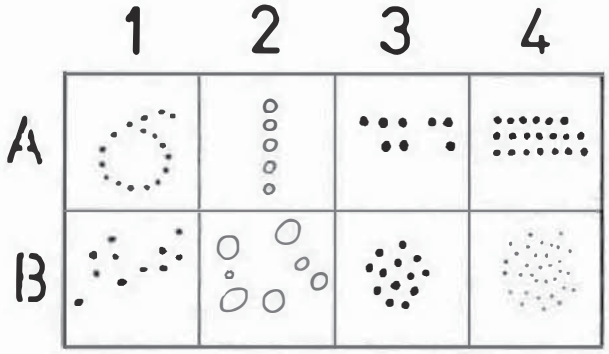


Figure 6. Motif Type D, picture forms.

ation and distribution of this motif type. A total of seven individual pictures are of this type, and all of them are painted. Their occurrence, therefore, is rare.

Motif Type D: pictures are engraved or painted, and consist of series of circles or dots. Of the 13 recorded painted pictures of this type, eight are either of the B2 or B4 variation (see Figure 6). This type of motif is also rare, and such depictions are always associated with other motif types. Table 4 tabulates the distribution of pictures of this type.

Motif Type E: pictures of this motif type are divided into two forms. Both forms consist of extended linear drawings or paintings. Type E1 is differentiated from Type E2 on the grounds that the former consists of variations of the vertical and/or horizontal line picture, often enclosed within a boundary line (Figure 7, Plate 3). A total of 146 Motif Type E1 pictures have been recorded, and their distribution is shown in Table 5. By far the most common variations of this theme are forms A4 and C3 (Figure 7), with 46 and 36 pictures respectively.

Motif Type E2 pictures are more diverse in their range of variation. 181 pictures represent 125 variations of the extended linear motif, with only form G6, with 21 pictures, being of numerical importance (Figure 8, Table 6. See also Plate 4 and Figure 9). The variation observable in this motif

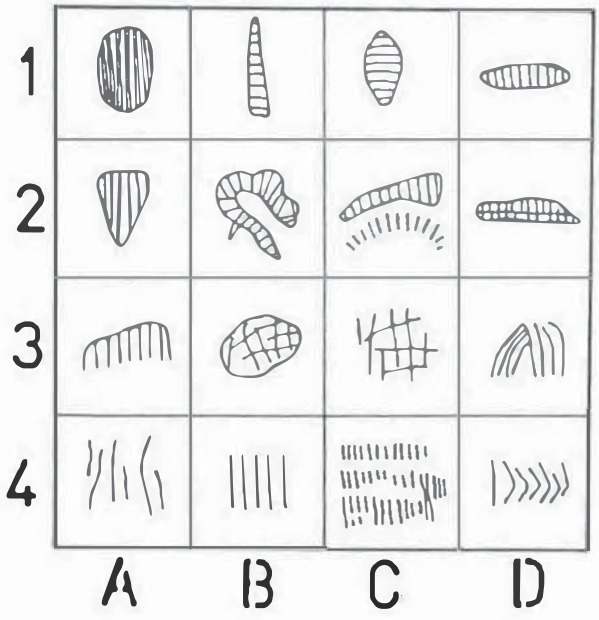


Figure 7. Motif Type E1, picture forms.

	1	2	3	4
A	1	1	-	1
B	2	5	-	3

Table 4. *Distribution of paintings of Motif Type D.*

type is extreme and, along with Motif Types F and G, Motif Type E is an important element of Chillagoe's rock pictures. Once again, this is an important point to remember in the comparative discussions below.

Motif Type F: pictures of this motif type are small geometric or near-geometric designs, most commonly variations of circles or star shapes (Figure 10). They are very common, with a total of 344 Type F paintings, including 31 of the A2, 67 of the K6, and 38 of the K9 forms. Together, these three forms are by far the most commonly represented of the Motif Type F designs. One circular object was stencilled next to a series of hand stencils. As its identity is unknown, it has been included in this motif type. Table 7 tabulates the distribution of Type F motifs.

Motif Type G: these paintings are believed to represent animal tracks. By far the most common are those resembling bird tracks (tridents), with 80 such designs out of a total of 107 Type G paintings (Table 8). Paintings of this motif type are numerically important in the Chillagoe region, unlike 'macropod tracks' of which only four have been recorded among the paintings. Tracings of paintings of this type are shown in Figures 12-14.

Motif Type H: a total of 48 hand stencils have been recorded. Only three examples of variant hand motifs have been noted, these being:
(1) a stencil of a fist;
(2) a stencil of a hand with thumb, first finger and little finger, ring and middle fingers being bent at knuckles into the palm of the hand; and
(3) a stencil of a hand where the little finger is bent down at first joint.

It is possible that these variant motifs are related in some way to hand signals (cf. Wright 1985). The meanings of hand signals have never been recorded for the Chillagoe region, but Wright (1985: 7-9) notes that the above variant motifs 1 and 2 have the following meanings in other parts of Queensland:
(1) snake, or black snake;
(2) evil spirit.

This is particularly interesting because variant stencil (2) is located at the entrance of a deep cave, in site CM26. Aborigines of the Chillagoe region never entered these caves because they were

A	B	C	D	
3	5	1	9	1
3	1	1	10	2
1	2	36	3	3
46	8	6	1	4

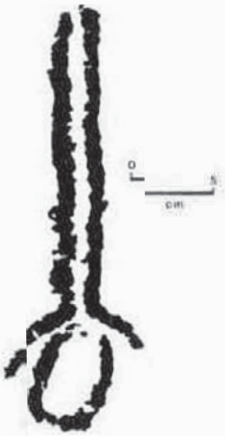
Table 5. *Distribution of paintings of Motif Type E1.*

Figure 8. Motif Type E2, picture forms.

	1	2	3	4	5	6	7	8	9	10
A	1	1	1	1	1	1	1	1	1	1
B	1	1	1	2	1	1	1	1	1	1
C	1	1	1	1	1	–	–	1	1	1
D	1	1	1	1	1	1	1	1	1	1
E	1	1	1	1	1	1	1	1	2	1
F	1	1	1	1	1	1	1	1	1	1
G	1	1	1	1	1	21	1	1	1	3
H	1	1	1	1	1	3	1	4	1	1
I	1	1	1	1	1	1	1	2	1	1
J	1	1	1	1	3	1	1	1	1	1
K	1	1	1	1	1	1	1	3	1	–
L	1	1	1	1	1	1	1	1	1	1
M	1	1	1	1	1	Miscellaneous = 26				

Table 6. *Distribution of paintings of Motif Type E2, Chillagoe.*

Figure 9. *(right) Painting, Motif Type E2.*



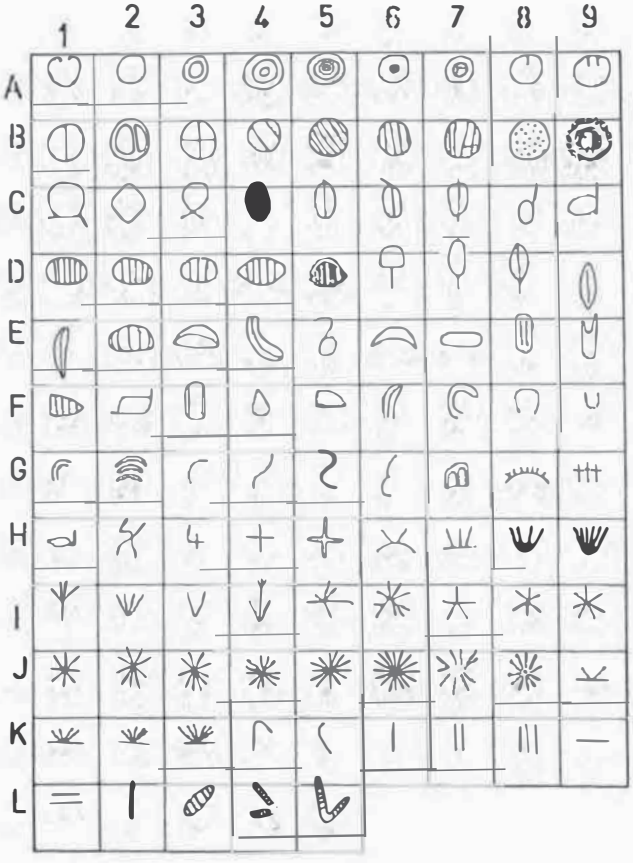


Figure 10. Motif Type F, picture forms.

	1	2	3	4	5	6	7	8	9
A	–	31	2	1	1	1	1	1	1
B	4	1	–	1	1	1	1	–	1
C	–	1	–	13	1	1	1	1	1
D	1	1	3	1	1	1	1	1	2
E	2	1	–	–	–	5	6	1	1
F	1	1	1	1	1	1	1	1	2
G	1	1	3	2	1	–	6	1	2
H	1	2	2	8	1	1	1	2	1
I	3	3	3	–	2	2	7	9	8
J	7	1	2	4	2	2	1	–	1
K	4	4	1	1	1	67	4	11	38
L	1	1	3	1	2	Miscellaneous = 6			

Table 7. *Distribution of paintings of Motif Type F (including one stencil), Chillagoe.*

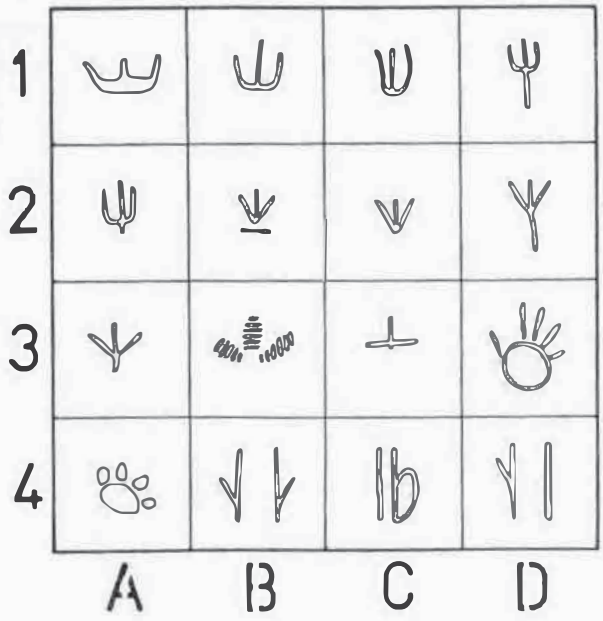


Figure 11. Motif Type G, picture forms.

	A	B	C	D
1	2	1	4	-
2	3	1	22	15
3	29	1	2	1
4	22	2	1	1

Table 8. *Distribution of paintings of Motif Type G, Chillagoe.*

(and indeed still are) believed to house evil spirits. It is tempting to relate variant motif (2) to this belief, and see it as reflecting the artist's cosmology. Unfortunately, and partly due to the limited number of variant motifs from the region, little more can be said of these stencils.

Elsewhere (David and David in prep.) we will show that hand stencil sites are related to mortuary practices. After death, the body is placed into natural cavities in limestone bluffs, and the traditional owner stencils his or her hand near the entrance of the cavity which contains the body. After a few days, the body is taken out and finally placed in a tree. Whilst the body is in the bluff it dreams, and in the process its mind is revealed to its closest friends. This process is tangibly shown to the people by a falling star, which is a manifestation of the mind's travels after death (Bobby Sellars pers. comm. 1987) (David and David in prep.).

Archaeologically, we were able to isolate hand stencil sites from non-hand stencil sites. The former are usually located high up in limestone towers, usually have rocky floors with an absence of occupational deposits, with stencils usually occurring at the entrance or near the end of natural rock cavities. This is in marked contrast to other picture sites, which are generally located at the base of towers (bluffs), are usually associated with occupational deposits, and whose pictures are usually painted or engraved away from the site's entrance, towards the centre of the wall space (David and David in prep.).

Figure 12. Painted trident (Motif Type G).

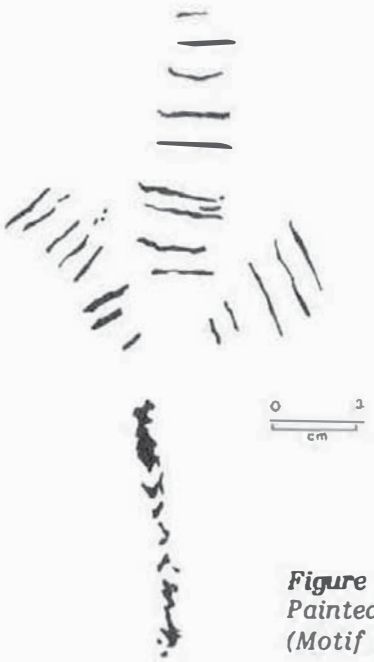


Figure 13. Painted trident (Motif Type G).

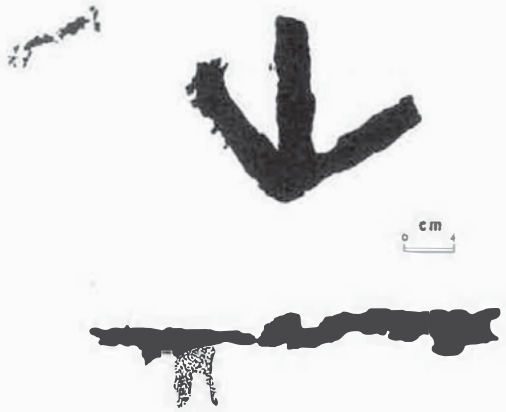


Figure 14. Painted trident (Motif Type G).

Investigations of hand stencil sites at Chillagoe have also shown that very large hands (adult males?) and very small hands (children?) are usually found at hand stencil sites with an absence of occupational deposits. All but one of the middle-sized hands, however, are found near occupational deposits. These associations are interesting and need to be further investigated.

The hand stencils are much less common in Chillagoe than in the Koolburra Plateau, Mitchell-Palmer and Laura regions to the north, with 48 hand stencils recorded from 41 sites at Chillagoe, 1829 stencils from 163 sites from the Koolburra Plateau, and 25 stencils from 10 sites in the Mitchell-Palmer region (see Figure 15 for locations of these regions). There is a slight predominance of left hands stencilled at Chillagoe, with 57.1% left hand stencils and 42.9% right hands. This distribution is similar to that from the Koolburra Plateau, which has 58% left hands, 38% right hands, and 3% both hands represented (see Flood 1987 for detailed discussion).

Discussion of Paintings

The Chillagoe paintings and stencils are usually monochrome (794 pictures, or 94.4% of paintings and stencils whose colours could be distinguished), executed predominantly in reddish-mauve (30.6%), white (42.6%) or black (10.6%). Other colours are yellow (4.3%), brown (0.1%) and orange (0.5%).

The bichrome paintings are rare (5.6%) and when present are usually red and white (3.9% of total paintings and stencils), with a red outline and white infill. No paintings have three or more colours. Natural features of the cave wall are often incorporated in the pictures (for an example see Plate 5). In addition, six paintings have short incisions incorporated in the painting. Five of these are of Type F motifs. In all cases the incisions are located either immediately above or below the ochre, and are clearly part of the picture. Item L4 on Figure 10 is an example of this (note that these incisions usually have U-shaped cross-sections, and are distinct from the V-shaped incisions believed to be the results of stone tool processing).

The size of the Chillagoe paintings varies dramatically from long, elongated linear designs (Motif Type E) of 4 m and more in length, to small Motif Type F and G paintings of a few centimetres

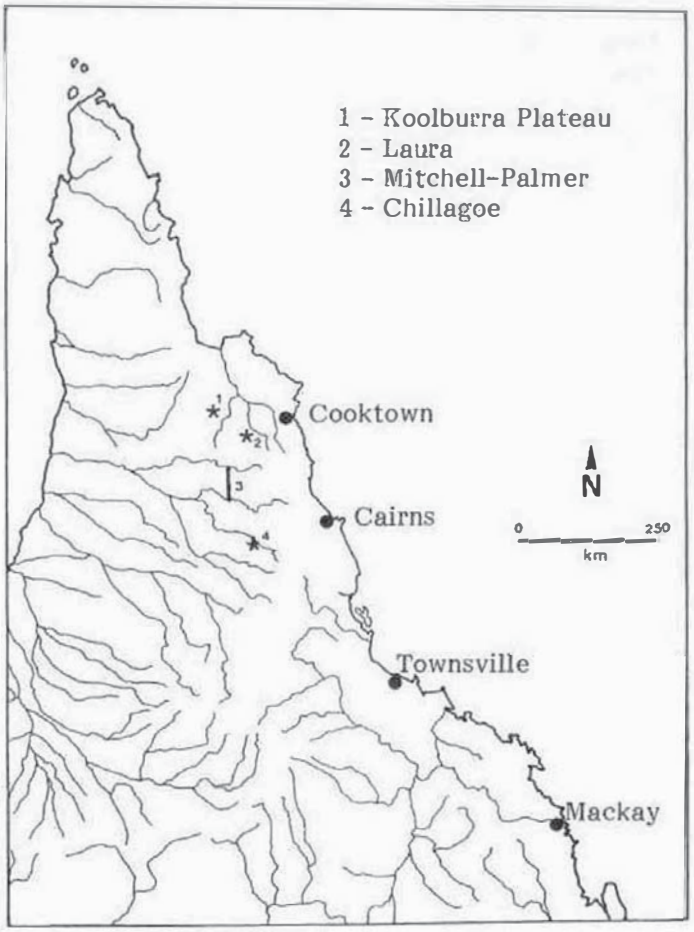


Figure 15.
Map of north Queensland showing the locations of the four rock art regions mentioned in the text.

length. Type A motifs (anthropomorphs, zoomorphs) approximate about 30 cm in length and, like at the Koolburra Plateau, no life-size anthropomorphous paintings have been recorded (cf. Flood 1987).

No x-ray style paintings have been found anywhere in the Chillagoe area. Naturalistic paintings are rare, and are usually monochrome infilled paintings. This contrasts dramatically with Flood's findings that 'the most numerous painted motifs [in the Koolburra Plateau region] appear to be therianthropes' (Flood 1987: 110), and differs from the predominance of anthropomorphous and zoomorphous paintings at both the Laura and Mitchell-Palmer regions to the north. What characterises Chillagoe's rock paintings is its suite of Motif Type E, F and G paintings. Together, these contribute 816 of the 910 recorded paintings and stencils from the region (89.7%). Most sites display little or no superimposition (see below), with most or all paintings produced in one colour only (see Table 10). A sequence of animal tracks occurs at two sites, in which case each track is counted as an individual picture. In one such site (CM87), which has a sequence of bird tracks that spans the length of the painted wall, the tracks eventually disappear into natural cavities in the rock wall.

To summarise: a number of features isolate the Chillagoe paintings from the painting traditions of the Mitchell-Palmer, Koolburra and Laura regions to the north. These include:
(1) a paucity of hand stencils at Chillagoe;

Motif Type	Motif Form	Number
D	A3	1
	B1	4
	B3	4
E1	B4	1
E2	C6	1
	C7	1
	G5	1
	G6	1
	I5	1
	K10	1
	L4	1
F	A1	1
	A2	10
	A6	3
	B1	2
	B3	1
	B8	1
	C1	1
	C3	1
	E3	1
	E4	1
F	E5	1
	F8	1
	G1	1
	G3	3
	G6	1
	H4	2
	H7	1
	I4	1
	I7	1
	I8	1
	J8	1
	J9	2
	K2	1
	K6	1
	K7	2
G	B1	1
	C2	1
	B4	2
	D1	1
	D2	2
	D3	1

Table 9.
Distribution of engravings by Motif Type.

Figure 16. *Pecked trident.*

(2) a sparse distribution of sites in the landscape;
(3) a predominantly nonfigurative painting tradition at Chillagoe;
(4) a lack of 'stick figures' in Chillagoe (with the exception of one picture), which are extremely common in both the Mitchell-Palmer and Koolburra regions, but rare at Laura;
(5) a predominance of line, monochrome paintings at Chillagoe, which contrasts with the high percentage of polychrome paintings in both the Koolburra and Laura regions.

Unfortunately, apart from the hand stencils which are known to have been associated with mortuary practices, the Aboriginal people of Chillagoe today are not versed with the meanings of Chillagoe's paintings. We must therefore rely on archaeological methods of investigation, and hence on etic forms of enquiry, if we wish to derive information from the region's paintings and engravings.

Engravings

As already mentioned, the fine vertical incisions commonly found on Chillagoe's rockshelter walls have not been included in this analysis, as they are believed to reflect stone tool manufacturing processes rather than picturing behaviour.

All of Chillagoe's engraved pictures are pecked. Peckings range from deep and heavily patinated pictures to shallow and fresh-looking ones. The deeper ones appear to be older, if degree of patination is any indication.

The range of motifs which are depicted in Chillagoe's peckings falls within the range of Chillagoe paintings. In Table 9 we tabulate the distribution of pecked pictures. They include a total of 67 peckings, all of Motif Types D-G. There are therefore no pecked anthropomorphous or zoomorphous figures. The most common engravings are of Motif Types D and F. Both macropod and bird tracks are present, but rare. Given the low numerical importance of peckings in general, however (7.1% of Chillagoe's rock pictures), any generalisation would be spurious. Figure 16 illustrates one of Chillagoe's peckings.

The sparsity of engravings at Chillagoe is in marked contrast with the great, numerical importance of peckings in both the Koolburra Plateau and Laura regions to the north. In the former, approximately 600 engravings have been recorded from 33 sites, giving a mean of 18 engravings per site (Flood 1987: 97). Similarly, the Laura sites recorded by Rosenfeld (1981) have large numbers of engravings. In all cases, circles and sets of dots are common, they are also present at Chillagoe. Mazes, which would fall within our Type E1 pictures, are often found amongst the Laura engravings (Rosenfeld 1981), but are absent amongst the Chillagoe engravings. Pecked star shapes are common in both the Laura and Koolburra Plateau regions, but are rare at Chillagoe. Tracks and tridents are common amongst peckings in all of the three regions, although macropod tracks are rare in Chillagoe. When they do occur in Chillagoe they are invariably heavily patinated.

In general, the engravings of the Chillagoe, Laura and Koolburra Plateau regions appear to be related more closely than these regions' paintings. This may reflect a greater antiquity of engravings than paintings, and closer cultural affinities in the more distant past.

Chronology

In this section we investigate the chronology of Chillagoe's picturing conventions through an analy-

White	over	black	6
Black	"	white	9
Red	"	white	29
White	"	red	47
Black	"	red	8
Red	"	black	4
Red	"	yellow	6
Yellow	"	red	2
Yellow	"	white	3
White	"	yellow	15
Black	"	yellow	4
Yellow	"	black	1
White	"	brown	1
Brown	"	white	1
Brown	"	black	1

Table 10.
Number of pigment colour superimpositions, Chillagoe paintings.

C	over	E1	1	E1	over	C	1
C	"	E2	2	E2	"	C	-
C	"	F	1	F	"	C	-
D	"	E1	2	E1	"	D	3
D	"	F	3	F	"	D	1
E1	"	E2	7	E2	"	E1	18
E2	"	D	2	D	"	E2	-
E1	"	F	3	F	"	E1	8
E1	"	G	3	G	"	E1	2
E2	"	F	12	F	"	E2	10
G	"	E2	1	E2	"	G	-
G	"	F	1	F	"	G	1
G	"	D	1	D	"	G	-
G	"	A	1	A	"	G	-

Table 11.
Number of superimpositions, by motif types.

sis of:
(1) the superimposition of pigment colours; and
(2) the superimposition of motif types.
We also note the degree of patination amongst the engravings.

Colour. We have recorded 138 cases of superimposition which involve paintings of different colours, or paintings and engravings. Their distribution is shown in Table 10.

There does not appear to be any significant trend in the pattern of superimpositions. Only one engraving is involved in superimpositions, and this is a Motif Type D pecking of shallow depth and fresh appearance. Unfortunately no cases of superimposition involving the heavily patinated Motif Type F engravings have been recorded. If patination is any indication, these appear to be of considerable antiquity and may well predate the surviving paintings. Further investigation is needed here, but this would necessitate the recording of a significant proportion of the remainder of Chillagoe's rock picture sites. The sample so far contains too few superimpositions to make any definite statement on chronological patterns, although this sample suggests that there were no significant changes in colour through time.

Motif Types. There are, in total, only 84 cases where different motif types are superimposed. Many of these (28) involve Motif Types E1 and E2, and this is mainly due to the fact that pictures of these types are large, and therefore more likely to be involved in superimpositions.

Once again, the number of superimpositions is too small to make any definitive statement on relative chronology. The pattern of superimposition (Table 11) does not, however, appear to reflect any significant chronological changes in motif type.

Conclusions

The rock pictures of the Chillagoe limestone belt are dominated by paintings of Motif Types E, F and G; by far the predominant motifs are geometric or extended linear designs. Stylistically, rock picturing in the region appears to be highly conventionalised, although there cannot be said to be any formalisation of the predominant design types. There is an enormous variation in the individual motifs represented, and these can be categorised into eight major motif types (A-H).

In comparing the rock pictures of the Chillagoe region with those of the Koolburra Plateau, Laura and the Mitchell-Palmer region to the north of Chillagoe, it has been noted that a number of important differences occur. Unlike Chillagoe, these regions contain a significant proportion of naturalistic and highly conventionalised anthropomorphous and zoomorphous designs. This has also been noted by Brayshaw (1977) and Clegg (1979) for the Townsville and Bare Hill regions immediately to the south-east of Chillagoe.

On the other hand, the rock pictures of the Chillagoe region show many similarities with the rock pictures of many regions to the south-west, including those of the Mt Isa area, about 750 km to the south-west (Morwood 1985), and of the Olary Province, 1700 km to the south-west (Nobbs 1984). Some of these similarities are particularly striking, and this is especially so of the extended linear designs from the Olary Province and those of Chillagoe.

The question naturally arises as to why the rock pictures of Chillagoe are so different from those of regions immediately to the north and to the east, whilst they show greater similarities to the rock pictures of regions sometimes thousands of kilometres to the south-west. We suspect the answer has something to do with the nature of Aboriginal relations, whereby the rainforest zone immediately east of Chillagoe may have acted as a cultural boundary, as possibly did also the major rivers to the north, such as the Walsh, Mitchell and Palmer Rivers. Limitations on social interactions between these regions may have resulted in the development of distinct cultural traditions, including the development of separate artistic conventions.

On the other hand, more freedom in the dissemination of ideas, through more extended social interaction with regions to the west, may have more standardised the stylistic conventions employed by the picture makers of Chillagoe and those to the south-west. This is not only reflected in the large sizes of the traditional territories reported in Tindale (1974) for the peoples west of the

rainforest, but also in the frequent mentions, in early journals, of less restricted social interactions amongst the peoples of the west.

It is beyond the purpose of this paper to investigate these issues in detail, but they are currently being investigated by one of us (B.D.). Included in these investigations is the undertaking of a series of statistical tests (including Cluster Analysis), aimed at comparing the motif distribution of a number of regions in north Queensland. The results of this work will form the subject of a separate publication. In this paper we have merely wished to report on the nature of rock pictures in the Chillagoe region. In doing so, we have but introduced some of the more interesting implications of our findings.

1) We have adopted the use of the word rock *pictures* rather than rock *art* in this paper, following Clegg, as we do not wish to imply a picturing behaviour of the type(s) normally associated with Western *art* analysis.

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Résumé. Dans la zone du calcaire Chillagoe-Mungana dans le Queensland septentrional a récemment eu lieu une prospection extensive; 41 sites d'art rupestre ont été systématiquement recensés. Leur art est tout à fait différent de celui des sites de la région de Laura et du Plateau de Koolburra, immédiatement au nord. Par contre, les figures rupestres des zones arides et semi-arides à l'ouest et au sud-ouest paraissent ressembler beaucoup plus à celles de Chillagoe. Les auteurs croient que ce fait est lié aux relations sociales dans le passé, et qu'un travail statistique sur les fréquences de motifs pourrait faciliter la reconstruction des anciennes unités et frontières culturelles.

Zusammenfassung. Die Chillagoe-Mungana Kalkstein Zone im nördlichen Queensland wurde kürzlich gründlich für Felskunst untersucht. 41 Fundorte wurden systematisch registriert und diese Kunst unterscheidet sich merklich von der in Laura und Koolburra Fundorten, die nahe nördlich davon liegen. Die Felsbilder der semiariden und ariden Zonen gegen Westen und Südwesten scheinen hingegen denen von Chillagoe ähnlicher. Die Autoren sehen darin frühere soziale Beziehungen wiedergespiegelt und glauben, dasz statistische Arbeit, gestützt auf Motiv-Verteilungen, die Rekonstruktion von früheren kulturellen Einheiten und Grenzen ermöglichen könnte.

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RAR DEBATES

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ORGANISATION, DISTRIBUTION AND TYPOLOGY OF THE CAVE ART OF MONTE DEL CASTILLO, SPAIN

Reynaldo González Garcia

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FURTHER COMMENTS

By JOHN HALVERSON

González Garcia has found an interesting statistical pattern among the depictions in the caves of Monte del Castillo, a pattern that clearly associates horses, cervids, ibex and handprints with concave surfaces, and bison and other bovids with convex ones. Co-ordinate patterns of colour and figure completion support the implication of deliberate selectivity. González himself draws no far-reaching conclusions from this discovery, but Bahn (1987a), in his Comment on this article, seems optimistic that we may have the 'makings of a breakthrough' if the Monte del Castillo pattern should turn out to be generally discernible in cave art. Just what significance it might have he does not say, but any such consistent schemas would certainly be provocative and would have to be taken into account henceforth. I would like to express my doubts that any breakthrough in this area is imminent; my doubts are based in part on a recent tour of some of the best-known sites, during which I tried to pay particular attention to painting and engraving surfaces, and in part on conceptual and theoretical problems.

Considering first whether the pattern extends beyond Monte del Castillo, I cite a paragraph by Bahn, apparently an approving paraphrase of González's thesis (1985):

> at Altamira, the bison on the ceiling are all 'convex' whereas the deer and horse are concave; at Font de Gaume, the bison and reindeer seem to be on convex surfaces; the Tuc d'Audoubert clay bison are on convex, the Cap Blanc sculptured horses and the Tito Bustillo painted horses on concave surfaces; while almost all the hand stencils at Gargas, like those of Castillo, are on concave surfaces. (Bahn 1987a: 131)

But little of this will hold up. I will take mainly bison representations as the test case. At Altamira only three of the ceiling bison are clearly on convex bosses (the 'wallowing' or 'leaping' bison, curled to fit the rock supports), and one other standing bison is on a slight convexity. Two are on distinctly concave surfaces, the rest on more-or-less flat rock. I have to say 'more-or-less', since there are no truly flat surfaces, but the remaining seventeen-or-so bison depictions are certainly not convex. And the great hind and the horse are on flat, not concave surfaces. At Altamira, then, it seems quite evident that there is no correlation of surface with animal. The same holds true of Font de Gaume, where the great majority of nearly forty bison are actually on concave surfaces, some on undulating surfaces, but none, I think, on convex ones. The Tuc d'Audoubert clay sculptures are indeed propped against a convex formation, but they could hardly be propped on a concavity. Whether the Cap Blanc horses were carved from a concavity or merely a slope is not clear (at least not to me). I take it as obvious that inwardly sloping surfaces do not count as concavities since this is the characteristic parietal surface of nearly all caves, containing within it the bulges and depressions that are at issue.

The surfaces of the Salon Noir of Niaux are entirely flat or concave, yet the greatest number of depictions are of bison, with a few horse and ibex intermingled. The Great Ceiling of Rouffignac, as flat as any natural surface is likely to be, contains, among other animals, both bison and horse, the bison outnumbering the horses. At Pech Merle the first 'chapel' on the left is a concave recess containing eight bovids (four bison) and three horses. At Lascaux the well-known 'crossed' pair of bison are sited on a very distinctively concave surface. Even at El Castillo one notable composition pairs two bison on adjacent convex and concave surfaces.

Thus at several sites there is a 100% location of bison on concave or flat surfaces, 0% on convexities. I realise that such an informal survey of selected sites (not entire caves) does not prove or disprove anything, but it does not bode well for more complete inventories. Even if the statistics for the Monte del Castillo complex should be paralleled elsewhere, or even universally (an unlikely prospect), what significance would they have? What could we make of such data? González Garcia presupposes, without argument, that a painted cave is 'a sacred collective place' and that its art is 'organised and developed in a particular and characteristic way'. The principle of organisation that he proposes is that certain surfaces, sites or 'panels' call for certain figures executed in a certain way most of the time (there will always be exceptions, as Bahn notes). Some sort of sacral ordering is implied. The idea of sacred places within caves has been around a long time, but González gives it a new twist: it is not particular topographical sites that have this quality but particular structures. It seems to me, however, that *no* assumption of sacredness, old or new, is warranted. González makes no attempt to justify his assumption of 'sacred collective space', but surely it can not merely be taken for granted, for it is by no means a universally acknowledged truth. If sacredness is to be taken seriously, exceptions become more seri-

ous, for they are then not merely expectable statistical variations, but violations of sacred rules. The Altamira ceiling would have to be considered a monument of heresy for deliberately juxtaposing right and wrong uses of the surface. In El Castillo itself, the paired convex and concave bison would appear to be a wilful flouting of sacred rule. Even from so incomplete an inventory as given above, it would appear that there are far too many exceptions to postulate such a rule.

One might ask, 'Well, how else can such impressive statistics be explained?' Of course no-one should ask such a question, and no-one has. I fabricate it only to point out that there are certainly alternatives. But let me begin with some observations on tallying methods. According to González's figures, a full 75% of the painted surfaces examined are concave and 22% convex, leaving only 3% presumably flat surfaces. Is the last figure credible? It suggests to me possibly over-refined or tendentious characterisations of surface structures. I would have to guess that extremely slight and often partial inward curvatures are assigned to the category of 'concave'. (Incidentally, I have no idea what it can mean to say that a concave support 'is a panel that has no impact with its forms or shape on the surrounding space' and that a convex support has 'an active impact on the space', so I use the terms in the ordinary sense of 'hollow' and 'bulging', as Bahn does.) A great many animals are depicted on what I would think of as undulating surfaces, but more flat than anything else. And how is a depiction to be categorised, part of which is convex and part concave or flat? For example, at El Castillo there is a bison the legs of which are rock swellings, though all the rest of the figure is flat. At Font de Gaume there is a bison figure using a pronounced ridge for the backbone, but the rest of it is concave. At Altamira one bison is flat with its head only on a boss, another flat with only its legs on a boss. Such examples could be multiplied endlessly. It is not here a question of 'panels' or 'zones' but the underlying support of specific, individual figures. It is hard to see how they could be assigned exclusively to one category or another. In many instances it would appear that the artists were attracted to a particular site by nothing more esoteric than natural features of resemblance or to general suitability for painting.

But, even accepting González's classifications and tallies, if 75% of the surfaces the artists thought suitable for drawing (for whatever reason) and certain animals were favoured for depiction (for whatever reason), most would inevitably be depicted on concave surfaces—a consideration that perhaps lessens the impressiveness of the statistics. Preference for certain animals in individual caves is well attested in a number of instances, for example, the mammoths of Rouffignac (65%), the horses of Lascaux (nearly 60%) and the bison of Altamira. If 'signs' were added to the inventory, I suspect that the proportion of concave surfaces would increase considerably. At Castillo, for example, there is a good-sized concave chamber full of signs and a long line of red dots on a passageway which I would take to be a more-or-less flat surface but which González might count as concave.

The basic question here is one of surface 'suitability'. Why is one location chosen over another for depiction? Is the reason ideological or practical (or perhaps both)? In favour of a practical explanation, it seems to me that the great majority of Palaeolithic cave paintings (and at least a majority of engravings) occur on relatively smooth surfaces and tend to cluster in what Vialou (1987: 37) calls *'cadrages'*—areas that have some sort of natural framing characteristic, such locations becoming even more attractive if they contain natural formations that suggest animal figures. There are a great many 'chambers' or recesses, set off from the surrounding area, that have been artistically exploited. They are natural concavities. But were they chosen because they were concave? It seems more likely that they were selected for *cadrage* and smooth-textured surfaces. Some of the most famous and richest examples of Palaeolithic art are sited in large 'rooms' with the same characteristics on a greater scale—the Lascaux Rotunda, Niaux's Salon Noir, Altamira's Gran Sala, Rouffignac's Grand Plafond—and chosen for the same reasons. Lascaux is no doubt the best example of the ideal site for surface colour, texture (finely stippled), expanse and natural horizontal framing, which may be the principal reason why it was so fully exploited for artistic purposes. It is sometimes asked, however, why some perfectly suitable cave surfaces were *not* painted. As an implied rebuttal, the question strikes me as rather inane. Why should the artists have been expected to utilise every available space? Surely they had their own limits of energy, ambition, time, materials etc. There are many obvious reasons why every suitable space might not be utilised, and no obvious reason why it should be. Of course there are also paintings on less than ideal surfaces. Lascaux again affords a good example in the Axial Passage and Meander, where the wall texture often becomes very knobby and must have presented a real challenge to the painters, though evidently an acceptable challenge. The impetus to fill the space was perhaps provided by the profusion and success of the numerous other figures in adjacent areas.

This last observation brings me to the matter of species statistics. Why are some animals represented in greater numbers than others in particular caves and particular locations within caves? Many possible explanations have been proposed, many quite plausible, ranging from dietary to spiritual. I do not discount cultural and psychological reasons, which must inevitably have played a part in selection, but I would like to point out another factor, which might be called the principle of imitative repetition. This is simply the likelihood that one representation will inspire further representations of the same subject, either by emulation if the first drawing is a good one, or in a competitive or ameliorative spirit if it is not. If there is a significant element of play in artistic activity, as is commonly held, it is well known that imitation and repetition have a major role in play. This principle would require yet further caution in drawing ideological conclusions from statistical data, for chance could well be a greater component in the

data than the percentages themselves suggest, and species preference have less significance than one might otherwise suppose. There is a natural tendency to think that high percentages, 'above chance', must have some significance, even if it be hidden, but at least in the present context this is not necessarily the case.

If nothing else, however, perhaps one might conclude from the Monte del Castillo data that 'Depictions of these species were clearly carefully planned', as Bahn (p. 131) says. But this seems to me a conclusion neither clear nor warranted. Even if there is a high correlation of animal species with concave and convex surfaces (which is not indubitable), it does not follow that any kind of planning was involved. In fact if González's general conjectures about 'sacred space' are right, they would preclude planning; a certain surface simply requires certain species to be represented on it: there would be nothing to 'plan'. If we are to speak of planning, I should suppose that at a minimum we must think of a Magdalenian artist looking at a suitable space and formulating in his mind how to utilise it—projecting a composition, in other words. But the results almost never show any overt sign of planning. On the contrary, when figures are clustered, the variability of size, position, completeness and superposition suggests anything but careful planning. It is difficult to conceive what sort of advance planning might have produced the panels of the Salon Noir, for example. In the third panel there, did the artist say to himself, 'Here is a good place for a two-thirds complete horse, a topless bison, an ibex, horse and bison all the same size, three overlapping bison', etc.? There are, to be sure, many instances where an extremely minimal kind of planning is conceivable, namely in the use or elaboration of natural rock formations, but these do not go beyond single figures, for which 'planning' seems an exaggerated word. If planning is so minimal for single figures, and indiscernible in panels, how likely is it in larger configurations? Freeman (1987) has recently argued that the Altamira ceiling is a planned composition, but this would be a major exception, and in fact imitative repetition could quite as well account for its features. As for entire caves, or cave clusters as at Monte del Castillo, are we to imagine our artists investigating these caves and deciding they are just the place to depict 81 horses, 88.8% of which will be on concave surfaces? Perhaps it is an unfortunate limitation of my own imagination, but I find it impossible to conceive that the very disparate depictions in so vast and complex a cave as El Castillo could ever have been the product of forethought. If there ever was such a plan, it is totally elusive.

Human behaviour, presumably then as now, has always an element of caprice—which is not to say it is unmotivated but that the motives may be impulsive and ephemeral, quickly forgotten and not taken seriously. One of the first images one encounters in the cave of El Castillo is a small, elegantly engraved feline and nearby it a rabbit head, and a few feet from that a deer and a deer head, all on different surfaces but in the same vicinity—an anomalous collocation by any theory of grouping. It is quite as good an assumption as any other that nothing connects them except the whim of the engraver (or engravers), someone who happened to be in the mood to make a few sketches at that place at that moment. The site need have no special significance, nor the particular surfaces; no planning need be supposed; certainly no grand scheme of overall cave design need have been in anyone's mind. What moved the engraver to make just those sketches probably not even he or she could tell us. Perhaps only an intensive psychoanalytic session would reveal some explanatory web of thoughts, feelings and stimuli at the moment, but it would in any case be highly personal and individual, idle to speculate about fourteen millennia or so after the fact, and unlikely to yield the kind of cultural 'meaning' modern researchers so often seek.

Attention to surfaces and sites should be basic in the study of cave art. I have no wish to undermine its importance; in fact it seems to me that such information should regularly be included in all inventorial descriptions as a matter of course. This should help us to understand why the cave artists chose the places they did to paint, engrave and sculpt. It might even someday help uncover ideological motives. And it might not. Thus far it seems to point, for the most part at least, to more immediate practical reasons. There are always exceptions, but sites seem to have been chosen most often for *cadrage*, for the presence of suggestive natural formations, and for surface texture, colour and uniformity. The ideal site would include all of these features, but any site is likely to have at least one. And all suggest the primacy of practical motives in site selection.

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Any methodological or even conceptual contribution to the analysis of Palaeolithic parietal art results from a process of reflection which tries to—or should do so—analyse a phenomenon in its different variants and characteristics. Dr Halverson, in a very laudable way, states several objections to my study of the caves of Monte del Castillo. They are evidently a reflection of his own method and process of reflection about Palaeolithic art, for which reason they are as susceptible to criticism as mine.

Leaving aside the very logical discussion about these aspects, Halverson's commentary can be summarised in some sections which I shall try to respond to.

One of the aspects of his commentary that surprised me most is the one relating to the sacredness of caves containing Palaeolithic parietal art. In this sense I note that, except for the interpretations around the turn of the century which supported the idea of 'art for art's sake' (Mortillet 1883; Piette 1907), the theories have always had some element of sacredness of the decorated caves. Two

of the best-known examples (Breuil 1952; Leroi-Gourhan 1965) take quite similar positions in this respect. In spite of that, the concept of the caves' sanctity and their designation as sanctuaries deserves more attention than it has received over the years. For this reason, one aspect of my work deals with the analysis and definition of the concept of Palaeolithic sanctuary.

Several early as well as recent authors (Alcalde del Rio, Breuil and Sierra 1911; Alcalde del Rio, Breuil and Obermaier 1913; Laming-Emperaire 1962; Bahn 1980; Giedion 1981) have attempted to define the internal characteristics of the cavities with Palaeolithic parietal decoration, which are related to their selection and their significance as special spaces. The brevity required in this debate prevents an exhaustive consideration of the development of my work, but its outcome is a series of concepts which can be summarised under two headings:

- Collective sacred environment (outside cave)
- Collective sacred space (inside cave)

Both have some minimum characteristics, identifying as sanctuaries the cavities where they occur. This method was applied to two large groups of cavities, those of Monte del Castillo (Castillo, Pasiega, Chimeneas and Monedas) and those of Monte Pando (Covalanas and Haza), and in some sections I made reference to other Spanish or French caves (Salitre, Santian, Pendo, Clotilde, Las Aguas, Meaza, Pindal, Hornos de la Peña, Sotarriza y Cobrantes for Spain, and Niaux, Laussel, La Madeleine, Mas d'Azil and Cap Blanc for France). The results led me to the conclusion that caves with paintings and engravings are sacred collective spaces, since a series of rules in their localisation, orientation and internal characteristics are repeated in them which join them in a common cultural tradition with little variation. This is proved by the selection of some caves over others for the location of the artistic manifestations. In Monte del Castillo, for example, only four of a considerable number of chambers are decorated. This trend is repeated in a great number of examples, so it seems obvious that there is a selective process indicating a common idea. The category of 'sacred' is derived from the exclusiveness, and presence of ordered artistic works that were executed under relatively precise rules. The majority of sacred spaces in the most diverse cultures is defined by the absence of light and sound, by temperature and localisation (Eliade 1981), and the presence of creative works which reinforce the idea of sacredness.

Halverson's second focus of criticism refers to the validity of the categories of concave and convex supports of the panels and rock forms in a cave, and the decoration they support. Halverson cites, among others, the example of the well-known ceiling of the polychromes of Altamira and claims an absence of relation between the convex panels and the bison. I am afraid I disagree absolutely. If we revise the definition of this kind of panel (and the one of the other models) we have to consider the physical form of the rock support, the figure it supports and how this is related to the surrounding space.

It is evident that the size, situation and the manner of execution make the bison clear convex figures and not concave ones, since their relation to the space is 'fundamentally aggressive'; they are far more visible than the rest of the figures on the ceiling and walls of the hall. Halverson ignores not only the manner of execution but also the physical characteristics of the place, the original access, and the small original height of the hall. We must remember that the central 'witness' shows a height of no more than 1.8 m at the highest point and the height is just 1.1 m in the innermost zone, precisely where the hind is located (Breuil and Obermaier 1935; Cabrera 1981). Therefore if we try to reconstruct the original state of the hall, which is feasible, we realise that, although the panel bearing the hind is physically flat, in its origins it was clearly concave since its form and figure had no impact at all on the space as I pointed out in the definition of this kind of panel. I must add that the definition of the flat panel involves neutrality in respect to the surrounding space.

The development of my work in the caves of Monte del Castillo and in others in the Cantabrian region permits verification of a true organisation in the way different Palaeolithic figures are distributed and executed, always admitting a certain originality for each one of the caves. In other words, the distribution of the figures and their manner of execution are determined by the place they occupy in the cavity. Unlike in Leroi-Gourhan's scheme, the localisation, kind of figure and evidently its manner of execution are related to the physical characteristics of the cavity. There is no model of distribution of figures, they are arranged in each cave according to its spatial characteristics. Certain figures coincide with physically prominent locations, and the position of the rest of the figures is established. There is no initial idea saying this or that figure belongs in the entrance, in the centre or in a diverticulum of a cave (Leroi-Gourhan 1965), but an elemental scheme presenting a number of variables (to be studied, but basically regional and typical of a hunter-gatherer society), and each representation or group of representations occupies an established and precise place. Thus the organisation and diverse typologies of supports would have a relative value per se. If we add to this the way the figures are arranged and placed in each cavity, we realise that their value can be more important than it could seem at first. As Leroi-Gourhan (1964) points out, 'prehistoric man has only left truncated messages'—but messages nevertheless.

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By PAUL G. BAHN

All Play and no Work . . . ?

I may have done a disservice to Reynaldo González in presenting the tentative speculations, made at the very end of his thesis, about the pos-

sible applicability of his findings to other caves. In any case, it is for him to reply to Dr Halverson's comments about these caves and about his work as a whole (though I would like to make the point that I see no reason why the Tuc d'Audoubert bison could not have been propped against a concavity!).

I prefer to address the issue of 'art for art's sake' which Halverson recently roused (1987) from the deep slumbers to which it had (deservedly in my opinion) retreated, and which he supports again here. I applaud his rejection of unsubstantiated assumptions about Palaeolithic sacredness, religion and so forth, having myself pointed out the huge amount of wishful thinking involved in theories about fertility and hunting magic (Bahn 1986, 1988; Bahn and Vertut 1988). However, I disagree completely with his view of cave art as something capricious, done as play or on a whim. On the contrary, I have become profoundly convinced that much of it has meaning, structure and often evidence of planning, and I believe that virtually all specialists in Palaeolithic art would concur.

Indeed, I find it somewhat insulting to Cro-Magnon artists to attribute their efforts to simple imitation, repetition or play (all somewhat reminiscent of infants or even chimpanzees), and demeaning to claim that Palaeolithic artists would not be able to tell us why they were making particular sketches. The tremendous sophistication of their work, its wide range of techniques, and the evidence of meaningful design (see below) all cry out against this.

To take the Altamira ceiling as an example: from de Sautuola onward most prehistorians have seen the final phase of its decoration—that of the polychrome animals—as being due probably to a single artist, and the recent detailed analysis by Apellániz (1983) has convincingly supported this notion, for the bison figures at least. Lorblanchet's study (1980) of the Frise Noire at Pech Merle suggests strongly that it too is a single piece of work by one artist. There are many other examples, sometimes whole caves (e.g. Gabillou).

We can never *prove* (though it is highly probable) that these examples were planned out beforehand; but since there are innumerable cases of multiple figures—similar or different, and almost certainly by one hand—arranged to fit the shape of bones, stones and antlers, why should we assume that the artists were incapable of doing the same on cave panels?

The lack of proof applies to any such panel. The recently reopened metro station of Cluny-Sorbonne in Paris has its plain, arched roof decorated with immense copies in coloured mosaic of the signatures of eminent figures from French history. They are made in different colours, orientated in various directions, and irregularly scattered across the ceiling. Would an archaeologist of the future see them (correctly) as being a composition by a single artist, or as a whimsical, random, imitative accumulation by many people? I believe the uniformity of style and technique would argue for the former hypothesis, and the same criteria apply in many Palaeolithic cases.

Besides, even if one were to swallow the 'imitative explanation' in a particular instance, how does this explain the repetition of essentially the same motifs through millennia and in widely separated caves? Even though Leroi-Gourhan's 'blueprint' for cave layout has been justly criticised and called into question, with recent detailed studies placing emphasis on the uniqueness of each cave (Vialou 1986), it nevertheless remains true that there is a certain thematic unity: animals depicted are of a very restricted range, almost always as adults in profile; some associations of species occur frequently, others never; and a certain amount of Leroi-Gourhan's data on positioning of animals (e.g. horse and/or bison dominating in some types of location) still stands firm. As he himself pointed out, if the art were truly a random, mindless accumulation, one might expect to see, for example, a frieze of lions and storks surrounded by hyenas and eagles, but this never happens (Leroi-Gourhan 1972).

Few scholars, I think, would deny that some Palaeolithic art could well be 'art for art's sake', or might have been done by youngsters—some finger tracings, hand stencils, simple signs or apparently crudely executed animals would be examples here; but the 'well-executed' animals and the complex signs are a different matter.

There is so much Palaeolithic art, and in such variety, that bits of evidence can easily be selected to support a wide range of notions, including those I have examined in recent papers. It is noteworthy that the few caves mentioned by Halverson are big, well-known sites where today one simply strolls in and looks around at painted walls—not surprisingly they do not look very mysterious, and some of them (such as Castillo or Altamira) have very complex decoration comprising many different phases, making it difficult to judge whether a purposeful layout was involved.

But these few are not truly representative of the phenomenon of Palaeolithic parietal art which is now known at about 275 sites in Europe (Bahn and Vertut 1988) and in which engraving, rather than painting, dominates. It is in those caves where, after crawling through *chatières* or squeezing up chimneys, one finds figures hidden in nooks and crannies that one becomes convinced that there is meaning and function here.

I have already listed some of the many objections to the 'art for art's sake' view in my earlier reply (Bahn 1987b), but they will bear repetition (not mindless, I hope) here.

First, the above-mentioned restricted range of subject matter; the extreme rarity of very young animals; the different content of parietal and portable art; the frequent lack of any correlation whatsoever between the animals depicted and those consumed at particular sites; the apparent re-use of portable and parietal images through time; the consistent locations of female, male and neutral human figures within caves; and so forth. I submit that these and other factors reveal a certain amount of systematic planning, of meaning and function. It was not simply a question of picking a nice smooth *cadrage* and then sketching whatever came to mind: the best available *cadrage* would surely have been chosen no matter what the art's function (or lack of it).

There were probably many different functions

and meanings within each phase and throughout the period. We do not know whether these functions were religious or secular; probably both are represented, and we shall never be able to differentiate them. But what we *can* do is learn more about the details of layout, associations and colour schemes within individual caves. This is one of the major avenues of current research, and that is why González's results—even if they prove applicable only to the Monte Castillo caves—are intriguing and potentially of great importance.

I feel it is just as mistaken to deny all meaning as it is to go all out to prove a single pet theory: the atheist is as biased, blinkered and illogical as the bible thumper. Where Palaeolithic art is concerned, I prefer to remain an agnostic, never expecting to find the solution but always trying to keep an open mind, to learn more, and to take all the facts into account.

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TWO VIDEOTAPES AVAILABLE

WIDMAKARA: ROCK ART IN OLARY PROVINCE

A 30-minute, 16 mm documentary film of Aboriginal painting and petroglyph sites in the Yunta-Olary area has been produced privately by AURA members. The sites are located in a north-eastern district of South Australia and have been the subject of major research papers published in RAR (including the article by Nobbs and Dorn, pp. 108-146, this issue).

The sites comprise granite rockshelters with paintings and drawings, and rock pavements with ancient, varnished petroglyphs. The range of motifs at these art sites falls within the range of the 'Panaramitee traditions'. There is a similarity of motif content between paintings/drawings and petroglyphs that possibly illustrates an adoption of ancient styles by later artists.

Reference is made to the region's natural and cultural landscape, and to evidence of a lengthy habitation of the area by Aboriginal people. Recent dating indicates that petroglyphs were produced in the area prior to 31 000 years BP, making them the oldest dated rock art in the world.

Video copies of this film are available at $A50.00 plus postage from Margaret Nobbs, P.O. Box 21, Burnside S.A. 5066, Australia.

OPENING OF FIRST AURA CONGRESS

Maningrida Media has produced a videotape of the spectacular opening ceremony of the First AURA Congress in Darwin. The video film includes Kunwinjku artists, whose works were exhibited at the Congress, describing their art. Also featured are the Barorroga Mimi Dancers as well as other features of the opening ceremony.

Maningrida Media is an Aboriginal video recording unit from central Arnhem Land, and all monies raised from sales of the tape will be shared between Maningrida Arts and Crafts, and Maningrida Media, to cover working costs of these Aboriginal organisations.

The cost per videotape is $A30.00 including postage within Australia (add overseas postage and nominate either air or sea mail delivery). To order a copy of this videotape please send a cheque (payable to Maningrida Media) or money order to Maningrida Media, c/o Post Office, Maningrida N.T. 0822, Australia.

BRIEF REPORTS

Reflections on the First AURA Congress

JACK STEINBRING

Even after a month it is difficult to digest all of the events and experiences of the Congress. In my case, the complexity of this task was increased by a four-day precongress field trip to the Victoria River region. Earlier pronouncements about the richness of Australian rock art may have seemed extravagant to foreigners, but this trip would have retired even the smallest doubts. The power and magnitude of the Australian rock painting idiom were instantly manifest, and the volume of petroglyph activity (susceptible to good scientific procedures) is inestimable. For myself, the intensity of rock art experience in Australian Aboriginal history places it in full equality with the celebrated concentrations of Europe, Africa and America, and the living context of this art adds immeasurably to its significance.

One person to whom I will always feel indebted for these new perceptions is Howard McNickle. With virtually no preparation, Howard got us to a goodly number of splendid sites—at least one that he had found only a week earlier! Nobody could know these places better, or give so selflessly in communicating them to others.

Another individual added greatly to my initiation into Australian rock art. This was Bruno David, a gifted young archaeologist/ethnologist from the Australian National University at Canberra. He took the trouble to provide critical interpretations at several sites. These were based upon his work as a participant observer in the Aboriginal communities. His empathy with these communities was obvious, a fact which greatly strengthened his conveyance of the rock art's cultural context.

From the purely practical standpoint of adapting to bush travel, Julie Drew, another graduate student at Canberra, deserves honourable mention. This young woman personified what I have come to regard as the best in Australians: cheerful always, astutely practical, inquiring and gracious. Along with her, I would like to list Mr and Mrs Colin MacDonald. They get my endorsement for 'perfect Australians' too: kindly, considerate, and steeped in sensitivity to bush life.

While the field trip made us acutely aware of a profound concentration of rock art, the Congress itself produced a renewed focus on world rock art research which will likely stand for some time. The publication plans for the proceedings make it clear that definitive material filling several volumes will find its way into both the theoretical and substantive researches of the international rock art community.

Only people can make things happen. In the case of this event it has been plain to all for at least a year that Robert Bednarik is the central force and direction of the AURA Congress. No superlatives would be inappropriate in identifying his colossal effort, and the sensitive intellectual perceptions which guided the emergence of key issues in debate. In the academic realm, nothing proceeds without debate, and Robert wrote this into the congress. The effect will be historic.

One thing that came to disappoint me about the congress format was my inability to participate in many different sessions. For example, with simultaneous presentations on the Americas, Australia and Europe, a significant number of delegates may have missed those apart from their geographic specialty. In future world congresses I would recommend a more continuous sessional system (well into the evenings) for the geographic emphases, with simultaneous offerings in procedural and conservation foci. An added benefit from this type of approach would be the better opportunity for delegates to practise some tourism. Nobody can deny that delegates are interested in their exotic surroundings. In fact this may be an important, partial motivation for some.

Another disappointment, of course, were the hotel's deficiencies in regard to projection equipment and projectionists. If it were not for the excellence of impromptu volunteers (like Breck Parkman, Clem Meighan and Helen Schuster, in sessions I attended) there would have been some real disasters.

Many of the numerous congress papers and presentations were stimulating and illuminating. Our new awareness of rock art in the Soviet Union and in China form an important contribution, as does that of India and Sri Lanka. For intellectual impact, the archaeopsychology session provided an incredible array of theoretical currents, many of them potentially historic themes. None of the debates in this area of research could equal that addressed by Alexander Marshack. His extemporaneous analysis (lasting more than an hour) of Lewis-Williams' thesis on shamanistic rock art was a spectacle of genius and personal vigour.

The biggest surprise of the Congress was my election as Vice-President of AURA. When Paul Bahn asked me if I would allow my name to stand in the nominations, I thought it was simply a case of insufficient numbers of nominees, that more names were being solicited to avoid the image of 'railroading' etc. The result was quite a shock.

Nonetheless, I will do my best to fulfil my duties, whatever they may be, at long distance. If there is the least hint of dissatisfaction with my foreign status I will step down immediately. On the other hand I can mention a benefit for the *type* of choice made. Some governments identify 'international' associations on the basis of percentage of foreign members elected to the executive. For Canada, at least, this choice satisfies the legal requirements, and will make it possible to send a Canadian to any future AURA congress.

Both my wife and I are enormously grateful for the opportunity of learning so much about so many things, and of meeting so many wonderful people.

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[NOTE: Professor Steinbring need not be concerned about his 'foreign status'. It had been decided before the GM that it would be most desirable to have at least one non-Australian on AURA's executive committee, in view of the large overseas component of AURA's membership. AURA has now about 200 members in North America, and it is most appropriate that they should be represented by a leading North American scholar.]

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Micromorphological recording of petroglyphs at Ingaladdi, Northern Territory, taken by François Soleilhavoup during one of the AURA Congress field trips.

DARWIN'S AURA: the Congress and the Excursion

PAUL G. BAHN

The Congress

Most of us who flew into Darwin had little idea of what to expect, apart from the heat and humidity. We made our way to the various hotels—the majority of participants were lodged as groups in the rooms of the Mirambeena Tourist Resort. These instant mini-United Nations taught us something of each other's cultures and nightwear. The Mirambeena's pool and jacuzzi were extremely popular, and the scene of some of the conference's liveliest, rudest and most enjoyable discussions.

The conference itself was held at the Beaufort Convention Centre, a great white elephant of an edifice which proved somewhat ill-equipped to host conventions: the unfortunate presenters of opening papers found that some rooms could not be darkened, and looking at slides is, after all, fairly crucial at a rock art conference. However, the problem was overcome with plastic sheet and sticky tape, and our visual problems were largely solved, though we still shivered occasionally in the glacial air-conditioning and shuddered at the price of refreshments.

The opening event was a spectacular display by the Mimi dancers, together with an exhibition of superb bark paintings. Many of the foreigners present had never seen an Aboriginal before, let alone a group painted up and in full action, so flashguns were popping from all directions.

The 5th CHAGS conference (on Hunters and Gatherers) was being held on the same dates in Darwin, at the Museum, and it had always been thought that everyone would be able to pop across from one to the other. As it turned out, the Museum was quite a distance from the Beaufort, so very few people other than an occasional fanatic on a bicycle tried to attend both. In fact there was such an abundance of riches on offer at AURA that the comparatively meagre fare at CHAGS was hardly missed. Thanks to the three concurrent sessions at AURA, there was always something of interest to listen to, and often very difficult choices to be made between two or even three excellent papers being given simultaneously.

This *embarras du choix* was pretty frustrating at times; one can certainly await the published version, but a live version with colour slides is inevitably far superior. However, it is hard to see a solution to this problem, since the number of papers offered at future AURA congresses will certainly increase rather than decline. They could be weeded out severely, but many people get travel grants on the express condition that they give a paper. The congress could be doubled in length, but few people could come for a fortnight, and a week of non-stop papers is already quite long enough. So it looks as if simultaneous sessions are here to stay. One partial solution might be to have sessions on conservation running alongside regional sessions, and not grouped together as at Darwin.

It is invidious to compare sessions, but there were some definite highlights such as discussions in John Clegg's and Whitney Davis's symposia (often featuring Alex Marshack), and the lively debate on the repainting issue. Andrée Rosenfeld and I were very gratified at the turn-out for parts of our 'Rock Art and Prehistory' session, which reached standing-room only for the important closing papers concerning women and rock art.

On every level the congress can be counted a triumphant success: the biggest rock art gathering ever held, with 343 people from dozens of countries present. As at every such event, innumerable friendships (and occasional enmities) were begun or confirmed. It was fun putting faces to the names one knows so well from **RAR** and elsewhere. One must salute all the hard work put into the organisation by Robert and Elfriede Bednarik (Austri-

ans Underpin Rockart Assembly), and by George Chaloupka and Pina Giuliani.

The Excursion

The first day of the Grand Tour turned into what seemed to be an attempt to enter the *Guinness Book of Records* for cramming the highest number of people into a rock art site: there were over 100 people present (from over 30 vehicles), all jostling for positions from which to view or photograph the pictures. Fortunately for the art and for George's blood pressure, this multitude quickly diminished as people left on private tours, returned to other commitments, or simply quit because of the heat and conditions. So after a couple of days the numbers were already more manageable, and wisely we were being divided into two groups: one, under Paul Taçon, got a thorough grounding in Kakadu art, while the other group tried to keep up with the tireless David Welch.

The organisation proved impeccable, and there were few mishaps: the only real hiccup occurred when Ken Hedges' vehicle got stuck in the crocodile-infested South Alligator River, but fortunately Ken and his party emerged unscathed (Americans Underwater Repulse Amphibians). There were some real treats—notably the barbecue where George and his Aboriginal friends cooked barramundi, buffalo, magpie geese and turtle for us, though a discreet veil should be drawn over events at the post-barbecue drinks party (Alcohol Unlimited Revives Australians). At the same event, held on George's birthday, he was given a magnificent bark painting to which all present had contributed.

There were certain challenges to be overcome: some people had never camped before, while others were not used to the humid heat: several faces and indeed bodies went very curious shades of bright red. One particularly overweight person tried to counter the conditions by wearing far too many clothes and consuming enormous amounts of food—with predictable consequences. And did the excursionists enjoy the scenic flight over Kakadu? Yes, sir, yes, sir, three bags full . . .

At times it reached 45°C in the shade, yet we would be out in full sunlight on exposed rocks: Mad dogs and rock art fans go out in the Northern Territory sun. We were very trusting and very willing—our guides had but to beckon and we would follow through the spinifex: Ours not to reason why, ours but to walk and fry (Archaeologists Undergo Real Agony). And we even developed a tolerance (though never a liking) for hot water and juice. The fortunate few with eskies or even fridges or freezers in their vehicles became extremely popular.

But it was all worth it for the wonderful landscapes, the superb art, the unbelievably starry skies, the frequent waterholes (even committed nonswimmers jumped in for the sheer relief), and the reward of cold drinks at every store we passed.

And we all got fitter as time went on (personally I lost c. 25 lb, so can recommend Australian rock art excursions to would-be dieters), and eventually were hopping from rock to rock like demented macropods. Many of the older members of the party aroused admiration for their stamina, especially Heinz Hunger, a 'long-standing member' of AURA, who belied his 83 years by climbing absolutely anything if there was a chance of seeing any pictures related to his 'preoccupancy', ritual sex. Professor Chen Zhao Fu also won great respect: he was in bad shape by the end of the first day, but stuck with it, and lasted all the way to the Pilbara.

From Kununurra down to Dampier the last two-thirds of the trip were led by Robert Bednarik. Members had dwindled to a couple of dozen, and by the very end, a month after the congress had started, there were eight of us in four vehicles. Despite fearsome warnings from Northern Territorians, we found that the Kimberley and especially the breezy Pilbara were less hot than Kakadu, and there were waterholes to be found, and even occasional cold drinks. There were far fewer stars, though.

We saw some fine Wandjinas in the Kimberley, through the guidance of site custodian Alec Campbell and his brother Paul Chapman. One site that Alec took us to, at Galvans Gorge, was called (much to Heinz's delight) *Darrangingnarri*, or 'Walk over to woman with open legs'. We were also taken to several of the recently repainted sites, and able to see for ourselves the damaging effects of whitewashing over previous figures—our Aboriginal guides seemed as indignant about this as we were.

The Burrup Peninsula made an awesome climax to the trip: thanks to Chris Mills of Woodside Petroleum and Ed Barnard of Dampier Salt, we were able to see some of the richest sites in the world, including Gum Tree and Skew Valleys, where almost every rock seems to have something marked on it. We also saw the 'rock groups at Woodstock'—Kevin and Rita Young accompanied us to the spectacular sites of Gallery Hill and Lukis Granites, where Heinz had fun again.

We were also shown the phenomenal complex of sites at Spear Hill by Howard McNickle, the discoverer. A huge man with an amazing gait, he also has an incredible memory (for which pictures are where, and when the sun is likely to be on them) and a unique logic—it was Howard who declined an offer to fix his faulty headlights because he only needed them at night!

Our camps were fun: they featured a scatter of vehicles, a hammock wrapped in a net like a cocoon, a couple of mosquito nets dangling from trees like bridal chambers, an orange tent that resounded with snores, and often the boom of an oratorial voice talking about rock art into the small hours. The occasional bit of 'tent swapping' did not go unnoticed by anyone.

All in all, the remarkable people on the trip contributed greatly to its overall enjoyment and value, and we were all sad when different characters departed towards the end. There had been no major accidents, no injuries, illnesses or other mishaps. And we had seen a tremendous amount of very varied rock art from which we shall all derive a great deal of information, insight and pleasure in future years.

Darwin '88 will be a very hard act to follow, but if anything can do it, it will be Cairns '92—in terms of size alone it should register high on the 'Beaufort scale'. Despite the success of the big trip,

the general consensus is that excursions should be shorter in future, with no more than a handful of people in them, and always strictly supervised, since even the most alert visitor may accidentally touch or tread on a picture. It may even be advisable to envisage different categories of tour, some fairly superficial for those who simply wish to see and photograph a range of sites, and a more intensive kind for those wishing to take a detailed and prolonged look at only a few.

In any case, it might be wise and profitable, on the basis of this excursion, for AURA to seek sponsorship or discount from the manufacturers of slide films, cold drinks, noodles and tinned pineapple.

The congress and excursion were both tremendous once-in-a-lifetime events. Never again can there be a first AURA gathering or, probably, such a long and varied trip in such pleasant company: we few, we happy few, we band of brothers (and sisters, of course) . . . And rock art people now abed shall think themselves accursed they were not here, and hold their manhoods cheap while any speaks that came to Darwin's AURA.

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The Getty Conservation Institute Course on Rock Art Conservation Los Angeles, 18 – 30 April 1988

ANDRÉE ROSENFELD

This course, organised by the Training Program of the Getty Conservation Institute, was intended to introduce conservators already experienced in other areas of conservation to the special problems of rock art. The GCI Training Program is interested in developing conservation interests and expertise in areas for which there is currently a shortage of specialised training. Their decision to concentrate on rock art conservation is a very welcome one.

Their previous two-week field course in rock art conservation held in 1987 in the Valltorta region in north-eastern Spain (RAR 4: 84) was a 'first' of its kind, and the 1988 course held in California naturally benefited from that experience; for instance, it was designed for a more specialised group. The GCI is also actively involved in establishing the one-year postgraduate course in rock art conservation to be held at the Canberra College of Advanced Education in 1989.

For the 1988 course the first week was held at the GCI in Los Angeles and was devoted to presentation of basic data, discussion and some laboratory work. This was followed by one week's practical instruction and experience at an important but very damaged Chumash rock art site in the Carrizo Plain, c. 300 km north-west of Los Angeles.

The twelve participants in the course had very varied backgrounds. Most were conservators of some experience with specialisations in areas such as paintings, objects or buildings conservation. Some had had some experience of working with rock art, either to remove damage or as rock art recorders, but others had never visited a rock art site. The course had a very international flavour: just over half the participants came from the U.S.A., but there was also one person each from Malaysia, Sri Lanka, Argentina, New Zealand and Australia. The instructors also were drawn from diverse fields and countries to cover a range of subjects pertinent to rock art conservation.

Because of the course participants' very uneven experience in rock art, the first week's program began with a general introduction to rock art. This was my main responsibility and I concentrated on showing the range of materials, techniques and site types from several regions and periods. I was also asked to introduce some of the social or ethical issues of conservation, particularly when the rock art is the heritage of a minority group within a larger political unit. This theme was not adequately covered and, being introduced on the first day, people seemed reticent to enter into discussion. It is clear that ethical issues and management practice could be developed to a far greater extent than is possible within the confines of a conservation course, and this possibility is currently under consideration by the GCI Training Program.

Jean Vouvé of Bordeaux University provided the necessary background in areas of surface geology, hydrology, site microenvironment, and topographic and geological map reading. This was a major part of the course, and it continued throughout the week's field instruction. Since Jean Vouvé had not been able to visit the site before the course, his analysis of the site proceeded during the week's instruction. His ability to involve the group throughout this process of analysis of the site's geology, hydrology and environment was particularly successful since it demonstrated the processes of investigation very vividly.

The relevance of buildings conservation to rock art was explored by Norman Weiss, a conservation scientist who has worked extensively on buildings conservation in the U.S.A. Buildings conservation is a highly developed field with a range of tried and established procedures. Norman Weiss emphasised some of the problems he perceived in relating this expertise to the rather different conditions of environment and materials of rock art, but he also argued that many of the principles and analytical methods used in monuments conservation have relevance to the problems of conserving the surface of the living rock face. This is perhaps an area of rock art conservation that has not been adequately explored.

Actual conservation practice and procedures, including methods for recording rock art and rock surface conditions, were covered by two conservators with very different experience in rock art work. Connie Silver's rock art conservation had been concerned largely with the cleaning of sites damaged by graffiti and other acts of vandalism. In her work for various management agencies in the U.S.A. a concern with restoring a clean and

aesthetically acceptable appearance seems to have been a major requirement. The rationale for this approach is twofold: on the one hand it enhances tourist appreciation of rock art sites; on the other, since graffiti tend to attract more vandalism, a clean and cared-for appearance of a site may diminish the risk of further damage.

In her course presentation, Connie Silver devised a useful laboratory assignment: first participants were presented with a piece of sandstone (from near the site to be visited) and with natural mineral pigments. They were thus able to experiment with some rock art materials and techniques. These works of art were partly stabilised by spraying with ethyl silicate to simulate natural mineralisation. They were then savagely vandalised with a range of classic vandal materials. Participants then attempted to restore their creations to a passable state. This experiment revealed very dramatically how difficult it can be to lift, rather than disperse, the vandal's paints and other pigments even after the appropriate solvents have been identified. Some further limited tests to remove vandal's paint were carried out at the site towards the end of the fieldcourse. This emphasised the added problems of working in field conditions, such as maintaining a poultice on a sloping or vertical surface and where rates of solvent evaporation are difficult to control.

Isabelle Dangas' experience in rock art conservation was primarily based on the Palaeolithic caves of France. There she had had to develop treatment for the problems of natural deterioration processes as much as for the removal of offending graffiti. However, in her work the emphasis had been on stabilisation of rock and/or pigment surfaces; maintaining the integrity of the archaeological document, even in a visibly damaged state, had been her primary constraint. Restoring an aesthetically acceptable appearance as required by American agencies has been of secondary importance to the French authorities. Isabelle Dangas' presentation concentrated on identifying the various conditions of rock surfaces and on discussing stabilisation procedures in relation to the hydrological and microenvironmental conditions of the rock face, thus integrating with Jean Vouvé's analysis.

The contrast in the methodological and ethical requirements under which these two conservators had developed their experience reveals something of the different approaches that seem to prevail in different countries. I believe these are issues which would benefit from greater international discussion and communication among those interested in the preservation and tourist promotion of rock art.

Our experimental site is known as Painted Rock and it must have been one of the larger and more spectacular rock art sites in that region. Several of the art styles of Chumash rock painting are represented (Campbell Grant 1965: Plates 4-5, 88-89 and 96) and at least three sequences of repainting were identified on one of the major panels of recent style. In fact from careful comparison with the photographs of c. 1870 and 1961 published in Campbell Grant's book and the dates of graffiti, Nicholas Stanley Price (the course organiser) was able to deduce that the most recent phase of repainting at the site occurred between the 1870s and 1925, presumably in the late 19th century. This discovery raises the possibility of the use of non-traditional pigments in the most recent art, and could lead to investigations of rates of development of natural causes of decay. With the benefit of hindsight, I believe we could have placed greater emphasis in the course syllabus on the relevance of documentation research on a site prior to its conservation.

The sandstone outcrop of Painted Rock forms a large horseshoe-shaped enclosure in which different facies of the rock are exposed. Weathering conditions vary according to exposure to the eddying air currents trapped in the enclosure and to moisture percolating along bedding joints and other fissures. It therefore presented a very interesting case for the demonstration of the relation between geological structure and the movement of moisture and also for showing different surface weathering effects. The site has been known for a long time and has suffered extensive damage over the years (Bednarik 1985: 172-3). It had been used as a corral for sheep, it contains innumerable graffiti in paints, in local ochre and carved into the rock. Some of the rock paintings had been highlighted in chalk, or elaborated in what appear to be mineral pigments. In addition people had attempted to chisel out sections of painted surface and finally had used it for target practice! Moisture, dust laden with salts from the salt lake in the valley, invading mosses, lichen (not on the art) and nesting birds add to the woes of the site. It certainly introduced us to as wide a range of preservation problems as any site might have. It was an excellent choice for the conservation course field experience.

Painted Rock lies in a large tract of land which has recently been acquired by the U.S. Nature Conservancy with a view to restoring its original vegetation and for the protection of certain endangered species. The Conservancy does not concentrate on archaeological resources, but is concerned to offer the site maximum protection now that it lies within their jurisdiction. To this end they asked us to propose some guidelines on future preservation and management. These formed a useful focus for discussion in the closing sessions of the course. There is now reason to hope that the site will be cleaned up and will remain as a testimony to the heritage of Chumash Indians and as a dramatic example of the fragility of such heritage when not adequately protected.

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REVIEWS & ABSTRACTS

L'Art des Cavernes: Les Sanctuaires de la Préhistoire, by DENIS VIALOU. 1987. Sciences et Découvertes, Editions du Rocher, Monaco. 117 pages.

Vialou, perhaps best known for encyclopaedic, minutely descriptive and analytic studies of cave art, has here produced a general work, highly informative but compendious, broad in scope, elegantly written, insightful and informed by a humane and aesthetic spirit. He reconstructs vividly what the appearance and feeling of deep caves must have been for Magdalenian explorers, their flickering oil lamps and torches revealing the fine details of rock and calcite texture, animating natural formations, and kindling their imagination. The caves became 'sanctuaries', not in the dubious religious (or even more dubious ecclesiastical) sense popular in the literature, but as a world of ultimate contrast to the open world of daylight, movement, change and mundane concerns, places where thought, belief and tradition could be purified or transcended. The caves make Palaeolithic art an art of shadow and light, and an art in which mass and surface must be integrated into depiction.

Vialou strongly emphasises the latter, not only citing the common incorporation of natural formations such as rock bulges, calcite flows and fissures into figurative representations (a kind of 'symbiosis') but also calling attention to the important phenomenon of *'cadrage'*: broadly, the spatial rapport between a representation and its support; more specifically, natural features of colour, texture, surface changes etc., that provide a frame or border setting off a potential 'graphic space' from the surrounding area. He refers to such large chambers as the Rotunda of Lascaux and the Salon Noir of Niaux as well as smaller examples—the Chapel of Mammoths of Pech Merle and the chamber of signs at El Castillo--but might have extended the list considerably: *cadrage* seems to be a common element in site selection. In general, he observes that 'images become incarnate in rock; a kind of powerful accord is established between matter and idea . . . Parietal art is not an art placed underground, hung on a wall like the pictures lined up in a museum; it is an art, absolutely original and indeed unique, born of the union of image and rock, a pure and total creation' (pp. 38-39).

Continuing with a sensitive discussion of style, technique and composition, focusing on specific examples, the author notes the variety of animal postures to be found, indicating the artists' intimate familiarity with their subjects; variation in stylistic conventions of representation, including the familiar topics of strict profile, twisted perspective etc.; the often clever use of surfaces—not only their shapes but also their colours being incorporated into depictions; and different means of suggesting three dimensions. In his discussion of composition, centring on a detailed analysis of the Salon Noir, he tends, in my opinion, to overestimate the element of conscious design, and is obliged to resort to a notion of abstraction to explain away incomplete and overlapping figures that would appear to belie careful planning.

After a sensible discussion of dating, in which he cautions against overemphasis on artistic homogeneity over millennia of depictive activity, and a brief account of prehistoric fauna, both subjects more-or-less required in a general work such as this, the author turns to the subject of human representations, which is treated with unusual fullness and is unusually interesting. Whereas animal respresentations consistently conform in a general way to reality, human representations, he believes, are deliberately anti-naturalistic, from the earliest statuettes to Magdalenian reliefs and engravings, and ideationally stylised. Pointing to the frequent prognathism that seems to turn human faces into animal muzzles, he finds a general 'bestialisation' of human figures that recurs also in a number of animal-headed 'sorcerers', a mingling of human and animal themes, as intimate as possible, which witnesses a remarkable intrusion of the imagination into figurative art, especially in human representation (pp. 80-83).

Reminding us frequently that Palaeolithic art is not simply animal art but has three clear themes—animals, humans and 'signs'—Vialou goes on to discuss the third theme in an informative chapter analysing the various types of signs and their distribution. In some places signs outnumber other figures by a considerable margin, suggesting that, whatever they are, they were considered important by their makers, and indicate the degree to which parietal art is not a reflection of the natural world. He offers no interpretation of the meaning of these signs, but suggests that they symbolised ideas, myths and visions borne by oral traditions: 'Parietal art is a construction of symbols made from two complementary realities: the one is visual, the other was oral' (p. 96).

Vialou is a bit disdainful of theories of cave art, and gives them hardly a glance. He laments how rarely caves are 'correctly and completely studied' (p. 109), arguing reasonably enough that only such study can provide any basis for generalisations. Thus the last two chapters of his book are largely devoted to close inventories and pattern analyses of select caves, replete with tallies and percentages and taking morphology and topography systematically into account. In design terms, he concludes that each cave is unique, indeed 'radically original', rooted in the psychological and metaphysical life of different ethnic groups. Within a single cave, careful topographical analysis may suggest quite different conclusions than those reached by gross tabulation. He uses Le Portel as an example, where an overall count would seem to justify Leroi-Gourhan's well-known horse-bison pairing, whereas a gallery-by-gallery analysis indicates a definite separation of the two, so much so that it is 'as if the Magdalenians had organised a double symbolic universe, the one attributed to the horse, the other to the bison' without any meeting or connection between the two (p. 112). The point is well taken and convincing.

In the end, however, Vialou seems to hold out little or no hope of understanding Palaeolithic art fully, for he is persuaded that it is a codification of representations based on language and metaphor which must remain forever irrecoverable. This does not seem to me a necessary conclusion, for the idea of a linguistic foundation for visual representations is no more than an assumption, which, though not implausible, also has no supporting evidence. Vialou's conviction appears to stem mainly from his interpretation of the so-called signs as a kind of symbolic quasi-script, but the signs by no means require such an interpretation. They may have had no symbolic value at all, linguistic or otherwise. They may have been, as recently suggested, depictions of entoptic phenomena. There seems to be no prima facie reason for supposing that oral tradition in the form of myth and narrative played any significant role in the graphic art of the Upper Palaeolithic. So perhaps the outlook is less dim than Vialou thinks. In the meantime, his own insight, sensibility and thorough knowledge make his small book valuable reading.

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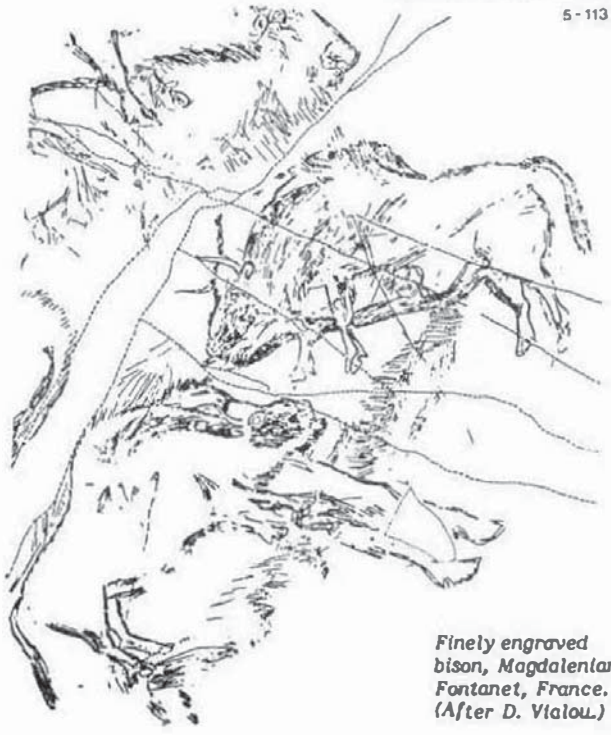

Finely engraved bison, Magdalenian. Fontanet, France. (After D. Vialou.)

Orígenes y Significado del Arte Paleolítico, by EDUARDO RIPOLL PERELLÓ. 1986. Silex Ediciones, Madrid. 183 pages.

This book offers the mature reflections of a senior statesman of Palaeolithic art on the state of the art. Ripoll Perelló's knowledge and experience are vast, the product of many years of close study of rupestrial and mobiliary art at first hand as well as intimate acquaintance with the research literature, to which he himself has contributed much. A student of Breuil ('*mi maestro*') and Leroi-Gourhan, he finds his strongest affinities with an older generation, and realises that his views may not always be compatible with those of 'young investigators', but he certainly knows what that younger generation is doing and for the most part seems to approve of it. His convictions, though strongly held, are not always thoroughly substantiated, but since exiguous evidence is endemic in this field there is all the more reason to respect and ponder intuitions derived from long and thoughtful experience. Basic to Ripoll's views is the conception of Upper Palaeolithic life as necessarily preoccupied, because of the nature of the climate and the principal food supply, with hunting—a reasonable assumption, from which not many, I suppose, would strongly dissent. Perhaps more controversial is the conviction that these ecological circumstances imply myth and ritual centred on hunting, a belief that pervades this book.

Like many others, Ripoll recognises the explosive character of Upper Palaeolithic culture, a 'revolution in human history', and begins by exploring possible antecedent conditions (Chapters 2-3). He believes that Mousterian burials are evidence for a capacity for abstract thought, 'religion', in the broadest possible sense, and at least some form of artistic manifestation. In a useful survey of the earliest marking activity he is disposed to infer ritual purposes. Art itself, with a natural basis in an '*inclinasión al grafismo*', probably began, as Breuil thought, with finger markings and hand prints, but Ripoll would add other factors such as the impulse to complete figures suggested by natural formations; he thinks it likely that the first artistic activity was in a spirit of play, only much later to be utilised for religion. For reasons not given the author maintains not only that language was being perfected long before art appeared, but also that vocal symbols must necessarily have preceded iconic symbols (p. 51).

In an informative 'Synthesis of the Characteristics of Palaeolithic Art' (Chapter 4) it is observed, among other things, that for figurative representations strict profile is the norm and that the majority, especially of earlier figures, are static. Indeed, 'it seems that movement was a discovery of the Magdalenians' (p. 62) and is of particular significance as representing the idea of time, since movement implies a before and after. Both figures and 'signs' fall into stylistically related groups and indicate traditional stylistic norms. While not denying, then, broad continuities, Ripoll recognises a multiplicity of temporal and regional variations that belie simplistic scenarios of the evolution of Palaeolithic art.

A compendious survey of theories of interpretation from Lartet and Christy to Leroi-Gourhan (Chapters 5-7) culminates in an extensive and apparently deeply felt plea for a hunting-magic interpretation *à la* Breuil (Chapter 8), a theory not very popular these days, as the author well knows. The arguments and evidence are familiar—possible 'sorcerer' representations, indications of wounded animals, sanctuary-like sites and various suggestions of ritual activity—but ably presented, and his approach is wide enough to include initiation rites. He is careful to keep his religious concepts very broad, but he has no doubt that Palaeolithic art is essentially religious, inspired by fear and devotion, and more specifically 'totemistic' (in the sense of any indefinite 'spiritual' relationship between man and animal), and by derivation shamanistic. The general position taken is respectable, but the arguments involve too many questionable assumptions to convert the unpersuaded.

In the remaining chapters the author discusses 'signs' again, concluding that they 'represent a very complex thematic' but are 'abstract symbols'; astutely suggests that the immediate motive for some depictions is a *horror vacui*, that 'one figure attracts another' to fill space (p. 157); reviews recent semiological work, particularly that of the Sauvets, agreeing that there is a 'syntax' in cave art that makes it more than the sum of its parts and that the underlying semantic must be mythological ('Art and myth would be in the Palaeolithic two aspects of the same thinking', which would be related to hunting activity—p. 170); and offers a very cursory, largely bibliographical review of other recent research.

For a generally positive and often enlightening book, one of the author's main conclusions seems rather melancholy: 'I may say that concerning the theme of interpretation of Palaeolithic art, I have the feeling that we have been and still are going around in circles in the dark' ('*Puedo asegurar que, en el tema de la interpretación del arte paleolítico, tengo la sensación de que hemos estado y estamos dando vueltas en una habitación circular y oscura*'). Here at least Ripoll Perelló can be assured that many, particularly 'young investigators', will wholeheartedly agree with him. The present reviewer, as it happens, is less pessimistic.

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Spirits of the Earth: A Study of Earthen Art in the North American Deserts. Volume 1: The North Desert, by JAY VON WERLHOF. Imperial Valley College Museum, El Centro, California, U.S.A. 303 pages, 360 figures, 2 tables, extensive bibliography. Cloth US$53.00, paper US$37.10.

This is the long-awaited first volume of what is projected to be a three-volume study of the earthen art of the North American deserts. In this first volume, von Werlhof has concentrated on the earthen art found along the northern edge of the California deserts, including Panamint Valley, Death Valley, Eureka Valley and Greenwater Valley. In Volume 2, the author plans to address the earthen art of the central region, including that found in the Mojave Basin, southern Nevada and the upper part of the Lower Colorado River Basin. Finally, Volume 3 will look at the earthen art of south-eastern California, west central Arizona and north-western Mexico. Volumes 2 and 3 are expected to be completed in 1989.

Von Werlhof defines earthen art as being an arrangement of rock or earth. He includes among this art form rock alignments, geoglyphs ('intaglios'), cairns, monuments, heaps, mounds, upended slabs, henges and barrows. The author points out that earthen art is very old, and that it occurs throughout the world. The focus of this study is on the rock alignments and geoglyphs of the American desert in the south-western United States and north-western Mexico, the area from which virtually all of the North American examples are known.

The author begins his study with a discussion of the history of earthen art research. This is followed by an examination of the beginnings of art, and of its associations with religion. This section is in turn followed by a description of the forms of earthen art. After a long and interesting discussion of shamanism, the author sets about to describe the individual earthen art sites, the emphasis of this report.

Fifty-nine earthen art sites are covered by von Werlhof in Volume 1. The sites are well described, and they are nicely illustrated in line drawings and photographs. The photographic coverage of the sites includes numerous aerial photographs, and the large number of illustrative figures makes this report especially useful for comparative studies.

Although relatively expensive, *Spirits of the Earth* is a significant study and one well worth the cost. It should be read by all who are studying similar aspects of prehistoric art.

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Parks and Recreation

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Stone arrangement at Site 1.9, Panamint Valley, California, 21 m long. (After J. von Werlhof.)

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Quatre cents siècles d'art pariétal. Les cavernes ornées de l'age du Renne, by H. BREUIL. 1985. Ed. Max Fourny Art et Industrie, 46, rue de Richelieu, Paris. 417 p., many photos, drawings and magnificent copies of rock art in paintings (reproduced in black-and-white).

This is a reprint of the classical study by H. Breuil, first published in 1952, dealing with Palaeolithic rock art in Spain, France and Italy. MS

Estudios en Arte Rupestre, ed. by CARLOS ALDUNATE DEL S., JOSÉ BERENGUER R. and VICTORIA CASTRO R. 1985. Museo Chileno de Arte Precolombino, Casilla 3687, Santiago, Chile, 425 p., line drawings, photos, 3 colour photos.

This volume contains the papers delivered at the 'Primeras Jornadas de Arte y Arqueología' (Santiago, 16-19 August 1983), dealing with theoretical or technical aspects of rock art research and area surveys of rock art in Chile and, in some cases, Argentina. I found the following articles of special importance:

Mario Consens (p. 37-58), on the application of a computer in determining rock art styles; Percy Dauelsberg H. and Calogero Santoro V. (p. 69-86), on the dating and cultural identification of a number of rock art designs in north Chile; José Berenguer et al. (p. 87-108), on the rock art sequence in Alto Loa, Chile, where they established a chronological sequence with a first, pre-ceramic phase and three later phases, the last in the 19th century; María Hernández Ll. and María Podestá (p. 109-130), on geometric compositions in rock paintings of Huamahuaca, Jujuy, Argentina, analysing their basic elements and the way in which they are combined to form complex designs; Hans Niemeyer F. (p. 131-172), on petroglyphs of Las Lizas, Atacama, Chile, which consist almost entirely of fishes allowing the recognition of specific species; Lautaro Nuñez A. (p. 243-264) on petroglyphs in the Chilean desert which he convincingly relates to traffic routes; Pablo Cerda F. et al. (p. 311-348) with preliminary information on 69 sites of geoglyphs in north Chile with more than 6000 figures.

Unfortunately, this edition consists of only 500 copies and may no longer be on sale.

Matthias Strecker

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Les premiers moutons sahariens d'après les figurations rupestres, by ALFRED MUZZOLINI. *Archaeozoologia*, 1987, Volume 12, pp. 129-148.

The dating of rock drawings is difficult, but in spite of 'cultural filters' they can give useful and accurate information to the zoologist. This paper provides first a critical survey of the African evidence, from excavations, for early ovicaprines. The traditional view of the lack of pre-Neolithic ovicaprines throughout Africa must be reappraised.

After a brief review of the chronology of the Saharan rock drawings, the two homogeneous forms depicted in them are described: (1) the 'adorned rams' of the Saharan Atlas range, which belong to the '*Bubalin naturaliste*' phase, c. 4000 - 2000 B.C., and are of the *longipes* type: indisputably domestic, hairy, with a long thin tail and ammonite-shaped horns (in one picture with *palaeoaegyptiaca* corkscrew horns); and (2) the sheep of the paintings of the 'Iheren-Tahilahi group', which belong to the Late Bovidian phase (c. 1500 to first half of 1st mill. B.C.). The possible relationships of these sheep with those of Fezzan, Predynastic or Pharaonic Egypt and Kerma are discussed.

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Poissons et contours de type pisciforme dans l'art pariétal paléolithique, by LYA R. ⊡AMS. *Bulletin de la Société Royale Belge d'Anthropologie et de Préhistoire*, 1987, Volume 98, pp. 81-132.

Notwithstanding the general opinion which relegates fish images to the periphery of Palaeolithic cave art, and though they represent a relatively small proportion of the depicted figures, their numbers exceed those of reindeer, for instance. They are mainly located in the Mediterranean area and their distribution seems to imply that they were an important food source. The paper is well illustrated with numerous examples of Palaeolithic fish depictions.

SIARB Boletín. Number 2, May 1988. Annual journal of the Sociedad de Investigación del Arte Rupestre de Bolivia. Edited by MATTHIAS STRECKER. Number 2 has 66 pages:

BEDNARIK, R. G.: El arte rupestre Boliviano visto desde el exterior. Three outstanding aspects of Bolivian rock art, and their potential as research subjects, are examined.

TABOADA T., F.: Arte rupestre de Chirapaca. A major rock art site near La Paz is described.

ENCINAS G., P. J.: Informe preliminar de los petroglifos de 'Marka Rumi' (Quila-Quila), Depto. de Chuquisaca, Bolivia. A preliminary report about a petroglyph site.

PIA, G. E.: Los distintos momentos estilísticos encontrados en las pinturas rupestres de las areas de Roboré, Santiago y San José en el oriente Boliviano. The stylistic sequence of rock art in an east Bolivian area is analysed, using superimpositions, cracks and patination differences.

FERNÁNDEZ DISTEL, A.: La cueva con pictografías de San Lucas, Depto. Valle Grande, Jujuy, Argentina. A preliminary report on a cave with paintings in north-western Argentina.

Enquiries to SIARB, Casilla 3091, La Paz, Bolivia.

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The Digging Stick. Biannual newsletter of the South African Archaeological Society. Edited by SHIRLEY-ANN PAGER.
Volume 4, Number 1 (April 1987) includes on its 8 pages the following:

WOODHOUSE, Bert: Bolas in the rock art of southern Africa.

KINGDON, Z. E.: The work of the Wits Rock Art Research Unit.

TOIT, M. du: Description of a unique set of rock paintings in the Karasberg, Namibia.

Enquiries to Shirley-Ann Pager, P.O. Box 81292, Parkhurst 2120, South Africa.

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La Pintura. Quarterly newsletter of the American Rock Art Research Association. Edited by FRANK G. BOCK.
Recent issues include the following scientific contributions, in addition to numerous notices and announcements:
Volume 13(4), northern spring 1987:

FAULKNER, Charles H.: Underground art in eastern North America: a reply to Campbell Grant.

WILLCOX, A. R.: The rock art of North America and southern Africa compared.

RAFTER, John: The spiral-anthropomorph designs.

Volume 14 (1987/1988):

STEINBRING, Jack: Rock art site classification.

BEDNARIK, Robert G.: Chalking of petroglyphs: a response.

BEDNARIK, Robert G.: No pictograph at end of Rochester Creek rainbow.

Enquiries to the Secretary, ARARA, P.O. Box 65, San Miguel, CA 93451, U.S.A.

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Quiboreña. Revista cientifica del Museo Arqueológico de Quíbor, Venezuela. Edited by MARIA I. TOLEDO. Volume 1 (1986) has 108 pages, two of its five articles deal with prehistoric art:

PALPIERIS, I., M. I. TOLEDO and L. E. MOLINA: Piedra de los Monos: un nuevo sitio de pinturas rupestres en el noroeste de Venezuela.

DELGADO, L.: Arte prehispánico venezolano: una historia, un análisis.

Enquiries to Museo Arqueológico de Quíbor, Avenida Pedro León Torres, Quibor, Estado Lara, Venezuela.

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Relaciones. Annual journal of the Sociedad Argentina de Antropología. Edited by CARLOS J. GRADIN.
Volume 15 (1983) has 235 pages and contains the following rock art papers:

ALFARO, Lidia C.: Investigación arqueológica en la cuenca del río Doncellas (Provincia de Jujuy).

CEBALLOS, Rita and Antonia PERONJA: Informe sobre el arte rupestre de la Cueva Visconti.

GRADIN, Carlos J. and Ana M. AGUERRE: Arte rupestre del Area La Martita (Provincia de Santa Cruz).

Enquiries to Carlos J. Gradin, Sociedad Argentina de Antropología, Domicilio Postal: Moreno 350, 1091 Buenos Aires, Argentina.

ORIENTATION

TASMANIAN HAND STENCILS

As reported in the *AURA Newsletter* of November 1988 (5/2: 12), a resolution of the First General Meeting of AURA concerning the hand stencils discovered in Tasmanian caves has been conveyed by me (as AURA's Secretary) to Senator Graham Richardson, M.P., the Australian Commonwealth Minister for the Arts, Sport, the Environment, Tourism and Territories. In it the Minister was informed that 'these sites are of deep international significance for the assessment of early human culture'. In addition, several member organisations of the International Federation of Rock Art Organisations (IFRAO) have provided strong letters of support, thus emphasising international concern about this issue.

In his reply of 13 January 1989, the Minister advises that 80% of the region considered in the Helsham Inquiry (the Lemonthyme and Southern Forests region of Tasmania) has now been nominated by the Commonwealth Government for World Heritage listing. This means that over half of the four last remaining pristine tall forest ecosystems in Tasmania will be protected, and that around 15% of that state's total area has now been nominated for listing. Management of the World Heritage area will accord with policies approved by a joint Ministerial Council of both state and federal governments.

On behalf of AURA I thank IFRAO's member organisations for their spontaneous and decisive support. The Late Pleistocene hand stencil sites in Tasmania are a unique aspect of Australian rock art, and their inclusion in a World Heritage area should ensure their perpetual preservation. R. G. Bednarik

SÉMINAIRE INTERNATIONAL 'REPRÉSENTATIONS PRÉHISTORIQUES'
Art Préhistorique, Musée de l'Homme
6e Session: 1989
DENIS VIALOU

3 Février 1989:
M. le Doyen A. Beltrán, Universidad de Zaragoza: *Problèmes de l'art rupestre dans la phase de transition du Paléolithique final à l'Epipaléolithique en Espagne.*
M. C. Wagneur, Président du GERSAR, et Mme Wagneur: *Les abrís gravés du massif de Fontainebleau.*
10 Février 1989:
M. le Docteur G. Delluc, Mme B. Delluc, chercheurs membres de l'U.A. 184 C.N.R.S.-M.N.H.N.: *La grotte ornée de la Cavaille à Couze-et-Saint-Front (Dordogne); sa place dans l'art pariétal archaïque en Aquitaine.*
M. le Pr. W. Davis, Northwestern University, Evanston ou M. D. Vialou, U.A. 184 C.N.R.S.-M.N.H.N.
2 Mars 1989:
Séance du Musée des Antiquités Nationales à Saint-Germain-en-Laye avec communications de MM. et Mmes: J.-P. Mohen, H. Delporte, J.-J. Cleyet-Merle, D. Buisson et G. Pincon, L. Mons.
3 Mars 1989:
Mme le Pr. M. Conkey, University of Berkeley (titre non communiqué).
M. le Pr. R. White, New York University: *Vers une compréhension contextuelle de l'origine de l'art et de la parure.*
10 Mars 1989:
M. le Pr. A. Moure-Romanillo, Universidad de Cantabria: *Réflexions sur quelques variantes de l'art rupestre dans la partie orientale de la Cantabrie.*
Mme A. C. Welte, Université de Toulouse: *L'art mobilier du gisement de Fontalès (Tarn-et-Garonne), nouvelles observations.*
16 Mars 1989:
Mme le Pr. Bueno et M. le Pr. R. de Balbin Behrmann, Université de Alcala de Henares: *Art mégalithique de la péninsule ibérique; recherches actuelles.*
M. le Pr. V. Villaverde, Universidad de Valencia: *La séquence artistique de la grotte du Parpallo; considérations sur l'Art Paléolithique de l'Espagne Méditerranéenne.*
A une date ultérieure, M. le Pr. J. D. Lewis-Williams, University of the Witwatersrand, pourrait présenter une communication dans le cadre du Séminaire.
Professor Denis Vialou is continuing his series entitled 'Prehistoric representations' at the Museum of Man, Paris, in the early part of 1989.

Notices

The INTERNATIONAL FEDERATION OF ROCK ART ORGANIZATIONS has been founded in Darwin on 3 September 1988. AURA is a founding member of the Federation, and RAR has been chosen as its official journal. For details see *IFRAO Report*, p. 174 of this issue.

INDIAN ROCK ART RESEARCH ASSOCIATION: The establishment of AURA's Indian equivalent was announced in Shantiniketan on 2 December 1988; see *IFRAO Report*, p. 175 of this issue.

EDITORIAL BOARD: E. Breck Parkman, a Californian rock art researcher, has joined the editorial board of RAR.

BACK ISSUES of RAR are available, at the normal subscription rate of $A10.00 per annual volume (2 issues).

Forthcoming Events

ARARA SYMPOSIUM 1989. To be cosponsored by the American Rock Art Research Association, the Witte Museum, the University of Texas and several other organisations, and to be held from 26 to 31 May 1989 at the Witte Museum, San Antonio, Texas.

WORLD SUMMIT CONFERENCE ON THE PEOPLING OF THE AMERICAS. An international gathering of scholars from more than a dozen countries will be held from 24 to 28 May 1989, at the University of Maine campus in Orono, Maine.

TRANSFORMATION OF EUROPEAN AND ANATOLIAN CULTURE IN PREHISTORY. Second interdisciplinary conference, involving archaeology, mythology, linguistics and physical anthropology. 25 August to 1 September 1989, Dubrovnik, Yugoslavia.

ANNUAL AURA MEETING 1989. To be held in Sydney, Australia, over a weekend, probably in September or October: one day of lectures and debates, one or two days of field trips. Dates and venue to be advised.

DIACHRONIC TRENDS IN HISTORICAL ANTHROPOLOGY and Third Anthropological Congress honouring Aleš Hrdlička. To be held from 31 August to 9 September 1989 at the National Museum in Prague, Czechoslovakia.

SECOND INTERNATIONAL TRIENNIAL OF MUSEUMS - TRIOMUS. To be held from 14 to 18 May 1990, in Rio de Janeiro, Brazil.

ARARA SYMPOSIUM 1990. Last weekend in May 1990.

LAVAS AND VOLCANIC TUFFS. A competition and meeting being organised by the World Monuments Fund, with the purpose of encouraging the study of the rock Easter Island monumental statues are made of, and which also bears the island's numerous petroglyphs. This rock is deteriorating and sculptures are in serious need of conservation measures. Abstracts are required by 1 March 1989, papers by 1 March 1990, the awards will be assigned by 1 June 1990, and the meeting will be held on Easter Island in October 1990.

ANNUAL AURA MEETING 1990. Time and venue to be advised.

SECOND AURA CONGRESS. To be held in Cairns, Queensland, in 1992; the major international rock art event in the foreseeable future, with around 200 research papers. The Second AURA Congress will seek nomination as the host of the 1992 IFRAO Meeting.

Letter to the Editor

Dear Sir,

We are all grateful to you for running the world's foremost journal on the study of prehistoric pictures.

Like all other great enterprises, your editorial practice has room for improvement, and I am writing this letter so that you will know how I feel about certain matters. You may feel differently, and it is your editorial privilege to practise as you wish.

(1) There are certain conventions of reference in scholarship and science, to the effect that the contributions of others should be recognised (lest there be any suggestion of plagiarism) and that the sources of data, arguments and ideas should be stated so that other interested parties may acquaint themselves with them. This convention helps to preserve scholarship and science from arguments by assertion and bullying.

No doubt the referees you consult before you accept a paper for publication are careful to check that contributions are properly referenced, but your present system allows an author to hide behind others' references. I suggest that the practice of *Current Anthropology* be adopted in appending the initials of the commenter who mentioned the item to bibliographic references. This practice allows readers to evaluate for themselves the scholarly and scientific status of contributions.

(2) It is disconcerting to discover that a text has been altered after it went out for comment, without telling the commenters. Resulting comments may be irrelevant to the published text. This is not fair to the commenters.

(3) All good journals have a house style. Please tell us what yours is. I would like you to correct my English, but not so as to alter the meaning. It is after all the person whose name appears in association with a paper who is usually thought responsible for its content.

I suggest that your journal be in Australian English. Editorial footnotes could provide translations for foreign readers if considered necessary.

(4) There is a long English intellectual tradition of making rigorous distinction between seriousness and solemnity. In other cultures one has to be solemn to be taken seriously, but in the Australian—particularly of Irish tradition—it is possible to write seriously in an unsolemn, or light-hearted mode. This can be particularly valuable in making it clear that a comment on someone's work or conclusions (through which science proceeds) is not a personal attack. For these reasons I consider it essential that a light-hearted serious contribution be not made solemn by editorial interference.

Yours sincerely,
John Clegg

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EDITOR'S RESPONSE

Dear John,

I am grateful for your kind words about the standard of our journal, and for your constructive criticism. In response to the points you raise I comment as follows:

(1) Your request is most pertinent and welcome; I shall go one step further and henceforth use a reference system superior even to that of *CA*. Not only will references introduced by commenters be identified by initials, those given by the principal authors only in their response will also be identified in this fashion. The long-term effect of this practice should be that authors of major papers will be obliged to include ALL relevant work in their initial reference list, i.e. before it is pointed out by commenters, and this should serve as a further safeguard against plagiarism.

I must stress, however, that responsibility for proper referencing does rest with the author, although the referees and I have often requested the inclusion of additional bibliographical references.

(2) This has been necessary on only one or two occasions in five years, and there were good reasons for it. Most importantly, when an author discovers, in time to alter the proofs, that he or she has been too brusque or severe, I am only too willing to comply with a request to moderate potentially acrimonious remarks. I strongly advocate both vigorous scholarly debate, and enduring personal rapport among those working in our discipline. To consolidate these two ideals we need to exercise some degree of restraint in criticism, and to also distinguish clearly between academic and personal critique.

(3) and (4) Spelling and punctuation in this journal follow the *Style Manual for Authors, Editors and Printers of Australian Government Publications* (known as the 'Editor's Bible') and the *Macquarie Dictionary*; in the few instances where the two disagree, the former has precedence. (For a summary of the main rules, please refer to the call for final drafts of papers presented in Darwin, *AURA Newsletter* November 1988.) But, to respond more directly to your request for clarification, and the issue it is related to: contributions must be free of Australian colloquialisms and idiosyncrasies that cannot be understood by non-Australian readers. This also applies to light-hearted contributions, and the peculiar Irish-Australian tradition you refer to. As the largest archaeological and anthropological journal in Australia, RAR could be expected to project a scholarly image befitting this position, but there are in fact more important issues at stake here. Only about one quarter of our present readership can be expected to understand 'peculiarly Australian terms', and informal contractions (such as I'd, which has three possible interpretations) are unacceptable in any scientific journal—precisely because they are ambiguous. Not only is RAR an international journal read in well over 40 countries, its recent nomination as the official organ of the International Federation of Rock Art Organisations renders it imperative that we cater primarily for the international reader. I think that Australian readers will forgo the small indulgences you advocate and value my policy as symptomatic of Australian leadership in the discipline of cognitive archaeology. Thus contributors wishing to avoid what you term 'editorial interference' will need to present their work in a format suitable for addressing not an Australian, but a global research tradition and readership.

R. G. Bednarik

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New AURA Members

We welcome the following new members who have joined the Australian Rock Art Research Association during the twelve months to November 1988:

Professor Francesco G. Fedele, Torino, Italy
Mr James F. Leckie, St Johns Wood, Queensland
Mrs Helen F. Leckie, St Johns Wood, Queensland
Mr Erwin Neumayer, Vienna, Austria
Mr Denis Rose, Halls Gap, Victoria
Mr Joe Schmiechen, Forestville, South Australia
Dr David M. Welch, Wulagi, Northern Territory
Professor Guy H. Wolf, II, Towson, U.S.A.
Western Australian Museum, Perth, Western Australia
Mr B. D. Nandadeva, Kadawata, Sri Lanka
Dr Yusuf M. Juwayeyi, Lilongwe, Malawi
Ing. Jacques Brunet, Champs sur Marne, France
Mr Bernard M. Huchet, Armidale, New South Wales
Mrs Kim Sales, Greenmount, Western Australia
Mr Ed Douglas, Grange, South Australia
Dr Yuri A. Savvateyev, Petrozavodsk, U.S.S.R.
Mrs Valerie C. Chapman, Mawson, A.C.T.
Instituto Nacional de Antropologia, Buenos Aires, Argentina
Mr John H. Hunt, Mount Gambier, South Australia
Université de Provence, Aix en Provence, France
Ms Alexandra Rivers, Camperdown, New South Wales
Dr Wendy Beck, University of New England, New South Wales
Miss Meenakshi Dubey, Ujjain, India
Dr Iain Davidson, University of New England, New South Wales
Mr Robert D. Catlett, Cambria, U.S.A.
Mr Nevin N. Ellis, Braidwood, New South Wales
Dr Prous Poirier, France
Mr Robert J. Ware, Adelaide, South Australia
Mr Ross Muegge, Adelaide, South Australia
Mr Neale Draper, Adelaide, South Australia
Mr Desmond S. Coulthard, Port Augusta, South Australia
Dr Christopher Chippindale, Cambridge, England
Professor Frederick Levine, Australian National Univ., A.C.T.
Visual Anthropology, Philadelphia, U.S.A.
Dr Michael J. Morwood, Univ. of New England, New South Wales
Drs Dirk L. Huyge, Leuven, Belgium

Ms Monica Hinrichs, Lake San Marcos, U.S.A.
Mrs J. P. Collins, Dunbogan, New South Wales
Mr Paul H. Schoonover, Cave Creek, U.S.A.
Professor Anthony Forge, Australian National University, A.C.T.
Ms Margaret Clancy, Chatswood, New South Wales
Dr Rakesh Tewari, Lucknow, India
A.N.U. Library, Canberra, A.C.T.
Ms Elizabeth M. Caldicott, Hawthorndene, South Australia
Ms Melody Tannam, Oakland, U.S.A.
Centro Studi e Museo d'Arte Preistorica, Pinerolo, Italy
Ms Lisa Hill, Glebe, New South Wales
Ms Su Solomon, University of New England, New South Wales
Drs Antonio Núñez Jiménez, Habana, Cuba
Mr Stanley C. McFarland, San Diego, U.S.A.
Mrs Maurine P. Dorman, Price, U.S.A.
Professor Robert I. Jack, University of Sydney, New South Wales
Ms Nancy Keating, Australian National University, A.C.T.
Mrs Joyce Warren, Greenwich, New South Wales
Mr Michael J. Rowland, Brisbane, Queensland
Dr Sudha Malaiya, Indore, India
Mrs P. M. Th. Smits-Gerritzen, Ellecom, Netherlands
Ms Cheryl L. Cooper, Univ. of New England, New South Wales
Mr Edward C. Chapman, Australian National University, A.C.T.
Professor Margarita Bru, Madrid, Spain
Ldo Andrés Diez, Madrid, Spain
Ms Natalia Diez Bru, Madrid, Spain
Mr Rowan Webb, University of New England, New South Wales
Dr Peter Stanbury, University of Sydney, New South Wales
Mrs Lydia Bushell, University of Sydney, New South Wales
Mr John Rudolph, Winslow, U.S.A.
Dr Dale W. Ritter, Chico, U.S.A.
Mr Geron Marcom, Fort Bragg, U.S.A.
Dr Yashodhar Mathpal, Bhimtal, India
Professor Emmanuel Anati, Capo di Ponte, Italy
Musée de l'Homme, Paris, France
Dr Giancarlo Negro, Segrate San Felice, Italy
Menzies Library, Australian National University, A.C.T.
Dr Graeme K. Ward, Canberra, A.C.T.
Dr F. N. Anozie, Nsukk, Nigeria
Ms Yoshiko Inui, Sapporo, Japan
New York Public Library, New York, U.S.A.
Mr Paul Peterson, Petaluma, U.S.A.
Mr Colin G. Macdonald, Aranda, A.C.T.
Mrs Pamela M. Macdonald, Aranda, A.C.T.
Mr David Roe, Australian National University, A.C.T.
Ms Linda S. Sehgal, Lompoc, U.S.A.
Ms Shirley Chesney, New York, U.S.A.
Professor William Noble, Univ. of New England, New South Wales
Mr Christian Wagneur, Perthes, France
Mrs Janine Wagneur, Perthes, France
Ms Maya Wright-Schmidt, Stockholm, Sweden
The Australian Museum, Sydney South, New South Wales
Mr A. Govendir, Dover Heights, New South Wales
Dr Friedrich Berger, Richmond, United Kingdom
Mr Robert C. Monteith, Forest Knolls, U.S.A.
Mr John A. Polglase, Castle Hill, New South Wales
Mrs Helen J. Polglase, Castle Hill, New South Wales
Mr Norman Fulton, Smiths Lake, New South Wales
Mrs Dorothy M. Fulton, Smiths Lake, New South Wales
Mr Andrew C. Roberts, Darwin, Northern Territory
Dr Nancy H. Evans, Newport Beach, U.S.A.
R.A.A.M., University of Winnipeg, Canada
R.A.A.H., Honolulu, U.S.A.
Ms Lesley Maynard, Grafton, New South Wales
Mr Donald R. Coles, Engadine, New South Wales
Dr Dietrich Mania, Halle, German Democratic Republic
Dr Shyam K. Pandey, Sagar, India
GERSAR Library, Perthes, France
Miss Margaret Puffett, Cootamundra, New South Wales
Dr V. H. Sonawane, Vadodara, India
Mr Alastair I. McGregor, Neutral Bay, New South Wales
Professor David R. Yesner, Anchorage, U.S.A.
Mr Ian Coates, Dickson, A.C.T.
Mr Andrew England, Torrensville, South Australia
Mr Terry Yumbulul, Nhulunbuy, Northern Territory
Mr Alex Peterson, Heathcote, New South Wales
Mme Fatima-Zahra Alaoui, Rabat, Morocco
Professor Miroslav Kšica, Brno, Czechoslovakia
Mr Andrew K. Lee, Bellevue Hill, New South Wales
Mrs Marilyn F. Stephens, Killarney Heights, New South Wales
Mr Andrew Thorn, Hawthorn, Victoria
Professor Yuan Chang-Rue, Taipei, Taiwan
Professor August Sladek, Freienwill, West Germany
Dr T. G. des Yvelines, France
Ms Paola Talbert, Marrickville, New South Wales
Miss Sylvia Leighton, Manypeaks, Western Australia
Mr A. E. Parsons, Gympie, Queensland
Ms Ann Poeszus, Forster, New South Wales
Ms Claire E. Smith, Booragul, New South Wales
Ms Angela McGowan, South Hobart, Tasmania
Ms Lyn de Berry Hughes, Gladstone, Queensland
Ms Libby Rossmoore, Sarasota, U.S.A.
Dr Lois Tilbrook, Northbridge, Western Australia
Mr Neil Berecry, Glebe, New South Wales
Mr Gordon Herbertson, Cairns, Queensland
Mr Maurice Miller, Ceduna, South Australia
Mrs Pamela M. Russell, Auckland, New Zealand
Mr Norton C. Redsell, Gymea, New South Wales
Dr Andrée Thenot, St Germain-en-Laye, France
Mr Andrew Border, Bardon, Queensland
Mr Christopher F. Haymes, East Brunswick, Victoria
Mr Peter J. Callaghan, Rockhampton, Queensland
Ms Edith Swift, Winnemucca, U.S.A.
Mr Arsen Faradzhev, Moscow, U.S.S.R.
Mrs K. Alexander, Hawker, A.C.T.
Ms Angela Amoroso, Santa Fe, U.S.A.
Ms Marie Potter, Bairnsdale, Victoria
Mrs Y. J. Knight, Armidale, New South Wales
Mr Engkos A. Kosasih, Jakarta Selatan, Indonesia
Professor Claude A. Vaucher, Portland, U.S.A.
Mr Lionel R. Baker, Engadine, New South Wales
Ms Karen E. Saenz, Venice, U.S.A.
University College of the N.T., Darwin, Northern Territory
Mr John Hanel, Como, Western Australia
Mr John H. Caldicott, Hawthorndene, South Australia
Ms Ann M. Robb, Lancefield, Victoria
Ms Christine Schelberger, Vienna, Austria
Mr John R. Austin, Alice Springs, Northern Territory
Mr Kenneth H. Watson, Pymble, New South Wales
Mr Kenneth Mulvaney, Katherine, Northern Territory
Ms Pia Panaretos, Bexley, New South Wales
Ms Jennifer K. Burden, Adelaide, South Australia
Professor Michael Greenhalgh, Australian National Univ., A.C.T.
Dr Kingsley Palmer, Canberra, A.C.T.
Ms Margaret P. Wilson, Kilburn, South Australia
Ms Sachiko Kubota, Suita Osaka, Japan
Professor John B. Campbell, Townsville, Queensland
Dr Yoshinaru Fujioka, Kobe, Japan
Mr Damien R. W. Foster, Hermit Park, Queensland
Ms Maree A. David, Villawood, New South Wales
Ms Isabel Ellender, Rosanna, Victoria
Professor David F. Branagan, Univ. of Sydney, New South Wales
Ms Elanie A. Moore, Idyllwild, U.S.A.
Mrs Kathleen N. James, Rockhampton, Queensland
Mr Edward Mitchell, Mount Morgan, Queensland
Mr Loren A. Shirar, Altadena, U.S.A.
Mr Patrick Somers, Palmerston, Northern Territory
Ms Roslyn Premont, Alice Springs, Northern Territory
Ms Wendy J. Lacoon, Woodroofe, Northern Territory
Dr André Prous, Belo Horizonte, Brazil
Ms Patricia Reis, Richmond, U.S.A.
Ms Linda James, Christchurch, New Zealand
Dr Tilman Lenssen-Erz, Köln, West Germany
Dr Pamela Vandiver, Suitland, U.S.A.
Professor Marcel Otte, Liège, Belgium
U.P. State Archaeological Organisation, Lucknow, India
Mrs Kerry Murray, Mittagong, New South Wales
Ms Bonita Ely, Erskineville, New South Wales
Ms Ann Taylor, Beecroft, New South Wales
Mr Eric Minnaèrt, Paris, France
Mrs Louise Coleborn, Springwood, New South Wales
Ms Julie Drew, Vaucluse, New South Wales
Dr Bonny Guylaine, Loureciennes, France
Mrs Peggy Vessey, Paraburdoo, Western Australia
Petroglyphs Provincial Park, Woodview, Canada
Sydney College of the Arts, Glebe, New South Wales
University of Western Australia Library, Nedlands, W.A.
Mrs Runa N. Johns, Gayndah, Queensland
University of New South Wales Library, Kensington, N.S.W.
Dr Gro Mandt, Bergen, Norway
Dr Kalle Sognnes, Tiller, Norway
Mr Bob Edberg, Pomona, U.S.A.
Dr Eike-Olaf Tillner, Stein, Switzerland
Ms Catherine Roberts, North Fitzroy, Victoria
Mr Bruce Ford, Watson, A.C.T.
Universitetet i Tromso, Tromso, Norway
Mr Guillermo Munoz Castelbanco, Bogota, Colombia
Dr Jean-Pierre Duhard, Biarritz, France

AURA's membership therefore stands at 603, after allowing for losses through death and non-payment of fees. The rapid growth in membership experienced throughout 1988 is continuing.

IFRAO Report

INTERNATIONAL FEDERATION OF ROCK ART ORGANIZATIONS FORMED

The one outcome of the First AURA Congress that may well be of the greatest consequence is the founding of a federation of rock art organisations. On the day following the close of the academic symposia, 3 September 1988, senior representatives of the nine major rock art organisations met in the boardroom of the Beaufort Convention Complex in Darwin to discuss international co-operation. A spontaneous decision at this impromptu summit meeting led to the immediate establishment of the federation. Here are the minutes of the inaugural meeting of the International Federation of Rock Art Organizations:

Present: K. Hedges (Immediate Past President, ARARA), Dr Donald E. Weaver (Vice-president, ARARA), R. Querejazu Lewis (President, SIARB), S.-A. Pager (President, SARARA), R. G. Bednarik (Secretary, AURA), Prof. J. Steinbring (President, RAAM), Prof. B. K. Swartz (President, ACASPP), Dr M. Lorblanchet (President, Groupe de réflexion), Dr S. K. Pandey (India), Prof. Z. F. Chen (People's Republic of China).
Apologies from Prof. E. Anati (President, CAR).

This is an informal meeting of ten delegates from nine rock art organisations (or organisations about to be formed) to discuss future co-operation among the global research community. The matter of forming an international federation is raised, and discussed at some length. Clear benefits are perceived.

(1) The need to maintain a noncentralised structure is emphasised, and there is general agreement that such a body should be structured only to the extent necessary to operate successfully, but should lack a constitution or executive beyond the combined executives of the member organisations, unless the need for any such features or structures is perceived.

(2) It is moved that a name be decided for this federation, and that such an international organisation be considered to have been formed.

(3) Next, it is suggested that the representatives present should report to their respective executive committees at the earliest possible time, and solicit responses, suggestions and requests, to assist in formulating the conditions within which the Federation will operate.

(4) It is asked whether RAR would be willing to act as a dissemination channel until such time as the Federation may produce its own periodical, and the necessary space is committed. It is requested that the results of this meeting be published in the forthcoming issue of RAR.

(5) Bednarik is elected as Convener or caretaker organiser of the Federation.

(6) The discussion then moves to the basic aims of the Federation, beginning with the exchange of publications. Several such exchange programs already exist between some of the bodies represented, and extension of them could proceed along the same lines.

(7) It is suggested that the Federation should consider the establishment of a computerised archive, but it emerges during debate that expansion of existing facilities such as those at UCLA and Centro Camuno would perhaps be preferable. Concern is also expressed that such a new archive could facilitate centralisation, which would be contrary to the policy of the Federation.

(8) The delegates are advised that Indian and Chinese researchers are establishing national rock art organisations in their respective countries.

(9) After considering the options for holding future meetings it is decided that no separate meetings should be held at this stage, but that the Federation should nominate as venues of regular meetings other major international rock art events, and that each Federation meeting be chaired by the host organisation. This would save time and travel costs, while significantly increasing the international attendance of host events.

(10) The need to produce a letterhead is considered next. It is agreed that there should be an initial period during which discretion be exercised in matters of official business, and that feedback from the member organisations be sought at the earliest possible time, to determine the rules applicable to correspondence and other inevitable requirements.

(11) There is discussion on the need to establish the Federation as an agency that can be effective in assisting member organisations in procuring funding for initiatives and projects, for groups or for individual researchers. It is believed that an international federation can approach funding agencies far more effectively than national or regional organisations, and it is envisaged that a major function of the Federation will be to endorse projects supported by member organisations, and to act as a clearing house in areas such as refereeing. The long-term effect of such a system will be that individuals or groups in any country with a national or regional organisation will seek closer involvement at such national/regional level, because it will be only through the endorsement of the affiliated regional bodies that they could expect the support of the Federation.

(12) It is requested that lists of sponsor agencies be made available to member organisations.

Summary: The results of this meeting exceed by far the terms of reference it commenced with, which amounted to an informal airing of views on improving international collaboration, and a discussion of the viability of a federation. Representatives perceived a clear need for a global body which, with no intent upon infringing on the autonomy of each member organisation, could act as a common forum and initiator, and project or represent the interests of member organisations and, ultimately, of individuals. Rock art researchers have not always been accorded the recognition they deserve, and our dependence on other disciplines has often facilitated a bias against our work or approach. The solution lies in closer international co-operation and solidarity, and in strengthening a structure acting as an international agency. While carefully avoiding interference in all internal matters of member organisations, the Federation will enhance their effectiveness and the influence of their elected executives; it will improve the ability of our discipline to compete, for instance in funding; and it will provide a means of focusing collective strength on individual and regional issues of many types. However, as the representatives present at the Darwin meeting need to consult their respective organisations on many details and policy matters, much remains to be solved.

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In the months following this meeting I have been in contact with several further rock art organisations desiring membership with IFRAO, and have been involved in various endeavours related to the establishment of new national organisations in different parts of the world. A number of developments are taking place, and will be reported here as soon as there are concrete results.

Membership with IFRAO is open to any organisation active in the scientific study and/or management of prehistoric rock art that has:

(1) An elected executive, and thus some form of democratic constitution;

(2) Non-profit status;
(3) No discriminatory rules relating to its own membership.

*

One of the first projects of IFRAO will be to co-ordinate discussions concerning the international sharing of archival data (see proposal below, by Dr Maurice P. Lanteigne, Canada). Specialist appraisal has been sought from six experts in Australia, Italy, Uruguay and U.S.A., and readers wishing to contribute to this debate are strongly encouraged to make relevant submissions. Other projects are also being considered, and negotiations are underway regarding a specific research project that can only be conducted with international collaboration and participation.

In December 1988, the executives of the member organisations have made the following decisions:

(1) A proposal to nominate the rock art conference in San Antonio, Texas, in May 1989 as an official IFRAO meeting is not accepted, because there is not adequate time to plan for this event.
(2) Member organisations agree unanimously that the next IFRAO meeting should be held in 1990. We invite suggestions for a suitable site for this event, as well as for the 1991 meeting (AURA will seek nomination of the Second AURA Congress, in Cairns, Australia, as the venue of the 1992 IFRAO Meeting).
(3) Of the nine responses received to a query concerning the Federation's name, seven favour the name International Federation of Rock Art Organizations.

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Executive Member Mario Consens (President of CIARU) suggests that the image of a hand print, one of the most ubiquitous motifs in rock art, be adopted as IFRAO's logotype.

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On behalf of AURA and the Australian community of researchers I thank Federation members for their support concerning the issue of protecting cave art in south-western Tasmania.

*

At the time of writing, the executives of the following organisations have ratified their affiliation with IFRAO:

American Committee to Advance the Study of Petroglyphs and Pictographs (ACASPP)
Australian Rock Art Research Association (AURA)
Centro studi e museo d'arte preistorica (Italy)
Centro de Investigación de Arte Rupestre del Uruguay (CIARU)
Groupe de réflexion sur les méthodes d'étude de l'art pariétal paléolithique (France)
Rock Art Association of Manitoba (RAAM)
Southern African Rock Art Research Association (SARARA)
Sociedad de Investigación del Arte Rupestre de Bolivia (SIARB)
Indian Rock Art Research Association (IRA)

The executives of the following organisations have not met to date to confirm their affiliation:

American Rock Art Research Association (ARARA)
Comité International ICOMOS pour l'art rupestre (CAR)

Four other organisations have indicated that they wish to join IFRAO, and applications from another two or three organisations are expected shortly.

Robert G. Bednarik
IFRAO Convener

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INDIAN ROCK ART RESEARCH ASSOCIATION FORMED

S. K. PANDEY

To fulfil the last wishes of Dr V. S. Wakankar (see Obituary on p. 146, this issue), a meeting was held at Shantiniketan, West Bengal, on 2 December 1988, to form a society for promoting the cause of Indian rock art studies. The potential of Indian rock art as a source material for the study of Indian prehistory has been well recognised, and it has been felt that the ecological disturbances are creating havoc in the preservation of these paintings. To co-ordinate the studies and preserve the art properly, the need for an organisational structure had been felt for some time. Wakankar and I had planned to found a society, but due to his sudden death it could not be realised during his life time.

In August and September 1988, seven Indian delegates attended the First AURA Congress in Darwin, Australia. They held several meetings with AURA leaders and a decision was made in Darwin to form the Indian Rock Art Research Association, and to announce it at Shantiniketan, in the joint session of the Indian Archaeological Society, the Indian Society for Prehistoric and Quaternary Studies, and the Indian History and Culture Society.

In all, thirty-seven delegates attended the meeting which was held at Purvapalli Guest House in Shantiniketan. Veteran scholars including Professor K. D. Bajpai, Professor V. N. Mishra and Dr S. P. Gupta expressed their views on the practicalities of forming the association. It was felt that forming a society is not a difficult task but it is difficult to keep it alive. A committee of seven members (all those who participated in the AURA Congress) was formed and the task of conducting a membership drive was assigned to it. Twenty-four delegates immediately became members of this association by paying Rs 20/- (less than $2.00) each as an annual subscription. It was also decided that the office of the Indian Rock Art Research Association (IRA) shall be at Sagar.

Your co-operation and membership are solicited.

Dr S. K. Pandey
Chairman, IRA

The members of the IRA Committee are:
Dr Giriraj Kumar, Dayalbagh
Dr Sudha Malaiya, Indore
Dr Yashodhar Mathpal, Bhimtal
Dr S. K. Pandey (Chairman), Sagar
Dr Vishwasrao H. Sonawane, Baroda
Dr Rakesh Tewari, Lucknow
G. S. Tyagi, Sagar

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SOVIET UNION

The negotiations of IFRAO Convener R. G. Bednarik with the Soviet authorities have resulted in the announcement that a rock art symposium will be held in Moscow in 1990, where the RARASU (Rock Art Research Association of the Soviet Union) will be formed. Further details to be announced.

JAPAN

The Japan Petrograph Association has been formed in Japan. It produces the newsletter *The Petrograph News.*

PEOPLE'S REPUBLIC OF CHINA

Professor Chen Zhao Fu, IFRAO's principal delegate in China, advises that preparations to found the Chinese Rock Art Research Association are proceeding.

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PROPOSAL FOR AN IFRAO ARCHIVAL DATA SHARING NETWORK

MAURICE P. LANTEIGNE

The recent formation of the International Federation of Rock Art Organizations during the First AURA Congress (Darwin, 1988) has provided an altruistic mechanism for the co-operative sharing of technical expertise and interests in the preservation, documentation and scientific analysis of rock art on an international scale. One of the driving motivations for the creation of such an organisation has been the recognition that the 'individual parts' cannot exert as much influence upon protection policies and granting agencies as can the 'sum of the whole'. It is recognised, and indeed applauded, that the 'bonding agent' of the organisation will be that of 'shared interests', rather than 'constitutional by-laws'.

One of the policies advocated during the founding meeting of IFRAO has been the rejection of creating a centrallsed archival data bank. The rationale behind it is obvious. Such a data bank would inevitably become the responsibility of a single organisation to create, maintain, redistribute and update it, and a financial mechanism would have to be installed for underwriting it which in turn would require a constitutional framework, antipathetical to the bonding agent of 'shared interests'.

At present, there are at least two large archival data bases devoted to collecting rock art documentation source material: the Rock Art Archive at UCLA, and CAR-ICOMOS, with many other organisations devoted to anthropology in general. Since 1983, the Rock Art Association of Manitoba has been developing a computerised information-retrieval data bank on an international scale. To date, 10 000 bibliographical references (15% of which are annotated) have been collected. It is estimated that this collection represents only 1% of the total inventory of published and unpublished papers, site reports and manuscripts. At the present rate of updating it would take RAAM 500 years just to complete a bibliographical inventory of rock art papers current to 1988.

It is assumed that similar provincial and national groups of rock art enthusiasts around the world are compiling their own archival data banks, and could benefit greatly by having access to other sources. Given the current state of telecommunication and computer technology, network interfacing with independent archival data banks is now as easy as dialling the telephone.

PROPOSAL: It is proposed that a discussion be initiated to establish an understanding for the possible exchange of respective archival data banks within the framework of IFRAO.

ADVANTAGES: The larger archival data centres would benefit from such a network by having access to remote regional data. The small data centres would have access to larger sources, yet also have the freedom to custom design their data banks at their own discretion, for their own particular needs, at their own pace. Additionally, no single organisation would have to bear the cost of maintaining, upgrading and redistribution. Rather, all participants would share responsibility, on a 'user-participation' basis.

DISADVANTAGES: Individual publication of bibliographies would effectively cease, since all archival data banks would become the property of all participants. The greatest losers would be those who have devoted years of their own time (and money) into developing their respective data banks. The only effective solution would be to publish all present data collected, prior to network interfacing.

TELECOMMUNICATION METHOD: To achieve efficient archival data bank sharing, several concepts should be considered:

- (a) The cost of upgrading should be the responsibility of the receiver (world community), and not the host (system owner). Therefore the receiver must be able to selectively access (read, write) host data sets, as well as leave mail messages. To date, the RAAM's archival data bank is not designed for selective world (read, write) accessing, but such a format is under consideration.
- (b) Modification of data format should be the responsibility of the receiver, not the host. The cost of modifying all archival data banks to one standard format (though ideal) would be unjustifiable, especially for large data banks. RAAM's data bank was initially designed for publication, and hence includes laserprint typeset control language. But present size is proving to be unmanageable and user-unfriendly. Data set modification is underway, and the format being considered is that of the Royal Anthropological Institute Library (e.g. continent - country - province, with a subject cross-referencing index). Whatever format is currently being used, or being considered, it should be as user-friendly as possible (i.e. the receiver should be able to selectively download pertinent data files without having to download the entire data set system). Yet at the same time the format should not have to infringe upon the needs and resources of the host.
- (c) Pertinent information about accessing data bank procedures, data set file structuring and sizes, operating system specifications, interfacing specification requirements, telecommunication instructions etc. should be provided through the agreed forum, which at present is *Rock Art Research*, journal of the Australian Rock Art Research Association.

Once all major data bases have been accessed and shared, the cost of upgrading respective data banks should decrease accordingly. For this, separate temporary updating files will have to be maintained by respective hosts so that receivers will only have to download the smaller updating files, the overall costs to the receiver being kept to a minimum. Initial downloading cost/time factors to the receiver may be estimated by consulting with respective telecommunication companies. For example, the RAAM data bank consists of 26 files in a sequential data-set of 6000 blocks. The data bank is equivalent to 1500 pages of 60 lines/80 characters per page. Downloading of the data bank through telecommunication lines takes approximately 45 minutes. The cost/time factor will vary according to the optimal peak periods, the interface window initiated by the receiver, as well as distance/satellite route.

HARD COPY METHOD: The cost of hard copy printout of just one copy was over Can$350. The cost of redistribution of the hardcopy by RAAM, including parcel carrying charges, to all presently participating organisations of IFRAO would exceed Can$4500. In contrast, it is estimated that the cost of redistribution by telecommunication networking would be one half to one-fifth that of hard copy distribution.

FLOPPY DISK METHOD: This would probably be the most cost-efficient method of redistribution. It is estimated that the RAAM data bank could be downloaded and distributed for less than Can$750, or less than one-seventh of the cost of the hard copy method. At present, RAAM has only Mag-tape format (10 times the cost of floppy disk), though downloading capabilities to disk format is under consideration.

However, there are problems with the two latter methods. Cost of upgrading becomes the responsibility of the host, not of the receiver. Larger archival centres become penalised for being larger, which is not the intent of the proposal. If the reciprocal exchange method is used (such as that being considered for newsletters), the host may not have the financial or logistic resources to complete the reciprocation at a particular point in time, which may inadvertently create discord.

It is not important at this stage of discussion to opt for only one method. All options should be considered as viable alternatives, custom designed for specific links in the network chain. What is important is an understanding in principle of the concept of free-information exchange, along the same lines as envisaged for publication exchange among IFRAO member organisations. The method should be flexible enough to accommodate the needs of individual participants, yet present no undue financial/logistic obligations upon host-participants who may not have the resources to accommodate receiver-participant needs.

Clearly, the proposal has many technical problems which have not been considered. Also, loss of prestige and owner privileges may pose the greatest stumbling block to any international network-interfacing agreement. However, one should consider the enormous potential in having such a network system. Indeed, it may assist in enhancing the professional stature of the discipline of rock art studies.

Dr Maurice P. Lanteigne
Chairman, Research Committee
Rock Art Association of Manitoba
Winnipeg, Canada

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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:

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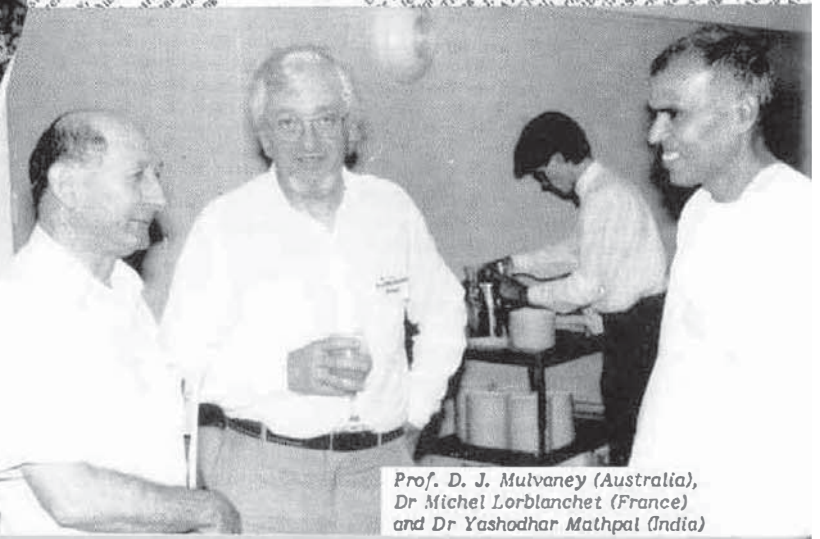
Three Spanish delegates in the congress exhibition: Andrés Diez, Dr Margarita Bru and Prof. Antonio Beltrán

Participants of the postcongress field trip to Kimberley and Pilbara reach the first of the Spear Hill petroglyph sites

Dr Agueda Vilhena Vialou, Prof. Denis Vialou (both France), Percy Trezise (Australia), Ariela Fradkin Anati and Prof. Emmanuel Anati (both Italy)

Dr Paul G. Bahn (England), in favourite pose, with Josephine McDonald, Natalie Franklin (both Australia) and María Podestá (Argentina)



Prof. D. J. Mulvaney (Australia), Dr Michel Lorblanchet (France) and Dr Yashodhar Mathpal (India)