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ROCK ART RESEARCH

Volume 8, Number 2 NOVEMBER 1991

First Palaeolithic art discovered in China

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

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

KEYWORDS: Notational systems - Portable art - SEM - Upper Palaeolithic - Europe

MICROSCOPIC AND STATISTICAL CRITERIA FOR THE IDENTIFICATION OF PREHISTORIC SYSTEMS OF NOTATION

Francesco d'Errico

Abstract. The criteria adopted in the past for identifying prehistoric systems of notation have never been clearly described or demonstrated experimentally. The analysis (by SEM and transmitted light microscope onto transparent resin replicas) of a series of experimental notches reveals that criteria exist for differentiating between the use of a single cutting edge and the use of different cutting edges on a given object. A computer system for profile measurements and evaluation is used to measure the angles formed by the sides of the experimental and archaeological notches. The variability of these angles demonstrates that the use of a single cutting edge or of several different cutting edges can be discriminated statistically. This paper summarises these criteria and presents the results of two blind tests which confirm the validity of the microscopic and statistical criteria. The examination of a Solutrean piece bearing a series of notches characterised by numerous and distinctive changes of the cutting edge raises the question of Palaeolithic systems of notation.

Introduction

From the end of the 1950s, several authors (Absolon 1957; Marshack 1964, 1970, 1972a, b, 1988, 1989; Bouchud 1968; Thévenin 1972; Couraud 1980; Couraud and Lorblanchet 1986; Campbell 1985; Dalmeri 1985; Larichev 1986 etc.) proposed or accepted the hypothesis that some series of engraved lines, incisions, notches and micro-cups visible on stone and bone objects dating from the Palaeolithic and Mesolithic could be interpreted as systems of notation (lunar calendars, 'hunting tallies' etc.).

Others suggested that these sequences might reflect a mathematical conception of the cosmos (Frolov 1965, 1970, 1978, 1979), the knowledge of a numbering or a calculating system (Tratman 1976; Heinzelin 1979; Gob 1983; Dewez 1974, 1975, 1981, 1987), a rhythmical support for traditional recitation (Leroi-Gourhan 1964; Corchon Rodriguez 1986) or musical instruments (Müller-Karpe 1966; Otte et al. 1982; Huyge 1990).

Recent comparative microscopic analyses (involving examination by different techniques and types of microscopes) of experimental and archaeological engravings (d'Errico 1987, 1988a, 1989a) have demonstrated the existence of certain clues enabling us - among other things - to demonstrate:

1) The direction of movement of the point.
2) Whether the lines were engraved by the same tool.
3) The order in which lines were made.
4) The time required for engraving the lines.

Using the methods described in d'Errico (1987, 1988a, b, c, 1989a), a recent analytical study of French Epipalaeolithic abstract art has demonstrated that these objects cannot be interpreted as lunar calendars or systems of notation, as several authors have claimed. Moreover, a re-examination of the bibliography on this subject (d'Errico 1989a, b; see also d'Errico 1991) has shown that the criteria on which such examinations are based have never been described or validated experimentally. In the latest work by probably the best known researcher on the subject of notational systems (Marshack in press), the identification of a probable example of a system of notation must be regarded as hypothetical. In effect no analytical criteria based on experimental replication are provided to support that proposed identification, or the verification of the tools used to engrave the archaeological specimen.

Our research on Azilian art covered a relatively short prehistoric period. Several objects dated to the Upper Palaeolithic have also been interpreted in the past as systems of notation. Although it is possible that such systems might have existed during this long period, their existence is far from proven.

The objects described as bearing notation include those with sequences of single-stroke lines (made by a unidirectional movement of a point), notches (produced by a to and fro movement of a cutting edge) and micro-cups (produced by the rotation of a point), and are of stone, bone, ivory, amber etc. Objects with series of notches are the most numerous.

Investigations were carried out in order to determine experimental criteria for the study of such notches. The purpose of this paper is to summarise these criteria and to demonstrate their usefulness as means of studying archaeological notched bone objects.

Microscopic criteria

Microscopic analysis (by SEM and transmitted light microscope, on transparent resin replicas, Rose 1983;

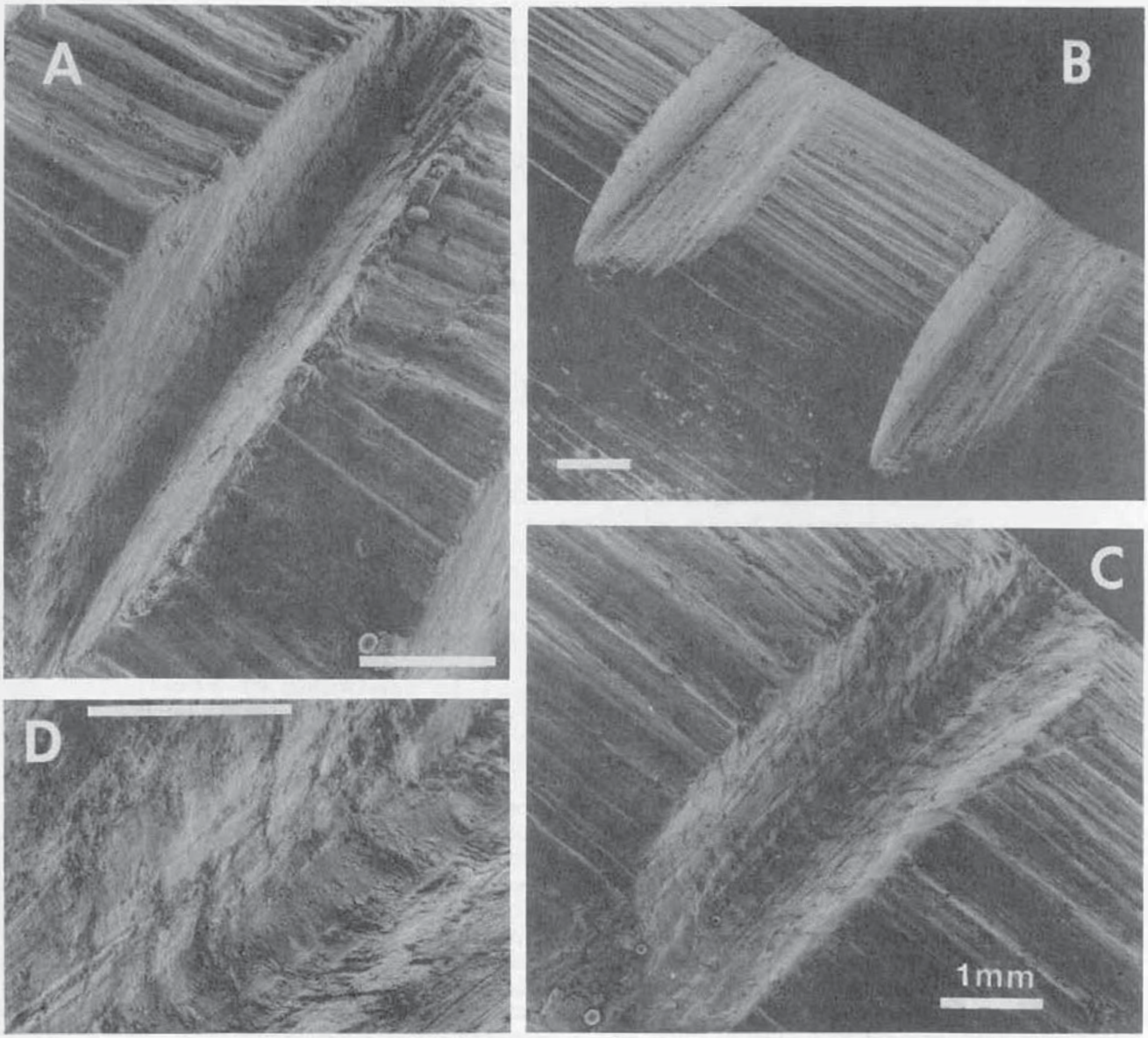

Figure 1. A: Notch made on bone by an unretouched and unworn stone implement's cutting edge; B: Notches made by the same, retouched cutting edge; C, D: Notch made by a worn unretouched cutting edge. Note the chiselled aspect of the bottom of the notch (D).

Bromage 1985; d'Errico 1988b, 1989a, 1991, in press a, b) of several series of experimental notches has made it possible to ascertain the engraver's way of working.

It is possible to differentiate notches made by unretouched cutting edges from those produced by retouched ones. The former (Fig. 1A) have flat sides, while the latter (Fig. 1B) have one flat side, and one 'terraced' side which corresponds to the retouched side of the cutting tool.

A sharp, unretouched cutting edge produces a notch with a narrow bottom (Fig. 1A). If the edge is worn, the bottom of the notch will be wide and may show a 'chiselled' aspect (Fig. 1C, D). Using the same unretouched cutting edge to produce several notches usually became difficult after 20 notches and very difficult after 30. We have selected criteria which make it possible to determine whether a series of notches was fashioned by a single cutting edge or by several different cutting edges. The use of a single cutting edge can be observed by

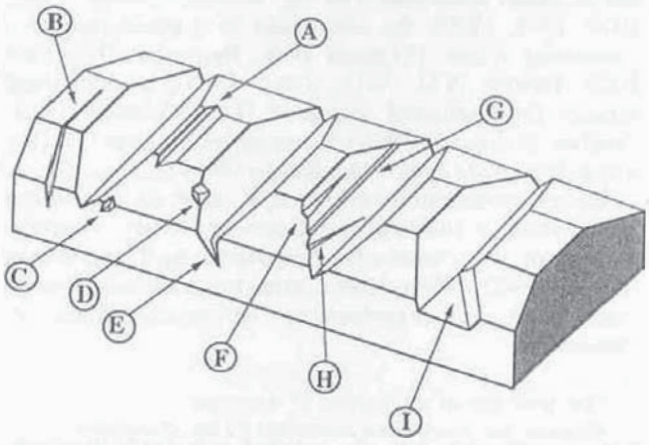

Figure 2. Microscopic criteria for identifying the use of the same cutting edge: external incision (A) and indentations (B, C) outside the notch; incision (E) and indentation (D) at the end of the notch; partial steps either parallel (F) or oblique (H) to the bottom; continuous step parallel to the bottom (G); change in the orientation of the notch side (I).

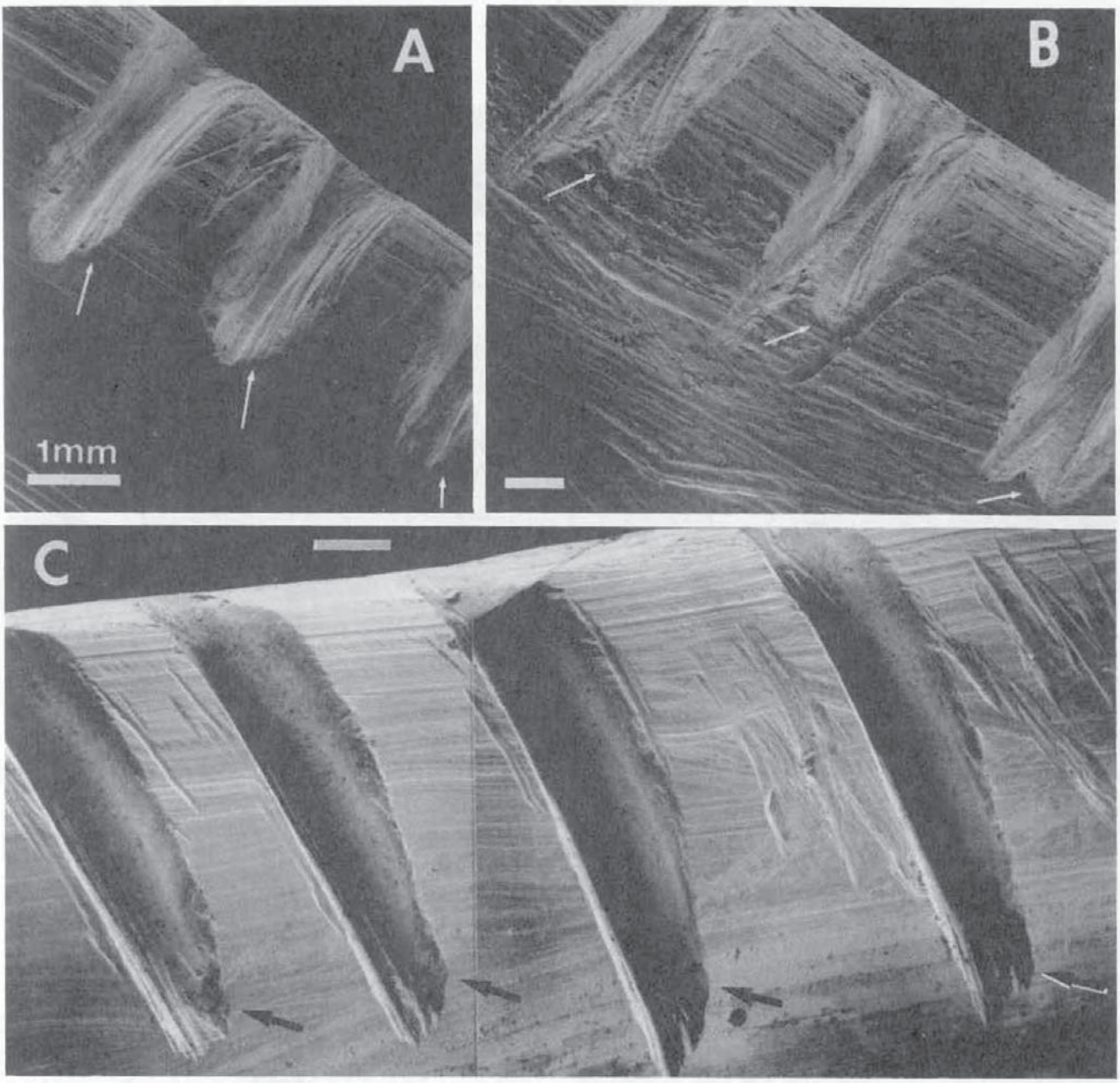

Figure 3. Experimental notches suggesting criteria for recognising the use of a single cutting edge. A: partial steps; B: large partial forked step; C: indentations.

examining the morphology of the micro-steps (Figs 1B; 2F, H; 3A) and the orientation of the sides of the notches (Fig. 2I). Incisions and indentations which occur at the ends (Figs 2D, E; 3C; 4) and outside of the notch (Fig. 2A, C) also indicate the use of only one cutting edge.

In order to understand the value of these criteria it is necessary to explain their formation and range of morphological variability. Each notch is the result of a to and fro movement of the cutting edge. This movement delimits three zones on the edge (Fig. 5): a central zone (A1-A4) which passes over the entire length of the notch at every stroke, and two peripheral zones (B1-B6) which enter the notch on their respective sides, but without passing over its full length. Figure 6 illustrates the manner in which different central and peripheral morphologies of the cutting edge affect the different forms of the notches. The peripheral zones of the cutting edge are thus active mainly in fashioning the ends of the notches. Owing to the possibility of changes in the movement of the cutting edge, the central zone is more liable than the peripheral zones to produce the same morphology in every notch.

In order to identify the use of different cutting edges in the production of a series of notches it is thus more important to take into consideration the lack of microscopic features located in the centre of the notches, than the lack of those towards their ends. Experimental notches show that the lack or the total change of microscopic features may correspond to a change of the cutting edge.

Morphological and statistical criteria

Experimental notches and resin replicas of archaeological notched objects were analysed using a computer system for profile measurements and evaluation (Conturograph and Conturoscop C4P MAHR, Göttingen, Germany). The Conturograph records a surface profile with a stylus arm connected to a drive unit. The Conturoscop C4P compensates automatically for measuring errors resulting from the radius of the stylus tip.

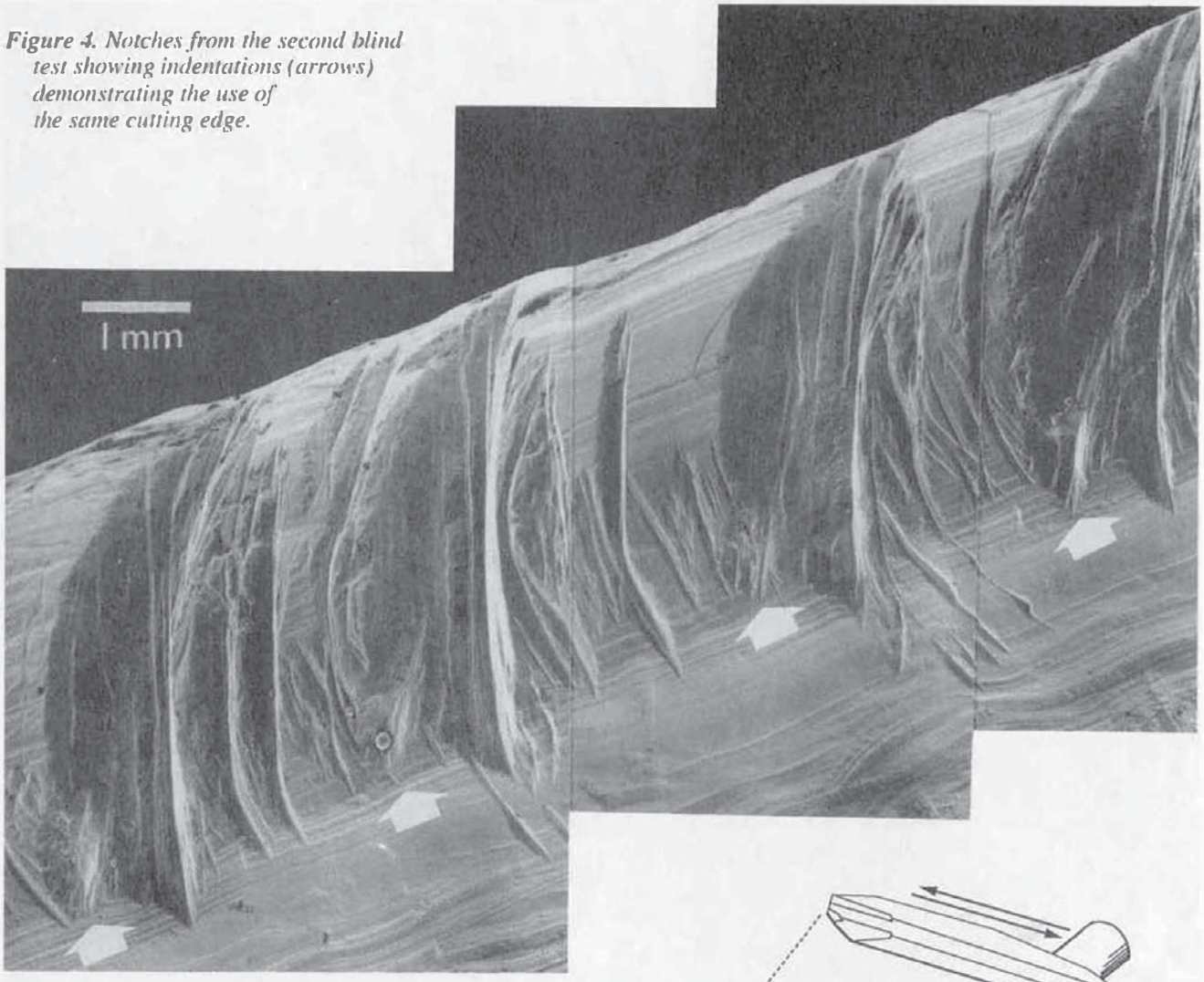

Figure 4. Notches from the second blind test showing indentations (arrows) demonstrating the use of the same cutting edge.

This apparatus can insert best-fit lines and best-fit circles onto profiles and compute geometrical elements such as points of intersections of best-fit lines, angles, radii, distances etc. This equipment was used to measure the angle formed by the best-fit lines adapted to the sides of the experimental series of notches. The statistical analysis of the measured values (Table 1) shows that a series of notches made by a single cutting edge can be statistically distinguished from those made by different cutting edges. In the former case, the values of the standard deviations are systematically lower than 10% (and in half the cases lower than 5%) of the mean value of angles.

Graphical representation of the values (Fig. 7A) shows that in each series of notches made by a single cutting edge, the difference between angles of adjacent notches is never higher than 17° and generally lower than 10°. Notches made by the same cutting edge thus show a low variability in the angle values. On the other hand, if the cutting edge is changed for each notch (Fig. 7B, C), the difference between the largest and smallest angles falls between 27° and 60.3°. The variation between angles of two nearby notches may reach 55°. The standard deviation is about 20% of the mean value. This indicates that, if the cutting edges are changed for each notch, the angle variability will be high.

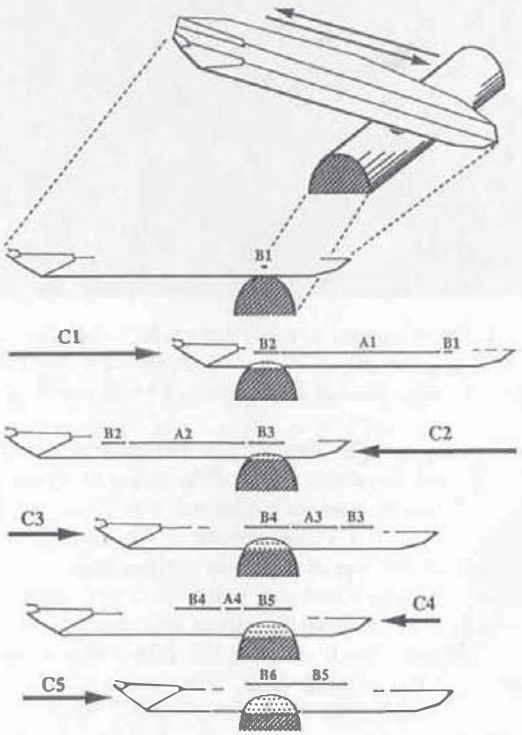

Figure 5. Each notch is the result of a to and fro movement of the cutting edge (C1-C5). This movement delimits three zones on the edge: a central zone (A) which goes across the whole notch at every stroke and two peripheral zones (B) which enter the notch without going entirely across it.

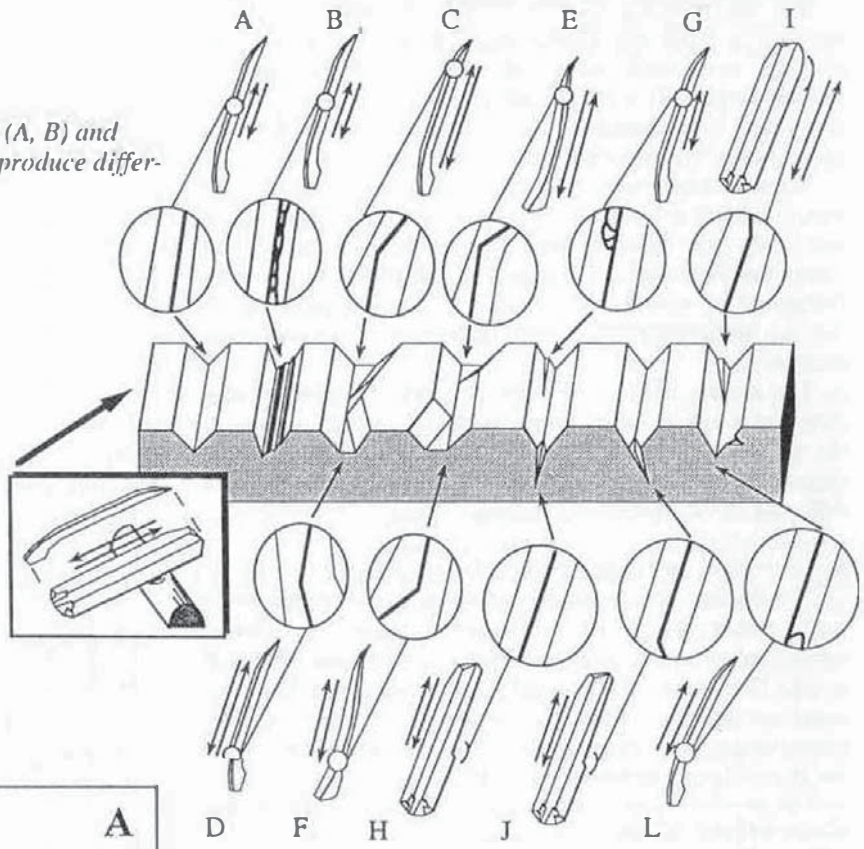

Figure 6. Different morphologies of central (A, B) and peripheral (C-L) zones of a cutting edge produce different kinds of notches.

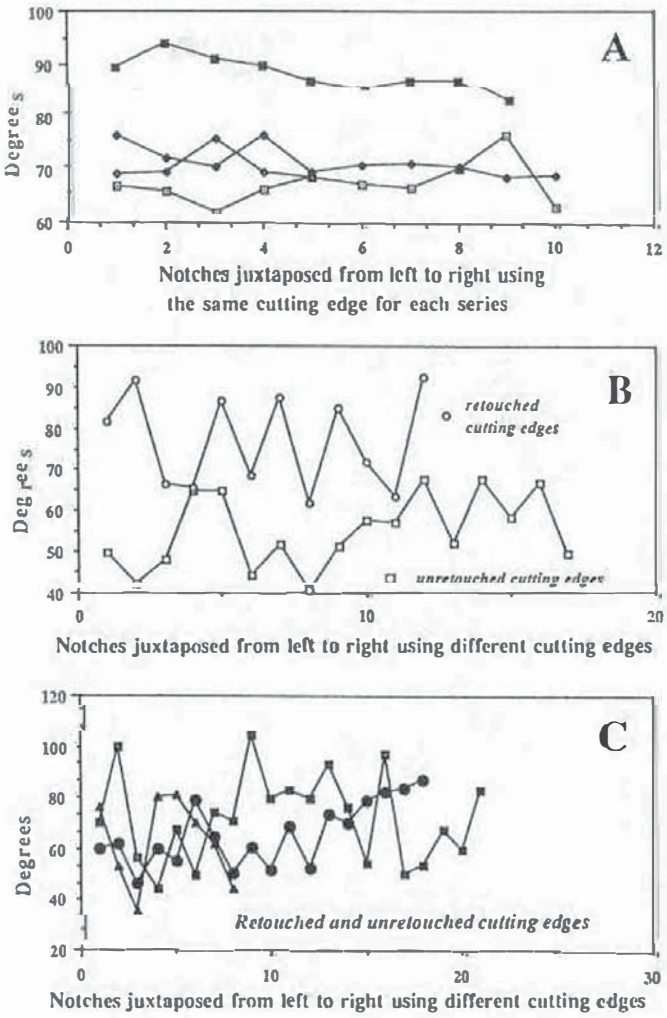

Figure 7. Variations of angles in experimental series made by using the same cutting edge (A) or by changing it (B, C) for each notch.

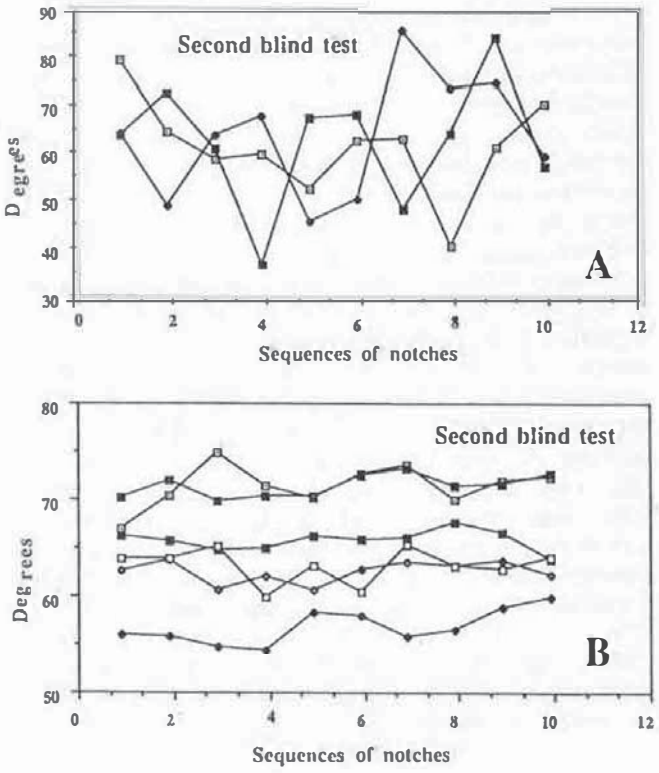

Figure 8. Variations of angles from the second blind test. Through observing the different variation range of the values, series made by the same cutting edge (B) can be clearly distinguished from those made by changing the cutting edge for each notch (A).

Fist blind test (Experimenter: F. Sellet)

The statistical and microscopic criteria cited above were tested in a blind test. In this experiment, sets of series of notches were made either by: (i) a single unretouched cutting edge; (ii) a single retouched edge; (iii) a single retouched edge sharpened during work; or (iv) a different cutting edge for every notch or group of notches.

These notches were then analysed microscopically and statistically (Table 1). Out of a set of ten series used in this test, only one failed to meet the criteria. However, this series was made up of a limited number (seven) of notches fashioned by similar microblades; moreover, three of the notches were too shallow to be reliably measured with any accuracy.

The results of the test were positive. The use of four different ways of work, however, increased the values of the standard deviation for the series fashioned by a single cutting edge. It was thus difficult to distinguish between different work on statistical grounds alone.

Second blind test (Experimenter: P. Paillet)

The thirteen sets of notches used in this blind test were made either by (i) a single, retouched or unretouched cutting edge; or by (ii) a different cutting edge for each notch. The values of the standard deviations (Table 1) are similar to those obtained for the experimental series, thus permitting a clear distinction between the notches made by the two different methods (Fig. 8).

The results of this second test (13 correct answers for 13 series) prove that the statistical criteria alone can suffice for differentiating the two possible working methods.

Archaeological results and future prospects

At present we are applying these analytical criteria to European and Middle Eastern Palaeolithic bone objects with series of notches. This investigation has allowed us to identify archaeological specimens with notches made by a single cutting edge (Fig. 9A) as well as pieces with notches made by several different tools. A Solutrean rhinoceros rib from the site of Solutré (Solutré Museum) clearly shows a series of 47 notches made by several different cutting edges (Table 1, Solutrean rib 'B'). Made with unretouched and unworn cutting edges, the notches on this object are far more numerous than what is possible to produce experimentally with a single cutting edge. Both comparison of the profiles and microscopic analysis reveal changes of cutting edge. Variations of more than 30° are sometimes visible between notches. The difference between the largest and smallest angles is 46.37°. Such large variations were never observed in the experimental series made by a single cutting edge. They are, however, typical of the experimental series made by changing the cutting edge for each notch. The value of the standard deviation (11.23) is far greater than 10% of the mean value. It is the same as that obtained by changing the cutting edge for each notch. All these considerations suggest that different cutting edges were used to fashion the notches on this rib.

Discussion and conclusions

Blind tests have confirmed that microscopic and statistical criteria can be used to identify series in which each notch was made by a different cutting edge. In a large series of notches fashioned by the same cutting edge it is possible that the tool may have been resharpened during the work, and the morphology of the edge be changed. Although more difficult to identify, this behaviour has been detected in the first blind test and may possibly be identifiable on archaeological specimens.

In general, flakes or blades present only two or three edges suitable for fashioning a notch. Consequently, numerous changes of the cutting edge will necessarily correspond to changes of tools. Series of notches fashioned by the two different methods are indiscernible to the naked eye. Moreover, the making of a single notch on bone is enough to damage a flint edge, even to the point of rendering it unsuitable for cutting soft material. Changing

	Series of notches	Same cutting edge	Different cutting edges	Retouched cutting edge	Unretouched cutting edge	Number of notches	Largest angle "A"	Smallest angle "a"	Difference (A - a)	Mean value	Standard deviation	Variation coefficient	Correct answers *
Experimental series	1	•		•		10	76,48	62,40	14,08	67,50	3,88	5,76	
	2	•		•		10	76,47	68,80	7,67	71,64	2,75	3,84	
	3	•				12	59,37	51,65	7,72	55,45	2,70	4,87	
	4	•			•	19	75,20	58,60	16,60	66,09	4,88	7,39	
	5	•			•	12	70,58	60,97	9,61	63,54	2,61	4,12	
	6	•		•	•	9	93,80	82,30	11,50	87,96	3,39	3,86	
	7	•		•		5	75,82	68,83	6,99	70,69	2,88	4,09	
	8	•		•		22	86,40	70,12	16,28	76,24	4,36	5,72	
	9	•			•	13	75,65	66,18	9,47	71,91	2,83	3,94	
	10	•			•	13	81,82	67,65	14,17	74,16	4,40	5,94	
	11	•			•	5	87,35	77,42	9,93	81,11	3,96	4,89	
	12	•			•	12	73,13	57,50	15,63	61,47	4,05	6,60	
	13	•		•		15	78,73	64,60	14,13	71,67	4,80	6,71	
	14	•			•	7	55,65	42,58	13,07	48,56	4,31	8,88	
	15	•			•	15	64,82	57,52	7,30	60,17	2,18	3,63	
	16	•		•		9	101,28	85,60	15,68	91,79	4,81	5,25	
	17	•		•		19	86,17	70,50	15,67	77,65	4,46	5,76	
	18	•			•	10	67,22	52,87	14,35	58,20	3,66	6,30	
	19	•			•	14	100,8	81,68	19,12	85,91	4,68	5,45	
	20		•	•	•	18	87,2	46,6	40,6	65,87	12,54	19,03	
	21		•	•	•	8	81,1	35,5	45,6	62,68	17,04	27,18	
	22		•		•	17	67,6	40,6	27	54,96	9,03	16,43	
	23		•	•		12	92,6	61,9	30,7	76,87	11,67	15,18	
	24		•	•	•	21	104,7	44,4	60,3	72,10	17,41	24,14	
First blind test	1	•			•	19	64,8	46,38	18,41	55,57	5,97	10,74	•
	2	•		•		8	101,7	81,28	20,42	92,92	5,95	6,4	•
	3	•			•	7	62,9	46,92	16,03	55,12	6,2	11,24	
	4		•		•	16	71,9	48,33	23,64	60,04	7,92	13,19	•
	5		•		•	17	76,8	37,15	39,73	53,62	11	20,51	•
	6	•			•	10	76,7	59,28	17,5	65,65	6,71	10,22	•
	7	•				11	84,6	69,57	15,03	78,98	4,97	6,29	•
	8		•	•		11	91,2	66,75	24,45	79,95	7,04	8,80	•
	9	•				9	89,2	79,15	10,07	84,41	3,39	4,01	•
	10	•				30	74,4	50,15	24,32	59,32	6,07	10,23	•
Second blind test	1		•	•	•	10	77,5	45,8	31,7	61,30	10,10	16,48	•
	2	•			•	10	75,2	67,3	7,9	71,80	2,20	3,07	•
	3	•			•	10	60,4	54,9	5,5	57,27	1,81	3,16	•
	4	•		•		10	73,8	58,9	14,9	65,63	4,55	6,94	•
	5	•		•		10	73,5	70,1	3,4	71,76	1,18	1,65	•
	6		•		•	10	79,9	41,6	38,3	62,05	10,09	16,26	•
	7		•	•	•	10	85,9	46,4	39,5	64,05	12,54	19,59	•
	8		•	•	•	10	84,7	37,4	47,3	63,01	12,94	20,54	•
	9	•			•	10	64,1	61	3,1	62,93	1,12	1,79	•
	10	•			•	10	68,1	64,2	3,9	66,17	1,07	1,62	•
	11	•		•		10	53,9	41,1	12,8	45,75	4,15	9,08	•
	12	•			•	10	65,8	60,3	5,5	63,58	1,77	2,78	•
	13		•	•	•	10	71,6	52,6	19,0	63,99	6,62	10,35	•
Upper Pal. objects	Solutrean rib "A"	•			•	6	94,8	74	20,8	85,43	7,49	8,76	
	Solutrean rib "B"		•	•	•	47	98,6	52,2	46,37	71,11	11,83	39,17	
	Solut. needle 1	•			•	17	59,9	42	17,9	52,18	4,34	8,31	
	" " 2	•			•	22	60,7	33,5	27,2	46,78	6,48	13,85	
	" " 3	•			•	20	69,3	55,2	14,1	63,31	3,75	5,92	
	" " 4	•			•	23	65,4	45,7	19,7	53,31	6,04	11,32	
	" " 5	•			•	23	58,1	32,6	25,5	46,36	5,76	12,42	
	" " 6	•			•	18	60,2	52,1	8,1	55,71	2,28	4,09	
	" " 7	•			•	19	60,1	39,4	20,7	48,40	6,53	13,49	

***Table 1.** Metrical and statistical results of measurements taken from experimental, archaeological and blind test series of notches.*

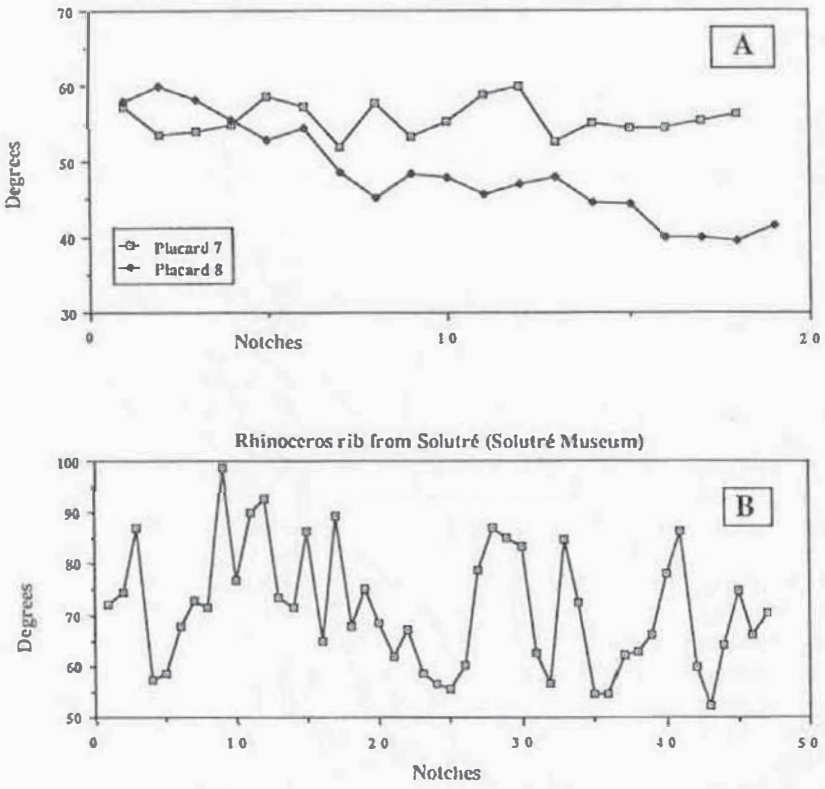

Figure 9. Variations of angles on: (A) a Solutrean notched needle with two series of notches made by the same cutting edge (Grotte du Placard, Institut de Paléontologie Humaine, Paris); and (B) on a Solutrean rhinoceros' rib with notches made by different cutting edges, from Solutré (Solutré Museum).

the cutting tool for every notch involves extra time. As such behaviour is uneconomical it cannot be accounted for by decorative purposes alone. It is more reasonable, therefore, to assume that the use of a different tool might be the result of a certain lapse of time between the making of the different notches. In this case, the engraver's intention was the addition of a new element to a pre-existing series and not the overall result of the action. The result of this behaviour cannot be interpreted as simply decorative. Unfortunately, very little information is presently available on determining just how much time elapsed between the cutting of one notch and that of another (Bromage 1984; d'Errico in press b, c). Such series of marks can tentatively be termed 'notations'. However, it is not the intention to propose in this paper interpretations of such behaviour, but merely to demonstrate that it is possible to distinguish series of notches made by different cutting edges from those produced by a single tool, and that such series can be shown to exist on European Upper Palaeolithic objects.

The existence of other types of notation (*sensu* Davis 1986) during the Upper Palaeolithic cannot be excluded *a priori*. It is, however, difficult for the moment to determine valid criteria for distinguishing 'notations' made by the same cutting edge from other kinds of expression (schematisations, abstractions, decorations etc.).

Acknowledgments

This research project was supported by a NATO Science Program grant. I wish to thank F. Sellet and P. Paillet for participating in the blind tests. I am also grateful to MAHR Mesure (France), ROLTRA (Italy), the Kodak Pathé Foundation and Bayer Dental France for their technical support. I am grateful to Thomas Kessler for revising the English text.

The initial results of this research were presented at Antibes (France), in October 1990 ('*25 ans d'études technologiques en préhistoire*') and subsequently at Liège (Belgium), in December 1990 ('*Les gestes retrouvés*').

*

COMMENT

By ROBERT G. BEDNARIK

Dr d'Errico's research is the logical continuation of Alexander Marshack's pioneer work: the systematic testing of innovative 'Theoretical concepts ... new analytic methods [and] modes of enquiry' (Marshack 1985). In several recent papers, d'Errico has shown that Marshack's basic premises concerning the microscopic and internal analysis of engraved marks are correct: their direction, order of sequence, and information about the tools involved in their manufacture are all recoverable under favourable conditions. In the present paper, one of his technically most brilliant, d'Errico goes one step further and demonstrates that marks of possibly notational content, i.e. series or sets of marks made by several different tools, should be identifiable on some objects.

The main difference between Marshack's and d'Errico's work is that the former researcher, while acquiring a vast store of knowledge about mark-making behaviour, 'symbolic activities', the techniques of analysing their possible traces, and cognitive aspects of archaeology, conducted little replicative work to underpin the theoretical constructs he based largely on close familiarity with the material. D'Errico, on the other hand, has initiated extensive experimental programs in mark production and related fields. Others, in discussing their work, have emphasised differences of opinion between these two outstanding scholars, which I think detracts from more important aspects: the parallels that are in fact emerging in the results of the two different approaches to the same set of problems.

D'Errico's present work is typical of his scientific approach: an example of solid, well documented and generally convincing research. The samples in both blind tests appear to be randomly prepared, in the sense that there was not a set number of each of the variables. This safeguards against the tendency of an analyst to guess the more difficult attributes from the sample remaining after he has confidently identified the part that permitted easy identification.

D'Errico mentions again that even 'the making of a single notch on bone is enough to damage a flint edge'. It cannot be emphasised enough that flint-like cherts are poorly suited for precision work on hard surfaces. Pressure always detaches microspalls, rendering the working edge or point ineffective. This, of course, is the principal difficulty with Marshack's early notational interpretations: points, in particular, are constantly modified in response to pressure flaking. I have found, for instance, that it is quite impossible to drill unweathered ostrich eggshell economi-

cally with a pointed tool of any type that consists of cryptocrystalline or even microcrystalline sedimentary silica. The materials that are most effective and experience the least wear are coarse-grained, well-cemented quartzites and quartz. With them I have drilled through ostrich eggshell in 70-90 seconds. In Kumar's replication experiments, heavily weathered Pleistocene eggshell was perforated with Mesolithic bladelets, which took 10 to 12 minutes (Kumar et al. 1990: 36; G. Kumar, pers. comm.).

The data presented here by d'Errico are so persuasive and obviously important, that it is necessary to caution against over-confidence, or their applications by over-enthusiastic but inexperienced researchers. I think d'Errico will agree that work of this type should not be based on 'textbook instructions', that it requires considerable experience with the materials and techniques involved. As usual, Marshack's warning against 'technology-oriented' research (Marshack 1986: 68) is relevant. It is only too easy to become absorbed by the beauty of one's technological means.

In my own microscopic or replicative work during the past decade, with markings on rock, stone, bone, ivory and eggshell, I have also found many of Marshack's basic premises confirmed. However, my work does not address the question of notations, it involves the more productive pursuit of determining 'mark production strategies', particularly in noniconic cave petroglyphs (finger flutings and tool marks). Here, the aim is to establish relative sequence, direction of execution, repeated use of the same tool, and similar attributes relating to the production of the art, and this is done by field microscopy, detailed recording, identification of engraving tool material, 'finger-printing' of marks to document repeated application of the same tool at the same time, and replication work (e.g. Bednarik 1984, 1986a, 1987, 1991, in press).

Figure 1 illustrates this type of work with a practical example. A cluster of tool marks on a wall in a limestone cave is analysed, several specific tool applications are identified, as is the relative sequence of all the marks present. The panel is 1.4-1.8 m above a rock ledge that was never covered by sediment, so the marks seem to have been made at adult eye level. They were executed when the cutaneous carbonate speleothem deposited on the wall was quite soft, but the precipitate stabilised before the marks lost any of their detail and became fully hardened. Thus even the faintest striations remain visible on many of the marks, which bear very distinctive 'signatures' of parallel striations. Where they are well enough preserved, repeated applications of the same tool can be identified if the tool was not rotated or the direction of the application not changed. Marks are often also deep enough to provide precise information about the contour of the tool point (at right angle to direction of movement), which helps in identifying markings of a single 'set'.

In this particular case, replication suggested beyond reasonable doubt that all marks were made with Tertiary limestone clasts (cf. Bednarik 1986a, 1987), while another series of marks, just 2 m away, had been executed with a microcrystalline material, presumably the locally occurring sedimentary silica. A number of the marks were too corroded to permit secure identification but two cross-sections of tool points were determined, and five 'striation prints' were recognised in the panel depicted in Figure 1. This does not necessarily mean that there were five different tools involved: all marks could still relate to a single tool. But the marks made by consecutive applications without altering the grip are clearly identifiable, and shown as Tools 1-5.

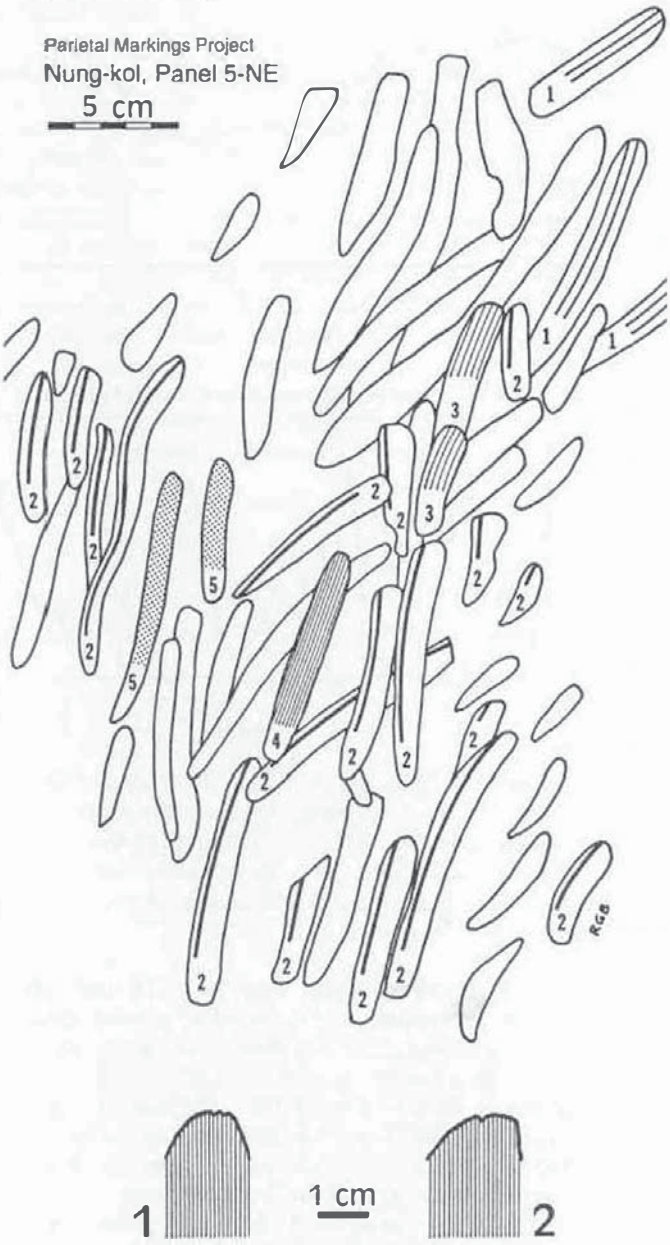

Figure 1. Internal analysis of a small panel of tool markings on formerly soft wall deposit in Nung-kol Cave, South Australia. In this example, the art production sequence has been reconstructed. Where striation patterns are preserved, five tool points have been identified, numbered 1-5. All marks were executed from top to bottom, using clasts of aeolian limestone. The cross-sections of the tips of two tools were determined, and are shown below (tool No. 1 and tool No. 2).

D'Errico has confirmed another of my observations elsewhere (1988c): that the direction in which finger flutings were made can be established from the direction in which the tiny transverse 'tear marks' in the compacted, soft medium are raised. He has observed corresponding, considerably smaller transverse 'tear marks' in experimental engraved grooves on limestone, and believes to have identified them on archaeological limestone pebbles, where they indicate in which direction the burin was moved. In the case of finger markings, these details are much better preserved and sometimes visible to the

unaided eye, although a magnifying glass is useful in their study (Bednarik 1986b).

But to return to the question of notations, which is considerably more problematic than my own work: in the final analysis, it cannot be conclusively resolved by d'Errico's present criteria. Notations can be made in a single sitting, or they can be cumulative, the result of reuse over a long time. The former type cannot be identified by determining the number of tools involved in their production (there is nothing to prevent an artisan or a whole group of people from using several tools or working edges to produce a set of marks in one sitting, nor is the use of several tools evidence for notational intent), and for the latter, the question of different tools is not relevant (if the marks were made on different occasions, identification of the tools involved does little to resolve the notational status). The linchpin of d'Errico's argument is that the use of a different tool for every mark in a single set should be taken as evidence that the marks refer to temporally discrete events. It would help to have some additional, independent corroboration of this postulate. This brings us back to Marshack's approach of comparing degrees of use wear between different marks, which would provide a means of sequencing and recognising cumulative sets that are *likely* to be notational. It is here that d'Errico's future efforts might be most profitably focused.

A few minor points: d'Errico does not mention details which would be of interest to those of us doing similar work: for instance, was the bone he used dry or green? Was the tool material European flint? How would the radius of the bone surface curvature or the compositional bone structure affect his experimental notches? What other variables might affect them? What happens with the detached micro-spalls during a cutting operation, or with any sand particles that are caught between the cutting edge and the bone? How do we account for the 'oblique partial steps' he mentions (Fig. 2H)?

The *anthropic* marks described by Huyge (1990) were never proposed to be notational, and it would make no difference to Huyge's tentative interpretation how many tools were used in their manufacture, or what their cutting edges may have looked like. One also notes from Table 1 that for three of the series in the first blind test (a total of 50 notches) it was not determined whether retouched or unretouched edges were involved.

D'Errico considers that Marshack's identification of the Taï specimen is 'hypothetical', which indeed it is. But by the same token, this also applies to his own determinations of marks on archaeological specimens. The fact that *most* of the results in the blind tests were positive does not establish identification attempts of Palaeolithic marks as proven facts. Nor does the apparent similarity between experimental and archaeological marks, i.e. a 'best fit' interpretation, conclusively prove a postulate as factual. These propositions, too, are hypotheses, and in the strictest epistemological sense they would remain so even if we conducted a hundred blind tests without a single error.

Finally, it needs to be emphasised that there are enormous variations in the preservation state of archaeological specimens, and most surfaces of bone, ivory, ostrich eggshell, antler, limestone etc. of the Pleistocene are so corroded that they would probably not provide the type of microscopic detail required by d'Errico's methods.

Robert G. Bednarik
RAR Editor

REPLY

By FRANCESCO D'ERRICO

First of all, I wish to thank R. G. Bednarik for his stimulating comments. His reflection concerning the different approaches taken by A. Marshack and by myself is 'historic', in that it puts an end to a debate which began three years ago.

Intuition and scientific research can indeed aid one another, provided that we recognise the difference between that which each can contribute to the problem at hand. Each has its merits. Nevertheless, the study of prehistoric art must demonstrate solid analytical methods and must produce verifiable results in order to improve its reputation among other disciplines that deal with the physical and cultural evolution of prehistoric populations.

Bednarik correctly states that notations can be made in a single sitting, or can be the result of reuse over a long time. The problem, however, is more complex in reality. A set of marks made by the same tool in one sitting can be of one of three types:

1. The marks differ from each other (as in the case of modern calendars where different numbers distinguish one day from another).
2. The marks themselves are identical but can be differentiated by their organisation on the support (example: organisation by groups).
3. The marks cannot be differentiated (identical equidistant marks).

In the last case, the set of marks might only constitute a notation used at one given time. After a certain lapse of time it becomes impossible for the engraver to differentiate one element from an other. The difficulty of identifying sets of marks as notations increases progressively from the first to the third type mentioned above. If the code which organises these notations is not known (as in the case of prehistoric objects), their interpretation as notations can be proposed but is difficult to demonstrate. Science has its limits and this must be recognised.

A different case is that of a set of marks which appear to be identical to the naked eye but which were made while the tool was continually changed. For this type of behaviour we should propose the most logical interpretation.

Bednarik states that - in the strictest epistemological sense - we can never affirm that the function of an object was that of a notational system. This is also true, however, for the function of nearly all of the objects and structures made by prehistoric societies. The scientist attempts to offer the most rational model of interpretation according to the results of his research. The development of the most logical hypothesis also requires that the chrono-stratigraphic and cultural context be considered. Are there, for example, Upper Palaeolithic civilisations in which certain objects occur regularly or systematically on similar supports? Do these objects occur everywhere or do they characterise a cultural province?

In Marshack's study of the Taï bone, the method of analysis is not explained, nor is it experimentally validated before the archaeological object is studied. Marshack does not present - in a strictly epistemological sense - the results

of scientific research but rather his observations and hypotheses. In my study, the method is explained and validated before the analysis. The method produces *results* that I attempt to interpret in a *discussion*. Other researchers can use this method and its results and are free to propose a different interpretation for the latter. To state that the two approaches lead to equally hypothetical identifications appears to me to deny the efforts undertaken over the last half a century to equip prehistoric research with a scientific method of analyses.

The wear on the objects can be an important element. My most recent experiments (d'Errico in press b, c) demonstrate that microscopic criteria do exist for differentiating traces of handling, transport, wearing, technological polishing etc. The identification of these traces on archaeological objects, however, presents several problems. Often it is only possible to exclude certain factors rather than to identify the factor responsible for modifying the bone surfaces.

It is true that numerous Palaeolithic portable art objects are corroded and are not suitable for microscopic analysis. Such a state of conservation, however, is not peculiar to these materials. It is an integral part of a science that is obliged to work with the remains of a very distant past. I can testify by my own experience that researchers working with bone working techniques, use-wear on bone and stone tools, taphonomy, dental wear, pollen etc. encounter much more serious problems than those which I have met with in analysing portable art. Furthermore, the analyses of several hundred Spanish Palaeolithic portable art objects has shown me that numerous objects lend themselves to metrical analyses, and that the great majority of them furnish data through microscopic analyses.

I am happy to provide the details Bednarik considers interesting. The stone implements were fashioned from European flint. It is clear that several factors play a role in the morphology of the notches: profile, section and length of the cutting edge, the ability of the worker, the cross-section of the bone object etc. These factors were evident during the course of the experimentation, and they certainly also affected the carving of notches by prehistoric people. This in no manner contradicts my results. I explain in detail elsewhere (d'Errico in press a) the role played by each of these factors in the cutting of the notch. Oblique partial steps, for example, are made by the micro-protuberances on the lateral zones of the cutting edge (Fig. 5B). The central zones of the cutting edge, on the other hand, completely cross the notch with each to and fro movement and thus produce parallel steps at the bottom of the notch. Any sand grains which might be present on the bone surface have a negligible effect on the morphology of the notch, due to the prevailing effectiveness of the tool during the cutting of the notch. The more curved the surface of the support is in cross-section, the shorter and deeper the notches will be. The inverse applies to lightly curved surfaces.

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NOTE: An additional RAR Comment by A. Marshack has not been received in time to be included here.

Résumé. Les critères adoptés dans le passé pour identifier des systèmes de notation préhistoriques n'ont jamais été clairement décrits ni démontrés expérimentalement. L'analyse (MEB et microscope à lumière transmise sur répliques transparentes) de séries d'entailles expérimentales révèle qu'il existe des critères pour reconnaître l'utilisation du même tranchant et pour repérer des possibles changements de tranchant. Un calculateur de mesures des profils est utilisé pour mesurer les angles formés par les parois des entailles expérimentales et archéologiques. La variabilité des angles montre que l'utilisation du même tranchant et le changement répété de tranchant peuvent être discriminés statistiquement. L'article résume ces critères et présente les résultats de deux tests aveugles qui semblent confirmer la validité de la méthode. L'identification d'une pièce solutréenne avec une série d'entailles caractérisée par des nombreux changements de tranchant pose à nouveau le problème de l'existence des systèmes de notation paléolithiques.

Zusammenfassung. Die früher zur Identifikation prähistorischer Notationssysteme angewandten Kriterien sind noch nie klar beschrieben oder experimental demonstriert worden. Die Analyse (mittels SEM und optischem Mikroskop an durchscheinenden Harznachbildungen) einer Serie experimentell hergestellter Kerben zeigt, dass es Merkmale gibt, um festzustellen, ob ein gegebenes Objekt mit Hilfe einer oder mehrerer Arbeitskanten bearbeitet worden ist. Ein Computersystem für Profilmessungen und Auswertung wird verwendet, um die Winkel, die zwischen den Seiten der experimentell hergestellten und archäologischen Kerben liegen, zu messen. Die Unterschiedlichkeit dieser Winkel zeigt, dass die Verwendung einer einzelnen, oder mehrerer verschiedener Arbeitskanten, ermittelt werden kann. Die vorliegende Arbeit fasst diese Kriterien zusammen und legt die Ergebnisse zweier blinder Tests vor, welche die Gültigkeit der mikroskopischen und statistischen Merkmale bestätigen. Die Untersuchung eines Solutrium Stückes, das eine Serie von Kerben mit zahlreichen und deutlichen Änderungen der Arbeitskante aufweist, leitet wieder die Frage der paläolithischen Notationssysteme ein.

Resumen. Los criterios adoptados en el pasado para identificar sistemas de notación prehistóricos no han sido claramente descritos ni demostrados experimentalmente. El análisis (MEB y microscopio óptico de transmisión sobre réplicas transparentes) de series de muescas experimentales muestra que existen criterios para reconocer la utilización del mismo borde de un útil y para localizar los posibles cambios de borde. Un medidor de perfiles ha sido utilizado para calcular el ángulo formado por las paredes de cada muesca experimental y arqueológica. La variabilidad de los ángulos permite discriminar estadísticamente entre la utilización del mismo borde y cambio repetido de borde. El artículo resume estos criterios y presenta los resultados de dos tests ciegos que confirman la validez del método. La identificación en una pieza solutrense de una serie de muescas realizada por varios bordes plantea de nuevo el problema de la existencia de sistemas de notación paleolíticos.

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KEYWORDS: Graffiti - Rock art - Central Queensland - New South Wales

THE USE OF GRAFFITI IN THE MONITORING OF COMMUNITY ATTITUDES TOWARDS ABORIGINAL ROCK ART

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Abstract. This paper is concerned with the problem and research potential of graffiti at rock art sites. Graffiti often detract from the cultural significance of place, but some are of considerable historic import. Dated examples can also be used as a means for monitoring changes in the way Aboriginal sites have been used and viewed in post-contact times. Two case studies are used briefly to illustrate these points.

Introduction

The urge to leave a personal mark on rock faces for posterity appears to be a basic human trait. It is well documented as one of the reasons for rock art manufacture in ethnographic studies of Australian Aboriginal rock art (e.g. Moore 1977: 322), and is also one of the principal causes of rock art deterioration associated with tourist visits to sites. Some graffiti at rock art sites are themselves of historic importance, such as that left by the explorers Burke and Wills at Burkes Cave in western New South Wales during their epic journey to the Gulf of Carpentaria, or those left by Aboriginal stockmen in central Australia (Walsh 1988: 261; Gale and Jacobs 1987: 6). These situations present a problem: removal of graffiti from rock art sites is now a standard site management practice as it discourages 'copy-cat' behaviour (e.g. Gunn 1983: 53). However, Australia is a signatory to the International Council on Monuments and Sites (ICOMOS) Charter which stipulates that in implementing conservation procedures, all aspects of the cultural significance should be taken into consideration and that the contribution of all periods to the place must be respected. This means that where graffiti are part of the cultural significance of a place, their removal needs to be undertaken only after consideration of the conflicting priorities.

In a recent review of the impact of tourists on places of cultural significance, Gale and Jacobs (1987: 64, 69) note that preliminary studies and monitoring of the effectiveness of mechanisms for modifying and ameliorating visitor impact at sites may be costly, but much less so than constructing facilities, then having to change them. They list a range of techniques for measuring visitor pressure on sites, including mechanical counting, observation, questionnaires and interviews, all of which provide information on visitor impact and attitudes over a fairly limited time period. However, changes in visitor impact and attitudes over a longer time period should also be of concern in the design of a conservation plan, otherwise measures taken may be a response to past rather than present patterns of visitor behaviour. It appears that different stages in the 'public awareness' of a site tend to attract different types of visitors with very different management problems (Walsh 1984a: 10), and at many sites graffiti appear to have been made when visitor numbers were low and attitudes to rock art were different (Gale and Jacobs 1987: 6). In such cases, graffiti may no longer be a problem, even though the results of previous defacement may still be very evident. This can be checked by examining the chronological distribution of dated examples.

At the more general level, over the past twenty years there has undoubtedly been a shift in community attitudes towards conservation of cultural and natural resources, some of which may have stemmed from legislative changes and some of which led to legislative changes. Clearly, methods for monitoring the long-term effectiveness of conservation procedures would be useful. Comparative studies undertaken at specific sites at regular intervals have provided such data (e.g. Lorblanchet 1986: 154; Walsh 1984b), but in Australia no study was undertaken for the important period from the mid-1950s through to the 1970s, when all Australian states passed laws protecting Aboriginal sites and the 'public archaeology' bodies responsible for their implementation were established (Ward 1983). One possible method for monitoring long-term changes in social attitudes towards Aboriginal art sites is to make use of dated graffiti, as the following case studies from the Central Queensland Highlands and New South Wales illustrate.

Example 1: the Central Queensland Highlands.

While engaged in Ph.D. research in central western Queensland during 1976-79, Morwood (1979) recorded defacement at the rock art sites visited during the project. The majority of such recordings came from sites on the upper Belyando watershed, especially Blacks Palace which is probably the most heavily vandalised site in Queensland (Fig. 1). For instance, in one of the first official reports on Blacks Palace, Staff Surveyor W. G. Drane (1918), wrote to the Queensland Surveyor-General 'I am forwarding per parcel post a painting of a hand', and in 1935, J. Bergin,

Figure 1: General view of a section of Blacks Palace, central western Queensland. This site has suffered extensive vandalism (e.g. high-powered rifle shots, 'souveniring', graffiti).

Acting Land Commissioner, complained that at this site 'visitors persist in defacing the work by carving their names over the drawings, and worse still by scattering the bones out of the caves'.

Other records used in this study came from sites on the upper Barcoo, Warrego and Comet Rivers (e.g. the Bull Hole, Goat Rock, Paddys Cave, Wanderers Cave, Kenniff Cave). The most recent recordings were undertaken in 1982.

All examples of graffiti which included a date were extracted from the defacement recordings and plotted up in histogram format (Fig. 2). The chronological patterning in the graffiti can be readily explained in terms of the history of European land use in the region, coupled with general changes in population mobility and attitude. Huge pastoral holdings in central western Queensland were first taken up in the early 1860s and from this time Europeans must have encountered Aboriginal rock art sites during stock work. However, apart from Parrot (1888: 271), who wrote of his discovery of a rock art site in the Expedition Range in 1864, very few of such early encounters were ever published. The earliest dated graffiti recorded are by J. Hunt who carved his name at a number of art sites in the Blacks Palace region between 1890 and 1893. Local tradition records that Hunt was a brumby-runner and, judging by the occurrence of his name inscribed in a number of rock art sites over a wide area of the Central Queensland Highlands between 1890 and 1919, he must have ranged extensively as part of his occupation (Walsh 1984b: 112).

Towards the end of the 19th century European knowledge of the region steadily increased, as did the number of reports on local rock art sites (e.g. Biddulph 1900; Maitland 1894; MacLellan 1901; Meston 1901; Tibbets 1902; Worsnop 1897). This period is not well represented in dated graffiti, possibly the frequency of illiteracy amongst station workers at the time was one factor in this. On the basis of a much larger sample size, Walsh (1984b: 112) notes that some graffiti in local rock art sites date to the massive man-hunt through the region for the Kenniff brothers, who murdered a Police Constable and the manager of Carnarvon Station in 1902 (Heap 1969). The influx of people associated with the man-hunt is not represented in the graffiti sample used in this study, although the Kenniff saga clearly motivated a (probably fraudulent) 'Pat/Jim Keniff 1891' inscription at one art site.

From 1908 there was a definite peak in graffiti as the country became more densely occupied. This peak continued through World War I. To the east the earliest evidence for European knowledge of rock art sites in Carnarvon Gorge comes from this time. 'E. Ward 22/2/1918' is inscribed at Cathedral Cave and is said to have resulted from use of the area by two local draft-dodgers, the Ward brothers, who 'went bush' to avoid the proposed conscription (Quinnell 1976: 18). Walsh (1984b: 113) also reports a depiction of a World War I type aircraft with German markings at one rock art site in central western Queensland which is likely to date to the same era. Some of identified graffiti names found in central Queensland rock art sites from the 1920s are those of well-known Aboriginal stockmen of local descent, indicating that the traditional significance and respect for rock art sites had been lost by this time (Walsh 1984b: 112, 1988: 281).

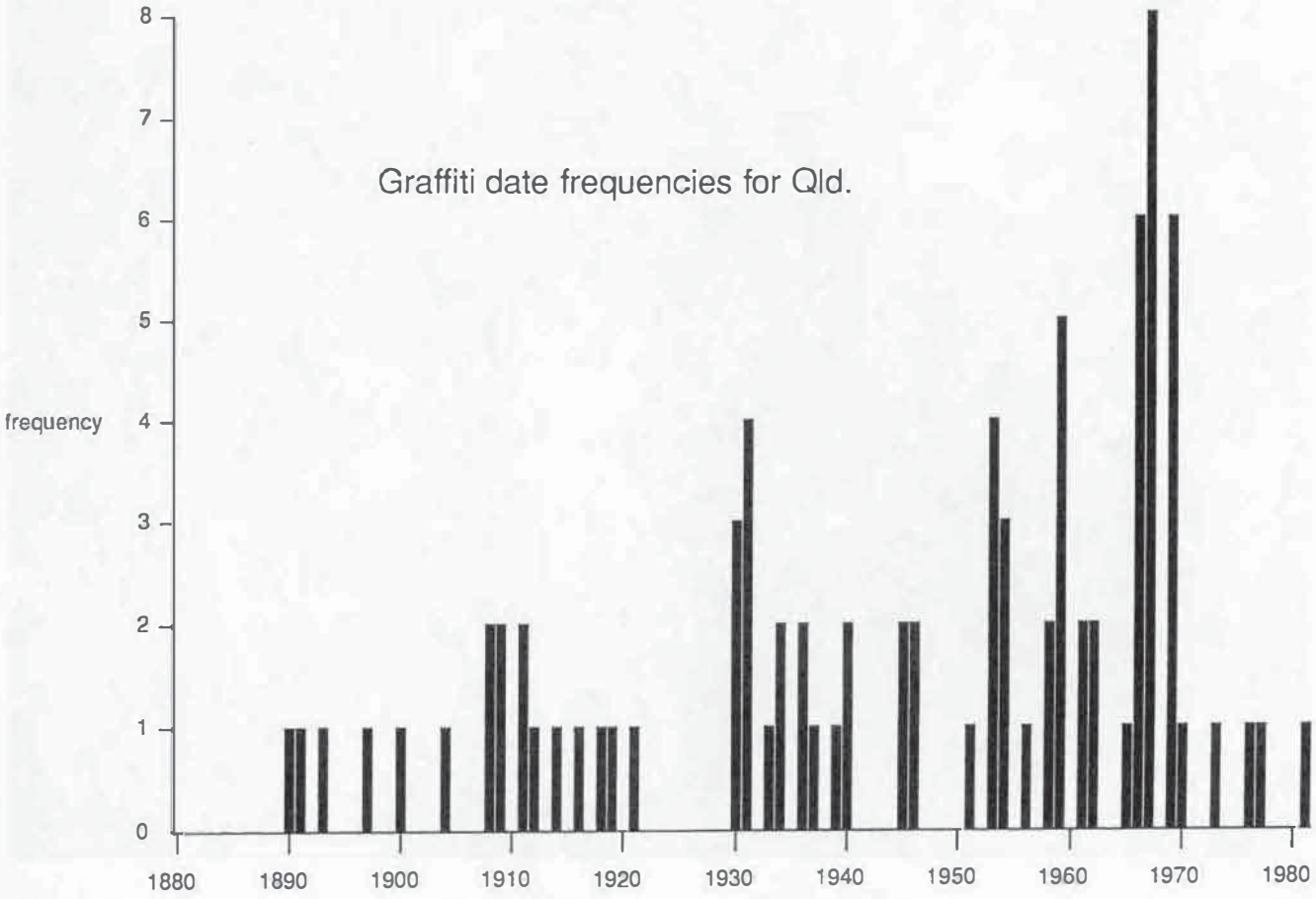

Figure 2: A histogram of dated graffiti from four rock art sites in the Central Queensland Highlands. Sample size 84.

The second peak in dated vandalism coincides with the Great Depression of 1929-39 when many Europeans relied on income gained from bounties paid on wild dogs, and from the trapping of possums and koalas for their skins. (The last legal season for possum hunting was 1936.) Scalpers, possumers and doggers often made use of rock-shelters for residence and storage, and some of the vandalism is associated with these activities. For instance, near Carnarvon Gorge, some of the vandalism at Wanderers Cave and adjacent shelters is not only dated to this period, but the individuals concerned are known to have been engaged in possuming (Grahame Walsh, pers. comm.).

From the 1950s an increase in affluence, leisure time and number of motor vehicles led to more visitors to sites, especially after the advent of four-wheel-drive vehicles in the 1960s. This period is marked by a steady increase in the graffiti at rock art sites in central Queensland which peaked in the mid-1960s. The subsequent decrease in the frequency of vandalism, despite continued increases in visitor numbers, seems to mark a major change in general public perception about the cultural value of Aboriginal rock art. The decrease coincided with the passing of the Queensland *Aboriginal Relics Protection Act of 1967*, indicating that the Act has been very successful in defining appropriate visitor behaviour at sites.

Establishing the identity and personal characteristics of the small number of people who have continued to deliberately desecrate rock art sites in central western Queensland since the Act was passed would now provide useful data for the design of preventative measures. In at least one case, when a name was carved over petroglyphs at the Bull Hole site near Tambo (Fig. 3), the offender was a local juvenile in an unsupervised group of her peers. Gale and Jacobs (1987: 45-7) have noted that children, poorly led groups and locals are all in the high-risk visitor category, and the Bull Hole incident involved a combination of all these. In this case penalties stipulated by the Act were not adequate, but a well-placed warning sign may have prevented the damage.

Example 2: New South Wales.

Kaiser-Glass (1986) obtained information on dated vandalism in New South Wales using the archives, photographs and tracings held by the Cultural Resources Section, National Parks and Wildlife Service for the following rock art sites: Dora Pinnacles, Bobadeen, Bull Cave, Bravo Handgon Rock, Angus Place, Girrakool, Grotto Point, Bents Basin, Loves Cave, Snake Rock, Wilton Cave and Wollemi. The earliest recorded graffiti date, 1869 from Bents Basin, considerably postdates the earliest European occupation of the Sydney-Penrith region. The sporadic nature of the graffiti record prior to 1914 reflects the low European settlement density over much of the region prior to this time. Overall, the resulting frequency histogram (Fig. 4) closely mirrors that from the Central Queensland Highlands. Specific differences, such as the absence of dated examples between 1908-13 and the slight increase in vandalism in New South Wales between 1982-4, are likely to be a reflection of the relatively small size of the samples, in which the activities of individuals at

Figure 3: A section of the Bull Hole petroglyph site on the upper Barcoo River showing post-legislative graffiti done by a local adolescent. Subsequent attempts by local residents to 'fill' the incised graffiti with a bonding agent have added to the damage.

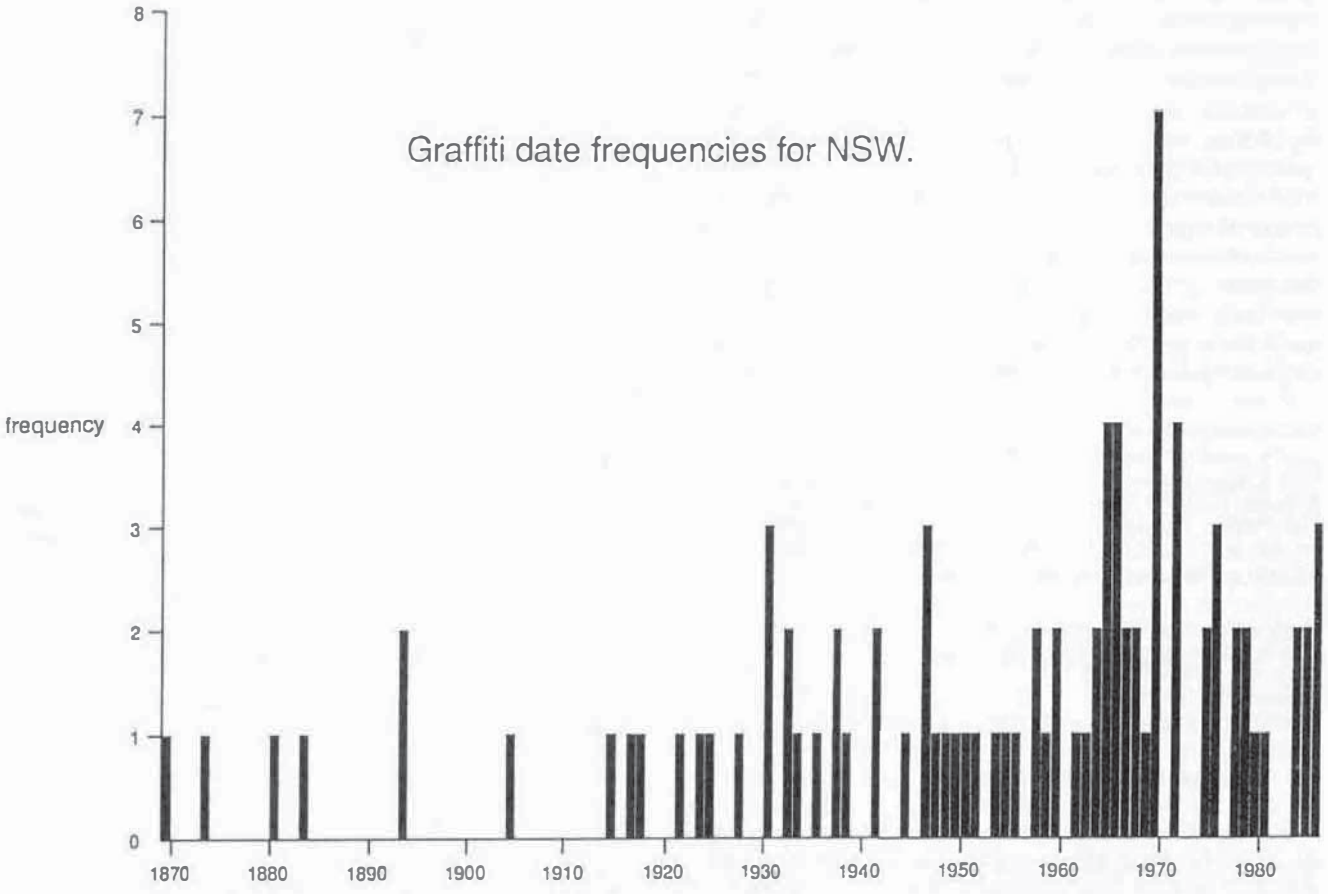

Figure 4: A histogram of dated graffiti from twelve rock art sites in New South Wales. Sample size 89.

individual sites can change apparent trends. More detailed interpretation of these differences would necessitate a site-by-site analysis of the data base.

Conclusions

Despite minor differences, the chronological distribution of dated vandalism in central Queensland and New South Wales indicates that a similar range of determinants was operating in each State; namely the closer pattern of European settlement of many areas from the early part of this century, the effects of the Great Depression on European land use, an increase in general population mobility and leisure time over the past thirty years, and the effects of changing community attitudes and legislation over the past twenty years. However, in New South Wales the peak in dated vandalism does not occur until after the initial legislation protecting Aboriginal sites was passed in 1967, indicating that the major change in community attitudes towards Aboriginal sites postdated the relevant legislative changes. This provides a stronger argument than the Queensland case study, that legislative changes generally led rather than followed changes in community attitudes towards protection of archaeological sites. In fact, the continued extent of vandalism at archaeological sites, especially around the Sydney region (Haigh 1979: 82), prompted amendments to the *National Parks and Wildlife Act* in 1974, with stricter penalties and provision for restricting access to, and use of, areas containing Aboriginal sites (Ward 1983: 26).

These two case studies suffice to show that the incidence of dated vandalism at rock art sites reflects the history of European use of specific areas and sites, as well as more general changes in ease of access and community attitudes towards Australian Aboriginal culture. If systematically undertaken, the recording of graffiti to monitor changes in its rate of accumulation at sites can provide information on the long- and short-term effectiveness of legislation, well-publicised punitive action on offenders, specific management procedures at individual sites, and so on. To this extent vandalism at art sites is an artefact with historical significance, research potential and management implications, and should be treated as such. Removal of European graffiti is clearly a necessary conservation measure at many Aboriginal rock art sites but, as this study has shown, graffiti contain useful information. Recording of graffiti prior to removal should be standard practice.

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Résumé. *Cet article est à propos du problème et du potentiel de recherche des graffiti aux sites d'art rupestre. Les graffiti diminuent souvent l'importance culturelle d'un site, bien que quelques uns aient une importance historique considérable. Les examples datés peuvent aussi servir à établir les changements dans la façon dont les sites des aborigènes de l'Australie ont été utilisés et appréciés durant la période d'européanisation. Deux cas sont documentés de façon à illustrer brièvement ces points.*

Zusammenfassung. *Diese Arbeit befasst sich mit dem Problem und den Forschungsmöglichkeiten von rezenten Felskritzeleien an Fundorten von Felskunst. Diese können oft die kulturelle Bedeutung eines Ortes beeinträchtigen, doch manche sind von beträchtlichem historischen Wert. Datierte Beispiele kann man auch dazu heranziehen, Änderungen festzustellen in der Weise, in der Aboriginal Fundorte in der Zeit seit europäischem Kontakt verwendet und gewertet wurden. Zwei Studienfälle werden verwendet, um diese Fragen kurz zu illustrieren.*

Resumen. *Este artículo concierne el problema y potencial de investigación de 'graffiti' en sitios de arte rupestre. 'Graffiti' con frecuencia se aparta del significado cultural del lugar, pero algunos son de una considerable importancia histórica. Ejemplos fechados pueden también ser usados como una forma de informar acerca de cambios en la manera en que los sitios aborígenes han sido usados y vistos en épocas post-contacto. Dos casos de estudio son utilizados para brevemente ilustrar estos puntos.*

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KEYWORDS: **Trance - Shamanism - Ethnography - San - Southern Africa**

A TESTAMENT TO THE SHAMANISTIC HALLUCINATORY TRANCE THEORY OF THE SOUTHERN AFRICAN ROCK ART

Cyril A. Hromnik

Abstract. The fundamental and unquestioned premise of present-day rock art studies in southern Africa is the widely shared, though never proved, belief that this art is of Bushman (as opposed to Quena or Hottentot) origin. The present article aims neither to prove nor to disprove this belief. Instead it poses a legitimate question about the validity of some of the most recent evidence in support of this belief's theoretical offshoot, namely the claim that 'the painted depictions were principally associated with the trance experience of [San] shamans', and that 'perhaps most of the paintings were done by shamans themselves'.

The Last Testament

Pieter Jolly, the principal of the two interviewers of the 'old Bushwoman', paraphrases her as saying that 'Medicine men did the painting'. The other interviewer, Lewis-Williams, adds that 'Nothing could shake her conviction that rock painting was the task of medicine men' (Jolly 1986: 6; Lewis-Williams 1986: 10). The old woman spoke in Xhosa and in a contrived language which consisted of Xhosa words without the prefix and with suppressed final syllables. This was in support of her claim of being of pure 'San' descent and of being a speaker of the 'San' language (Lewis-Williams 1986: 10). While a shaman can be described as a 'medicine man', not every medicine man can be described as a shaman. Katz, who made a detailed study of the magico-medicinal practices of the Kalahari !Kung Bushmen with whom he stayed for several months, did not find among them a single medicine man who could rightfully be called a shaman. Their healing practices were devoid of trance (Katz 1982: *passim*). A shamanic figure and the name shaman are totally absent from the historical record of the southern African Bushmen. The word shaman is in fact absent, not only from the existing Xhosa and Bushman dictionaries, but also from the vast majority of African languages. It is a word imported into southern Africa in recent times mainly through the work of Lewis-Williams. The 'old Bushwoman' certainly could not have used it. She may have said *gqirha*, meaning 'medicine man', 'witchdoctor'. Inserting the word 'shaman' into her vocabulary shifts her entire evidence from one historical and cultural plane to another, from the African context to the context of Eurasia. Yet nowhere in the cited studies do we see any admission that the Bushmen may have had a mixed Afro-Asian or Afro-European origin.

Taking the liberty of substituting the word 'shamans' for the term 'medicine men' in what is claimed to be recorded evidence is inadmissible. Similarly inadmissible is the second interviewer's assertion that 'Again and again she insisted on a shamanistic view of the art' (Lewis-Williams 1986: 11). As we shall see, the 'old Bushwoman' had no knowledge of the word shaman and no notion of the shamanistic view of the art.

A re-examination of the Testament

The far-reaching conclusions based on the largely paraphrased statements of the 'old Bushwoman' call for close scrutiny of this rather unexpected source. The simplest procedure would be to visit the 'old Bushwoman' again and check her background and her story. This, however, has been made impossible, because her interviewers felt it necessary not to reveal her true name and to 'refer to her as M. to protect her identity' (against whom?). Even her residence has been given only as 'in the Tsolo district of Transkei' (Jolly 1986: 6). In addition, the statements of the 'old Bushwoman' have been called 'the last testament of the Southern San', with the ominous implication that there will be no more San to tell us their story, which may confirm or contradict her story, and that we should accept her interpreted word without further questioning. This is a strange way of conducting research.

But if we are to accept the interpreted word we had better make sure we know the original statements. We must also be sure that our source is an authentic Southern Bushwoman and that she grew up in an environment where she was able to acquaint herself with details of the Bushman religious and cultural life, as well as with the artistic traditions and artists' conventions of religious symbolism. It is also important to note that the testament of the 'old Bushwoman' appeared exactly at the time when 'the absence of southern informants has, for some [rock art] workers, cast doubt on this [trance and shamanistic] interpretation [of the rock art] and, indeed, the viability of the whole undertaking' (Lewis-Williams 1986: 10; cf. Willcox 1990).

The 'old Bushwoman' claimed to have been born in 'the river shelter' from a 'San' father but, 'in fact, she was born amongst the Mpondomise', most probably of a Mpondomise mother (Jolly 1986: 8). Her mother has not

been identified, yet it is the mother who shapes the cultural outlook of her children, especially among the Bantu-speaking people like the Mpondomise. She claimed to speak 'the San language of the area' but she did not. (It is doubtful that she ever used the name San.) It is not known how she acquired the idea of concocting a so-called 'San' language, but there is a strong indication that she wanted to convince the interviewers about her dubious 'San' identity. That she had thought about it and prepared herself in advance is confirmed by her use of the concocted 'San' language without 'contradicting herself' (Lewis-Williams 1986: 10). At this stage it would seem more appropriate to call her Mpondomise M. rather than San M.

At the time of the interview M. was 'about seventy-five years' of age and, if she ever visited the rockshelter near the river (which river?) where her father was allegedly born, it would have been, in the interviewer's opinion, 'in about 1918 or some time after this date' (Jolly 1986: 8). She would then have been seven years old or a little older, scarcely an age to learn about the hidden meanings of the conventional symbols in the rock art that depicted the visions of shamans in a hallucinatory trance, as has been claimed. (To counter this objection M. is said to have been 'well into her eighties' in a subsequent publication [Lewis-Williams and Dowson 1989: 36].) When shown reproductions of paintings depicting therianthropic figures which, according to Lewis-Williams, were 'painted metaphors for trance', Mpondomise M. only guessed that 'they could be a hunting disguise' or 'decorative headdresses worn by the medicine men at dances' (Lewis-Williams 1981: 114; Jolly 1986: 7-8). Apparently she had to guess, because in her life she had had no opportunity to experience life in a culturally flourishing Bushman community. We have no evidence that such a community could have been found in Transkei, or anywhere else in the Eastern Cape, in this century. The interviewer's wishful statement that 'It is not at all impossible that there were still San living as a community near the river at that time' is mere speculation with no claim to being evidence (Jolly 1986: 8). Quena (Hottentots) 1) and the marauding Quena called descriptively 'bushman Hottentots' (with lower case 'b') or Soaqua were the only indigenous non-Bantu speaking inhabitants of the Eastern Cape in the late 19th and early 20th centuries, 2) and it is most probable that her father was one of them.

Comparisons and analogies

It is claimed that because Mpondomise M.'s ethnographic statements matched to some extent the information supplied by some of Bleek's informants in the last century, and because that information provided the clues for the interpretation of rock art in a trance context, 3) 'we can go so far as to speak of corroboration of the shamanistic interpretation' (Lewis-Williams 1986: 11). This, however, is a circular argument that lends no support to the hallucinatory trance theory. In both cases the data have been subjected to a process of adjustment and re-interpretation and only then yielded a semblance of support for the theory.

For instance, Mpondomise M. described medicine men who would 'drop into the river' in order to capture a river snake which was then used for ritualistic purposes. Unaware of the hallucinatory trance theory, Mpondomise M. interpreted this incident in realistic terms, saying that the medicine men would then eat the snake's fat and 'rub the fat into cuts made on their body'. She did not believe that dropping into the water meant 'being in trance', as her interviewers would have it. Thus she produced a statement which is very realistic and most probably verifiable, but totally unsupportive of the hallucinatory trance theory. Instead of seeking further verification, the interviewers charge Mpondomise M. with having 'missed the metaphor implicit in the account' and of advancing a 'naive' interpretation of the therianthropes. Orpen's Soaqua informant Qing, who was interviewed in 1874, is said to have missed the same metaphor (Orpen 1874: 10; Jolly 1986: 7). However, it is much more likely that in historical terms there was no such metaphor, as is confirmed by its absence from the historical record. These details have eluded dozens of keen missionaries, administrators, ethnographers, scholars, travellers etc. in the previous centuries, for none of these observers and researchers has ever found any allusion to trance or shamanism in the rock art and in the historical record of the Bushmen of southern Africa. Jolly himself states that the 'old Bushwoman' is 'one of only two or three ['San'] ever to have been interviewed at all' (Jolly 1986: 6), and we have no record of a genuine Bushman (a speaker of a Bushman language like !Kung, not a Quena language like Nama or Soaqua-Quena spoken in southern and eastern Cape Colony or by the MaSarwa) who spontaneously claimed that the rock art was created by Bushman people like himself.

The obviously solicited and paraphrased statement of Mpondomise M. that the blood of an eland was 'mixed with paint, to make lines and patterns on the rock face of the shelter next to the river', has been invoked to give credence to the latest claim of the hallucinatory trance theory, that rock art not only depicts the invisible world of the hallucinating and trancing shamans, but that by adding blood or some other 'magical' substance from ritually killed animals to the paint, the paint itself becomes a link with the religious thought and its trance symbolism (Jolly 1986: 6, 8). Her evidence is said to be supported by another case from, by implication, San ethnography. How (1962) is cited as saying that Mapote, a Sotho man (not a

1) Quena is an historically attested indigenous name for the people of southern Africa whom Europeans came to call Hottentots. Other names presently used, such as Khoi, Khoikhoi, Khoe and Khoisan, have no historical validity as they were created by European travellers and scholars out of the misunderstanding of certain records (Hromnik 1988).

2) Documentation on this point is voluminous, and only a sample can be cited here (see Haupt 1752 in Godée-Molsbergen 1922, III: 288; Sparrman 1977, II: 123; Hromnik 1990a: 32-5). The name 'San', as employed by Lewis-Williams, is practically synonymous with 'hunter-gatherer', regardless of the ethnic or linguistic identity of the subjects thus defined. This looseness of definition facilitates the attribution to the so-called 'San' of most if not all cultural characteristics of the Quena. Thus, for instance, a sacrificial burning of ox flesh in the Quena rain-making ritual, practised by the Korana with some participation by their Soaqua Quena clients, has been transferred to the 'San' (grouped with the !Kung) by saying that, 'when possible, the San too used oxen in their rain-making rituals. Those oxen were probably either stolen or given to them by those desiring their assistance'. Next, the Quena ox is replaced by the San eland, because, allegedly, 'there is a much clearer indication of an association between eland and rain', and a typically Quena ritual has been transformed into a 'San' ritual. This then becomes one of the fundamental building blocks of the hallucinatory trance theory of the 'San rock art' (Lewis-Williams 1981: 105-6 ff.), even though no Bushmen (capital B) were involved, and the original Quena ritual involved no trance at all (Engelbrecht 1936: 175).

3) None of Bleek's writings cited by Lewis-Williams (1981: 103-16) expressly states that any of the described practices, dreams or beliefs involved trance, and Bleek's *A Bushman dictionary* (1956) has no entry for trance.

San), allegedly asked 'for the blood of a *freshly killed eland* as this was one of the ingredients of the paint', and 'prepared the pigments for the paint in a ritualistic manner, suggesting that the paint was considered to possess a magical power - which could, in all probability, have been transferred, as M claimed, to the paintings themselves' (Jolly 1986: 7).

In reality the Sotho man Mapote, born of a Mpondomise mother, was hired by Mrs How, a missionary wife at Qacha's Nek, to paint for her on rocks, which he reluctantly agreed to do for a reward of 'a pair of strong, stout leather boots' (How 1962: 32, 39). Mapote asked for the 'blood of a freshly killed eland', not because it 'was considered to possess a magical power', as claimed by Jolly, but because 'otherwise it will coagulate and will not mix with the pigment or soak into the rock'. Eventually he was quite happy to use the 'ox blood ... from the butcher' (How 1962: 37-8). His asking for fresh blood had nothing to do with the claimed San belief in a transfer of potency to the paint and paintings, and How's account contains no evidence or even a suggestion of this kind. How only speculates that 'Bushman paintings must have been executed immediately after a successful hunt and a good meal, thus prompting cheerful and energetic thoughts' (How 1962: 38).

Mapote wanted to use a body ointment called *qhang qhang* (from Quena *//kan* = to rub [with ointment]) as the red paint for his pictures, but was satisfied to paint with the red ochre called *letsoku la Makhooa* ('*letsoku* of the white people'), purchased from the local European store (How 1962: 34). Obviously Mapote knew something about body ointments - not about rock art paints and painting. His paints were ad hoc concoctions, yet he declared them to be 'completely sun-proof for ages'. Not surprisingly his paintings disappeared completely in less than 30 years (How 1962: 39). Careful reading of How's description of Mapote's artistry leads to a strong suspicion that Mapote, as a rock artist, was a fake, and his testimony, like that of Mpondomise M., leaves the trance theory quite exposed. It is hard to believe that mixing paint from an European shop and ox blood from an European butcher, and then painting on dressed blocks of stone from the missionary house construction and on a boulder could ever constitute a ritualistic act able to transfer 'a magical power ... to the paintings themselves' (Jolly 1986: 7).

Commenting on certain 'crude blotches and strokes above the heads of a series of finely painted dancers' in the shelter, Mpondomise M. said that they were 'believed by the San to ward off lightning'. In the interviewer's opinion these marks may have been made at a 'late stage of its occupation by the San and at a time when they were dependent for their livelihood ... on their reputed ability to bring rain for the Mpondomise'. Mpondomise M. claimed that her father 'was a medicine man and had painted some of the depictions in the rock shelter' (Jolly 1986: 7; Lewis-Williams 1986: 10). He therefore may have painted the marks in question. If this is true, what was the origin of his inspiration? The interviewers, arguing that shamanistic San mythology was the source, link these lines to the 'haematite stripes like those of a zebra' painted by a /Xam girl at puberty on the bodies of young men of the band to protect them from lightning. This information is said to have come from the Bleek material, but only the unpublished analysis of it by Hewitt is cited (Hewitt 1976: 212; Jolly 1986: 7). The real source of the inspiration appears to have been elsewhere. '*Qhang Qhang* ... is regarded as a powerful medicine by superstitious Basotho', writes How, 'and is used, among other things, to ward off hail and lightning'. The *qhang* was apparently applied to the body (it was an ointment, after all) and a Sotho gaoler at Qacha's Nek in fact came to Mrs How, wanting to buy her pigment 'as a protection against lightning and other evils' (How 1962: 34-5).

Concluding observation

In view of this evidence we can only conclude that Mpondomise M.'s comments on rock art and related ethnography originated by and large from the Mpondomise and Sotho cultural environment, and perhaps even from the culture of modern townships. Attempts to use her testimony in support of an academic theory is something she obviously did not bargain for. The themes of her comments, whether on religious practices or on the content of the rock art, are so closely akin to the major themes in the hallucinatory trance theory that they cannot be taken as spontaneous. Throughout her interview she appears to have been guided by questions that are of supreme importance to the theory but which are only remotely related to the historical reality and to her own memories. Thus she was asked: 'What did you see when your sister made rain?' Her answer was simple and well understandable in the context of the known practice of rain making among the south-eastern Bantu. She knew nothing about it because her sister simply 'disappeared'. She disappeared in the forest, on the mountain or wherever else she was making rain. But the interviewer expected something else from Mpondomise M.'s answer. To him the rain-making sister 'entered trance and made rain in that invisible realm' (Lewis-Williams 1986: 11). Mpondomise M. did not oblige in this case and, what is even more surprising, considering that her father was a medicine man and her sister a rain maker, she never once in the entire interview made the slightest mention of trance, hallucination or shamanistic symbolism. This 'defect' in Mpondomise M.'s testimony is briskly rectified - without any further interview or new evidence - three years later in *Images of power*. There she is said to have insisted 'that *all* the paintings had been done by shamans', and it is forcefully stated that 'This woman's father had been a *shaman* [my italics], and her sister ... had been the last of the line of rainmakers' (Lewis-Williams and Dowson 1989: 36).

San M. is only an abstraction from Mpondomise M., created for the sake of a theory. Her answers are embraced when they fit into the hallucinatory trance theory, and when they diverge from the anticipated shamanistic line they are made subject to the 'final arbiter' of the 'ethnography' that has been compiled in support of the theory (Lewis-Williams 1986: 11). The San M. is a figure that has been created 'to give her *imprimatur*' to the shamanistic trance understanding of the southern African rock art (Lewis-Williams 1986: 11), but her comments, derived from her Mpondomise environment, do not bear any reference to that theory.

Summary

'After decades of misunderstanding, San rock art is being restored to its rightful position: it is probably the most detailed and complex shamanistic rock art in the world' (Lewis-Williams 1986: 11). This bold declaration by the founder of the presently dominant hallucinatory trance school of southern African rock art rests upon three main premises:

1) That southern African rock art was produced by 'San' (Bushmen, such as the !Kung of the Kalahari).
2) That it is largely a product of trancing shamans.
3) That it is purely indigenous and to think otherwise is unscientific (Lewis-Williams 1983: 3-13).

After half a decade of unprecedented progress there is a growing realisation that the product of this school is a theoretical construct based on very little evidence, especially from the presumed artists themselves, the Southern Bushmen. Just when some rock art researchers have begun to wonder about the viability of the whole undertaking, a living 'San' witness is claimed to have been discovered, ready 'to give her *imprimatur* to this understanding of the art'. This witness is an old woman, said to be 'a first generation descendant of the Transkei San' and her interpreted story is called 'the last statement of the southern San' (Jolly 1986: 6) or entitled 'The last testament of the Southern San' (Lewis-Williams 1986). In it she is said to have fully endorsed the trance origin and a shamanistic view of the rock art, but her identity is withheld from scholars interested in her background and the substance of her testimony. She is called M. This article tests the historical soundness of the unexpected testament.

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COMMENT

By J. D. LEWIS-WILLIAMS

Critiques of widely accepted beliefs about, and explanations of, the past are always welcome; they keep researchers on their toes, clear up ambiguities and advance research. This is, however, a disappointing contribution.

Hromnik's scholarship may be judged by five of his statements. The first two are comparatively trivial, but they point to his superficial knowledge of the literature; the next three show that his argument is fallacious in its basic assumptions.

1) Hromnik: 'Mrs How, a missionary wife at Qacha's Nek'.

'An appreciation' by G. T. Chaplin at the beginning of How's book gives the facts: 'Those ... who remember Mrs How ... will always associate her ... with her husband, the late Lt. Col. Douglas Walsham How ... He served in every district of Basutoland, first as Police Officer and later as district Commissioner' (How 1962: 5).

2) Hromnik: 'This information is said to have come from the Bleek material, but only the unpublished analysis of it by Hewitt is cited.'

Hewitt's work was published in 1986. It was reviewed in *African Studies* by Deacon in 1988. The Bleek and Lloyd Collection is available to researchers in the Jagger Library, University of Cape Town.

3) Hromnik: 'Their [!Kung] healing practices were devoid of trance (Katz 1982: *passim*).'

In some publications, Katz prefers to retain the !Kung word *kia* (*!kia*), which he defines as '*that altered state of the consciousness* which is the key to healing' (Katz 1982: 8, emphasis added). He explains why he prefers to retain *!kia*:

> *!Kia* has been translated by other fieldworkers as 'trance'; hence the label 'trance dance' ... The term 'trance' has been used to describe a variety of altered states of consciousness, including possession states and meditation states. Its referents remain ambiguous, its use inconsistent. We prefer to keep the !Kung word *!kia* and to build an understanding of the phenomenon through examining the !Kung's discussion of it (Katz and Biesele 1986: 221).

Katz also translates *!kia* as 'transcendence' because he believes the state involves enhanced perceptions and peak experiences. I have reservations about using this word, as do most other writers.

Katz describes the physical effects of *!kia* thus:

> Kinachau is sweating; his face is beginning to take on a pained appearance. He starts to tremble, his legs quivering ... He swoons and falls softly into the sand just outside the dance rut. He has entered kia, sharply and quickly. Kinachau sits up and remains sitting for several minutes. His look is glazed, his body trembles spasmodically. He ... begins to heal. As he pulls sickness from each person, Kinachau's whole body shakes roughly and his legs tremble violently, the tendons sticking out ... he shrieks out the characteristic deep howling sounds which express the pain involved in pulling out sickness ... After more healing, Kinachau dives for the dance fire ... Just as his head touches the flames, his hair smoking, he is dragged back by two women and held tightly (Katz 1982: 65-6).

Healers say that, during such an experience, they visit God and see the spirits of the dead, experience out-of-body journeys and turn into lions (Katz 1982: 100-1, 115-6).

Most other writers who have witnessed this sort of Kalahari San healing use 'trance' instead of *!kia* to denote the altered state of consciousness in which healing is performed. It is a commonly understood word, and its undoubted ambiguity is perhaps exaggerated by Katz. The following quotations show how ten writers, all of whom have worked with the Kalahari San, use 'trance' to denote the altered state of consciousness experienced by the San. Lee (1984: 103): 'Specially trained healers are able to enter trance and heal the sick.' Lee also entitled an article 'Trance cure of the !Kung Bushmen' (1967). Howell (1979: 51): 'During a crisis of acute illness or serious injury, one of the responses of the group is to organise one or many sessions of trance curing.' Shostak (1981: 10): 'The central ritual event in traditional !Kung life is the medicinal trance dance.' Marshall (1969: 349): 'After an hour or two the medicine men begin to go into trance and to perform their curing rite.' Guenther (1986: 253): 'The explicit purpose of the shamanistic trance dance ... is to heal.' Marshall Thomas (1988: 124): 'Gai was in deep trance.' Heinz (1975: 28): 'Thus while the women create the men's frenzy and trance, so also do they protect and keep them from harm.' Silberbauer (1981: 176): 'He begins to go into trance ... In some men the trance is cataleptic in character, with a low pulse rate and skin temperature, whereas others have very fast pulse rates (up to 204 beats per minute) and sweat profusely.' Barnard (1979: 75): 'When in full and violent trance, immediately prior to the curing stage, medicine men walk towards the fire, and often into it.' Biesele (1978: 929, 933): 'If a young man is being given the power to trance and cure, he might feel his *n//au* tingle ... Then he launched into a long story describing his trance-journey to sky using the *n/um* of the supernatural giraffe.'

Hromnik's assertion that !Kung San healing is devoid of what most people call 'trance' is clearly nonsense. A good deal of his argument is undermined by recognition of

this fact.

4) Hromnik: 'The word shaman is in fact absent not only from the existing Xhosa and Bushman dictionaries, but also from the vast majority of African languages.'

It would be absurd indeed to seek 'shaman' in a Xhosa or a Bushman dictionary or in 'the historical record'. To the best of my knowledge no one has claimed that M. used this word. Nor is the use of it by anthropologists and archaeologists in the southern African context as recent as Hromnik claims: Hewitt used it as long ago as 1976. In any event, Dowson and I have explicitly commented on its origin and stated what is meant by the word:

> 'Shaman' is a Tungus word from central Asia. It has been accepted in the anthropological literature to mean someone in a hunter-gatherer society who enters a trance in order to heal people, foretell the future, control the weather, ensure good hunting, and so forth. Some societies have only a few shamans, whereas others, like the Bushmen, have many. Some - not all - shamans in North America and Asia seem to suffer from psychological disabilities and play little part in general life. By contrast, Bushman shamans are ordinary people who also perform the everyday tasks that those who do not have their supernatural abilities perform. Bushman shamans are not a privileged class (Lewis-Williams and Dowson 1989: 30-1).

Other writers who use 'shaman' to mean San healers include Winkelman (1989: 19), Hewitt (1986) and Guenther (1986). Moreover, Winkelman and Dobkin de Rios (1989: 57) write of 'the strong similarities of the !Kung Bushman medicine activities with shamanic activities in other parts of the world', and Halifax (1980) includes Biesele's transcription of a !Kung healer's account of trance experience in her book *Shamanic voices*.

Hromnik himself may not like the use of 'shaman', but there is no point in implying that Jolly and I thought M. used the word, or that the San do not have people who can be called shamans. The word's defined use in southern Africa is certainly not 'inadmissible'. This kind of debating trick that plays on readers' supposed ignorance of the literature (though in this instance they are probably not ignorant) has no place in academic discourse (see also 3 above).

5) Hromnik: 'None of Bleek's writings cited by Lewis-Williams (1981: 103-16) expressly states that any of the described practices, dreams or beliefs involved trance, and Bleek's *A Bushman dictionary*, 1956, has no entry for trance.'

Having tried (surely unsuccessfully) to persuade his readers that Kalahari San curing is 'devoid of trance' and that the San do not have people who can be called 'shamans', Hromnik tries to clinch his argument by extending his 'tranceless San' myth to the 19th-century southern San, but his assertion, given above, merely betrays his lack of understanding of ethnography in general and of the Bleek family's work in particular. To look up the English word 'trance' in the Bleek *Bushman dictionary* and then, on not finding it, to argue that the /Xam San did not experience what we understand by 'trance' is naive in the extreme.

The W. H. I. Bleek and L. C. Lloyd Collection includes, amongst other documents, a literal translation in English of nearly 12 000 pages of texts taken down verbatim in phonetic script from /Xam San informants, largely in the 1870s. The *Dictionary*, though started by W. H. I. Bleek, was finalised by his daughter, D. F. Bleek. The /Xam entries were compiled, not from first-hand interviews with San, but from the manuscripts. Each /Xam word used in the manuscript was entered, together with its translation and some examples of its use. The *Dictionary* is therefore not a comprehensive work.

No /Xam word is translated as 'trance', and it is doubtful if W. H. I. Bleek and Lloyd understood the full significance of what they were taking down in the accounts of shamanic activities. For instance, when W. H. I. Bleek was transcribing what I argue is an account of the trance experience in which 'shamans of the rain' (*!khwa-ka !gi:ten*) captured a 'rain-animal' (*!khwa-ka xoro*) (Lewis-Williams 1981: 103-16), he noted that it did not appear to him to be 'literal', but 'the sense is apparently the reverse' (B. 2540 rev.). The San themselves did not distinguish clearly between 'real' experiences and trance experiences, and this posed difficulties for the early ethnographers. We must remember that Bleek never lived with San and thus did not actually witness the activities of which his informants spoke (Lewis-Williams 1981: 25-37).

The /Xam did, however, have a word (*/xau*) that the Bleek family translated into 'go on a magical expedition' (e.g. D. F. Bleek 1935: 22-3, 29, 31; 1936: 132; 1956: 363-4); in other words, what we would call a sense of dissociation from one's body, one of the fundamental features of trance. The 'magic power', or 'sorcery', as the Bleek family had it, employed on these occasions was variously called *!gi:* (e.g. D. F. Bleek 1935: 12, 29, 32, 35, 36), *//ke:n* (e.g. D. F. Bleek 1935: 13, 26, 28, 31, 33), and */ka:ode* (e.g. D. F. Bleek 1935: 11, 28, 35; for a discussion of these three words see Lewis-Williams 1981: 77). Lloyd translated *!gi:-ta didi* as 'sorcery' (D. F. Bleek 1935: 12); literally, it means 'the deeds (or activities) of supernatural power', what we, in an etic way, would call the hallucinations of trance.

A person who possessed this power was called a *!gi:xa* (pl. *!gi:ten*). The suffix *-xa* means '-ful' (D. F. Bleek 1956: 255), as in the English word 'powerful'. W. H. I. Bleek and Lloyd translated *!gi:xa* as 'sorcerer', and D. F. Bleek devoted three instalments of extracts from the manuscript collection to the activities of these people (D. F. Bleek 1933, 1935, 1936). A *!gi:xa* was thus a person who was 'full' of supernatural power that he or she could use to heal, go on extracorporeal journeys, put sickness into people, turn into a lion or other creature, make rain, protect people from thunderstorms, and so forth (Lewis-Williams 1981: 75-116), all tasks performed by shamans in various parts of the world. The Kalahari !Kung use the phrase *n/um k"au* (pl. *n/um k"ausi*) which means, literally, 'owner of supernatural power', clearly a similar concept to the /Xam *!gi:xa*. Kinachau (see point 3 above) was a *n/um k"au*.

The /Xam *!gi:ten* are said to have entered a frenzy in which they bit people (e.g. D. F. Bleek 1935: 1, 23), trembled so violently that they had sometimes to be held down (*ibid.*: 2, 13, 23), sniffed sickness out of people (*ibid.*: 3), turned into animals (*ibid.*: 7, 16, 17-9, 23, 43), bled from the nose (*ibid.*: 12, 19; 1936: 137), and in which their veins 'stood up' and became rigid (1935: 23). During this dangerous time other people danced and sang (*ibid.*; L.V.3.4124 rev.).

The /Xam also believed that shamanistic activities took place in less violent circumstances. One of these states was dreaming, as is the case in numerous shamanistic societies. Dreaming is an altered state of consciousness in its own right that some shamans do not distinguish clearly from trance induced in very different circumstances. The /Xam, for instance, believed that a *!gi:xa* who trembled at night was combating dangerous *!gi:ten* in the spirit realm (Bleek

1935: 13): 'That is why we sometimes hear a sorcerer shivering at night; when other sorcerers come to him, then he shivers, because the others want to see whether his veins are still alive.' In this account *!khauken*, the word translated as 'shiver', means to beat or tremble; frequently it means to tremble in trance (Bleek 1956: 425, see section 3 above). One of W. H. I. Bleek's informants put it like this: 'He lies asleep by us, his magic [*//ke:n*] walks about while we sleep' (Bleek 1935: 30). Moreover, the /Xam word for dream, *//kabbo*, could be used in an active way in the sense of causing things to happen. One of W. H. I. Bleek's best informants (whose name was //Kabbo, 'Dream') said, 'I dreamt that I told the rain to fall for me ... The rain assented to me, the rain would fall for me' (L.II.6.625). It is worth recalling that in REM sleep it is possible to induce and manipulate dream imagery (Noll 1985: 44). Indeed, it is not always easy to distinguish between shamans' accounts of dreams and trance visions (Eliade 1972: 33-66). Hromnik's assertion that /Xam dreams did not involve trance is thus a further misunderstanding of altered states of consciousness, shamanism, and the /Xam texts (for more on San dreaming see Lewis-Williams 1987a).

The behaviour of, and beliefs about, the *!gi:ten* show that they were the /Xam equivalent of the !Kung *n/um k"ausi* - in other words, what I and other writers believe can be called 'shamans' who enter 'trance'. Trance was indeed a central and pervasive component of San belief, and the connection between it (together with its corollaries of supernatural power, shamans, transformations and out-of-body travel) and rock art should come as no surprise.

These five examples illustrate the striking lack of care in Hromnik's piece. Indeed, it is curious that the referees, if they were at all informed about southern African rock art research, allowed these and other points to pass. [One of the two referees is a specialist on southern African rock art, the other on the ethnographic study of rock art; both recommended publication of Hromnik's paper. Ed.]

As regards Hromnik's criticism of my own work and that of many other writers, all I can say is that it is clearly impossible for me to rehearse it all here; points 3, 4 and 5 above give some idea of the complexity of the material. Suffice it to say that M.'s 'contrived language', her descent (no one claimed she was 'pure "San" '), the fact that she 'has been isolated from a functioning San society' for a 'long period' (Lewis-Williams 1986: 10) were points all discussed in Jolly's and my original reports. Certainly, she herself did not use 'San' which, as Hromnik knows, is a Nama (Khoikhoi or, in his terminology, 'Quena') word that has been used by anthropologists and archaeologists to avoid 'Bushmen', a word many consider pejorative (this is a much debated point). She was known locally as, and claimed to be, 'Batwa', the Xhosa word for 'Bushman'.

The historical record shows, as Jolly and I pointed out, that San were living in caves in the area towards the end of the 19th century: 'There is a family of Bushmen still living in a cave ... who are considered the great rainmakers of their country' (Gibson 1891: 34). For this and other historical reports see Jolly (1986: 6). Subsequently, these families lived with the Pondomise. There is therefore evidence that M. had access to what Hromnik calls an 'environment where she was able to acquaint herself with details of the Bushman religious and cultural life as well as with the artistic traditions and artists' conventions of religious symbolisms'. The remarkable fact that she knew things that she could not have known had she not had access to a genuine San tradition cannot be circumvented. The argument is certainly not 'circular'. That all her remarks must nevertheless be very carefully assessed is a point that both Jolly and I were at pains to emphasise:

> Because her tradition is quite independent of the ethnography with which we have been working, I believe we can go so far as to speak of corroboration of the shamanistic interpretation ... On the other hand, where her remarks contradict the ethnography and where she remarked on matters on which the ethnography is silent we shall have to reserve judgement. Too much of her testimony was false to allow us to accept novel explanations with confidence. We must remember that, if it were not for the San ethnography, we would not be able to discriminate between acceptable and doubtful statements. Because the ethnography, a compilation of many informants from different times and places, is consistently more reliable than M's testimony, it must remain the final arbiter (Lewis-Williams 1986: 11).

We should similarly assess Hromnik's tendentious and selective recension of southern African rock art research. I therefore suggest that readers rather peruse my publications and those of numerous other writers (not just the ones Hromnik cites) first hand and then make up their own minds about the 'meaning' and the social conditions of the production of southern African rock art.

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REPLY

By CYRIL A. HROMNIK

To judge by Lewis-Williams' response, which is about the same length as my original article, my questions about the admissibility of certain evidence behind the hallucinatory trance theory of the shamanistic origin of southern African rock art must have shaken the core of the theory. Instead of limiting himself to the defence of the dubious 'San' and 'shamanistic' credentials of the still mysterious Mpondomise M., Lewis-Williams attacks my entire 'scholarship', although my rock art portfolio represents only a small fraction of my historical work. He does this at the most trivial level.

The correction that Mrs How was the wife of a police officer rather than a missionary's wife can scarcely affect the flow of shamanistic power to the rock art produced by a fake Sotho 'artist' with ointment concocted from the paint and ox blood supplied by an European shopkeeper and a butcher in town. I take Lewis-Williams' failure to say a single word in defence of this piece of evidence, so crucial for his theory, as a straightforward admission of its inadmissibility. How trivial can a self-proclaimed first 'scientist' in rock art studies (Lewis-Williams 1983: 5) be when the rotten roots of his theory, growing from the soil of manipulated evidence, are revealed?

Diversion is a useful tactic in military planning, but it does not answer my question as to which part of the shamanistic Kung (Bushman) 4) mythology inspired the

4) I use the indigenous name Kung (unglicised spelling) for the aboriginal hunters of southern Africa who used to be called 'Bushmen' in early and 'San' in more recent literature. Unlike other names, given to these hunters by either the Europeans or the Quena, Kung was used by the very people it designated. !Kung with the initial click is the name of a

father of Mpondomise M. to paint 'crude blotches and strokes above the heads of a series of finely painted dancers'. Recent research by Prins confirms my assessment when it reveals that both M.'s father, Lindiso, and his Mfengu wife 'lived in a traditional southern-Nguni dwelling', and 'both M and her oldest sister, Chitiwe, were born amongst the Mpondomise and were treated as such'. They all were accepted as Mpondomise. By all indications, Lindiso's non-Bantu ancestry, as well as that of all other so-called 'San' of the Eastern Cape and Transkei, was Quena or Soaqua Quena, NOT Kung (Bushman). Furthermore, none of those interviewed by Prins had any 'knowledge of rock art' (Prins 1990).

Mpondomise M. (Maqhoqha) claimed that her father had painted in Ngcengane cave, but this is not certain because, quite apart from her young age, she was 'never allowed to watch while he was painting'. [Concerning the name of this 'cave', it is to be cautioned that Prins uses three differing spellings in the one paper. Ed.] Her explanation of the paintings was 'realistic or empirical', not shamanistic, hallucinatory or trance-related. In fact, 'none of the accounts given [to Prins] by the informants make explicit mention of trance in relation to rock art and rainmaking rituals'. The published results of Prins' investigations came to my attention only after I had submitted my assessment of Lewis-Williams' 'The last testament of the Southern San' to *Rock Art Research*, but it fully vindicated my conclusion that Mpondomise M. has been created for the sake of giving an imprimatur to the pseudo-scientific hallucinatory trance theory that credits all rock art to the trancing Bushman ('San') shamans. The testimony of Mpondomise M. does not bear any reference to that theory. In her experience, as well as in that of other informants, the rainmakers did not go into trance when they submerged themselves in the river: they literally jumped into the real, physical water (Prins 1990).

That the Bleek and Lloyd Collection is at the University of Cape Town is an obvium. That Hewitt's work has been published and reviewed has been noticed. But we still do not know the source of the alleged shamanistic inspiration of the Mpondomise Lindiso. Jolly (1986: 7) cites Hewitt (1976: 212, p. 284 in Hewitt 1986), and Hewitt refers to 'Bleek 1931-63, Part V, pp. 298', where there is no reference either to shamanism or to the zebra stripes. That Lewis-Williams used Hewitt's adapted, and therefore more convenient, information without ever finding the original text seems highly likely; hence his reluctance to give the page of his information here. This procedure is beyond the pale of the historical profession but, so far, it has been a good gamble in the pseudo-scientific shamanistic rock art studies. No archaeologist has so far questioned Lewis-Williams' sources, and he is understandably upset by the failure of the present referees to censor my critical article. He is used to the practices in *The South African Archaeological Bulletin*, where no article of this kind ever passes through the sieve of the editor and her shamanistic and hallucinatory trance club of referees.

That at least ten writers have used the word 'trance' in connection with Kung medicine dancing (Lewis-Williams' phrase 'the altered state of consciousness experienced by the San' is not necessarily used by all the writers he quotes) is in no way confirmation that Kung healing practices involved shamanic trance. Marshall Thomas, possibly the earliest to use the words 'trance' and 'shaman', does so without a single word of explanation of what she means by these terms. From her text we gather, however, that every boy who had been 'initiated into manhood' is 'a medicine man', i.e. 'shaman', and what these so-called 'shamans' experience in their often very casual medicine dances is apparently a 'trance' (Marshall Thomas 1959/69: 130). Absence of any reference to the studies of Shirokogoroff, Eliade and other serious students of shamanism in the pre-1987 writings of Lewis-Williams is a sure indication that his adoption of the term 'trance' and 'shamanism' in the Kung context was more casual and convenient than considered. His own definition of 'shaman' is an afterthought (Lewis-Williams and Dowson 1989: 30-1), tailored so closely to the needs of the Bushmanocentric shamanistic hallucinatory trance theory that it is useless as far as the objective study of shamanism is concerned. It was published in a book, which, in the opinion of its reviewer, Professor Argyle, was addressed to 'an audience presumed to be largely, if not wholly, ignorant of the [rock] art' (Argyle 1990: 64).

What Lewis-Williams' ten writers describe as 'trance' often did not add up to more than a state of exhaustion after a heated dance. The *moqoma* dance, performed by the Maluti Quena (not Kung) in 1837 and used as an important building brick in Lewis-Williams' theory that 'Nasal bleeding is indeed one of the clearest diagnostic features of painted men ... [in] trance', was described by Arbousset and Daumas only as an ordinary 'amusement', a 'ball' and a favourite 'merry making' in which one man sank to the ground 'exhausted and covered with blood' (1846: 246-7). Recasting it as a shamanic dance with 'San dancers falling in trance' (Lewis-Williams 1985: 31) falls nothing short of a deliberate distortion of the historical record.

The main theoretical error lies in the false synonymy between the trance and the ordinary heated and religious dances of the Quena, which reappear among the Kung in the form of *!kia*. Lewis-Williams appears to be aware of this false synonymy and, while quoting Katz (1976) in his fundamental work on the hallucinatory trance theory and elsewhere, he carefully avoids mentioning *!kia* - the linchpin of Katz's findings - preferring to use terms like 'trance experience' or 'hallucinations of trance' which are culture-free and unfettered by the historical record. This is a surprising and unexplained avoidance in a book that uses no less than 66 Quena and Kung words to promote its hallucinatory thesis (Lewis-Williams 1981: 78, 81, 114 and *passim*). Thus, Katz writes about a 'teacher ... transferring the sweat' and snapping fingers at the student while teaching her 'to kia'; but in Lewis-Williams' rendering, 'Katz noted that, in teaching a novice to *trance*, an experienced *shaman* may snap his fingers' (Lewis-Williams and Dowson 1989: 48, emphasis added; cf. Katz 1982: 168). The distortion of the reality in this rephrasing and in numerous quotations and citations from Katz and other scholars is obvious. This preference is well thought through: *!kia* leaves no room for shamanistic rock art, hallucinatory trance seemingly does.

But the non-hallucinatory and non-shamanic *!kia* would not remain under the carpet, and Lewis-Williams is forced to take a stand on it, for the first time perhaps (I may have missed an article of his). Firstly, he insinuates that only 'In *some* publications, Katz ... prefers to retain the !Kung word *kia*' (in which does he not?). Then he quotes extensively from Katz (1982: 8, 65-6) to prove the *!kia* = trance equation. However, he carefully avoids the sentence which states that 'kia has been translated as "trance", which is

particular Kung group in the Kalahari.

misleading' (Katz 1982: 95, emphasis added). One of his disciples is much less bashful and inserts the word 'trance' as an explanation of *!kia* into a quote from Katz: 'In *!kia* [trance] ...' (Dowson 1989: 88; cf. Katz 1982: 46). This kind of manipulation of the evidence is inadmissible in respectable scholarship. *!Kia*, being much less than a shamanic trance, is accessible to any ordinary participant in the dance. Healers are 'more harmonious with kia' than non-healers (Katz 1982: 235). And Katz's observation that 'Through kia, the Kung participate in what Westerners call the "religious-spiritual" dimension' is quite sobering (Katz 1982: 43). Weighing the casual use or calculated misuse of the word 'trance' by non-experts against the considered and well pondered (over ten years) (Katz 1976: 284) opinion of the only expert in the field, a clinical psychologist, I reject Lewis-Williams' charge that Katz 'exaggerated' when he refused to accept the *!kia* = trance equation. Introducing the concept of trance and shamanism into Kung society is an exaggeration that distorts the cultural identity and the historical record of the Kung. Only a San ethnography created by Lewis-Williams, aptly called 'his own ethnography' by Argyle (1990: 65), can accommodate it.

W. H. I. Bleek was well versed in the religions of India, the cradle of trance and shamanism. Yet he found nothing in the stories of his Kung informants that could be described by these terms; and Lewis-Williams' allegation that Bleek and Lloyd did not understand 'the full significance of what they were taking down in the accounts of shamanic activities' is preposterous. Bleek's daughter, Dorothea, consulted contemporary scholars on this subject before she published her father's and her aunt's materials, and the only terms she found adequate to describe the Kung healers were 'witchdoctor', 'medicine man' and 'sorcerer' (Bleek 1924: 26, 1935: 1 and *passim*). These healers bore no resemblance to the shamans of Asia, for 'they do not seem to have any special dress or go through any ceremony, they are just individuals who have managed to make others believe in their supernatural powers, probably having acted as assistant to some former sorcerer' (Bleek 1924: 26). The 12 000 pages of Bleek's text, counted and misrepresented by Lewis-Williams, do not alter this fact.

D. F. Bleek recognised that some of the religious ideas of the Quena (Hottentots), the Indo-African origin of which has already been at least partially explored (Hromnik 1985, 1990b: 26-34, 1991: 283-90), had penetrated into Kung religious thinking (Bleek 1924: 26). It is also significant that a greater part of the stories about the 'sorcerers' collected by Lucy Lloyd came from the mouth of her Quena informer, *Diä!kwain* (Bleek 1935: 1-47). The central figure in Lewis-Williams' theory, the 'sorcerer', bears a Hessaqua Quena name, *!gi:xa* (pl. *!gi:ten*), and the same could be shown to be true of the majority of the so-called Kung (Bushman) terms that have crept into our literature dealing with the rock art and the religious beliefs of indigenous southern Africa. The limited space available to me precludes any further comments or analysis in this direction. These will have to be presented in a separate essay or essays.

No 'etic way' allows us to transform borrowed magics and naive beliefs, e.g. deceased Dr Bleek coming to visit his children in the form of an owl (*Diä!kwain* in Bleek 1911: xv), into the 'hallucinations of trance' or 'shamanistic activities' of the Kung as suggested by Lewis-Williams. Otherwise every preacher in the 2000 years of Christendom could be called a cardinal or a pope and every boy who prays on his knees would be a Jesuit father. Sergeants could be called generals and the relationships of power would lose their earthly anchorage. The chaos that this frivolous treatment of evidence would bring into our historiography is scarcely imaginable. My insistence on the use of authentic and justified terms such as *!gi:xa*, 'medicine man', 'sorcerer', *!kia*, *moqoma* etc., and avoiding the culturally and historically incompatible terms 'trance' and 'shaman' is not a matter of liking or disliking the word 'shaman', as Lewis-Williams would like to impress upon his readers. It is a matter of the obligation by both of us to preserve the sense and right proportion in the historical and rock art studies of Africa.

In this context Lewis-Williams' belief expressed here, that shamanism and trance constituted a 'central and pervasive component of San belief', is no more than wishful thinking. Hewitt, in his study of *Structure, meaning and ritual in the narratives of the Southern San*, treats this 'central component' as an afterthought relegated to a short appendix (is it included in the original 1976 thesis?). He employs the term 'shaman' casually and sparingly, and points out that the term ' "magician" ... makes the sense clearer' (1986: 39, 99, 138, 287, 296, 298-9). Lewis-Williams' repeated claim that this fiction is stroke for stroke reflected in the rock art (Lewis-Williams and Dowson 1989: 32, 38-164) must come as a surprise to every scholar trained in critical and careful handling of the evidence. The chasm between the rock art which occurs by and large in the area of ancient Quenaland and the Kung of the Kalahari, who have no tradition of painting on rocks and know nothing about the origin of this art, remains unbridged. The shamanistic hallucinatory trance theory of the 'Bushman' or 'San' rock art is based on an admirably skilful use of the English language and a great deal of manipulated and distorted evidence. As one reviewer has sadly observed with regard to Lewis-Williams and Dowson's *Images of power*, 'There is no scholarly objectivity' in this theory: 'it is the work of a clever advocate making a case, not of a truth-seeking judge' (Willcox 1991).

Thus, Biesele was told by an old !Kung 'curer' named K"xau about his *!kia* healing experiences, which she rendered as 'trance'. Apparently in the absence of better stories from Africa, Halifax included the story in her *Shamanic voices* in 1979, but neither she nor Biesele dared to call the old 'curer' a shaman (Biesele 1975: 151-3; Halifax 1979: 54-62). Wisely, Halifax left this story out of her more recent book about shamans (1982). However, Lewis-Williams and Dowson do not shrink from retelling the story as if the old K"xau had referred to himself as 'shaman' (1990: 14). The trick played here and elsewhere is that of inserting a convenient word in texts where it does not belong. Can we call a band of armed Kung *Wehrmacht*? Lewis-Williams charges here that !Kung healing devoid of trance 'is clearly nonsense', but I take comfort in the fact that Biesele, who casually translated *!kia* as 'trance' in 1975, has by 1986 recognised the error and adopted Katz's stand that this translation is 'misleading'. Significantly, their combined study of !Kung healing is absolutely devoid of any mention of shamans (Katz and Biesele 1986: 197; Katz 1982: 95).

The fundamental evidential weakness of this speculative theory, which grew out of an apartheid environment that favoured an extremely narrow Afrocentric and Bushmanocentric interpretation of the rock art and to this

purpose allowed for the unrestrained shamanic flights of fancy, is admitted by its founder, who feels it necessary to appeal to readers to do what they have been doing for the last ten years: to read again his many hallucinatory publications before they make up their minds. Apparently, doubts about the viability of the shamanistic trance interpretation of the rock art are serious, and are growing. This is not surprising, for there is 'a certain carelessness regarding truth' in the writings of the hallucinatory trance school (Willcox 1991). The 'shamanic rock art' illustrating Lewis-Williams' tractates is so stereotypical and emphatic on exactly the points called for by the hallucinatory trance theory that it looks factory made. 'I noted at the University of the Witwatersrand in 1988, at their rock art symposium', writes Genge, 'that the printed copies of tracings did not match the tracings themselves. It therefore seems that the copies of the tracings may not even be useful in many respects iconographically' (Genge 1990). Slowly, Lewis-Williams' shamanic trance path is veering off the field of serious scholarship and into the academic wilderness. Fortunately, in true scholarship and science, no shaman or self-appointed 'scientist' is allowed to tell his colleagues when 'the debate has in fact ended' (Lewis-Williams 1987b: 174).

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Résumé. *La prémisse fondamentale et incontestée dans l'étude contemporaine de l'art rupestre du Sud de l'Afrique est l'opinion générale, cependant jamais prouvée, que cet art est d'origine boschimane (plutôt que quena ou hottentote). Cet article n'aspire pas à prouver ni à réfuter cette opinion. Plutôt, il pose une question légitime concernant la validité de l'évidence récente en faveur d'une ramification théorique de cette opinion, c'est-à-dire, la déclaration que 'les représentations peintes étaient principalement associées à l'expérience de la transe par les shamans [boschimans]' et que 'peut-être que la plupart des peintures ont été exécutées par ces shamans'.*

Zusammenfassung. *Die grundsätzliche und unangezweifelte Voraussetzung in derzeitigen Felskunststudien im südlichen Afrika ist die weitverbreitete, aber nie erwiesene Annahme, dass diese Kunst von Buschmann- (im Gegensatz zu Quena oder Hottentott) Ursprung sei. Der Artikel sucht dies weder zu beweisen noch zu widerlegen. Statt dessen erhebt er die berechtigte Frage über die Gültigkeit mancher der jüngsten Evidenz in der Unterstützung eines Seitenzweiges dieses Glaubens: die Forderung, dass 'die gemalten Darstellungen in erster Linie mit den Trance-Erfahrungen von [Buschmann] Schama-nen verbunden sind', und dass 'vielleicht die meisten Malereien von den Schamanen selbst hergestellt wurden'.*

Resumen. *La premisa fundamental e indisputable de los estudios actuales de arte rupestre en el Sud del continente africano resulta la ampliamente compartida, aunque nunca probada, creencia de que este arte es de los 'Bushmen' (en oposición a los 'Quena' o los 'Hottentot') en origen. Este artículo no pretende probar ni refutar esta creencia. En cambio, plantea una pregunta legítima acerca de la validez de una parte de la más reciente evidencia en favor del origen teórico de esta creencia, a saber, la afirmación que 'las representaciones pintadas estaban principalmente asociadas con la experiencia de trance de los chamanes [Bushman]', y que 'tal vez la mayoría de las pinturas fueron hechas por los chamanes mismos'.*

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RAR 8-207

KEYWORDS: **Petroglyphs - Rock paintings - Kola Peninsula - Arctic - U.S.S.R.**

ROCK ART OF RUSSIAN LAPLAND

Vladimir J. Shumkin

Abstract. The few rock art sites discovered on the Kola and Rybachiy Peninsulas in the extreme north-west of the U.S.S.R. are among the northern-most sites in the world. Petroglyphs and rock paintings are both represented, and the sites are briefly described here. The art is seen as an eastern extension of the northern Scandinavian/Finnish art body, it bears little resemblance to the Karelian rock art to the south.

The discovery of petroglyphs in the central part of the peninsulas Kol'skij (on the Chal'mn-Varra River, in 1973) and Rybachiy (on the Pjaive and Majka Rivers, in 1985-86) join eastern (Russian) Lapland to the rock art region of northern Finno-Scandinavia (Fig. 1).

The Chal'mn-Varra petroglyphs are located on the right bank of the Ponoy River, about 72 km from the village Krasnoschel'e, in what used to be Ivanovka country. The motifs have been produced by percussion and occur on ten variously sized boulders at or near the river bank. The contours of two former water levels can still be recognised clearly. We have been successful in defining different styles, techniques and compositions and in relating these to the former water levels.

The first group of petroglyphs occurs directly at the present shore. They are characterised by a 'silhouette and line style', comprising pictures of deer-like quadrupeds, shown in profile with one pair of legs, and three anthropomorphous figures in frontal view. One of the anthropomorphs displays three fingers at each hand, a tripartite headdress and a phallus (Fig. 2), another seems to have a tail. Pecked lines join anthropomorphs to animal figures, perhaps hinting at a compositional relation, but there are no superimpositions.

Approximately 150 motifs are located at a higher level. This group consists of line-contour figures of 'deer', anthropomorphs and fantastic beings, so-called 'solar marks' and cupules (Fig. 3). The arrangements are complex and the figures possessing little depth are faint, human figures tend to be more detailed. The density of the images is such that there are often superimpositions, in which existing figures are not supplemented or altered; they resulted from the creation of similar but later figures.

Geological and geomorphological observations suggest that the lower of the two groups may date from the end of the 3rd or the beginning of the 2nd millennium B.C., while the upper group may be of the 2nd or 1st millennium B.C.

Two sites with rock art found in the north-west part of Russian Lapland, on the Rybachiy Peninsula, were named the 'Gallery' and the 'Cave'. One (Fig. 4) is situated on the Pjaive River's right bank, some 1.5 km from its mouth. The art is located on the blocks under the roof of a rock-shelter. It consists of complex geometrical arrangements (25) and 'deer' figures. All paintings are in red ochre, applied with fingers in a linear technique (Fig. 5). The

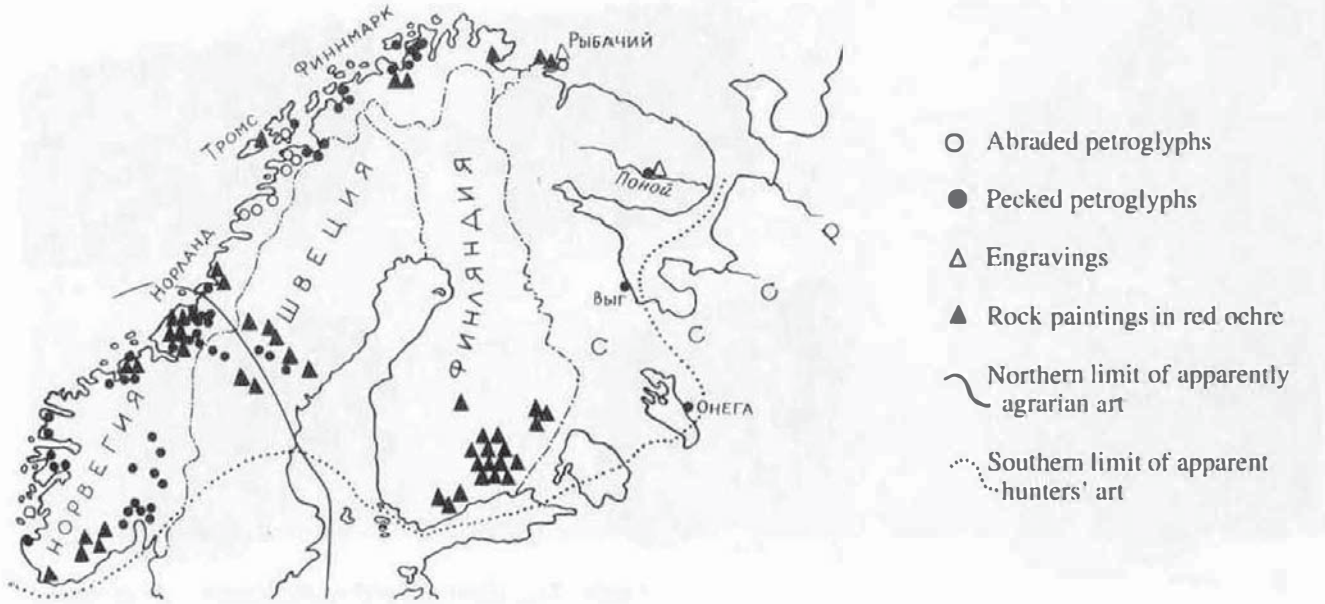

Figure 1. The principal occurrences of rock art in Finno-Scandinavia.

Figure 2. Petroglyphs on Rock 1, Chal'mn-Varra, early group.

Figure 3. Rock 5, Chal'mn-Varra, late group.

Figure 4. *The 'Gallery', Rybachiy Peninsula.*

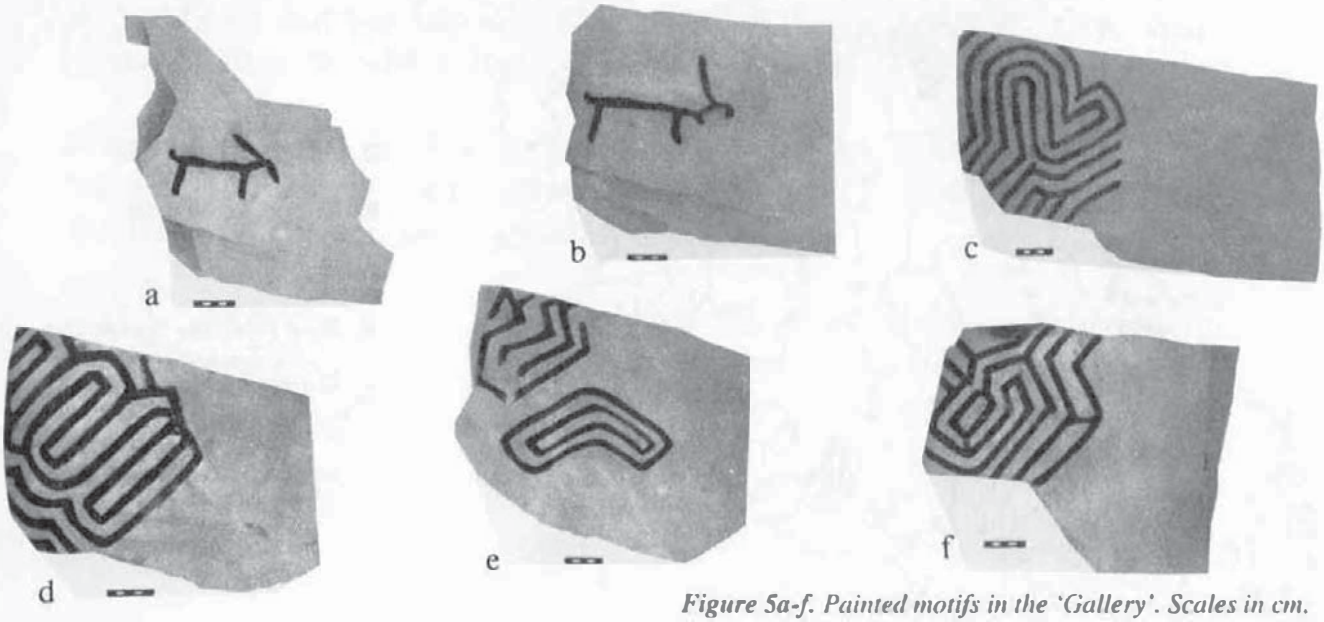

Figure 5a-f. Painted motifs in the 'Gallery'. Scales in cm.

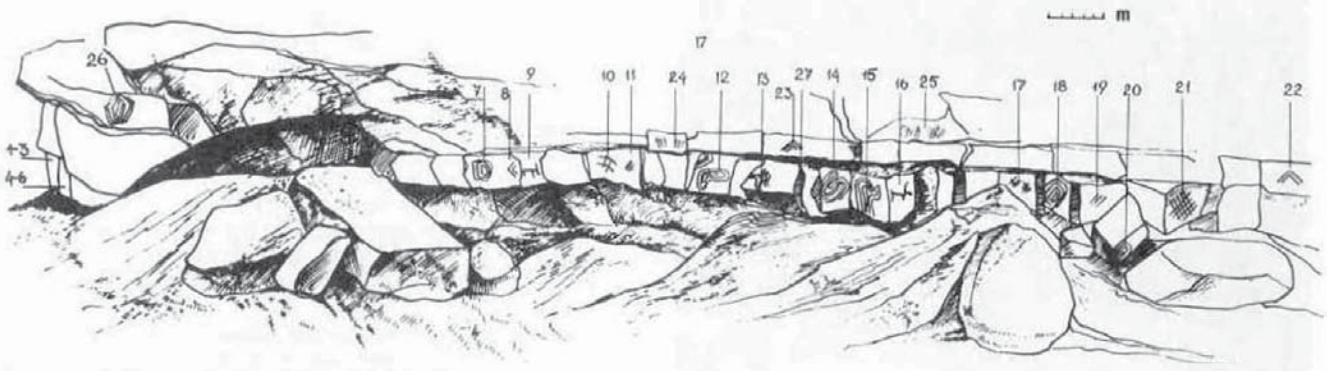

Figure 6. General view of the 'Gallery'.

uniformity of style and technique suggests that the figures are all of similar age. Our preliminary dating, based on archaeological reasoning, places them in the 5th or 4th millennium B.C. (Fig. 6).

The engraved figures in the 'Gallery' are of geometric forms. Superimposition of incised lines over painted marks is shown in Figure 7a. The majority of these motifs, located higher than the paintings, appear to have been made with sharp metal tools. The stylistic similarity of the engravings and some of the painted motifs is evident, but the authors of the more recent marks seem to have imitated the paintings, probably without completely understanding their original symbolic content. Evidently the presence of the earlier paintings was more significant in prompting the production of the engravings than the orientation of surfaces, their disposition or the suitability of the rock surface. The lower age limit of the petroglyphs is suggested to be the 1st millennium B.C.

The 'Cave' (Fig. 8) is a hollow formed under a horizontal rock stratum, situated

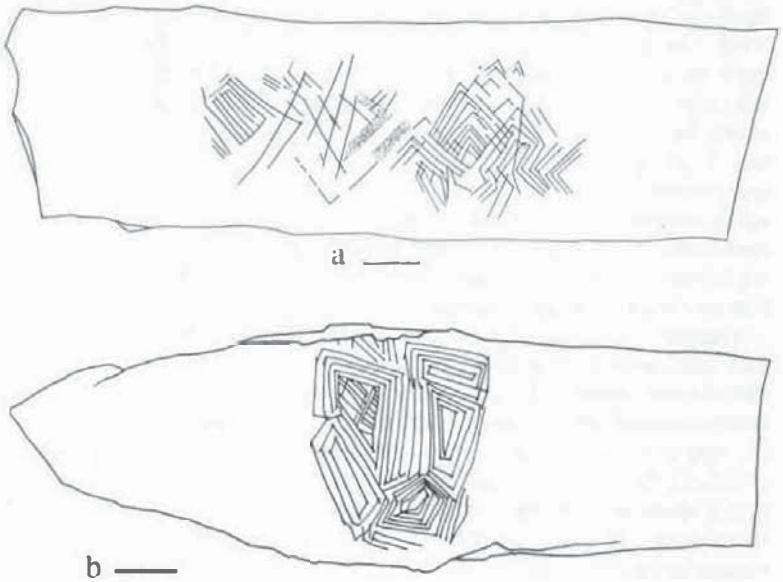

***Figure 7a, b.** Incised petroglyphs in the 'Gallery'. Scales in cm.*

Figure 8. The 'Cave', Rybachiy Peninsula.

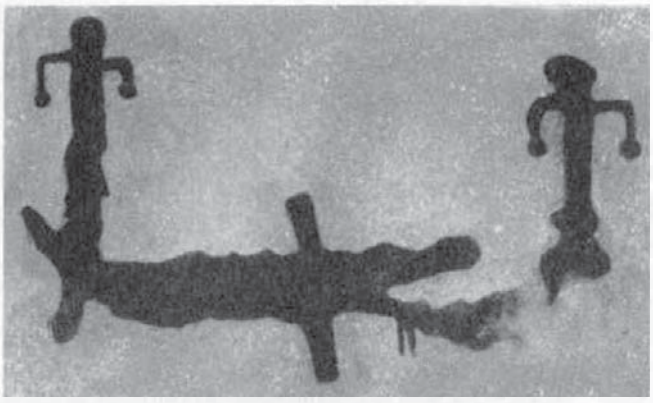

Figure 9. Painted motifs in the 'Cave'. Scale in cm.

about 1.5 km from the 'Gallery'. It is located on the left bank of the Majka River, at an altitude of 23 m above sea level. The rock art is found in a remote, dark part of the cave, on a flat vertical panel above a large rock slab with a flat upper surface. The figures are again drawn in red ochre, but in a technique quite different from that used in the 'Gallery' drawings. Three figures seem to form a composition (Fig. 9). The two vertical anthropomorphous images evidently depict a male and a female figure, with round heads, high shoulders and closed legs. The central horizontal figure resembles a sea animal or a fantastic monster with wide-open mouth.

Despite some stylistic peculiarities in the rock art of Russian Lapland, its similarity with the art of Finno-Scandinavia is apparent. A particularly important objective for future research in the general region is the examination of the apparent discontinuity in the development of the art traditions from the early periods to recent times. However, this gap in the record may simply reflect a gap in our knowledge about the ancient history of northern Finno-Scandinavia.

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Résumé. Les quelques sites d'art rupestre découverts sur les Presqu'îles de Kola et de Rybachiy à l'extrême nord-ouest de l'U.R.S.S. sont parmi les sites les plus au nord du monde. Des gravures et des peintures rupestres sont illustrées et les sites sont brièvement décrits. On maintient que l'art représente une extension orientale du corpus d'art du nord de la Scandinavie-Finlande. Il ne ressemble guère à l'art rupestre carélien au sud.

Zusammenfassung. Die wenigen Felskunst-Fundorte, die auf den Kola und Rybachiy Halbinseln in äussersten Nordwesten der Sovietunion entdeckt wurden, sind unter den nördlichsten Fundorten der Welt. Petroglyphen und Felsmalereien sind beide vertreten, und die Fundstellen werden hier kurz beschrieben. Die Kunst wird als eine östliche Ausdehnung des nordskandinavischen-finnischen Kunstgebietes betrachtet, sie hat nur geringe Ähnlichkeit zur karelischen Felskunst gegen Süden.

Resumen. Los pocos sitios de arte rupestre descubiertos en las Peninsulas Kola y Rybachiy en el extremo Nor-Oeste de la U.R.S.S. están entre los sitios más septentrionales del mundo. Tanto petroglifos como pinturas rupestres están representados, siendo los sitios brevemente descritos. El arte es visto como una extensión hacia el Este del conjunto artístico del Norte Escandinavo-Finlandés. Contiene poca semejanza con el arte rupestre Karelian al Sur.

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INDIGENOUS ART AND THE LIMITS OF SOCIAL SCIENCE

Kingsley Palmer

Abstract. The paper discusses attribution of meaning and value to indigenous art, using examples collected from sites in Europe and Australia. The methodology and cultural circumstances in which this meaning is developed ensures that it is a product of the encounter with the art and not an absolute account of a symbolic graphic system. Social science, by using an epistemology that can only determine meaning and value in relation to cultural context may produce misleading results. While other approaches for analysis are available, they too are fraught with difficulties.

There are no true interpreters or sacred guardians of our culture. We are all entitled to our truths, insights and intuitions into interpretations of our culture.

(Wendt 1980: 27)

Introduction

There has been a tendency in recent times to regard rock art abstractly and analytically, leaving interpretation as a matter for final comment (e.g. Lorblanchet 1980). There have also been writers who have developed a terminology and system for classification for rock art which avoids interpretation altogether (e.g. Maynard 1977). Halverson postulates that Palaeolithic man produced the art in an inchoate cultural and psychological state that preceded conscious reflective thought (Halverson 1987: 63-71) and that the art had no specific meaning as such 1).

However, there remains a substantial school of thought whose adherents, however else they may proceed analytically, seek to accord meaning, value, interpretation and intention to surfaces that have been engraved or painted by men (or women) who are no longer around to be questioned about their work. The problem of understanding this so-called 'silent art' has been discussed by Groenfeldt (1985) who also reviews a number of analytical approaches to 'prehistoric' art.

Before proceeding there are two preliminary points to make. First, I use the term 'art' throughout this paper to mean any design (usually parietal, but not exclusively so) executed by men or women. The term does have connotations which may prejudice the understanding of what is viewed but I use the term definitively. Second, I use the term 'meaning' in the following way: meaning is some form of message or communication comprehensible to the viewer or listener.

This paper explores the attributions of meaning and value accorded by various parties and seeks to understand these attributions within their social context. In so doing I examine the relationship between ascribed meaning and value (however discovered) and the original meaning of the art. In this I attempt to determine the contribution that social science can reasonably be expected to make to this endeavour, and to demarcate some of the limitations that must be recognised in the process.

Culturally bound interpretation and the accretion of meaning

A distinction between original meaning (what the artist meant) and our own meaning (interpretation of the art) is not easily made (cf. Gallus 1985: 29). The artist may consider there to be a whole range of meanings which might constitute an understanding of his work. Some of these he might not have appreciated at the time of executing his work. Probably, most artists would not wish their art to be limited to their own particular range of meanings, even if they were prepared to specify them. An audience will respond to an art form and may (in fact probably will) do so regardless of the artist's intention 2). The range or scope of their interpretation is not limited by the art, though representational art may impose certain boundaries. It may be tempered by the artist's title, comments or the context in which the work was undertaken (or presumed to have been undertaken). A painting of a reindeer or a petroglyph of a kangaroo which is pictorially representative of something we recognise in the real world is immediately comprehensible to us as conveying an easily assimilated meaning. However, we still do not know why the painting or petroglyph was executed. Nor do we know whether the design was executed with respect to a range of symbolic referents accessible only through a knowledge of the culturally bound communication system operative at the

1) Critics of Halverson's approach note that there is no evidence that Palaeolithic man was in fact in some state of mental precivilisation (see especially Davis 1987: 75). Halverson's assertion is certainly not supported by evidence derived from a study of contemporary Australian Aboriginal art that parallels prehistoric art, for this art is clearly not the product of a system without referents (see for example, Keen 1977; Munn 1973). Just because we have been unsuccessful in understanding Palaeolithic art does not necessarily mean that the artists did not think symbolically (Davis 1987: 75).

2) Some artists might argue that their art is not meant to 'do' anything. However, once a phenomenon is created it must be taken into account by those who see, feel or encounter it. A work of art can only exist independently if no-one else encounters it - including the artist him or herself, after finishing the work.

time of execution.

The question of meaning does not end here. Ask a child why he scratches his name on a desk top, or a vandal why he wrote his name on a rock or a poem on a lavatory wall. The graffitist may not know the answer, while other specialists (psychologists for example) may presume that they have the skills to discover the implicit meanings of the designs.

There is an additional and complicating element in the process of assigning meaning: that of value. There is a tendency to attribute value to art, and so to rank it according to such things as size, cultural significance to a population, dramatic effect, sacredness, perhaps even obscurity. Attribution of value is a concomitant of prescribed meaning, and the one is derived from the other. But once prescribed, value then also becomes a part of meaning and so additionally compounds the sum of the response to the original phenomenon.

Interpretation of 'prehistoric' art is then a task complicated by the formulation of a number of necessary distinctions. 'Prehistoric' art is a topic whose content multiplies with successive commentary or description since every observation supplies a new sediment of meaning. The observer receives stimuli which are active in his or her interpretation of the art. These may come from the cave guide, the archaeological record, the Aboriginal informant, the *paysan*, the perceived replication of forms familiarly understood from their own experiences of the natural world. The final meaning is a product of a whole range of stored data concerning the ordering and rendering intelligible of the individual's known world. Even the analytical tools for describing art are themselves a part of the perception of art and bear directly upon how it is subsequently interpreted. In short, meaning is a concomitant of time, place, culture and context and is developed from (and as a condition of) our account of the original. Meaning is the product of the experience and the encounter of an individual with art. It is an account of an interaction and therein lies its intrinsic worth. 'Meaning', in this instance, tells us about a human relationship with a cultural phenomenon, not about an absolute symbolic system for graphic representation.

There is a misunderstanding on the part of some commentators about the relationship between an artist's original meaning and a viewer's response to the work of art. Indeed, there is a misunderstanding of the complexities of those very meanings and the interactional relationship between artist, art, viewer and the individual's cultural universe. Early science was dogged by a similar confusion. Some alchemists believed that base metals could be metamorphosed to gold. They expended much effort in their vain attempts to realise such a dream. Their pseudo-science was founded upon a basic misunderstanding of the nature of the elements. However, lead could not be turned into gold and neither can derived meanings of indigenous art be turned into the original meaning and purpose of the art. Physically lead bears no necessary relation to gold, neither do derived meanings bear any necessary relation to original meanings. Before progress could be made in the field of understanding elements, scientists first had to understand the nature of elements and the fact that each element was a discrete and unique substance. Similarly, in order to understand the epistemology of meaning, derived meaning and original meaning need to be differentiated as individual phenomena that are not necessarily related or relatable.

By way of illustrating these and other problems I turn to some examples of ascribed meaning and value. The first two can be broadly termed 'ethnographic parallels'. The third illustrates how the allocation of value is not absolute, but is also dependent on social context **3)**.

Ethnographic parallels: the Fortescue River petroglyphs, Pilbara, Western Australia

Ucko (1977: 8) discusses terms of reference necessary for what he considers to be the pursuit of scientific inquiry into interpretation of rock art and contrasts these with previous research methods (see also Ucko and Rosenfeld 1967: 116-49). One of the methods proposed is what Ucko and Rosenfeld term the use of ethnographic parallels (ibid.: 158). There are occasions when scientists seek the opinions of others who they feel are closer than they are themselves to the indigenous culture. The following data are used to demonstrate why the use of ethnographic parallels is inherently problematic and to explain why it may be counter-productive.

There is an initial and essential distinction to make. There exists, of course, contemporary indigenous art, created by artists who are readily available to comment upon the 'meaning' of their work. Examples of such indigenous art and the comments of the artists concerning their work are well documented in the Australian literature (see for example Keen 1977; Morphy 1983). So-called 'living art' may well provide a better field for ethnographic comparisons than the art whose creators (and most other clues to provide meaning) are no longer there for the asking **4)**.

A Fortescue River petroglyph site

The Fortescue River rises south of Mt Newman in the eastern Hamersley plateau region of the north-west of Western Australia. It flows generally westward to the Indian Ocean. On leaving the plateau the river cuts into the bedrock forming a spectacular gorge with large tumbled boulders which have been engraved - probably in 'prehistoric' times. Petroglyphs are pecked into the predominantly reddish-brown rock revealing the lighter coloured unweathered surface beneath. Designs were both fully pecked or pecked in outline. Local Aborigines regard the river, its pools, natural formation and rock art as an integral part of their socio-religious system. Indeed, in the mid-1970s they mounted a vigorous campaign locally to prevent the damming of the gorge to provide a reservoir and drinking water for newly developing coastal towns (see Wright 1968; Palmer 1977a; Clarke et al. 1978). When I worked in the region between 1974 and 1980 I had the opportunity to visit these rock art sites with local Aborigines and to discuss the petroglyphs with them.

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3) Data used in this paper were collected during fieldwork in northern Australia between 1973 and 1978. I also draw on observations made during a visit to four Palaeolithic cave sites in the Dordogne area of France in 1984.

4) Even if the distinction between living art and prehistoric art in ethnographic parallels is born in mind, the process of comparison remains fraught with difficulties. First, the relevance of a central Australian culture in 1988 to a European Palaeolithic culture of 20 000 years ago is in no way demonstrable (Ucko and Rosenfeld 1967: 151). Moreover, in Aboriginal Australia men and women may sometimes assign and reassign meaning to traditional designs rather than create new ones (Munn 1973: 112). Meaning in these circumstances is relevant to context and in no way absolute. Ethnographic parallels with living art in what may be loosely thought of as comparable cultures may produce a better qualified guess as to intended meaning than those not so informed, but the result remains a guess all the same.

Aboriginal comment

The response of the Aboriginal helpers was to associate the petroglyphs with a series of mythical figures who were held to be responsible for creating the art. This response was encountered at other sites in the region during fieldwork and has been noted elsewhere (Palmer 1977b). The subject of the petroglyphs was explained by my helpers to be one of the following:

1. Self portrait of the mythological figures.
2. Paraphernalia and esoteric objects used by the Aborigines today when commemorating in ritual the supposed actions of mythological beings.
3. Natural species believed to have been created by the ancestral beings. Some of these species are now regarded by my helpers as extinct.

An example of the first sort of explanation is as follows. We examined a petroglyph which resembled a human figure. My Aboriginal helper commented:

> Look, that sort of shape that man must have been. Doweramada [mythical ancestor]. Doweramada that's the two man that started. He must have been shaped like that. You often see that everywhere. Well, its a drawing of him ... that's a carving of him, now, I reckon.

However, the petroglyph in question also had a 'tail'. My helper noticed this and said, 'But that fellow's got a tail, isn't it?' I asked him if it could be a goanna (a sand monitor, called locally *bungara*). He went on:

> I thought it was a human being but he looked like *bungara* 'cause he got that tail ... Well look, looks like to me this way. This fellow a kid, that fellow's a father. His standing over him like that. Got his hand over just like he going to take a photo of him.

In this first example my Aboriginal helper made a series of related statements. These can be summarised as follows:

1.1 The petroglyph represents a mythological figure who 'started' everything.
1.2 The petroglyph might be a goanna.
1.3 The petroglyph is perhaps a father and son in a particular pose which looked as if someone (the father?) was going to take a photo of him/them.

The second sort of explanation included comments made about esoteric paraphernalia that related the petroglyphs to known ritual objects. Many comments made by my helpers were complex and involved long exegesis. I summarise one example:

'I don't know what that suppose to be, but from that side, human being side, man that one, I/he's a dancer, but, er, what do you call it again that thing?'

'Headdress?'

'Outfit, drawing of it ... you fit it 'em round the face, that's what you call a *gundu*.'

The man then went into great detail as to the method of manufacture and use of the head decoration, and how it was used in a ritual dance. This second extract can be summarised as follows:

2.1 I don't know what the petroglyph is *really*.
2.2 At a 'human level' it is a dancer.
2.3 The dancer has a headdress.

A third set of petroglyphs prompted the following series of comments:

> You know the old people used to tell me, the different things what is not living now are in this country, that they know, when they was young ... there's a lot of things, a long time ago, gone out of sight, never been seen again. Not breeding anymore, there's nothing about ... Properly, when the Law started, the fellow, whether its the Mingala [mythical ancestor] done this. Well they ... put something that some bird or anything that, they all got names, some of them ... used to live in this country, early time, well, properly, this is it here. Birds or anything at all, little birds used to be in that world when it first started. So this could have been one of those? Could have been one of them.

This third extract can be summarised as follows:

3.1 There were animals formerly in this country which are now extinct. The 'old people' told me this.
3.2 The mythological ancestors named animals and put them in the country.
3.3 The petroglyph may be a representation of one such animal.

My Aboriginal helper was cautious about being definitive about his interpretation. However, he chose to contextualise all petroglyphs within his own social and religious framework by attributing them to the work of the mythological ancestors of the great Aboriginal creative period, commonly known as the Dreaming. He then used the petroglyphs as a prompt while he described and discussed in detail aspects of his own known world. In short, they became an artefact that he used both to explore his own world and to indicate, both to me and the other Aborigine present, his knowledge and expertise in traditional matters. He had presumably seen a father and son pose for a photograph or remembered such a photograph being taken in his presence. He knew the myths which told of the ancestors who established the Aboriginal culture and traditions. He had witnessed a ritual in which headdresses were used, he was a ritual expert in their construction and use and explained to me at great length the method of their manufacture. Finally, he referred to the names of now extinct fauna and failing to specifically identify an 'animal' or 'bird' in a petroglyph, contextualised his lack of recognition by reference to a body of knowledge to which he had partial access. All this information was provided in the context of explaining the 'meaning' of the petroglyphs.

My helper provided an additional example which further illustrates what he was doing:

'Further down they reckon there's donkey in the rock.'

'What, a donkey *in* the rock?'

'No, the rock itself looks like donkey.'

Whether the donkey rock is supposed to be product of a metamorphosis in the creative era (perhaps a problem because there were traditionally no donkeys in Australia) or a mere accident of nature - and what its relationship is to the petroglyphs - did not emerge in the discussion. However, the comments were consistent with a process whereby my Aboriginal helper was expressing his understanding of a form by reference to his own experience of the natural world rather than to any reference at all to the original intentions of the artist. In this final example there had never been an artist, but the 'meaning' of a rock form was developed by my helper on his own account. In all cases however, I, by asking my questions, was putting my Aboriginal helper in a position where he could talk about something that he was supposed to know more about than myself. The response was to provide a singularly Aboriginal account of the art form predicted upon his understanding of his own socio-religious world in which he lived.

The Durba Hills: Gibson Desert, Western Australia

In the Gibson Desert in central Western Australia lies a

small range of Permian sandstone hills known as the Durba Hills. Rain collects in pools in the deeply eroded rocky gorges which cut the hills, and there are several permanent springs which seep from the base of the rocks and remain important water sources for Aborigines to this day. These supplies of water also attracted drovers in the early decades of this century, who sought to bring cattle through the desert from the Kimberley region using a series of wells created in the early 1900s which formed what became known as the Canning Stock Route.

There are a number of rock painting sites in the Durba Hills. One of them, known as Durba Springs, is situated on a rock wall close to a pool of water, lies at the broad entrance to a gorge on the northern side of the range and was easily accessible to the early drovers. The paintings at this site are mainly concentric circles, but in a space between them a drover engraved an obscene poem notable both for its racism and its sexism. There are also numerous other pieces of graffiti, including several names and the date 1906.

When I first visited the area in June 1975 with Aborigines from Jigalong, one of the party expressed concern over the graffiti at Durba Springs. My helpers were also able to make extensive comments on the painting sites in the region, relating them to their mythological beliefs and current ritual practices.

I returned to Durba Springs with a colleague and several Aboriginal helpers from Jigalong in June 1978. In response to Aboriginal requests and consistent with a policy of removing graffiti from art sites, the Western Australian Museum Department of Aboriginal Sites had asked that the site be cleaned up. However, members of an historical society who knew of the graffiti had opposed its removal, arguing that the dated names at least represented an example of early colonial history and should be preserved along with the rock art, as a part of the Australian heritage.

The incident is notable, not only for what it tells us about the standards of parochial Australian history, but also because it illustrates how rock art is variously valued. For my Aboriginal helpers and for my colleagues with whom I worked, the European rock markings were not only in the wrong place but were a negative and little valued feature that should be removed.

For a group of historians, however, the European markings were meaningful and had a value which made them worthy of preservation. While they did not wish to destroy or diminish the Aboriginal art, they felt that the European contribution should remain because of its historic significance. They assigned meaning to the art, then, not merely by reference to its literal interpretation (i.e. a date, a drover's dream of an Aboriginal woman in his otherwise womanless world) but also for its perceived contribution to 'history' and 'heritage'.

Interestingly enough, the European contribution was more 'meaningful' in the sense that it could be readily interpreted by anyone literate in the English language, though its intended meaning is equally problematic in relation to the present meaning. The Aboriginal art, on the other hand, although associated by my Aboriginal helpers with the Dreaming, could be provided with nothing like such an exact meaning. Readily comprehended meaning did not, then, add to the worth of the designs in at least some people's eyes. The problem with the European Australian contribution was that some thought it crude, unnecessary, and perhaps sacrilegious and in any case not old enough to be of any consequence - though the last point was of course hotly contested by those who supported its conservation.

On the ceiling of Rouffignac cave in the Dordogne area of France I noted graffiti in some quantity, including a name and a date almost exactly 100 years older than that in the Durba Hills example. As far as I know no-one has suggested in this case that the names and dates are of any significance, and I had been told that Rouffignac had suffered from considerable vandalism (M. Lorblanchet pers. comm.). When I visited the cave in October 1984 the guide, who was also the owner of the cave, remarked on the graffiti negatively. The cultural and historical context is entirely different from that of the Durba Hills. The history of the Gibson Desert in the first decade of this century is still more or less undocumented prehistory. The history of the Dordogne in the early 1800s, on the other hand, is well known. Consequently the 'meaning' and related significance of graffiti at the former location is of a different order than that located at the latter location [5].

Rock art, then, is valued in different ways by different people. Maybe if the date A.D. 1006 was discovered in the Rouffignac cave it would be brought to the attention of tourists and given a positive value. The criterion of age is of critical importance, but it is judged by individuals according to their own scale of values, and absence or presence of easily deduced meaning has nothing to do with this process whereby value or worth is attributed.

Lascaux II, Périgord

Lascaux II is a skilfully conducted and ingeniously created business permitting thousands of tourists to encounter a copy of the real thing. Despite the fact that the region has numerous other cave sites which are not facsimiles, Lascaux II remains the most popular 'cave'. It would seem that, for a majority of cave visitors, a copy of the real thing is preferable (or at least as good as) seeing paintings that were actually executed by Palaeolithic people.

The reasons are not hard to fathom. Lascaux II is a masterpiece in which the visitor is gently removed from the outside world, through darkened rooms to the main gallery that, with very little imagination, could be a painted cave. Moreover, it is Lascaux which, over the years, has been the name associated with Palaeolithic cave art in the region, and for most tourists it is where the coach operators, tour promoters and issuers of information will direct them. But these people do not go in their thousands to see rock art, they go to see copies of rock art. And, so it would seem, this discrepancy is of no consequence in the long run, since it is generally accepted that it is not possible to visit Lascaux I for all the very good reasons that the guide explains in great detail before allowing one to see Lascaux II.

There is a qualitative difference between Lascaux II and other Palaeolithic art sites in the region (including Lascaux

5) Another interesting example of the ascription of different values to art occurred in 1987 in the Kimberley region of Australia. Aborigines repainted an art site which they claimed was their traditional responsibility. They justified their actions by reference to traditional practices (Mowljarlai and Peck 1987: 71-8). They argued that the action had helped preserve their culture and revitalise the paintings and the myths associated with them. Critics of the repainting, who were mostly non-Aborigines, considered that the art had been desecrated and its integrity ruined (see *Northern Territory News* 12.9.87; *West Australian* 18.8.87). The value of the repainting differed radically according to the perspectives and cultural background of the observer.

1). Palaeolithic art is thousands of years old, and is the product of a finger stroke or hand movement of a Palaeolithic man or woman. That, simply, must be a part of its intrinsic appeal. Moreover, the physical context of a cave - its temperature, dripping water, distances and depth - are a part of its mystery. One cannot de-contextualise Palaeolithic art without diminishing it substantially. It needs to be experienced to be appreciated. I suspect that is what the makers of Lascaux II attempted to do and they made an admirable attempt. However, a copy is a copy, and an underground chamber with plywood rocks at the entrance and ferro-cement inside is no substitute for the real thing.

I concluded when I visited the 'cave' in October 1984 that most people were not concerned so much with the real thing (a Palaeolithic site that was open to the public) as with a phenomenon that has been presented, marketed and sold as having value and therefore - *vaux la visite*. In short, the reason why Lascaux II is the most visited 'prehistoric art site' in France is not because it is a Palaeolithic site (which it is not) but because the prevailing social attribution of value current among its visitors (and those who promote them) asserts that it is a site of significance. The character, size, clarity of its recorded art satisfy the tourist imagination, despite the fact that, in a sense, it is not at all what they came to see.

Discussion

My three examples have been chosen to illustrate the product (expressed as interpretations) of a relationship between viewer and rock art within the context of the social and cultural world he or she inhabits. In examining the sets of interactions I have made a series of statements about the attributions of meaning and value. First, I demonstrated that the generation of meaning had to do with the preoccupations of the viewer, and little if anything with the (now unknown) intentions of the artists. Even in a case where my helpers could be seen as a part of a tradition which perhaps had its origins in the same cultural arena as those of the artists responsible for the petroglyphs, their value-laden generation of meaning has no necessary connection with the original intentions of the artists. Nor, from the point of view of my helpers, did this matter add to their appreciation of the art or their understanding it. Second, I noted that the value of art (that is, how it is deemed to be of worth or to be worthless) is similarly determined by an individual's cultural perspective: social, historical and spiritual. Finally, but in the same vein, I noted that the estimation in which art is held does not necessarily have anything to do with the art itself, but rather is determined by socially attributed preoccupations, priorities and an imposed or assumed worth. Despite lack of authenticity some art becomes all-important because it is generally accepted among its viewers (and those who direct them) that this is the way things should be.

Our systems of attributing value are caught up with our preoccupations with age, our preference for one culture rather than another, or one set of historical interests rather than another. Even our distinction between graffiti and art belies our unequal treatment of one sort of design compared with another. These choices are all legitimate, by our own standards, but by making them we mark out our world into segments and ascribe relative worth according to our own priorities. The conclusion then is this: in the case of the Palaeolithic cave art (a preoccupation of many archaeologists in Europe), or Australian petroglyphs or rock paintings, understanding original intention is fraught with so many pitfalls and traps that, being cognisant of them, we might be excused for abandoning the enterprise.

One implication of this might be that understanding proto-meanings of art is beyond the limits of social science. I do not think that this necessarily follows, but it certainly raises some questions which need to be satisfactorily addressed. This is not a negative procedure, but rather allows us to proceed in other ways or by reference to other theoretical frames which, in their turn, might prove more fruitful.

Authorities like Ucko have developed and explored recording techniques that analyse style and form (Ucko 1977: 8). An analysis of this style and form can reveal patterns, structures of a visual system, that enable the analyst to develop hypotheses about their relation to other aspects of the cultures that used them and produced the art (H. Morphy, pers comm.). A strictly scientific analysis of the art form will reveal, or at least give a valuable clue to, the original meaning (i.e. the artist's meaning; see Gallus 1985). Analysis of motifs is suggested by one writer as the 'safest' procedure in this regard (Groenfeldt 1985). This of course presumes that the artist was conscious of what he 'meant', or that his work had only a limited meaning or a set of definable meanings. But art does not suffer this attempt at analysis without transformation. There is a crucial distinction to be made between topic (that which is the object of analysis) and resource (the means by which we attempt the analysis). In the endeavour to interpret rock art these two components have been conflated. The difficulty is that the very act of description (revealing the 'facts' of the art), whether analytical or ethnographic, become the topic, while frequently being mistaken for the resource (Sacks 1974).

Recording procedures, however refined, however rigorous, still concentrate on specific sorts of art, on one design over another or on one technique rather than another. The recording is framed by the observer's preoccupations and the act of recording itself alters the original and replaces it with another version. What is recorded and then presented as an analysis of the art is not an analysis of the art itself but of another version of that art. Using Sack's (1974) terminology, the account of the art is a transformation of the original which thereby eludes being the topic (that is, the object) for the analysis. Moreover, neither the provision of ethnographic parallels, diacritical categories or the attribution of value, however derived, can provide that resource (except subjectively) because each is culturally bounded and has epistemological relevance only to the viewer.

Clearly, we are in danger of throwing the baby out with the bath water by concluding that in the relativists' shifting world we can understand nothing except that everything is relative. To resolve this apparent impasse requires a return to basics and a reconsideration of just what social science (and science in general) is capable of achieving.

First, we need to remind ourselves that social science (and science in general) is activity that works towards the generation of understandings 6). It is not (as the naive inductivists might have argued) about establishing facts and truth. It is a process of working towards an understanding of phenomena by the generation of statements

6) An excellent and critical review of the differing views about science and their implications for the pursuit of knowledge is found in Chalmers 1978.

helpful to this process. These statements are not immutable, nor are they ever claimed to be. Popper wrote:

> The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest on solid bedrock. The bold structure of its theories rise, as it were, above a swamp. It is a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being (Popper 1968: 111).

Building the structure may include making cautious assumptions about the original meaning of the art, but they will be couched in terms of tentative conclusion worked from the complexities of the inter-relationships apparent in any encounter with the art.

Second, and crucial to this open-minded approach is the generation of a paradigm 7) that incorporates the notion that meaning is never absolute, that allows for (and accommodates) the transformation of art by the act of recording. The paradigm must also comprehend a domain where structures are erected as heuristic devices for explanation that are not founded after reality but *a priori* from the interpretation that derives from them.

With this bundle of concepts the Kuhnian paradigm equips us for the task of working towards an understanding of the art. The end is not to understand the meaning of rock art (in the narrow sense of a single referent) but to understand how meaning and value are derived at different times and in different places. The creation of this paradigm does not preclude the generation of apparatus that could help us understand Palaeolithic cave art, provided we keep an open mind about the serious limitations of our data, the contexts in which it was gathered and the devices used to formulate and present it. Such an approach avoids retreat into the cul-de-sac of total relativism while avoiding the worst pitfalls of naive inductivism. At best it allows for a process whereby art can be understood and re-understood as new materials and new ways of seeing challenge existing paradigms.

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Résumé. *L'article discute l'attribution de signification et de valeur à l'art indigène, s'appuyant sur des examples tirés de sites européens et australiens. La méthode et les circonstances culturelles dans lesquelles cette signification est développée assurent que celle-ci est un produit de la rencontre avec l'art, non pas un exposé absolu d'un système graphique symbolique. Les sciences humaines peuvent conduire à des résultats erronés en utilisant une épisténologie qui peut seulement établir la signification et la valeur par rapport au contexte culturel. Bien qu'il y ait d'autres méthodes analytiques, celles-ci sont aussi problématiques.*

Zusammenfassung. *Der Artikel erörtert and Hand von europäischen und australischen Beispielen die Ergründung von Bedeutung und Wert indigener Kunst. Die Methodik und kulturellen Umstände, in denen diese Bedeutung entwickelt wird, bewirken, dass sie ein Ergebnis der Begegnung mit der Kunst ist, und nicht eine absolute Berichterstattung über ein symbolisches graphisches System. Sozialwissenschaft mag durch die Verwendung einer Epistemologie, die Bedeutung und Wert nur in Bezugnahme auf kulturellen Zusammenhang bestimmen kann, nur trügende Ergebnisse einbringen. Während andere Möglich-keiten für Analyse zwar vorliegen, sind auch sie von Schwie-rigkeiten umgeben.*

7) In what follows I rely on Kuhn 1970 (esp. pp. 174-210) and Chalmers's critique of Kuhn (1978: 83-96). Kuhn's idea as to what constitutes a paradigm are consistent with those postulated here. The inexact nature of the paradigm is a matter Kuhn discusses in the context of Wittgenstein's definition of objects (Wittgenstein 1953: 31-6; see Kuhn 1970: 45).

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KEYWORDS: **Upper Palaeolithic - Portable art - Engraving - Perforation - China**

PALAEOLITHIC ART FROM CHINA

Robert G. Bednarik and You Yuzhu

Abstract. The discovery of the first Palaeolithic art object from China is reported. An antler fragment, decorated with three elaborate, non-figurative engraved patterns, has been excavated at an Upper Palaeolithic site in Hebei Province, north-eastern China. It is about 13 000 years old. A second find is also introduced here, a perforated stone object of c. 28 000 years BP from the Shanxi Province. Relevant Chinese and other finds are discussed, and they are considered within the framework of what is currently known about the Palaeolithic art of Asia.

Introduction

The two principal theories on the earliest development of art are quite incompatible, yet there are some specific points on which they happen to agree. While one model has art emerging at the beginning of the Upper Palaeolithic in western Europe in the form of iconic depiction, and marking the beginning of 'reflective language' (Davidson and Noble 1989, 1990; Noble and Davidson 1991), self adornment (White 1989a, 1989b) and an assumed 'quantum jump' in social and cultural evolution, the other school of thought sees the 'discovery' of iconicity (Davis 1986) as having developed from earlier noniconic art forms and from a variety of non-utilitarian activities which promoted the gradual development of human models of reality (Bednarik 1986, 1988a; cf. Lindly and Clark 1990). One of the issues on which the two schools, or at least individual proponents of them, agree is that the first Australian settlers are likely to have possessed an art tradition. Of course the reasons for the postulate differ between the two models: while in the first case complex language is seen as a prerequisite for a seafaring capability, and was itself preceded by art developed from gestures, the hypothesis of noniconic art origins requires in any case that art production preceded the Upper Palaeolithic of Eurasia by a considerable time span. Based on the current knowledge about the first settlement of the Americas (Bednarik 1989), it is probable that similar conditions applied there: the first settlers to reach the New World can also be assumed to have possessed some form of art.

It is frequently suggested, and indeed highly probable, that both the Americas and Greater Australia (including New Guinea) were colonised from eastern Asia, the former via Bering Strait, the latter via the islands separating Australia from Southeast Asia. Most likely this occurred at the time of a stadial peak, i.e. at a time when the sea level was considerably lower than at present.

As a rule, dialectic about art origins is conducted purely in terms of developments within Europe, and it often considers nothing other than the Upper Palaeolithic art traditions of that continent. But in order to learn about early art evolution it is essential to consider also the early arts of other regions, most especially of Asia. The models of European and North American archaeologists maintaining the claims that western European art of the Upper Palaeolithic provides the earliest evidence of 'symbolic behaviour' should be categorically rejected (cf. Bednarik 1992a).

In particular, the significance of Asian evidence of the Pleistocene is so self-evident that there can be no excuse for its continued neglect. A recent review of Palaeolithic art in Asia (Bednarik 1990a) resulted in the refutation of numerous claims concerning both portable art and rock art. It was found that unequivocal evidence is restricted to just a few areas in this vast continent (in India, Japan, Siberia). Claims concerning finds in Israel (e.g. Goren-Inbar 1986), China and Korea have not found general acceptance. For instance, Sohn Pow-Key (1981, cited in Bahn and Vertut 1988) discerns animal figures on Middle Palaeolithic bones from Korea, and attributes pecked petroglyphs in a number of Korean sites to the Palaeolithic (Sohn Pow-Key 1974, in Bahn and Vertut 1988), but none of these claims have been substantiated.

The Siberian evidence of Palaeolithic art is by far the most impressive in all of Asia, although it seems restricted to portable art now that the few claims for Palaeolithic rock art on the upper Lena river have been rejected (Bednarik 1990a, 1992b). An extensive body of mobiliary art objects has been described from, at the latest count, 18 sites (Abramova 1990). These occur mainly on the upper Yenisei and the Angara river, and include the well-known sites Mal'ta and Buret'.

In Japan, evidence of Pleistocene art is restricted to the several engraved pebbles from the cave of Kamikuroiwa. They were found in Layer IX which has been dated to 12 165 years BP (Aikens and Higuchi 1982).

Finally, Palaeolithic art is known to exist in India, although the claims by Wakankar (1983) and others, concerning the dynamic-style, often green rock paintings of central India (especially near Bhopal), remain unsubstantiated (Bednarik 1990a). However, India has provided some of the most tantalising clues for the earliest art. For instance, at least one of the haematite lumps from the Acheulian of Hunsgi bears a facet covered by striations that suggest its use as a crayon (Bednarik 1990b). The apparent petroglyphs at Bhimbetka (Bednarik 1990a; Bednarik et al. 1991) were covered by Palaeolithic occu-

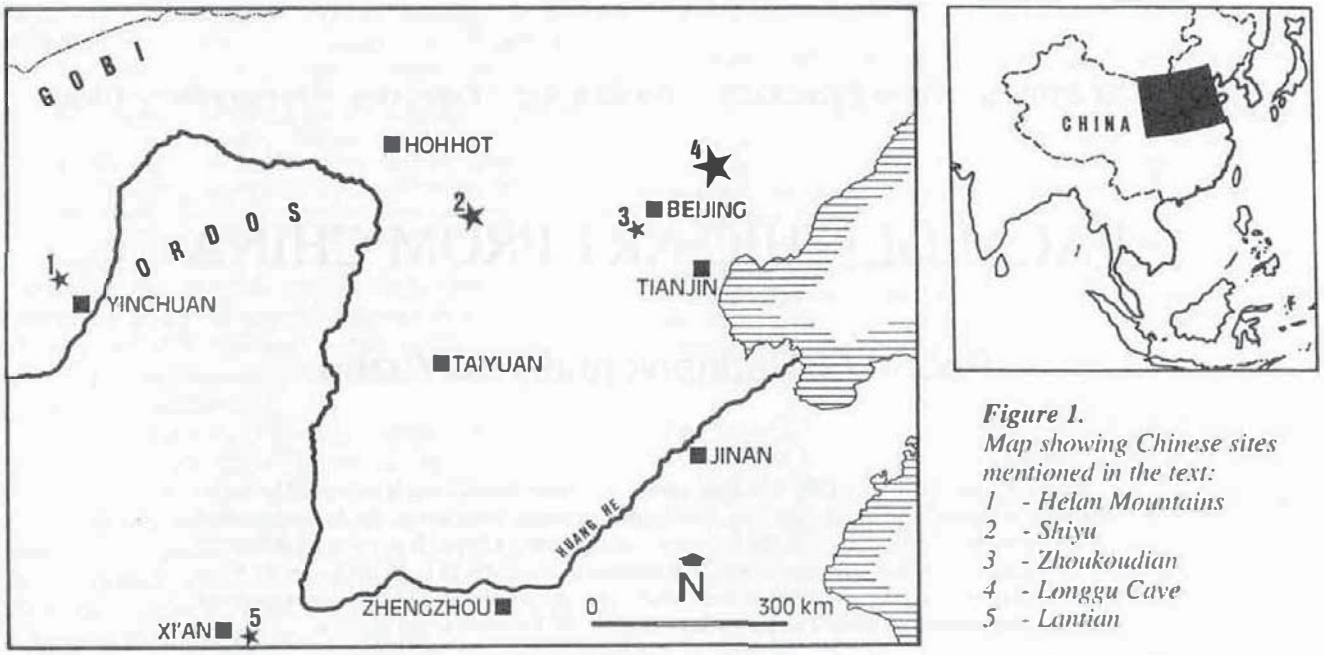

Figure 1.
Map showing Chinese sites mentioned in the text:
1 - Helan Mountains
2 - Shiyu
3 - Zhoukoudian
4 - Longgu Cave
5 - Lantian

pation deposits and may be among the oldest rock art in the world. Considerably more recent are the engraved ostrich eggshell fragment and the two perforated disc beads from Patne, and the similar ostrich eggshell beads from Bhimbetka, all of which are from the Upper Palaeolithic. The Patne engraved fragment bears part of a non-iconic decoration that was engraved with stone tools (as shown by replication experiments conducted by RGB), and it is partly covered by calcium carbonate encrustation. It has been dated to 25 000 ± 200 years BP (Sali 1978). Numerous other marked ostrich eggshell remains of the Late Pleistocene in India have been found to bear natural rather than anthropic markings (i.e. 45 of the 46 specimens examined; Bednarik 1991). The Indian rock paintings currently regarded as the earliest are the totally noniconic patterns Tyagi (1988) has described as 'intricate designs', which occur in central India and precede the dynamic human figures (cf. also Bednarik 1990a).

Palaeolithic art in China

It is clear from the above that Palaeolithic art does exist in Asia, but that the number of authentic finds remains very small indeed. We suggest that this pronounced paucity of evidence is a reflection of neglect rather than of true distribution, and that it would be resolved by a more determined approach and by the introduction of a methodology of direct dating for petroglyphs (Bednarik 1992c). This applies to most parts of Asia, and particularly in China.

The known evidence of non-utilitarian activities from the Palaeolithic of China is very meagre indeed, and has so far not included any true art productions. It seems limited to the finds from Zhoukoudian (near Beijing), particularly from the Upper Cave, and to the markings one of us has reported from bone fragments of a site in Shanxi Province (You Yuzhu 1984). Disc beads made from ostrich eggshell, similar to those found in India and Africa, occur in the Gobi as surface finds, and are attributed to the final Palaeolithic industry exemplified at Shabarak-usu.

The Upper Cave in the hillside above the town of Zhoukoudian (Figure 1) contains two occupation horizons, dated to about 13 000 and 18 000 years ago. They are separated by a layer of loess and rocks, and the upper stratum is overlain by a talus slope of cryoclasts. The occupation layers yielded human burials, haematite lumps and over 120 small perforated objects. These beads consist of perforated teeth (of deer and fox, and very similar to Palaeolithic specimens in Europe and Siberia) and shells, pierced fish vertebrae, perforated pebbles, and apparently polished, tubular sections cut from the long bones of a fairly large bird species. The latter bear distinctive cut marks along their sides, which YY suggests number from one to five on the original five specimens found. He also thinks that these objects were worn on bracelets rather than necklaces, as the remainder of this material probably was.

All the perforations we have examined on these objects are conical in section, similar to those on perforated Palaeolithic objects the world over. In the Upper Cave, 'ochre' has been found around interred skeletal remains. We have examined lumps of haematite from the site which measure about 25-45 mm. They are irregularly shaped, bear no wear facets or striations but must have been carried into the cave intentionally. Petrologically, these lumps are homogeneous and very hard, resembling the haematite from the Lindner site in Australia (Jones 1985). Minor traces of iron oxide have been detected on some of the perforated specimens from the Upper Cave.

Zhoukoudian Site 1 (the *Homo erectus* site) has yielded some 20 quartz crystals, including a crystal prism with all facets intact (Pei 1931: 120). It is relevant to recall that quartz crystals have been reported from three Acheulian occupation sites, including two in Asia (Bednarik 1992a). Also, stone spheres have been frequently found at Zhoukoudian, as well as at other Lower Palaeolithic sites, but while they tend to be grapefruit-sized elsewhere (at Lantian, for instance), they appear to be somewhat smaller at Zhoukoudian, of around 6-8 cm diameter.

Here we introduce a new find, an art object recently excavated by YY from the Upper Palaeolithic occupation site Longgu Cave, Northwest Mountain, near the town of Xinglong, Hebei Province (110 km north-east of Beijing). The occupation layer, in sandy clay, had earlier been dated to about 13 000 BP via charcoal, and an AMS date obtained directly from the newly found object has just confirmed that age.

The object is fashioned from antler of *Cervus elaphus*

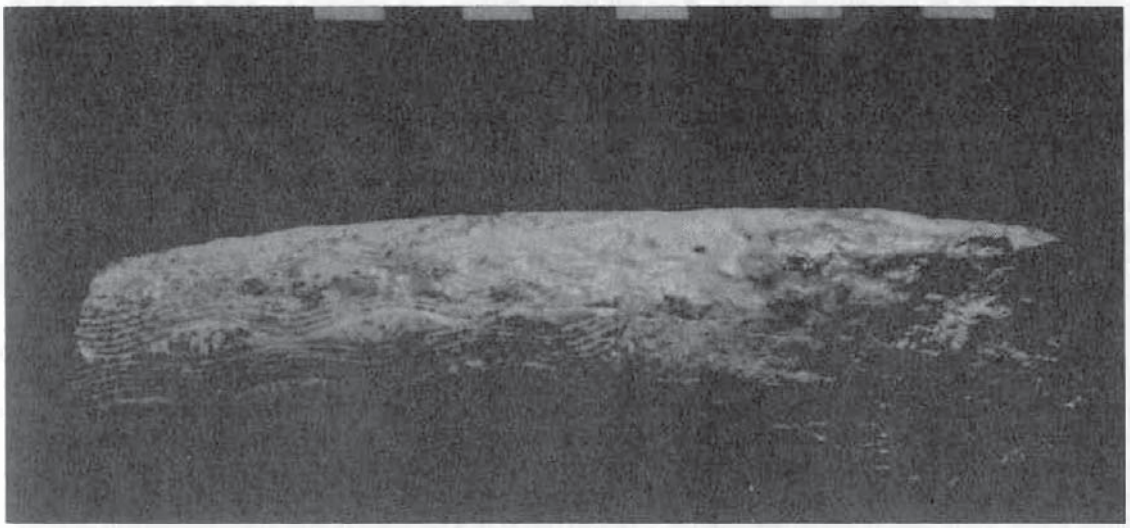

Figure 2. *Longgu Cave antler object, design 1: sets of multiple parallel wave lines.*

Figure 3. *Design 2: figure eight motif.*

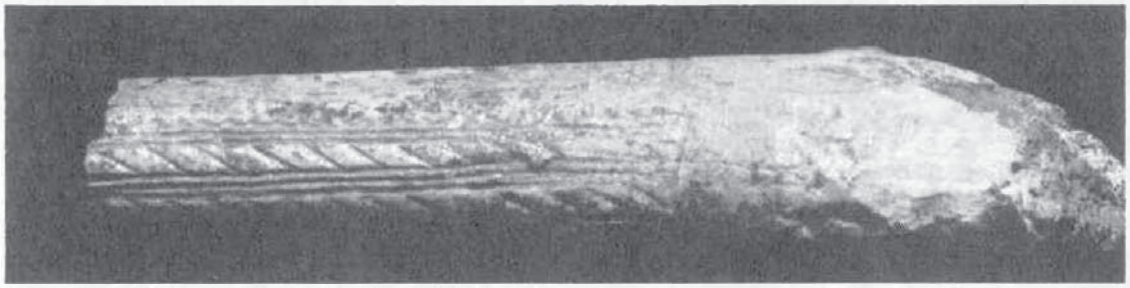

Figure 4. *Design 3: composition of parallel, zigzag and oblique lines.*

canadensis, consisting of a 134 mm-long section of the main stem, with the natural antler surface still partly intact. The specimen is oval in section, bearing three distinctive engraved patterns on its longitudinal surfaces: one each on the two flattened faces, and a third along one of the two sides (Figures 2-4). There is no perforation, and neither the object's shape nor the arrangement of the markings provides any clues about its former use. The specimen has experienced surface corrosion and has faded to a bleached, off-white colour, but it remains mechanically sound and weighs 104 g. The high-pH loess matrix and partial carbonatisation have no doubt contributed to its comparatively good state of preservation.

Part of the surface appears to have been coated with a brilliant-red, cinnabar-coloured deposit, presumably of haematite. Most of this seems to have peeled off, but traces remain over much of the surface. The pigment even occurs in the pores of the spongy interior where it is exposed at one end of the object. This end was obviously broken before the object's use, whereas the other end truncates all three engraved designs, and thus postdates its primary use period.

The original encrustation of calcium carbonate still covers various parts of the antler fragment. The precipitate conceals red colouring material, and in one location has facilitated the retention of the ochre completely: here it can be seen that the red paint entirely fills the engraved groove, which raises the possibility that all of the engravings were originally filled in this fashion.

The engraved lines were superbly crafted with stone tools, their groove patterning matches that on European and Siberian portable art objects. Line widths and depths, as well as the spacings between the lines, have been carefully maintained throughout. The layout of the three patterns is bold and confident, suggesting the hand of a highly experienced artisan with a repertoire of very distinctive and sophisticated designs. One pattern consists of four sets of six or seven parallel wave lines, competently arranged to form a consistent design; the second is an elaborate figure eight motif; while the third is an arrangement of parallel and zigzag lines enclosing two elongate panels of oblique cross hatching. The artist

demonstrates an admirable control not only of even spacings and groove depths, but also of the technical aspects of constructing complex curved arrangements and integrating them seemingly effortlessly into a rectangular layout. The definition evident in all three patterns and the distinctness of the designs suggest that well-defined meanings were attached to them.

A small quantity of the inner spongy material has been removed from the more recently broken end of the antler fragment (the end that bears no ochre traces) for direct dating by the AMS method. The result, just received, confirms the previously assumed age of the find: 13 065 ± 270 years BP.

The engraved patterns on the antler object are of a structural complexity which exceeds by far that found in comparable European art of the Upper Palaeolithic. It is always risky to draw far-reaching conclusions from a single and 'unique' find such as this one, but since corresponding finds from early Asian and Russian art exhibit similarly complex noniconic designs one may be justified in speculating about the emerging pattern. That pattern suggests a predominance in the Pleistocene graphic art east of Germany of highly elaborate designs that appear to hold considerably greater research potential than the simplistic animal figures that are so prominent in western European 'cave art'. Marshack has long recognised that the 'geometric signs' in the far west are the most significant component of that art, but in the east (Russia, India, Siberia, and now China), this noniconic art is conceptually much more complex, and has been developed to an extraordinary sophistication that has certainly defied any interpretation attempts so far. Nor is it likely to yield its metaphysical contents or its symbolism to the simplistic empiricism of Western scientists. So far, this rich art tradition has been best exemplified at Eliseevichi, Mezin, Kirillovskaya and Mezherich, but it also occurs, less pronounced or in smaller numbers, at Patne, Mal'ta, Afontova, Kavkaz, Balinkosh, Klinets, Timonovka, Suponevo, Novgorod-Severskaya, Avdeevo and Gagarino.

Another object recently excavated by YY is part of a perforated stone disc from Shiyu, near Huairen, Shanxi Province, c. 300 km west of Beijing. The presumed pendant, measuring 80.5 mm diameter, has broken in half through the central hole, which in section is very wide and flat-bottomed, drilled only from one side. The surface of the flat natural pebble is rough and unmodified, and the object is reminiscent of two of the three drilled stone pendants from Kostenki XVII, which are similarly flat and fractured through the hole.

The specimen is from the uppermost of two Palaeolithic layers, both of which occur in a single sedimentary unit of 0.8-1.5 m thickness which consists of greyish-black clay with sand. It is overlain by, from the top, a substantial loess deposit and a sand stratum, and in turn rests on a layer of gravel, below which is a palaeosol floor. The upper occupation deposit has yielded about 30 000 stone artefacts, the lower level contained some 40 000 lithics. The two occupation levels seem to be typologically similar, appearing to combine Middle Palaeolithic and Upper Palaeolithic elements. Radiocarbon dates have just been provided for them, of 28 135 ± 370 BP (upper occupation horizon) and 32 220 ± 625 BP (lower occupation horizon).

It follows that the presumed stone pendant from Shiyu is one of the earliest drilled stone objects known in the world (Bednarik 1992a), and is therefore an important find in the context of non-utilitarian evidence from the Palaeolithic of Asia.

The Shiyu site has also provided some 600 bone fragments with various types of markings. We distinguish four basic categories among these marks of which only one type, the linear incisions, could conceivably include anthropic marks. While in some cases these do resemble incised marks that have been described as 'intentional' or 'symbolic' from European Palaeolithic sites, we do not regard any of the Chinese specimens examined by us as bearing intentional engravings, even where series of similar marks occur together on a surface, or where their spatial arrangement is suggestive of a syntactic sequence. In fact our findings question the interpretation of various early markings reported from European sites, for instance those on one of the objects from Cueva Morín (Freeman and Gonzalez Echegaray 1983: Fig. 1a). The observations on some of the Shiyu specimens, together with their implications for identifying bone markings elsewhere, will be discussed in a separate paper.

The extensive rock art of China remains undated, except for two instances in Yunnan Province in the far south (Qin Shengmin et al. 1987: 230-2; Woo Sheh Ming 1991). In particular, the chronological sequences mooted for the major petroglyph regions of northern China are based largely on speculation, 'stylistic identification' and iconographically derived but often unconvincing models. In regions with Palaeolithic occupation evidence it is quite possible that a component of these petroglyph corpora could be of the Pleistocene. So far this possibility has not received any serious consideration in Chinese rock art studies, and it is also noted that the most likely candidates among the motifs are sometimes ignored by students of Chinese rock art, due to their simplicity or deep patination.

For instance, in the Helan Mountains, Ningxia Province, the earliest petroglyphs at Helankou or Suyukou (as identified by microerosion criteria; Bednarik 1992c) appear to be deeply patinated, noniconic motifs. They occur alongside much more recent and elaborate figures, and they are regularly ignored by rock art recorders in favour of the more picturesque and prominent, but also much younger, motifs, such as human faces ('masks'), anthropomorphs and animal figures. A preliminary examination suggests that the early motifs are at least several times as old as those figures which are attributed to the Bronze Age (Shong and Xi Zhou States). At Shui-tung-kou, near Yinchuan, an occupation site under 15 m of loess has provided very early Palaeolithic remains, and both the Ordos and Gobi regions have provided ample evidence of Palaeolithic occupation. Considerably more work and a considerably more rigorous approach will be required to establish a rough chronology for the pre-dynastic and preceramic rock art of China, but preliminary considerations suggest that the possibility of Palaeolithic beginnings should not be excluded for this sequence of rock art traditions (Bednarik in prep.). The finds introduced in this paper serve to underline the need for examining the possibility that Palaeolithic rock art may exist in China.

Conclusions

Intensive Palaeolithic research has been conducted in China for many decades. The search for Palaeolithic art, too, has been on since the discovery of non-utilitarian objects at Zhoukoudian, but so far it has remained fruitless. The first indisputable Chinese art object of Palaeolithic age is introduced here, and it is clearly noniconic. It consists of an engraved section of antler, covered by three

beautifully crafted, most elaborate geometric designs. The object was probably coated with red paint, which has survived especially where it became covered by an accretionary deposit of calcium carbonate. It is possible that the engraved lines were originally filled in with colouring matter for contrast.

The decorated antler fragment seems to be safely dated to about 13 065 years BP. Its complex decoration confirms what has been observed in the Palaeolithic art of Siberia, India and Russia: the early graphic art of these regions seems to consist almost entirely of intricate noniconic patterns. As observed previously (Bednarik 1990a), there are only two or three examples of two-dimensional iconic art from the Palaeolithic of Russia (apart from the paintings in Kapova Cave), and there are none available from anywhere else in Asia at the present time. Moreover, the earliest rock arts of Australia and the Americas, which presumably derive from eastern Asian Middle Palaeolithic art, are exclusively noniconic and strikingly uniform (Bednarik 1987, 1988b, 1989).

A second object introduced here is a perforated stone disc, about 28 000 years old and therefore one of the earliest evidences of its kind. This find confirms that the drilling of hard materials, which was already practised in the Middle Palaeolithic of eastern and central Europe, is likely to have been part of a well-established technology across the entire breadth of Eurasia by the end of the Middle Palaeolithic (Bednarik 1992a).

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Résumé. On documente la découverte du premier objet d'art paléolithique de Chine. Un fragment de ramure, orné de trois motifs gravés, compliqués et non-figuratifs, a été découvert au site paléolithique supérieur de caverne de Longgu, situé dans la province de Hebei, au nord-est de la Chine. Il date d'environ 13 000 ans. On introduit aussi un deuxième objet de la province de Shanxi qui est perforé, de pierre et datant d'environ 28 000 ans. Des trouvailles pertinentes chinoises et d'ailleurs sont discutées, et considérées vis-à-vis du cadre des connaissances contemporaines de l'art paléolithique d'Asie.

***Zusammenfassung.** Die Entdeckung des ersten paläolithischen Kunstobjektes von China wird berichtet. Ein Geweihfragment das mit drei komplizierten, nicht-figürlichen gravierten Mustern verziert ist, wurde in dem oberpaläolithischen Fundort Longgu Höhle in der Hebei Provinz, Nordost-China, ausgegraben. Es ist etwa 13.000 Jahre alt. Ein zweites Objekt wird hier ebenso vorgestellt, ein durchbohrter Steingegenstand von etwa 28.000 Jahren BP aus der Shanxi Provinz. Sachdienliche chinesische und andere Funde werden besprochen, und im Rahmen dessen, was derzeit über paläolithische Kunst in Asien bekannt ist, erörtert.*

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RAR DEBATES

Rock Art Research 1989, Vol. 6, No. 1, pp. 17 - 53.

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

ALEXANDER MARSHACK

FURTHER COMMENT

San informants on the practice of rock art in the Transkei, South Africa

By A. R. WILLCOX

> *The very nerves and sinews of science consist in believing nothing rashly.*
>
> Epicharmus, c. 500 B.C.

> *Ignorance is preferable to error and he is less remote from the truth who believes nothing, than he who believes what is wrong.*
>
> Thomas Jefferson, c. A.D. 1800

Lewis-Williams (1990), in his response to my critique (Willcox 1990a) of Lewis-Williams and Dowson (1988), and in support of his claim that 'virtually all San rock art is shamanistic', refers to the reports by Jolly (1986) and Lewis-Williams (1986). For this reason and because these interviews allegedly elicited important information it is desirable that the reports be evaluated critically and set against the background of statements by eyewitnesses and other evidence.

Comparison of the two reports discloses contradictions and ambiguities, and that important statements by the informant mentioned in one report are not mentioned in the other.

Jolly's account does not state that the woman (referred to as M.) said that her father was a rain doctor or medicine man, only that in his early life he had lived in a rockshelter which had paintings on its wall, nor did she say that he was one of the painters. Lewis-Williams, on the other hand, states that she said her father 'was a medicine man and had painted some of the depictions in the rockshelter to which she took us'. Lewis-Williams continues:

> Nothing could shake her conviction that rock painting was the task of medicine men. She denied that other people also painted. This part of her testimony is of special importance because it is directly relevant to the current polemic on whether San art was in any way shamanistic.

This was very good news to Professor Lewis-Williams who had committed himself to the proposition that 'virtually all' the rock art was executed by 'shamans' to depict what they had hallucinated in a state of trance. According to Jolly she only said, referring to some specific paintings, 'Medicine men did the painting' - no 'all' about it.

Lewis-Williams refers to 'the interviews Jolly and I had with M'; Jolly seems to imply only one. Lewis-Williams refers to M. as 'probably the last surviving southern San', Jolly states only that her father was San. Jolly established that M. was not, as she claimed, born in the painted shelter, and that she could not speak a San language.

Lewis-Williams, apropos of shamanism, writes 'Fortunately, as Jolly shows, the general reliability of her knowledge of San medicine people is established by her descriptions of dancers bending forward, bleeding from the nose, and so forth'. Jolly's report does not quote M. as mentioning bleeding from the nose, nor does Jolly say anything about M. demonstrating, as Lewis-Williams states, the way dancers faced the paintings when they wished to intensify their power etc. This and other discrepancies must have arisen from Lewis-Williams' independent questioning and depend for acceptance on M.'s credibility.

To clear up these points and others I wrote to Jolly and put some questions to him:

1. How many interviews with M. took place and who was present at each?
2. Was M. pure San, i.e. on her mother's side as well as her father's?
3. When M. said 'Medicine men did the painting', was she referring to particular paintings in the shelter, or all of them, or paintings generally?
4. In what language were the questions put and how were they and the answers recorded?

I received from Pieter Jolly a courteous and helpful reply to my questions above:

1. There were several interviews, the first of them between Jolly, M. and Lewis Matiyela (who took Jolly to M. and interpreted). Lewis-Williams was not present but later visited the area and interviewed M., and was taken with Jolly by her to the rockshelter where M. or her family had lived. Jolly was not present when M. told Lewis-Williams in effect that only medicine men painted.
2. M. and the chief of the area both stated that the mother and father were San, but the father of the interpreter asserted that her mother was a Mpondomise woman.
3. When M. stated that 'medicine men did the painting', she was referring specifically to the paintings which had 'medicine' in them, which were many but not all of them.
4. M. could not speak English and the questions were put in the Mpondomise tongue through the Bantu interpreters, who were first Lewis Matiyela and later a local girl who was fluent in English. Jolly's recording was done by taping questions and answers and taking detailed notes, but only the latter when the recorder broke down.

Jolly also tells me that, although not stated in his article, M. did say that her father was a medicine man and that he painted in the rockshelter.

Both Jolly and Lewis-Williams make it clear that M. was a bad witness, contradicting herself, telling untruths, and changing her mind. It seems that she was following the amiable but exasperating habit of telling a questioner what she thought he would like to hear.

From the supporting evidence noted in the two articles, and in the light of other reports not mentioned, it is reasonable to accept as true M.'s evidence in the following respects:

That San living in the area acted as rain doctors for the Mpondomise until later than 1886, in return for gifts. This is attested by Sir Walter Stanford in his memoirs. As a magistrate he well knew how to elicit information from indigenous witnesses without putting leading questions (Stanford 1962). Stanford frequently met San, and every day met Mpondomise and Bantu of other tribes. He lived among them as Chief Magistrate from 1885 until

1907. He must be the Chief Magistrate referred to by Jolly. There is evidence for San acting as rain doctors for Bantu also in the Northern Transvaal and Zimbabwe (Willcox 1963). Whether the shelter of M.'s father was one of those referred to in the various accounts as lived in by San rain doctors is uncertain - there are scores of painted rockshelters in the area - but is not material. Regarding the use by the San of blood as a paint medium there is also other evidence. Ellenberger (1953) was told by Basotho that San used the blood of an animal to paint (*employaient aussi pour peindre le sang d'animal*), and he mentions various media, including grease and plant saps.

Analyses of paint from San rock paintings by Denninger (1971) showed that in many cases, amino acids from blood were present. An experiment by Dr Denninger and Professor O. Schroder using ground-up iron oxide mixed with fresh sheep's blood and a little water proved the mixture to serve well as a paint (Willcox 1971).

M's evidence on the San use of animal skins and/or heads as hunting disguises and for dressing up by dancers is discussed by Jolly and Lewis-Williams. There is further evidence from Stanford's memoirs. One of his informants was a certain Silayi, a Tembu man who lived with the San for nearly three years and took part in their stock stealing raids on Bantu. A Bushman would put a sheepskin over his shoulders to get among the flock.

M's accounts of eland hunting rituals and the use of eland fat as medicine are quite acceptable: the eland was certainly a very special animal to the San, for good hunters' reasons as well as mystical ones.

It is notable that M. did not say that the painters went into trances; and that Lewis-Williams' statement that M. 'again and again insisted on a shamanistic view of the art' is not confirmed in Jolly's report or in his letter to me.

To evaluate M.'s evidence and Lewis-Williams' medicine men/artists-in-trance theory it is necessary to make comparisons with the accounts by eyewitnesses of San artists at work.

One of the Bantu whom Stanford closely questioned was the man Silayi already mentioned. Stanford stated the questions as well as the answers. One could wish that Jolly and Lewis-Williams had done the same, and that Stanford had asked more questions. However, two are relevant:

Q: Where did they [the San] get the material with which the caves are painted?

A: It was taken out of the ground. Some kinds were prepared at the fire. They could paint very well.

Q: Had they [the San] witch doctors?

A: No; they had rain doctors and the Kaffirs in the dry seasons employed Bushmen to make rain for them.

There was no mention of rain doctors being the artists or of the artists (whoever they were) painting in a state of trance.

An old farmer in the Cape questioned by Otto Moszeik (1910) only related how the Bushmen made their paint and how they used pointed pieces of bone in applying it. No help there!

As reported by Galton (1883), Dr R. J. Mann, Director of Education of the Province of Natal, questioned a Bushman youth, given the name Sam, who was captured in the Drakensberg in 1862 and put to work on a farm. Sam proceeded to demonstrate how a San artist executed a painting in the manner in which he said it was invariably pursued by his people. He was obviously an artist himself - Mann comments on his fine bold line - but he was also obviously not a medicine man, and he did not go into a trance, nor did he say any artists of his people did.

In 1873, J. M. Orpen received proffered explanations of some rock paintings in Basutoland from a Bushman, one Qing. He appeared to imply that some of the figures depicted were medicine men, but did not give any information about the status of the artists. Surely it is probable that he would have said that the painters were medicine men if that was the case (Orpen 1874).

Theopolis Hahn found San still painting in the region of the Orange River in Namibia (Hahn 1869). He was told that the old people (as translated by Sollas 1924) 'teach their children and they exercise their art for the pure pleasure of representation'. Again there is no mention of painting being restricted to medicine men and the aim is representation of the actual, not the depiction of hallucinations. In the present writer's view, pleasure in the exercise of skill to recreate on the rock face what gave pleasure at first sight is sufficient motive to account for nearly all South African rock art.

The mention of teaching the children to paint is pertinent. There are many examples of simple paintings very low down on the rock face, and usually imitations of the good paintings higher up, which almost certainly are the work of children. They are not found higher than the reach of the tiny San children who presumably were not shamans (for examples see Willcox 1956: Pls 12, 37, 38, 74; 1963: Pls 22, 58). Lewis-Williams has not to my knowledge taken cognisance of these paintings [but cf. the comments of several critics of the 'shaman hypothesis', on the possible involvement of children in the production of rock art. Ed.].

S. S. Dornan, writing in 1917, related that the Masarwa Bushmen, actually mixed people, of Sansokwe River in Bechuanaland (Botswana) still continued to paint (Dornan 1917). There were three painters among the tribe and *one* of them was a rain doctor, which implies, of course, that the other two were not. Given that they had painters and rain doctors it is to be expected that occasionally a man might fill both roles, and it is clear that two of the painters were not doctors. Dornan gives a detailed description of how the three painters went to work. He was an eyewitness in at least one case in which the half-bred artist was not stated to be in a trance, and proceeded in a systematic way (Dornan 1925).

Victor Ellenberger was also given a detailed account of painting procedure by a Basotho woman who claimed to have watched the Bushmen painters at work. There is no mention of any of them being medicine men or going into trance (Ellenberger 1953).

It was the same with the Mosotho who told Marion Walsham How that he had learnt to paint from the Bushmen, sometimes painting at one end of a shelter while San painted at the other (How 1962). He made some paintings for Mrs How.

Thus apart from M. as reported no eyewitness has been recorded as saying that the artists were medicine men except in the case of one of three artists mentioned by Dornan, and no eyewitness at all has said that going into a trance or hallucinating was part of the artists' procedure.

It is not in dispute that some San went into a state of trance, and still do today (see for example Biesele 1978), brought on by prolonged rhythmic dancing. This is not done only by medicine men. Biesele states that about half the adult men do so at some time in their lives. Some of the paintings can be interpreted as men having fallen into this state.

M.'s evidence, although acceptable in some respects, supports Lewis-Williams' shaman/artist contention only in that he reports her as saying that *all* the art was executed by medicine men. Jolly, however, explains that she meant only certain paintings. It appears that she had lived at the village all her life and could not know about the innumerable paintings in other parts of the Drakensberg art region, let alone other regions.

Apart from the unsatisfactory nature of the evidence for shaman artists recording hallucinations following trance, there are three main reasons for rejecting the theory as accounting for the production of San rock art. Note that Lewis-Williams does not say 'some' of it, or 'most' of it, but 'virtually all' of it.

In various publications Lewis-Williams and Dowson (e.g. 1988, 1990) state that entering trance or 'altered consciousness' proceeds in three stages. In the first people see what these writers call *entoptics* [the correct term is phosphene forms or phosphene motifs, because the term entopties covers all visual experiences generated autogenously, i.e. without sensory input; Ed.], geometrical shapes that include zigzags, chevrons, dots, grids, vortexes and nested U-shapes. This is stated to explain the presence of such forms in the rock art of southern Africa and elsewhere, and is given as evidence for the trance theory. This stage, it appears, is inescapable. In the second stage of trance, people elaborate the

'entoptics' into objects with which they are familiar, and in the third stage they hallucinate the forms depicted most commonly in the rock art. It is therefore extraordinary that in the richest area of San rock art, the Drakensberg of Natal and north-east Cape Province, and the most studied (Willcox 1956, 1990b; Vinnicombe 1976; Pager 1971 etc.) which is also the area of Lewis-Williams' own field work, no 'entoptics' (to my knowledge) have been reported. Some may exist in the thousands of paintings in the hundreds of rockshelters but, if so, it must be a minute percentage of the entire corpus of rock art.

The second fatal objection is that the theory makes no allowance for the motives that have moved artists the world over and throughout history - the urge to create, the use of skills and the exercise of the normal, undrugged, conscious imagination.

Thirdly, the idea that the art depicts hallucinations fails to explain the high degree of naturalism found in the representations. Studies chiefly in America show that in work purporting to depict hallucinations experienced by the artist - drug-induced or otherwise - naturalism is conspicuously lacking (Siegel 1977; Reichel-Dolmatoff 1978; Harner 1973).

Lewis-Williams' supporter Hammond-Tooke has emphasised that 'the study of rock art has entered the field of scholarship'. Scholarship, to my mind, requires looking objectively at evidence which does not support a theory as well as what may give it support. Selection of evidence is a scientific crime. In assessing M.'s testimony neither Jolly nor Lewis-Williams mention the accounts by Moszeik, Galton, Dornan and Hahn cited above, and in mentioning those of Orpen and Ellenberger do not make clear that these reports make no mention of shamans as painters, in trance or otherwise.

Lewis-Williams states that 'The Bleek Collection shows that nineteenth century /Xam medicine men entered trance to make rain'. I can find in the work cited nothing like such a definite statement: it depends again on accepting Lewis-Williams' postulated metaphors, symbols and 'analogical relationships', but even if accepted there is apparently nothing in the vast corpus of the Bleek's and Lloyd's translated texts of San folklore, and their comments upon it, to suggest that the medicine men were the painters or engravers. Much of the material is unpublished: the best readily available source is Bleek and Lloyd (1911). It has six references to sorcerers, three of them to rain-sorcerers. None of them implies that trance was involved, or that they were artists.

Against the weight of the negative evidence for the shaman-artist theory as a generalisation, M.'s answers, considered objectively, appear at best to show that one man was both a rain doctor and a painter; and that some paintings in one cave (and possibly others) in part of one region were also executed by medicine men. No acceptable evidence for the trance hallucination theory emerges.

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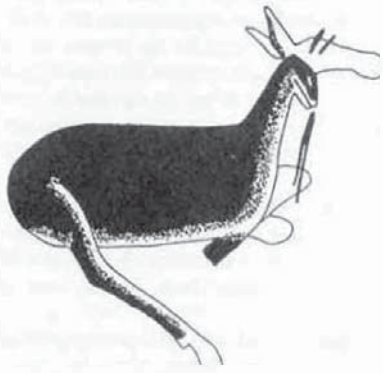
RAR 8-211

EDITOR'S COMMENT: The identity of M. has been disclosed in a recent paper by F. E. Prins (*South African Journal of Ethnology* 1990, 13: 110-6), which also provides a photograph of, and reliable ethnographic details about, Maqhoqha and other indigenous consultants who are or were knowledgeable about San rock art. Prins' results of interviewing M. differ significantly from those of Lewis-Williams ('The last testament of the southern San'): the art is portrayed as a record of the way the San used to live, and a form of 'sympathetic magic may be expressed' in some San rock art. Moreover, M. stated to Prins that, apart from two exceptions, none of the painters were medicine men.

In order to resolve this crucial matter once and for all, *RAR* will present Comments by several more scholars in the debate begun by C. A. Hromnik, two of whom were directly connected with the interviews of M.



'Reused' rock painting, north-eastern Cape Province, South Africa (after J. D. Lewis-Williams and T. A. Dowson).

Rock Art Research 1988, Vol. 5, No. 2, pp. 108 - 146

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

MARGARET F. NOBBS and RONALD I. DORN

FURTHER COMMENT

Style, statistics and the Karolta petroglyphs

By MAURICE P. LANTEIGNE

The following is a clarification of several points and questions raised by Nobbs and Dorn (1990) in response to my critique (1989) of their initial paper (1988). I restrain myself solely to some of the issues advanced by Nobbs and Dorn (1990).

> We do not understand Lanteigne's rigid acceptance of the 95% confidence interval ... Why not 90%? Why not 80%? (*RAR* 6: 51)

My 'rigid acceptance' was not based upon any personal criteria, but that of the researchers themselves:

> The extreme error range listed in Table 2 incorporates the errors at a 95% confidence level for all variables. (*RAR* 5: 116)

What is considered an acceptable confidence interval depends upon sample size (n_y), estimation of population variability (σ), confidence one needs to have in estimating the population mean (μ) and in generating inferences based upon error estimates (σ_x) of this mean (cf. Baillie 1990: 365). Although some statisticians suggest that a relaxation of comrol over Type I errors, in favour of reducing Type II errors, is appropriate for small sampling, this relaxation should not be considered for research designs with 'serious substantive consequences' (Stevens 1986: 4).

> Lanteigne's basic confusion appears to be over the difference between sample and population. This confusion is both in mathematical terms and in inferential statements. (*RAR* 6: 51)

Understanding the difference between sample and population parameters is crucial to the generation of any inferential statements (cf. Dwyer 1983), and corroboration through mathematics. The term 'sample', as used by the researchers (*RAR* 5: 112), is more appropriately referred to as a case of the sample randomly selected from the population. The term 'sample' has a colloquial meaning quite distinct from the statistical context as applied by myself. Some statisticians do refer to cases as samples (e.g. Runyon and Haber 1976), but such use tends to become very confusing to non-statisticians. Bio-statisticians refer to cases as individuals; whereas some general statisticians refer to cases as observations. In the cation-ratio example, my use of sample refers collectively to the 24 cases (or glyphs) from which three different observations (PIXE assays) were taken. Inferences generated from the sample (n=24) are deemed to represent best-fit estimates of the population (n=1826), in accordance with the limitations of probabilistic decision theory.

> Each cation-ratio date is a minimum age and is bracketed with a 95% confidence interval. This is how 'data' and confidence intervals are handled in dating in archaeology and in earth science. (*RAR* 6: 51)

The 95% confidence interval (*RAR* 5: 112, Table 2, Column 5) associated with the extreme CRs (Column 2), and the 68% confidence interval (Column 6) associated with the average of the three CR dates per case (Column 4), measure the degree of random observational error associated with the cation-ratio procedure, not the minimum date range of the individual case (glyph). How well the cation-ratio procedure is able to estimate the random observational error variability associated with producing each individual case (glyph date) can only be ascertained through the equations I provided (*RAR* 5: 145).

> Lanteigne has formulated a model of sampling which requires 84 000 samples! It would cost over a million dollars to test - an implausible expense. Plausibility is an important criterion of any scientific endeavour, be it sampling strategy or method for evaluating hypotheses. (*RAR* 6: 51)

I recognised the enormous amount of time and money invested into the cation-ratio procedure during the past seven years, and also that the sampling strategies necessary to sustain the inferences being conjectured by the researchers would entail an enormous capital expenditure (see *RAR* 5: 145). I therefore offered an alternative statistical modelling approach, one which could evaluate the probabilities of distribution differences between sample groups, where the variances of these sample means are unknown: Student's T-test (ibid., 145-6). I also recognised that many rock art researchers around the world would use (and are now using) the cation-ratio procedure without knowing about (nor understanding) the many intricate problems associated with it, and the inferential statements issued from it. To have remained silent would have cost these other researchers potentially millions of dollars without producing any tangible results.

> We disagree strongly with Lanteigne's interpretation that we are unable to make inferences concerning style duration ... To have a single motif present is enough to make an inference. (*RAR* 6: 51)

The presence of 'one' is of significance if and only if the characteristics of its singularity are 'concrete' (e.g. a coin with a mint date), and the presence of its singularity is corroborated (e.g. a 19th century coin found in a geological strata known to contain only 19th cemury material). If this same 19th century coin were found in a Precambrian Shield Archaic culture strata (c. 6500 - 2500 BP) we can safely infer that the presence of the coin is an anomaly created by processes other than those normally associated with its distinctness (e.g. rodent activity, pot-hunting etc.). We should not infer that its presence is due to a stylistic duration of 19th century coins from 6500 BP to 90 BP. When the characteristics of a singularity are due to mathematical processes, then decision theory must enter the assessment and statistically evaluate the probabilities of the event being present due to random errors associated with these mathematical processes.

The misunderstanding of the researchers was embedded in the assumption that the average of the three cation-ratio dates per glyph represented an 'actual' date (e.g. a coin's mint date), not a mathematically derived measurement (an assumption which similarly plagues archaeological inferences generated solely from ^{14}C dating). As it stands, the researchers sometimes discarded singularity events:

> one frieze in a corpus of rock art does not indicate a change of 'style' (ibid., 52).

and sometimes retained singularity events:

> To have a single motif present is enough to make an inference (ibid., 51).

not according to any stated 'objective' criteria (e.g. a 95% confidence interval) but only as it supported the 'subjective' needs of the model being tested. Evaluating numerical data on the basis of subjective interpretation most often results in inconsistencies when attempting to corroborate hypotheses.

> We take complete exception to Lanteigne's understanding of scientific methods. It is quite appropriate to generate hypotheses about chronological distributions without sampling more extensively. (*RAR* 6: 51)

The misunderstanding of the researchers in chronologically ordering the individual averaged dates was embodied in the assumption that these dates represented a population of individual scores, and not a sample of means. The utilisation of decision theory criteria within conditions of uncertainty, arising from samples containing incomplete information, must be assiduously considered at all times. This becomes especially relevant when we turn to the question of whether or not 'style' has changed in the Karolta district.

The problem with generating inferences from numerical data is that most decision theory criteria operate under assumptions of normal distributions. Violations of normality, linearity and homoscedasticity can pose significant inferential errors, unless the data is comprehensively screened and 'fixed'. These violations cannot be detected unless the sample size is sufficient for estimating random error bias. This is especially pertinent when we consider the use of only three points for establishing the AMS

^{14}C calibration curve. Any predictive solution generated from such a small sample will *always* appear to be perfect, and therefore meaningless. Most statisticians agree that a minimum of 20 cases per IV is essential for developing prediction equations (cf. Tabachnick and Fidell 1989). This ratio needs to be increased when the DVs are skewed, effect size is small, or substantial measurement errors are expected.

In the chronological ordering of the CR dates we need to take into consideration their nature as means (μ), not scores, and assess the probability that the difference between any two means is greater than zero; i.e. that the 'perceived' difference between the two means is not due to random sampling biases. We can accomplish this by calculating the standard error of the sample distribution of differences between the means of each pair of independent groups, where:

$$\sigma_d = \sqrt{(\sigma_{x1}^2 + \sigma_{x2}^2)},$$
$$\text{and } x_d = (x_2 - x_1),$$
$$\text{and } H_0\text{: } |Z| \geq |Z_{.001}| = 3.291$$

(cf. Hammond, Householder and Castellan 1970: 266). Since we are attempting to refute H_0 by demonstrating that non-random factors are the major contributors to differences in means, and the predicted number of misses for the Karolta rock art is quite high, a $|Z_{.001}|$ should be selected in the absence of additional sampling. This is justified by an unwritten 'rule of thumb' when performing multiple comparison tests: to divide the stated confidence interval ($p < .05$) by the number of tests conducted (n=23) to obtain a more valid interval ($p < .002$); because the probability of obtaining spuriously significant results will increase according to the Bonferroni Inequality effect (cf. Stevens 1986: 7). The results (Table 1) indicate extreme difficulty in chronologically ordering the glyphs on the basis of the data presented.

What Table 1 demonstrates is that differences in date means between various glyphs may be as high as 8900 years (Group K-12), or nearly 1/3 of the total sample range, before one could say with any statistical confidence that they represented real differences not attributable to random errors generated by the cation-ratio procedure. Clearly, this is not grounds for constructing sound chronological inferences for individual glyphs.

Case	x_d	σ_d	$H_0 \leq 0$	Prob.	Error % p < .05
		---------- Group K-13 ----------			
K-6	860	328	-219	p < .01	22.5
K-5	3378	625	+1322	p < .001	24.9
		---------- Group K-5 ----------			
K-17	826	770	-1709	p < .32	16.7
K-8	899	778	-1782	p < .32	16.9
K-15	2640	1010	-684	p < .05	21.4
K-14	4353	1113	+690	p < .001	20.0
		---------- Group K-14 ----------			
K-16	1004	1244	-4094	p > .32	15.9
K-9	1399	1368	-3102	p < .32	18.6
K-24	2367	1448	-2399	p < .32	18.8
K-2	2597	1496	-2327	p < .10	18.6
K-4	6085	1458	+1286	p < .001	14.5
		---------- Group K-4 ----------			
K-20	997	1802	-4935	p > .32	17.1
K-11	3771	2080	-3073	p < .10	18.0
K-19	4368	1943	-2028	p < .05	15.9
K-18	4453	1925	-1881	p < .05	15.7
K-10	5954	2434	-2058	p < .05	20.0
K-12	8715	2026	+2047	p < .001	13.9
		---------- Group K-12 ----------			
K-22	915	2696	-7957	p > .32	16.7
K-3	1718	2639	-6968	p > .32	15.5
K-21	1936	3036	-8056	p > .32	19.2
K-1	5407	2896	-4125	p < .05	15.7
K-7	6644	2818	-2631	p < .05	14.5
K-23	7821	2709	-1094	p < .01	13.1

***Table 1.** Macro-groupings of glyphs whose perceived differences in means are due to random processes (i.e. $|H_0 \leq 0|$, $p < .001$). (Caution: these macro-groupings are artificially constructed, with considerable overlap. Interpolations as to site 'phases', as traditional in archaeological contexts, are not valid.)*

> We do not consider the concept of continuous convention in the representation of motifs is illogical ... What we said was 'the range of dates shows that the stylistic tradition at Karolta has remained unchanged for at least 30 000 years'. (*RAR* 5: 123, 6: 52)

I personally disagree with the Binfordian logic of 'continuous convention' embedded in the characterisation of the transmission of knowledge as 'descent without modification' (*RAR* 6: 51-2). There are few things in life which are constant. Change is one of these constants. Weather changes, seasons change, faces change, generations change, individual perceptions change, cultural ideologies change. While we may find many examples in the real world where 'no change' is apparent, the inability to measure change is not a verification that no change has transpired, merely an indication of our measuring instruments' inadequacies to detect the changes which are occurring.

For the Karolta rock art assemblage, the question is not whether or not 'style' has changed during the past 30 000 years, but whether or not the preliminary cation-ratio data collected sufficiently supports the 'continuous convention' hypothesis. In order to proceed with the question, we set up the Null Hypothesis (H_0) - the cation-ratio sample supports the contention that 'style' *has not* changed in the Karolta rock art. The alternative hypothesis (H_a) would be considered viable if the Null Hypothesis is rejected. In testing these hypotheses we may reduce the definition of 'style' to its basic morphological parameters (shape), a position supported by Nobbs and Dorn (*RAR* 5: 119-20; 6: 51).

I have already assessed the Null Hypothesis before, using Student's T-test (*RAR* 6); but as the significance of its results were ignored by the cation-ratio researchers, I shall attempt to do so again using Analysis of Variance. Designed by Fisher (1942), the procedure is especially useful in analysing experimental data, when one wishes to measure variations in response (e.g. date) as an effect of a classification variable (e.g. motif) with an ability to control for extraneous variation resulting from random error vectors (e.g. cation-ratio preparational strategies). Essentially it compares the within-group (motif class) random variance (measurement error) to the between-group (motif classes) estimate of variance. If not significantly different from each other, then one must conclude that all class means were sampled from the same population (H_0): any differences in variance being due solely to random error vectors of the measurement process.

Of the 24 glyphs selected by Nobbs and Dorn (1988), four motif classes are distinguishable: 'tracks' (n=9), dots (n=5), lines (n=4) and circles (n=6), with 68 cation-ratio dates. (Only the anomalies identified by the researchers were excluded, in order to validate any comparison between our respective conclusions.) Since the Analysis of Variance procedure relies upon a balanced block design, and the line motif has a maximum of only 12 cation-ratio dates, 12 dates were randomly selected from each of the remaining three classes, for a total of 48. (Random numbers were generated by computer, assigned to all cation-ratio dates per class, sorted in ascending order and the first 12 dates per class output for analysis.) Four separate analyses of variance were conducted:

(a) a single main effects model which tests the hypothesis that the mean of the dependent variable (date) is the same for each motif class (Table 2);
(b) a Waller-Duncan K-ratio T test which minimi-ses the Bayes risk under additive loss (Table 3);
(c) the Pairwise T test which is comparable to Fisher's Least-significant Difference test (LSD) (Table 4); and
(d) Tukey's Studentised Range test (HSD) which controls the Type I experimentwise error rate (Table 5).

CLASS LEVEL INFORMATION

CLASS	LEVELS	VALUES
MOTIF	4	CIRCLE DOT LINE TRACK

NUMBER OF OBSERVATIONS IN DATA SET = 48

DEPENDENT VARIABLE: DATE

SOURCE	DF	SUM OF SQUARES	MEAN SQUARE	F VALUE	PR > F	R-SQUARE	C.V.
MODEL	3	1438528218.72917000	479509406.24305600	8.60	0.0001	0.369512	45.1676
ERROR	44	2454520054.25000000	55784546.68750000	ROOT MSE		DATE MEAN	
CORRECTED TOTAL	47	3893048272.97917000		7468.90532056		16535.97916667	

SOURCE	DF	ANOVA SS	F VALUE	PR > F
MOTIF	3	1438528218.72917000	8.60	0.0001

Table 2. Analysis of variance: main effects model for variable: date.

K-RATIO=100 DF=44 MSE=55784547 F=8.59574
CRITICAL VALUE OF T=1.91
MINIMUM SIGNIFICANT DIFFERENCE=5832

MEANS WITH THE SAME LETTER ARE NOT SIGNIFICANTLY DIFFERENT.

WALLER GROUPING	MEAN	N	MOTIF
A	24483	12	CIRCLE
B	17644	12	LINE
B			
C B	14702	12	DOT
C			
C	9316	12	TRACK

Table 3. Analysis of variance: Waller-Duncan K-ratio T test for variable: date.

ALPHA=0.05 CONFIDENCE=0.95 DF=44 MSE=55784547
CRITICAL VALUE OF T=2.01537
LEAST SIGNIFICANT DIFFERENCE=6145.2

COMPARISONS SIGNIFICANT AT THE 0.05 LEVEL ARE INDICATED BY '***'

MOTIF COMPARISON	LOWER CONFIDENCE LIMIT	DIFFERENCE BETWEEN MEANS	UPPER CONFIDENCE LIMIT	
CIRCLE - LINE	693	6839	12984	***
CIRCLE - DOT	3636	9781	15926	***
CIRCLE - TRACK	9022	15167	21312	***
LINE - CIRCLE	-12984	-6839	-693	***
LINE - DOT	-3203	2943	9088	
LINE - TRACK	2183	8329	14474	***
DOT - CIRCLE	-15926	-9781	-3636	***
DOT - LINE	-9088	-2943	3203	
DOT - TRACK	-759	5386	11531	
TRACK - CIRCLE	-21312	-15167	-9022	***
TRACK - LINE	-14474	-8329	-2183	***
TRACK - DOT	-11531	-5386	759	

Table 4. Analysis of variance: T tests (LSD) for variable: date.

ALPHA=0.05 CONFIDENCE=0.95 DF=44 MSE=55784547
CRITICAL VALUE OF STUDENTIZED RANGE=3.776
MINIMUM SIGNIFICANT DIFFERENCE=8141.1

COMPARISONS SIGNIFICANT AT THE 0.05 LEVEL ARE INDICATED BY '***'

MOTIF COMPARISON	SIMULTANEOUS LOWER CONFIDENCE LIMIT	DIFFERENCE BETWEEN MEANS	SIMULTANEOUS UPPER CONFIDENCE LIMIT	
CIRCLE - LINE	-1303	6839	14980	
CIRCLE - DOT	1640	9781	17922	***
CIRCLE - TRACK	7026	15167	23308	***
LINE - CIRCLE	-14980	-6839	1303	
LINE - DOT	-5198	2943	11084	
LINE - TRACK	187	8329	16470	***
DOT - CIRCLE	-17922	-9781	-1640	***
DOT - LINE	-11084	-2943	5198	
DOT - TRACK	-2755	5386	13527	
TRACK - CIRCLE	-23308	-15167	-7026	***
TRACK - LINE	-16470	-8329	-187	***
TRACK - DOT	-13527	-5386	2755	

Table 5. Analysis of variance: Tukey's Studentised Range (HSD) test for variable: date.

All tests indicate that there are significant differences between some motif class dates, most assuredly between circles and tracks by as much as 15 000 years, with probable distinctions between circles and dots and lines. There is some overlap between tracks and dots, with probable distinctions between tracks and lines. Since the perceived differences between class means cannot be attributed solely to random error vectors of the cation-ratio measurement process, we have no choice but to *reject* the Null Hypothesis (H_0) in favour of the Alternative Hypothesis: the cation-ratio sample supports the contention that 'style' *has* changed in the Karolta rock art.

Tentatively, a developmental sequence of rock art styles may be posited for the Karolta assemblage along basic morphological parameters:

Circles 24 483 ± 1466 BP
Lines 17 644 ± 2732 BP
Dots 14 702 ± 2397 BP
Tracks 9 316 ± 1827 BP

(Standard error point estimators are given at one unbiased standard deviation of the population mean.)

The temporal distinction between various 'styles' of this proposed development sequence is statistically significant between circle and track motifs ($p < .0001$) and between circle and dot motifs ($p < .002$), but only marginally significant between circle and line motifs ($p < .04$) and between line and track motifs ($p < .02$), keeping in mind that the Bonferroni Inequality effect compensation criteria would be ($p < .008$) for the sequence as a whole. How accurate this preliminary sequence reflects reality can only be ascertained through more comprehensive sampling strategies. The extent to which regional archaeologists should place stock in this temporal classification is dependent upon how much they are prepared to place upon the cation-ratio procedure itself. In the absence of additional sampling, if a general correspondence of this sequence is similarly reflected in a cultural seriation of the archaeological record (say, in lithic typologies), then greater weight could be placed upon the conclusions here tendered. Not being familiar with regional cultural parameters, I am unable to generate any relevant inferences as to this potential correspondence, aside from recommending that all future absolute dating experiments (be they the cation-ratio or any other technique) must take into consideration the need for assessing regional correlations when devising their sampling strategies. To not do so would be akin to conducting archaeological excavations without due attention to the local geological strata.

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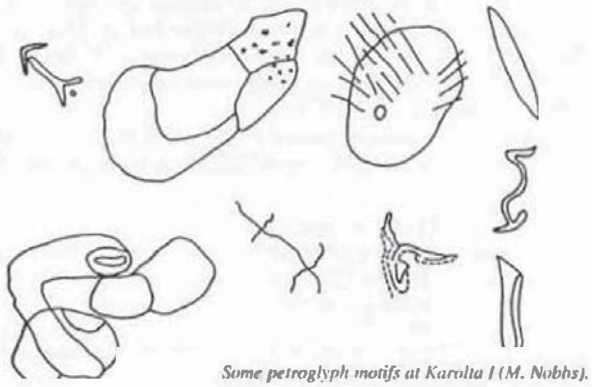

Some petroglyph motifs at Karolta I (M. Nobbs).

Rock Art Research 1991, Vol. 8, No. 1, pp. 36 - 46.

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

Howard P. McNickle

COMMENTS

On form and meaning in rock art research

By BRUNO DAVID and JOSEPHINE FLOOD

In commenting on McNickle's recent paper, we would like to dwell on a single but crucial point. In doing so we do not wish to reduce the value of general syntheses of regional bodies of rock art such as McNickle's useful outline, but merely wish to briefly highlight a common confusion in rock art analysis. We refer to the confusion between concepts of '*meaning*' and the analysis of rock art *forms*. More specifically, and having undertaken detailed archaeological and anthropological research in the Victoria River District (VRD) since 1988, we wish to comment on aspects of McNickle's interpretation of large, paired anthropomorphs found at Delamere and Yingalarri (McNickle's Ingaladdi; Aboriginal words used in this paper are spelt according to conventions used by linguists working in the region, and may differ from names on maps, such as Ingaladdi).

During our research in the area, we have undertaken six excavations of rock art sites and recorded in detail over 150 sites, including over 30 000 rock pictures. The significance of many of these sites to the local Wardaman traditional owners was recorded, including the meaning(s) of particular sites and paintings to numerous individuals under various contexts. A particular emphasis that was repeated many times and by many Wardaman people, including *all* of the individuals who claim primary Dreaming affiliations with the sites in question, is that the large, paired and often striped figures around Yingalarri and in nearby locations are *not* directly associated with the Lightning Brothers. Such pictures usually depict the identity of the principal Dreaming beings on whose land they occur. For instance, the paired figures at Murning, in the Yingalarri area, are *gulirida*, peewees. These are located in *gulirida* country. Nearby, two other similar figures occur, but these are known to be *gornbu*, hawks. A few kilometres away, two other similar figures are *menngen*, white cockatoos, whose old-man husband lives nearby. These paired striped figures are female, with clearly depicted vulvae, so are unlikely to be Lightning Brothers. Further away again, on the northern edge of McNickle's 'Innesvale-Willeroo' area, similar figures are elderly *yirmi-nyonong*, Dreamtime beings who travelled south-east from Port Keats during the Dreamtime (the site described by McNickle [p. 41] as having 'young lightnings'). The only place that we know of where the Lightning Brothers, Yagjagbula and Jabirringgi, are present as large paired anthropomorphs, is at Yiwarlarlay, the Lightning Brothers site reported by Davidson (1935), Arndt (1962), and excavated by us in 1989 (David et al. 1990).

In short, the Dreaming interpretation of large, paired and often striped figures from the VRD is largely a product of the Dreaming beings who give identity to the land. But for 'outsiders' to interpret this body of rock art with respect to its meaning(s) requires knowledge of the narrative tradition, and knowledge of individual sites themselves. Similar paintings in different places (or even in the same place for that matter) do not necessarily mean the same things to the people who claim 'traditional' affiliation with the sites. On the contrary, we have found that similar painting forms often mean different things, and that different painting forms often mean similar things. This issue has been addressed by us in greater detail elsewhere (Frost et al. in press), as well as by Merlan (1989).

The problem of interpretation of rock art is only a problem because researchers (interpreters) often confuse a number of very different issues:

1) What did/does the art mean to the person(s) who created it.
2) What does the art mean *today* to those who claim traditional affiliation with it. Importantly, a painting/petroglyph can have different meanings to different individuals, it can have more than one meaning to any given person(s) at any particular point of time, and its meaning(s) can change with the context of interpretation (e.g. a painting can be interpreted in one way when uninitiated people, or researchers for that matter, are present, but its meaning can be very different in the context of initiated individuals from the correct Dreaming affiliation). Any particular type of interpretation cannot be merely seen as right or wrong, for the concept of correctness must be understood in relation to context of interpretation.
3) What does the art mean to the observer as archaeologist, rock art researcher etc.

In short, every painting can have a different meaning, and the rules for interpretation can only be understood by reference to the narrative tradition. Without wanting to undermine our appreciation of McNickle's paper it is stressed here that his repeated interpretations of large, paired anthropomorphs as Lightning Brothers wherever they occur in the VRD are not an accurate reflection of the meaning(s) of this art to local traditional custodians, to whom such paintings usually reflect the identities of the principal Dreaming beings from the areas in which they occur. Furthermore, in all cases documented by us, large, paired anthropomorphs of the type discussed here are always seen by traditional owners to be the Dreaming beings themselves, who 'sat' in the rock in the Dreaming. They are *buwarraja*. Dreaming, and were never created by people, although they may be retouched to 'make them look good'.

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Rock painting sizes in the Kimberley and Victoria River District

By GRAHAME L. WALSH

The recent publication in *RAR* by McNickle has provided a valuable overview of the major rock art body of the Victoria River District (VRD), Northern Territory, and given researchers an appreciation of the techniques, sizes and diversity of rock art motifs existing in this little-known region of Australian rock art. Based on the author's own recent research in north-west Australia he would like to clarify some of the points discussed by McNickle.

Some emphasis has been placed on the unusually large proportions of many motifs within the VRD. In the Abstract it is noted that 'particularly noteworthy is the occurrence of the world's largest known paintings', and this is emphasised by the comment that 'the largest rock paintings in Australia are found in this region' (*RAR* 8: 45).

At that time the largest recorded motif was indeed the bichrome snake motif located in the Keep River area by McNickle, and illustrated in Walsh (1988: 248, Pl. 309). The measurements of this and other figures in McNickle's report depend on the measuring device used, as is evidenced by the length of 25.26 m reported by the author, compared to that noted by McNickle (p. 39), of 24 m.

During the 1991 field season (May to August) a bichrome (white infill and red outline) painting was recorded in the west Kimberley. Like the Keep River snake and many other huge rock paintings, it had been subjected to partial deterioration. The figure has deteriorated at either extremity where it is not adequately protected by the shelter's overhang, but still remains identifiable for at least 43.7 m. The availability of adjacent suitable surfaces and faint traces of outline indicate that originally it may have been eight to nine metres longer. This is not a recent figure, as is evidenced by the loss of most of the less stable white infill paint, and by the degree of overpainting which has in sections camouflaged much of the perimeter line work.

The deteriorated extremities make it difficult to positively identify the subject of this massive motif. As this area of the west Kimberley does not contain large snake motifs but does contain distinctive, long torso anthropomorphs from the early Wandjina Period, the motif has most probably been a huge example of these figures with small limbs and Rayed Headdress (the use of capitals for certain motif or period names in this paper refers to my terminology in the detailed recording work in this region, and these terms are defined in Walsh in prep.). When compared to the largest recorded rock painting outside Australia, reputedly a figure of approximately 10 m in India (R. G. Bednarik pers. comm. in McNickle, p. 39), the Kimberley figure exceeds four times its length.

Apart from the claim concerning the largest individual motif, some comment is warranted on the more conventionally sized rock paintings. McNickle (p. 40) refers to contorted anthropomorphous figures 'up to 7 m long' from the Pinkerton Range and Keep River areas of the VRD, which appear to be the largest anthropomorphs recorded. In the west Kimberley examples of a distinctive group the author refers to as Prominent Digit Slim Figures (Walsh in prep.) have been recorded in lengths of up to 7.32 m.

In the area of zoomorphic figures McNickle (p. 42) refers to 'salt-water crocodiles of 7 m', and again the measurements possibly reflect the devices used to obtain this information. On accompanying McNickle to the site containing the reputed largest known VRD crocodile motif, accurate measurements with a tape measure showed it to be 6.23 m long. In the west Kimberley a very well preserved ceiling depiction of a crocodile measures 7 m. This spectacular figure from the late Wandjina Period bears considerable mythological significance and is not regarded as the picture of a real crocodile by a knowledgeable person.

Excluding the exceptional examples, among which the Kimberley 43.7 m figure far exceeds any other recorded painting in size, the difference between the largest recorded Kimberley and VRD motifs is currently only 32 cm for anthropomorphs and 77 cm for zoomorphs. It may appear trivial to draw attention to these details but it could be of value to reconsider the claims concerning the largest known paintings in the light of the new evidence.

The composite painted-engraved technique of rock art production, referred to by McNickle as 'lacking in most rock art regions ... one of the most distinctive features of VRD rock art', is also present in the Kimberley art body. It is most commonly associated with facial features of some Wandjina figures, where artists have used it to emphasise features such as eyes and nose, which are then usually infilled with black paint. This modification is evident on some of the Wandjinas illustrated in Walsh (1988: 184, Pl. 216, and 192, Pl. 228), and is a feature earlier referred to on the coastal Kaiara figures (a variation of Wandjinas) by Crawford (1968: 78).

In addition to the discovery of very large paintings in the Kimberley the author would like to draw attention to other little-known features of that region, including a wide range of very fine grass prints (mentioned in Welsh 1990), string and object prints now recorded along the length of the Drysdale River, east to Wyndham and in areas at the Northern Territory border, south through the Baines head-waters and north-east through the VRD.

The author has now recorded regional variations of definite groupings of anthropomorphs from the Kimberley Bradshaw Period not only in the Pentecost Range border area, but further east in the Fitzmaurice River area of the Northern Territory. Although these Bradshaw variants display less refined application techniques, as is common in the many phases of VRD rock art, the author's research has indicated that they are unmistakable links to the Kimberley mid-late Bradshaw Period art. These differ from the 'early paintings' figures referred to by McNickle (p. 42) and first published by Lewis (1984).

A range of bees wax art has also been recorded through the west Kimberley, mostly of small anthropomorphous figures, ranging from patinated, deteriorating and apparently relatively ancient forms, through to a very recent Wandjina head depiction.

A range of frequently occurring, unusual and ancient zoomorphic images (possibly depicting extinct species) have been found, largely in the Irregular Infill Animal Period (Walsh in prep.), and also a fine example of a 1.93 m-long 'thylacine' showing fine head detail.

Earlier researchers (Davidson 1936: 119; Mulvaney 1975: 261) have drawn attention to artistic influence of Kimberley traditions on those of the VRD, although Lewis and McCausland (1987: 76) commented that 'this has yet to be convincingly demonstrated'. Without elaborating in this brief report on important facets of technique and feature detail, the author feels confident that he now has sufficient documented evidence to verify that there has been artistic (and probably mythological) influence extending from the Kimberley region to the VRD during various prehistoric times, and possibly as far east as Pine Creek. In more recent prehistoric times there is also evidence of a cultural 'reverse flow', involving the diffusion of distinctive physical features and decorative designs from the innovative artistic region of the West Baines into the southern Kimberley. These matters will be dealt with in a later publication.

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RAR 8-213

BRIEF REPORTS

Bead-like fossils from an Acheulian occupation site, Israel

N. GOREN-INBAR, Z. LEWY and M. E. KISLEV

Introduction

The Gesher (= bridge) Benot Ya'aqov Acheulian site occurs within the Benot Ya'aqov Formation (in Arabic: Jisr Banat Yaqub) and is located in the northern sector of the Dead Sea Rift, some 4 km south of the Hula Valley. It is exposed at c. 70 m above m.s.l. along the course and on both banks of the Jordan River. It is bordered by the Golan Heights in the east, and by the 'Korazim Saddle' in the west (Belitzky 1987) (Fig. 1).

Due to its Acheulian material culture, remains of vertebrates and freshwater gastropods, the Middle Pleistocene Benot Ya'aqov Formation (Horowitz 1973) was subjected to intensive study (Stekelis 1960; Hooijer 1959, 1960; Picard 1963, 1965; Geraads and Tchernov 1983; Gilead 1970, 1973; Tchernov 1973; Bar-Yosef 1975; Horowitz 1979). The stratigraphy and the tectonic history of this region of the Rift have lately been described in detail by Heimann (1990).

The age of the Benot Ya'aqov Formation is determined by radiometric and palaeomagnetic methods, as well as by bio-stratigraphic and archaeological considerations. It is presently considered to be between 0.73 myr (the beginning of the palaeomagnetic Bruhnes normal epoch) and 0.24 myr (Goren-Inbar and Belitzky 1989; Goren-Inbar et al. in prep.).

In two recent field seasons (1989, 1990) dedicated to the study of the Benot Ya'aqov Formation, seven Acheulian archaeological occupation floors were discovered. The two floors excavated yielded most impressive finds, such as the earliest wooden plank with man-made polish (Belitzky et al. 1991), and extremely rich and diverse lithic assemblages, including typical Acheulian artefacts such as hand-axes, cleavers, flake tools etc. (Goren-Inbar et al. in press; Goren-Inbar et al. in prep.).

Two bead-like objects (henceforth termed 'beads') were found in the extremely rich Acheulian occupation Area B, layer II-6, level 1. The sediment from this horizon was wet-sieved (2 mm mesh) in the Jordan River, dried and sorted. The beads have a low cylindrical shape with two flat faces and a hole in the centre, which could be suggestive of a cultural function. Naturally shaped fossils were used by prehistoric hunters-gatherers for various purposes, and crinoid segments have been described as ornamental beads (Leroi-Gourhan 1965; Marshack 1989). The determination of either the function or the incidental occurrence of the crinoid segments in the Benot Ya'aqov site is thus of great significance.

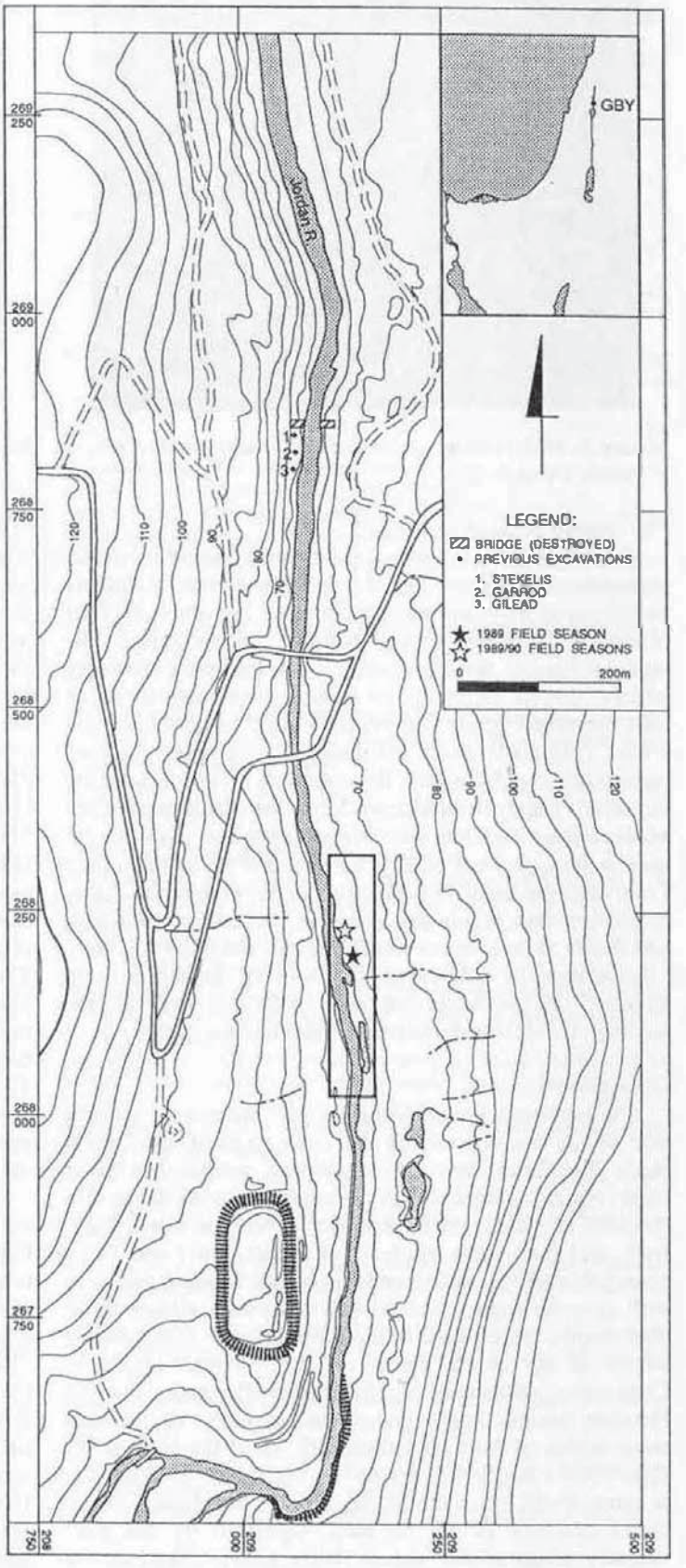

Figure 1. Location map of Gesher Benot Ya'akov site on the northern Jordan River, Israel.

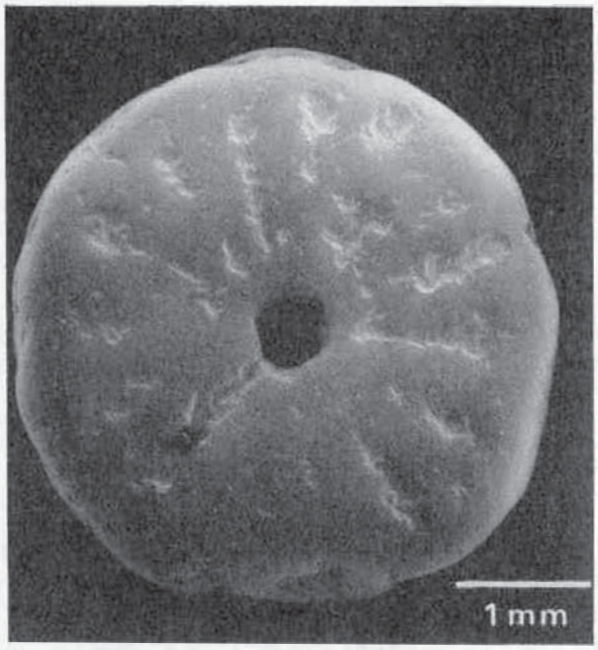

Figure 2. Millericrinus columnal segment from Gesher Benot Ya'aqov.

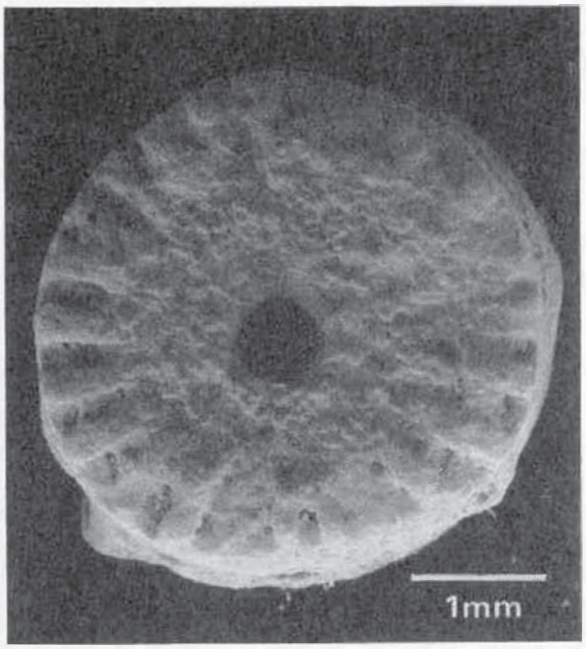

Figure 3. Jurassic Millericrinus columnal segment from Makhtesh Hatira.

The crinoid columnal segments

Two small (d = 3.5 mm and 3.8 mm) crinoid columnal segments were found (Fig. 2). In spite of their complete silicification they are heavily corroded but some of their characteristic morphological features are preserved. The articular face of these segments is covered with relatively widely spaced radiating crenulae, resembling those in well-preserved Upper Callovian (Middle Jurassic) *Millericrinus* columnals from Makhtesh Hatira (Fig. 3). This pattern differs from the fine, densely spaced radiating crenulae in coeval Aplocrinitidae from Makhtesh Hatira. *Millericrinus* are Jurassic crinoids, present also in the marine Jurassic beds of Mt Hermon, mainly in the Upper Callovian, the highly fossiliferous upper part of the Zohar Formation and in several levels in the Oxfordian Kidod and Be'er Sheva Formations (Picard and Hirsch 1987). Silicification of calcareous skeletons of fossils is quite common in the Callovian and Oxfordian beds in the outcrops of Makhtesh Hatira and Mt Hermon.

Discussion

The processes whereby the 'beads' became an integral part of the site deposit are the main issue of the present study. The 'beads' are of a minute size, smaller than many other typical sedimentological components of layer II-6 (pebbles of basalt, chert, molluscs, pieces of wood, bark, fruit, and man-made artefacts of basalt, chert and limestone). Numerous well-rounded, corroded quartz grains as well as some angular (fresh) quartz crystals, all of similar dimensions, were found in the same deposit. The possible source of the quartz grains are the outcrops of Lower Cretaceous sandstone of the Hatira Formation at Mt Hermon. A similar provenance was suggested for the sandstone lenses in the Late Quaternary fill of the Hula Basin (Kafri and Lang 1987). However, the Benot Ya'aqov site is some 25-30 km south of the Jurassic and Lower Cretaceous outcrops of Mt Hermon, separated by the sedimentary basin of the former Hula Lake. Therefore the mode of transport of the crinoid segments to the Acheulian site is puzzling.

Several studies (Horowitz and Horowitz 1985; Horowitz 1989; Heimann 1990) demonstrate that there was no topographical low (Hula Valley) between the Gesher Benot Ya'aqov site and Mt Hermon during the Late Miocene-Lower Pliocene times. This setting may have enabled drainage from the Mt Hermon area to transport fluvial particles (including 'beads' and quartz grains) southwards into the Benot Ya'aqov area.

Palaeogeographic and palaeoenvironmental data indicate that during Middle Pleistocene times, a freshwater body already existed in the Hula Basin (Picard 1963, 1965; Ehrlich 1973; Horowitz 1973, 1979; Horowitz and Horowitz 1985). Peat, lignite, fine-grained sediments and extensive communities of freshwater molluscs point to marshy and paludine conditions (Avnimelech 1986; Brener et al. 1978; Moshkovitz and Magaritz 1987; Picard 1952; Tchernov 1973). Based on the study of many core drillings carried out in the Hula Valley, there is evidence that a high-energy, fluvial sedimentary environment was confined to the margins of the basin (Kafri and Lang 1987: 77) until 1951 when the Hula Lake was drained. Therefore it is unlikely that the 'beads' could have been transported across the water body which existed in the Hula Basin to the area of the Gesher Benot Ya'aqov site.

Alternatively, the beads as well as the quartz grains could have been eroded from the Jurassic and Lower Cretaceous rocks of Mt Hermon during Middle Pliocene times, when Mt Hermon was south of its present position, opposite the site of Gesher Benot Ya'aqov (Kashai and Crocker 1987: 58).

Although we are assuming a natural transportation of the 'beads' to the Acheulian site, this does not mean that Acheulian hominids lacked the physical and mental abilities essential for the 'identification' of the objects. The mental abilities, a controversial issue in Middle and even Upper Pleistocene times, are beyond the scope of the present article (Bednarik 1990; Chase and Dibble 1987; Mania and Mania 1988 and comments there; Marshack 1989, to mention only a few).

Allochthonous items of different materials and shapes

are frequently reported from prehistoric sites (Leroi-Gourhan 1965). Additional data were recently reported from several Lower Palaeolithic sites which include fossil objects from Europe (Marshack 1989, 1990) and Israel (Berekhat-Ram, pers. obs.), quartz crystals (Bednarik 1988: 98; d'Errico et al. 1989) and coloured minerals (Chase and Dibble 1987 and references therein; Bednarik 1990). A crinoid columnal segment, described as a 'perforated object' was discovered in the Chatelperronian (Upper Palaeolithic) cave sediments of Arcy-sur-Cure, France (Leroi-Gourhan 1965; Bahn and Vertut 1988: 72; Marshack 1989: 10). It was considered as a piece of a decorative object, an integral component of the Chatelperronian material culture. Nevertheless, no beads or pendants of such small size were reported from Acheulian assemblages.

Summary

The 'beads' found at the Acheulian site of Gesher Benot Ya'aqov were identified as crinoid fragments. Their size, shape and central holes are all of natural origin and not a product of human cultural activity. The most probable source of the crinoids is the Mt Hermon Jurassic deposit. Palaeohydrographic considerations allow us to assume that the crinoids reached their depositional site earlier than Late Pliocene times, when a hydrographic system could have transported materials from Mt Hermon southwards, with no sedimentary traps along its course. Nevertheless, in other archaeological and sedimentological contexts, such objects could be considered as beads.

Acknowledgments

This study was supported by the Hebrew University, the Basic Research Foundation administered by the Israel Academy of Sciences and Humanities, and by an anonymous donor. We wish to thank S. Belitzky for his helpful and professional comments and the students who sorted the materials. Figure 1 was drawn by G. Hivroni.

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EDITORIAL COMMENT

After Marshack's valid proposition that Palaeolithic beads or pendants need not necessarily have an artificial perforation (Marshack 1991), this paper provides a timely warning that in certain sediment types such naturally perforated objects may occur naturally at an occupation site. In the case of the two objects introduced here, neither possibility is proven.

It is interesting that crinoids have continued to be used as decorative beads right up to the ethnographic past of the region. A recent publication of the Stuatliches Museum für Völkerkunde Dresden, a paper on Palestinian jewellery, includes a photograph of a bead described as a 'fossilised sea urchin?' (Helmecke 1990: Pl. 13, item f), which is clearly a crinoid cast and resembles those described by Goren-Inbar and colleagues.

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RAR 8-214

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A new petroglyph recording method

EVE SELISAAR

One can encounter many difficulties in the recording of petroglyphs. The new technique suggested here may be of some help when observing or photographing engraved patterns on rock surfaces.

The investigation of petroglyphs is often complicated by the presence of erosion marks or shallow natural grooves. Copying on paper does not necessarily provide objective recordings because the researcher may subconsciously modify the patterns according to previous experience or bias.

Even photographs may provide only distorted results as their accuracy depends on factors such as the direction of lighting and the angle of viewing. Some details are accentuated, others subdued. The result are cumulative difficulties in the interpretation of the less distinct features of the petroglyph.

A piece of damp thin cotton cloth can facilitate the study of petroglyphs considerably. The light-coloured damp cloth is placed on the rock surface, preferably in the favourable weather conditions of sufficient warmth and sunshine. In these conditions the cloth dries more rapidly over the engraved grooves, and the petroglyph motif appears on the cloth as a lighter area against the darker background created by the damp areas of the cloth. Also, the features of the rock surface around the petroglyph become apparent in greater detail (Fig. 1).

The resulting image is a faithful reflection of the original, created with the help of Mother Nature: with water, air and sunshine - and a piece of cloth.

Unfortunately the image so created does not persist for very long, it disappears as the cloth dries. If it is removed when still damp, the rock around the petroglyph is darker than the grooves and the motif therefore stands out. This contrast also vanishes soon (Fig. 2).

***Figure 1.** The damp cloth covering the petroglyph has dried over the motif.*

***Figure 2.** The petroglyph after the damp cloth was removed. Lake Onega, U.S.S.R. Photographs by Erkki Suonio, Finland.*

The method is most suitable for comparatively even and horizontal rock surfaces. They must be cleared of sand and vegetation before the cloth is applied.

The accompanying photographs were taken on the shore of Lake Onega, Karelia, U.S.S.R. The petroglyphs there are said to date from about 4800-4000 BP. They were pecked with stone tools into the granite pavements. The depth of the grooves ranges from 0.5 mm to 2 mm, the size of motifs from 5 cm to about 4 m.

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EDITORIAL COMMENT

From the rock art conservator's point of view, the method proposed by Selisaar is clearly preferable to the practice of producing 'rubbings' of petroglyphs, which is now confined to Scandinavia, China and North America. Various writers have condemned it on the basis of the mechanical damage it causes

(such as granular attrition), but recently Professors Dorn and Loendorf have noted that this practice can even change the chemistry of patinae (Dorn 1990: 10), which has the potential of prejudicing future analytical work.

Some obvious limitations would be that Selisaar's method must only be applied to petroglyphs that are exposed to rain under natural conditions; and that the cloth used should not be dyed, and should be thoroughly washed or boiled before use to remove any residues of water-soluble substances that remain from the manufacturing processes (such as mercerisation chemicals).

Subject to these simple precautions, the method proposed here seems a viable alternative to the method of night photography, the currently preferable 'simple' technique of recording petroglyphs. Comments are invited from rock art conservators.

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RAR 8-215

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

The symposium rationales for five of the Cairns '92 symposia appeared in the May 1991 issue of *RAR*, and in other journals around the world. Here are those for the remaining symposia. Abstracts of less than 200 words are invited, and should be submitted either to the AURA Editor, or to one of the authors of the rationale *and* the AURA Editor.

The dating of rock art

ALAN WATCHMAN and JACK STEINBRING

One of the most electric aspects of rock art research is the placement of the motifs in time. Establishing a chronology for rock art is a significant element in all rock art studies around the world. Various means were used, with varying degrees of success, in order to place rock pictures in practical and reliable time-frames. Chronology is imperative to the elucidation of changes in processes relating to rock art, in the determination of origins and interchanges, and in the assessment of innovation. Practically no study of rock art can be fulfilled without attention to its absolute and relative antiquity.

In order to bring the world community of rock art practitioners abreast of recent substantive and procedural developments in this speciality a symposium on the dating of rock art will be conducted at the Second AURA Congress in Cairns.

All interested persons are invited to forward their intentions to present a paper at the Congress on the dating of rock art. Abstracts of approximately 200 words or less should be sent to:

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We invite papers on recent technical advances, chronological data from sites, prospective methods and work in progress. The deadline for submission of abstracts is 1 April 1992. We also request each author to submit two copies of their paper at the symposium in Cairns for final editing in order to expedite publication. Such a deadline and request may now seem remote 'in time', but do not be misled, 'time flies'!

For more information, if required, contact Dr Jack Steinbring at the above address or on telephone: Canada (204) 786 9719 or (204) 786 7811.

RAR 8-216

Preservation of rock art

ANDREW THORN and NICHOLAS STANLEY PRICE

Following on from the successful Symposium M (Conservation and site management) of the Darwin congress (Pearson and Swartz 1991), many advances have been made in the understanding and implementation of preservation technology appropriate to rock art.

In 1989 the University of Canberra, in conjunction with the Getty Conservation Institute, held a one-year graduate diploma course in the conservation of rock art (Pearson 1991). This was the first course of its type in the world aimed specifically at the preservation of rock art.

In the same year ICCROM funded a conservator to conduct a training project at Nourlangie Rock, Kakadu National Park, Australia, while in 1991 the GCI, as part of an ongoing commitment to rock art preservation training, held a one-month course in preservation intervention at Painted Rock, California.

Together with these training projects has been the ongoing funding of rock art protection research by the Australian Institute of Aboriginal and Torres Strait Islander Studies in Canberra (Ward 1989; Ward and Sullivan 1989).

All of this recent input into protection has increased the awareness of preservation technology and the amount of work being carried out to protect sites.

This session *Preservation of rock art* will focus on the research and field work being undertaken in various countries that contributes to the preservation of sites. Papers on environmental analysis, materials analysis, method testing and research related to preservation will be encouraged as the basis of this symposium.

In addition more general protective procedures, such as the construction of walkways and signs that protect a site, will be considered. Management strategies that ensure that a site is maintained in the future, and the co-operative role of researchers and custodians in achieving long-term preservation policy will also be explored. Reports on training, past and future, will also be welcome.

Abstracts of less than 200 words should be submitted to the first-named chairperson, and a copy be forwarded to the AURA Editor. Completed papers should be submitted prior to the conference.

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RAR 8-217

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The ethics of rock art research

ROBERT G. BEDNARIK and MARIO CONSENS

A separate discipline of rock art studies, palaeoart studies or cognitive epistemology has begun to emerge since 1988, through the fusion of specific research interests, avocational activities, advanced analytical methods, multidisciplinary concerns, epistemological needs and the concerns of indigenous societies. To some extent, these recent developments have been quite spontaneous, they are often of altruistic dynamics but with little attention to the philosophical undercurrents, heuristic practices or ethical imperatives involved. In the creation of this new discipline (Odak 1991) we have several quite unique opportunities. For instance, it is still feasible to assemble a complete bibliography of this field (Bednarik 1991a). It is also possible to guide the formation of the discipline from the outset by establishing *a priori* ethical standards.

Every scientific discipline has some form of ethical standards, but their significance differs greatly according to the social, academic or philosophical responsibilities of the respective discipline. In our case, several unique conditions apply. The academically most important is that in trying to explore the basis of the human constructs of reality through the study of early expressions of consciousness, we should be particularly conscious of epistemic standards. The socially most important responsibility is that in conducting our work, we must be responsive to the needs of, and respecting the prior claims by, indigenous people.

The Cairns congress presents an opportunity to solicit relevant proposals from those who wish to study rock art, those who wish to appreciate it, those who wish to preserve it, and those who have been its traditional custodians. Since these groups have strong common interests it should be possible, through constructive discourse, to develop a set of ethical guidelines that are acceptable to all the interest groups concerned. To this end it is proposed that a symposium be held, consisting of paper presentations and debates.

The subjects to be addressed should include, but not necessarily be limited to:

1) Ethics in rock art recording methods: exploring the reasons for the rejection or acceptance of different methods (Bednarik 1990).
2) The removal of samples from rock art: what would be acceptable ethical guidelines for removing samples of paint, geomorphic deposits etc. (Bednarik 1991b).
3) Indigenous curatorship of rock art: how are the rights of traditional owners or curators of rock art to be preserved or enhanced? (Bednarik 1991c).
4) Standards in academic dialectic and presentation: debate procedures, research and publication standards, avoidance of plagiarism, refereeing etc.
5) Ethics in dealing with the public: announcement of research results, site visitation, exhibitions, promotion of indigenous cultural achievement etc.
6) Ethical standards in conservation and management: conservation philosophies, established guidelines (e.g. ICOMOS and Burra).

Abstracts of no more than 200 words, of papers on any of these subjects, are invited and should be sent to the *RAR* Editor.

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Workshops on management: indigenous experiences and perceptions

These workshops will encourage indigenous peoples from Australia, New Zealand, the Pacific, the Americas, Asia and elsewhere to recount their experiences of rock art management practices, and also encourage them to express their views on how rock art research and management should progress in the future. It is hoped that these broad discussions might enable the development of a code of ethics and practices (see above). Participants in these workshops are encouraged to produce brief and stimulating reports in an informal atmosphere with an emphasis on discussion. An extended announcement will appear in the May 1992 issue of *RAR*.

The Conveners: Natalie Franklin and Michael Rowland, Heritage Branch, Department of Environment and Heritage, P.O. Box 155, North Quay, Queensland 4002, Australia.

REVIEWS & ABSTRACTS

'AURA BOOK OF THE YEAR':

The rock paintings of the upper Brandberg. Part 1: Amis Gorge, by HARALD PAGER. 1989. Compiled by Tilman Lenssen-Erz, edited by Rudolph Kuper, with contributions by Peter Breunig and T. Lenssen-Erz. Heinrich-Barth-Institut, Cologne. 502 pages, 8 colour plates, 16 monochrome plates, 260 pp. of rock painting reproductions, 2 maps and numerous line drawings, 119 pp. of tables. Includes also cassette with 12 folded plates, maximum size 100 x 140 cm. Hardcover, DM125.00. ISBN 3 927688 01 2.

Until this publication by the University of Cologne, Harald Pager was best known for his exemplary recording of rock art sites from a selected area in the Natal Drakensberg in South Africa, published in 1971 as *Ndedema: a documentation of the rock paintings of the Ndedema Gorge*.

Following on the success of this volume and the acclaim that he received, Pager subsequently devoted his life to an unshakeable commitment to recording rock art for posterity. He was a superb draughtsman, and his work is meticulous to a degree difficult to surpass. His final challenge was a great water-less mountain, the Brandberg, surrounded by the Namib Desert in what is now Namibia, formerly South West Africa. Here Pager fully recorded 879 of the 1045 sites he and his helpers located, meticulously tracing no less than 43 000 individual figures and using approximately 6 km of plastic foil in the process. He died in Windhoek in 1985 (refer obituary in *RAR* 2: 174-5), just short of completing his task, as a result of renal failure induced by inadequate water supplies.

This published work on the Amis Gorge, the first in a series of eight to ten projected publications, is therefore a posthumous compilation of his dedicated labours. Completing the unfinished work of another is not an easy task, but was bravely taken on by a team of colleagues from Cologne University in Germany, the institution which financed Pager's field work.

The Amis Gorge publication, which covers 66 sites documented during 1979, comprises two large volumes: a singularly heavy bound book of over 500 pages, and a folio containing 12 folded black and white illustrations of varying sizes on parchment-coloured paper. A comprehensive foreword, in English, is written by Professor Rudolph Kuper, followed by a chapter in both English and German by Peter Breunig on archaeological investigation into the settlement history of the Brandberg. Harald Pager contributes a chapter in German, with an English summary, on observations concerning the selection of rock art sites in the Brandberg. This is followed by a brief and anonymous chronicle of research and documentation of the sites.

The bulk of the volume, the central pages on cream-coloured paper as opposed to the white printed pages, reproduces the paintings in black and white stipple, together with plans of the recorded rock shelters. The rock paintings are reproduced consistently in a scale of 1:4 throughout. The final section of the book, all in English, is the substantial catalogue by Tilman Lenssen-Erz in which 240 features for each of the 5635 figures in the Amis Gorge have been tabulated. While a laudable effort has been made by non-English speakers to write in a foreign language, some quaint expressions and grammatical constructions are occasionally used that are not native to the English language.

Archaeology

The focus of archaeological investigations in the Brandberg during 1984, 1986 and 1987 covered excavations at 10 sites, with Amis Gorge receiving the most detailed attention. Rich stratigraphies established proof of occupation dating from the Middle Stone Age (between 100-200 000 and 30-40 000 years ago). Dates in the Later Stone Age range from 6000-1000 years B.C., with the greatest frequency of occupation spanning 1500 to 750 B.C. An excavation carried out under the slanted roof of a rock shelter designated Amis 10, one of the richest painted sites in the Brandberg, yielded 35 small granite flakes showing definite traces of paint. One of these could be fitted back on to an exfoliating painting of a human figure on the ceiling above, a rare and exciting find (see Plate 6.1, p. 76). Charcoal samples taken from near the fitting piece gave a ^{14}C determination indicating that the painting dates from at least 1000 years B.C. Photographs show that the exfoliating painting is part of a superimposed panel, and many of the sequences appear to be considerably earlier than the dated figure (see Plate 7, p. 77).

Illustrations

For those who enjoyed the sumptuous colour reproductions in Pager's *Ndedema* book, *Amis Gorge* may prove visually disappointing.

For the purposes of reproduction, the paintings traced in outline by Harald Pager were patiently re-drawn in black and white graphics by Sonja Rupps and Klaus Gumher who laboriously colour-coded each of the 5635 recorded figures. While Rupps is to be congratulated on the complexity and comprehensiveness of the 30 different codes, 24 of which represent shades of red, the system can be confusing and does not lend itself to enhancing the sensitivity of many of the original paintings. To take an example, the three human figures reproduced on p. 218 (Amis 34, Plate 1, Fixes B, C) are not decorated with little V-shaped body patterning - this happens to be the code which represents a dark reddish-grey colour (Munsell Soil Colour Chart 10R 3/4). There is thus a tendency to reduce the images to a mechanical formula which the black and white stippled drawings produced by Harald Pager himself managed to avoid. Certainly, very conscientious efforts were made to re-create Pager's style as far as possible, for Marie-Therese Erz re-drew all the site plans and shelter elevations from Pager's field note books, attempting to preserve Pager's characteristic stroke. It is sad that there is so little of the original Harald Pager in this volume, despite the fact that the publication is in his name.

Amis 34, human figures between Fixes B and C (H. Pager).

There are nine pages of colour plates which establish the setting and reproduce some of the painted images as they appear on the rock face, but unfortunately there is no index of either plates, plans or photographs which, incidentally, have no credits. The black and white photographs are included among the total of 101 figures (no index), but photographs that illustrate new headings have neither figure numbers nor captions and are therefore no more than pleasant visual images.

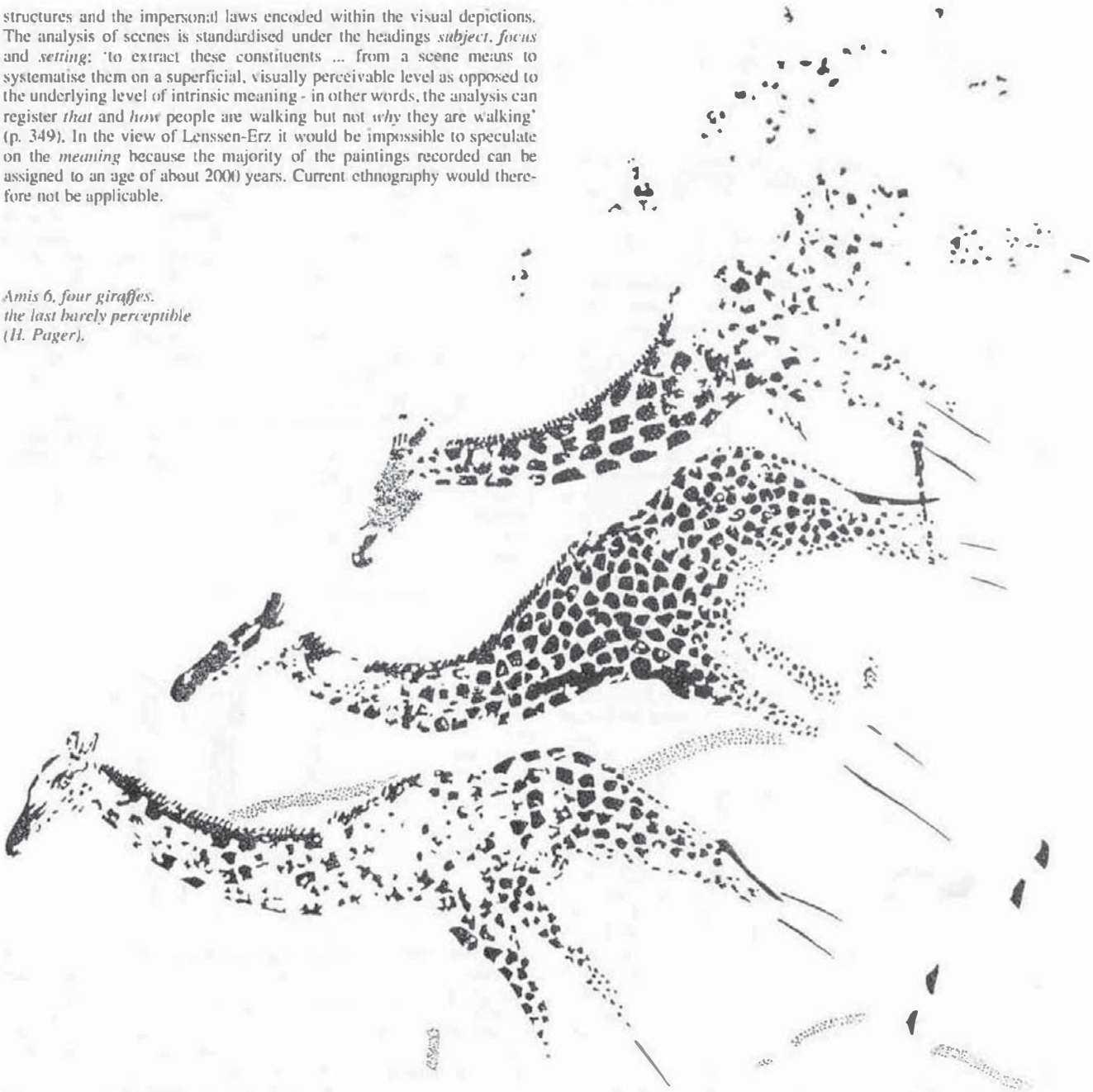
The objective of completely and accurately recording every aspect of the paintings in Amis Gorge has undoubtedly been met, but the result is that some of the illustrations are of such fragmentary art that they appear to be incomprehensible, for example p. 112, Amis 6, Plate IX, Fix B. Indeed, if casual viewers were to pick up the bound volume only, and did not go to the trouble of unfolding the detailed and impressive panels reproduced in the separate cassette, they would probably be disappointed in the quality of the art from Amis Gorge. There are, be assured, some exquisite paintings, in particular the elegant giraffes which descend from the ceiling of Amis 6 (p. 117) and the patterned zebra from Amis 56 (p. 308).

Catalogue

In addition to the visual presentation of the paintings, there is an immensely detailed and comprehensive catalogue of attributes by Tilman Lenssen-Erz which is tabled in the final 225 pages of the book. The columns of these tables cover technical data such as state of preservation as well as subject, species, style, clothing, activity, superimpositions etc. Another table presents a standardised analysis of 'scenes' which is more difficult to follow than the analysis of individual figures.

The principal thesis advanced by Lenssen-Erz is the application of a linguistic model to his analysis; the spoken word as well as communication by painting constitute 'a delineated pattern which has to be interpreted according to its inner connections'. Action traits are therefore 'similar to the structure of the speech-act and which make doing a kind of utterance' (p. 369). His analysis is therefore a descriptive one, the purpose of which is not to elucidate the *meaning* of a depiction, but rather the

structures and the impersonal laws encoded within the visual depictions. The analysis of scenes is standardised under the headings *subject, focus* and *setting*: 'to extract these constituents ... from a scene means to systematise them on a superficial, visually perceivable level as opposed to the underlying level of intrinsic meaning - in other words, the analysis can register *that* and *how* people are walking but not *why* they are walking' (p. 349). In the view of Lenssen-Erz it would be impossible to speculate on the *meaning* because the majority of the paintings recorded can be assigned to an age of about 2000 years. Current ethnography would therefore not be applicable.



Amis 6, four giraffes, the last barely perceptible (H. Pager).

This sounds all very laudable but the question is, if meaning cannot be deduced, is there any point in establishing structure? If, after all this meticulous and painstaking analysis, we can only say that such and such a structure is recognisable within the paintings, what then? It seems to me that the collection of data for the sake of data without attempting to interpret the significance or meaning of the data is a rather self-defeating and non-productive exercise.

From personal experience I can now, with some humility, be critical in my evaluation of painting analysis as a purely academic exercise, having myself misguidedly attempted to present data for all people for all time. I rather naively imagined that in the hypothetical future, some wizard researcher would *do* something worthwhile with all the intrinsically important 'objective' and 'factual' corpus of material I so laboriously gathered. For years, before the general introduction of computers, I steadfastly persisted in punching information on to data cards for subsequent hand-sorting, relating 23 attributes to a total of 8478 individual representations. It was a mammoth task, only a small proportion of which was eventually published in tabular form (Vinnicombe 1976: 362-4). In hindsight, analysis of a quarter of the sample would have produced the facts and figures which I utilised to good purpose. Nevertheless, I still idealistically persuaded myself that I had performed a great service for posterity.

The reality is that the tables I published have been reproduced by other researchers, the results have been quoted, some of the interpretations I suggested have been accepted, refined or expanded, but no-one in the 15 years that have elapsed since the data was eventually published, has approached me for access to the original analysis. Indeed, I can now confess a mishap which deeply distressed me at the time: one entire box of my punched cards somehow got lost during a move between continents! I agonised over all the irreplaceable data that I imagined would be mourned by researchers the world over, but indeed, it has not been missed by anyone, and I doubt that *I* shall ever need recourse to it again. I have already used the data for the purposes I required.

Harald Pager, during his recording project in the Ndedema Gorge, diligently improved the analytical procedure I developed (Vinnicombe 1967). He refined it, added to it and produced an immensely detailed descriptive catalogue of every individual painting he recorded. With the assistance of Richard Evans who developed a scheme for computer processing based on my manual quantitative analysis (Evans 1971), Pager fed all his data into the 'magic machine'. The results were disappointing. No magical answers were forthcoming because, as everyone now knows, a computer's response is governed by the relevance of the data with which it is fed. Furthermore, to my knowledge, no-one has ever used any of Pager's analytical work for further detailed research.

And now Lenssen-Erz has taken Pager's catalogue, again refined it and added to it, and has produced an even more complex and all-embracing data base expressly designed to allow 'any kind of "processing" of the art, from mere admiration through studies of metaphors to downright statistics and empiricism' (p. 370). I can only hope that Lenssen-Erz finds more enthusiastic researchers eager to use his data than did either Pager or myself.

There are, nevertheless, many aspects of this scheme that are worthy

of close study, and which may well be usefully adapted to other bodies of art. As far as the linguistic approach goes, with the subject being placed first in the paradigm, followed by action which is dependent on the subject, and the object of the action as the last link in the chain of interconnected events (p. 363), I can think of alternative combinations or emphases which could alter the meaning radically. However, I am not a linguist, and the references cited are not familiar to me. It may prove instructive for the editor of AURA to invite a specialist forum of assessors to evaluate the usefulness of the logic propounded by Lenssen-Erz in its application to the analysis of rock art. 1)

If there is a moral to data collection, it would appear to be that the person who amasses the data is the person who can most usefully understand and interpret that data. Everyone is able to persuade themselves that their methodology is more reliable and less biased than that of anyone else's, yet remains conveniently blind to the fact that all humans (and this includes the artists as well as the researchers) are regulated by selective processes. If we were not human and selective, the art we are so desperately trying to understand would not reflect any patterns for us to study.

At the end of the line I have the uncomfortable notion that if there is to be a fruitful outcome to the prodigious catalogue of information compiled by Lenssen-Erz, then it is Lenssen-Erz himself who will be the most appropriate person to interpret that data. I look forward to the results, but do not envy the process.

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EDITOR'S NOTE

1) Lenssen-Erz had submitted a paper on his work to *RAR* even before this review was received. The refereeing process of his paper is complete, a modified version has been accepted for publication and will be debated by an international specialist panel of rock art researchers and linguists.

NOTE: The first edition of the volume by Pager is already out of print. However, a second edition is being prepared and orders are invited from readers. The address for ordering copies of the second edition is: Heinrich-Barth-Institut, Jennerstrasse 8, D-5000 Köln 1, Germany.

Payment of DM125.00 (US$76.00, $A100.00) plus postage of DM60.00 (the volume weighs 5.6 kg!) is preferred by credit card (Visa, MasterCard, American Express), a surcharge of DM15.00 applies to cheques.

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L'Image des animaux dans l'art préhistorique, by HENRI DELPORTE. 1990. Picard, Paris. 254 pages, 289 illustrations. 550 Francs.

This is one of the best and most stimulating books of its kind to come out of France for some time, so let's get the quibbles out of the way before starting the praise.

Inevitably, the major problem is the price which is excessive for a book consisting almost entirely of monochrome illustrations. The production is glossy and of excellent quality, but that does not excuse such a high figure. There are recent books, including one which tact forbids me to name, that are almost entirely in colour and yet are priced at a far more reasonable level.

The book's title is very French in that, like Leroi-Gourhan's *Préhistoire de l'art occidental*, what it actually means by *préhistorique* is Upper Palaeolithic, and exclusively European at that. Delporte does include a certain amount of material from central and eastern Europe, but the book is very heavily dominated by French examples. His citing of non-French authors, both past and present, is similarly restricted in its cast-list, and becomes somewhat monotonous.

Finally, there are quite a few typographical errors, many missing accents (odd in a French volume), a couple of photographs upside down (163, 254), one on its side (144) and one back to front (242); and many pictures are frustratingly unsourced as to the museums which house the objects.

Despite its shortcomings, however, this is undoubtedly Delporte's finest book to date; for whereas his earlier *L'Image de la femme dans l'art préhistorique* was more or less a catalogue of female depictions, albeit a very useful one (soon to be reissued in an updated edition), the new volume is totally different, despite its similar title. Rather than a catalogue of Palaeolithic animal figures, which would be a vast undertaking, it is a more general study of some aspects of Palaeolithic art. Indeed much of the time its title is irrelevant, since the discussion tends to be about technique and style rather than content. There is no study of the different animals depicted.

The book's great strengths are the topics in which Delporte has specialised for so long: the history of the subject, the technology of portable art, and the relationship between depicted species and exploited species. The emphasis on portable art is welcome (in view of its frequent neglect in books on Palaeolithic art) and unsurprising from a man who was for so long curator of the world's finest collection, at the Musée des Antiquités Nationales. His knowledge of, and love for, this class of material shine out of these pages.

One also finds a most welcome inclusion of other previously neglected items such as the Neschers horse (mysteriously omitted from Sieveking's catalogue of the British Museum's collection, see *RAR* 7: 76), and an emphasis on Max Raphael's role as the precursor of Laming-Emperaire's and Leroi-Gourhan's theories.

There are some quite puzzling absentees, however. Despite a brief mention of the possibility of open-air rock art, Delporte makes no mention of the series of five such sites found in France, Spain and Portugal in recent years. And given the book's chronological, geographical and topical parameters, it is simply amazing that the sites of La Marche and Gönnersdorf, each with hundreds of engraved plaquettes, are accorded a single illustration each.

The book's structure is in three major chunks: first, the history and the basics of chronology; second (and at greatest length), the different techniques used, especially in portable art; and finally, some of the major theories developed over the past century. His extended treatment of the question of depicted vs exploited is timely in its demonstration of how much more complex this problem is - with portable art differing from parietal - than in the simplistic view currently being promulgated by Mithen with, alas, some success among readers knowing little or nothing of Palaeolithic art.

For that reason and for its other virtues outlined above, this book deserves to be read widely, and to be studied carefully by anyone hoping to use Palaeolithic art to support a pet theory.

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Images of the past. A guide to the rock carvings and other ancient monuments of northern Bohuslän, by JOHN COLES. 1990. Bohusläns Museum, Uddevalla. 96 pages, 116 monochrome illustrations, 6 maps. SKr60.00, ISBN 91 7686 092 2.

In association with Lasse Bengtsson, the English archaeologist John Coles has written a brilliant guide to the rock carvings and other ancient monuments of northern Bohuslän, one of the richest areas of rock art in Europe. The reader is invited to visit some of the most famous rock art sites to be found anywhere, with petroglyph motifs such as the 'Lovers' (Vitlycke), the 'Winged Warriors' (Kallsängen), the 'Charleston Dancer' (Smörsten) or the 'Football Team' of 11 people and a ball (Aspeberget).

The humorous nicknames given to these pictures, of which *Images of the past* contains a few, invite reflection. They remind us that there are far too many images in this rock art of which far too much is being said, but which we know far too little about.

The book is essential for anyone considering to visit any of the almost 90 sites mentioned. Without guidance most of them would be as hard to find as the proverbial needle in a haystack.

Apart from a brief description of sites with maps, illustrations etc., *Images of the past* contains an excellent introduction to the prehistory of the region, followed by a list of images to be seen. The book ought to be found in any university library and the low price makes it an ideal gift for anyone interested in the past. With the increase of tourism from southern Europe, French and Spanish translations would seem worthwhile. The volume certainly deserves many readers.

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RAR 8-221

The forgotten artist: Indians of Anza-Borrego and their rock art, by MANFRED KNAAK. 1988. Anza-Borrego Desert Natural History Association, Borrego Springs, California. xiv and 117 pages, 68 colour plates, 5 monochrome plates, 2 maps, bibliography and index. Large format (280x244 mm), paperback, $A17.95 + tax. ISBN 0 910805 04 0.

This volume documents the rock art of the Anza-Borrego Desert State Park, near San Diego, California. Although the purpose for writing the book and the audience Knaak proposes to target are not made explicit, it is clear that this audience is not primarily intended to be academic but the wider public. Nevertheless, rock art researchers will find the book useful as an introduction to the Anza-Borrego rock art.

The book is attractively presented and incorporates a generous number of colour photographs of, overall, good quality, although lacking scales. Most photographs were taken by the author and reveal much detail on the painted and engraved motifs. The text is remarkably free of spelling mistakes but the use of some terminology is questionable; for instance terms like 'he', 'man', 'draftsman' and 'primitive man' are used repeatedly to refer to the artist. Also, the text is often embellished by a poetic style and the use of words conveying an aura of mystic and magic around the art.

The body of the book is organised into three sections. The first one is about the land and the people, and includes three chapters. The first chapter describes the physical environment and summarises the archaeology and prehistory of American Indians generally. The second chapter presents a summary of traditional life in two local tribes, the Cahuilla and Cupeño, while a third chapter discusses the same topics but for two other tribes: the Northern Diegueño and the Kumeyaay. The second section, 'Drawing with hammer and brush', has three chapters. The first one gives an outline of the nature of American Indian art generally. It also discusses the nature of the local art, its meaning, function and antiquity. The second chapter defines art styles of the Anza-Borrego district and correlates their distribution to that of tribal territories. The third chapter presents speculations on the functions of rock art in hunter-gatherer societies. The last section, 'Ritual and ceremonies recorded on stone', has six chapters which mostly describe past ceremonies of various kinds as well as myths. In these chapters the author attempts to draw on this ethnographic information to establish the meaning and function of the art. The last chapter discusses artistic practices recently revived by contemporary American Indians.

In the foreword by Dr Spencer Rogers the reader is informed that the meaning and function of the rock art can only be guessed by becoming familiar with the ethnography of the local indigenous people. That there is a general lack of ethnographic information about the art itself is clear. It is only in a few places (pp. 70, 71, 73, 94-5) that cursory evidence is presented on the function of the local art, as recorded in the past. This evidence shows that the purpose of producing some of the rock art was associated with initiation ceremonies and shaman healing practices. No ethnographic data are available to ascertain the meaning of individual motifs.

This lack of information on the meaning of the art, and the very limited amount available on its function does not deter the author from making bold statements throughout the book about the possible meanings and functions of the art, a task which seems to represent the central concern of Manfred Knaak. This the author tries to achieve by drawing on his extensive knowledge of the ethnography of local tribes as well as the nature of the art and its context, blending the two to present speculations as if they were facts. For example he writes about the possible meaning of a particular bird motif: 'A glimmer of the symbols' meaning comes from the outlines of a bird, drawn in black. According to Kumeyaay legend, a beautiful bird lived here in the waters of the spring, but it was a spirit that could be seen only in a vision' (p. 40). And, regarding function, the author emphasises that much of the art was probably made by men, in particular shamans, and linked to their healing practices, and to the important role these men played in initiation ceremonies. This point is reiterated and implied in many places. For instance, on p. 41, Knaak writes: 'Anthropomorphs hidden in caves ... were probably painted by the shaman-artist'; and on p. 98: 'rock art was sacred art, imbued with supernatural power that only the shaman could understand'. It appears that Knaak convinced himself more and more of this as he progressed through the book, towards the final revelation on p. 99: the shaman *is* the forgotten artist.

But were shamans the only artists or even the individuals primarily involved in the production of the art of the Anza-Borrego area? This is not so: Knaak himself provides ethnographic data indicating that young girls, at least, also took part in activities of art production (p. 73).

As the author does not provide statistical data on the relative occurrence of motif types it is not possible to know to what extent the rock art illustrated in the book is representative of the local art, or whether the art motifs are presented simply because of their aesthetic value.

What, then, is the strength of this book? The volume is perhaps of value to those interested in an introduction to the ethnography of the Anza-Borrego Indians, as well as to those interested in a brief, though perhaps biased, overview of the local rock art. It is, however, of little interest to those wishing to learn about the ethnography *of* the local rock art. While such ethnographic information is generally lacking in the written sources on the region's traditional Indians, the author does not systematically test his interpretations and inferences about the meaning and function of the rock art. The book is, nevertheless, refreshing and easy to read; I found it enjoyable.

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Die Tonfiguren der Hohokam und ihr zeremonieller Kontext, by J. SCHWABE. 1989. Franz Steiner Verlag GMBH, Stuttgart. 250 pages, monochrome plates, drawings, bibliography. DM48.00, ISBN 3 515 05489 8 (printed in German).

In his text, the ethnologist Johannes Schwabe, directed by Wolfgang Lindig and Eike Haberland of the Frobenius Institute of Frankfurt, proposes to shed light on some of the mysteries of the Hohokam culture. This culture has long been known and the investigations conducted by H. Gladwin and E. Haury between 1930 and 1940 were decisive in defining it. However, since then a lot has been written about the Hohokam, who together with the Mogollón and Anasazi represent the most important cultural traditions existing in the Southwest of the United States from 500 B.C. until the arrival of the first European settlers in America.

With such representatives as McGuire, Wilcox, Sternberg, Gregory and Nials, Seymour and Schiffer, Anderson, Tainter, Teague (and many others), it can be said that the 'New Archaeology' took this culture as its ideal field of study, be it by carrying out new excavations with very sophisticated field techniques, or by going over the materials obtained from old and classical excavations such as those at Snaketown, Gila Butte, Sacaton etc. The latter approach is the one attempted by the author. He lived in the United States, especially in the Southwest, long enough to be able to review museum collections, to index catalogues and leaf through the fieldwork notebooks of the first archaeologists who dealt with the Hohokam. His objective was to make a detailed analysis of the functions which the figures might have had in their context, both at burials and in the homes, in all the sites of the Hohokam culture, and also to study these small enigmatic clay figures which appear so frequently. They are anthropomorphous, between 5 and 20 cm tall, with masked faces, special hairstyles and clothes. They occur in all the periods or phases of the Hohokam culture and seem to have been manufactured to very strict patterns, as the variations are insignificant over 1700 years of development. They appear in two contexts: among the home refuse in pits or heaps (especially during the 'Pioneer period') or inside houses and at burials with cremation which suggest luxurious and elaborate ceremonies ('Sedentary and Colonial periods'). At the beginning of the 'Classic' these figurines are very rare.

In 1985 J. Schwabe published a typological study of the Hohokam figurines, whereby he confirmed their uniform manufacturing techniques: they were always made of two parallel clay strips, the clay was extracted from particular streams, the figures always appear broken and burnt, and they are of uniform size. These features and many other 'regularities' did not allow him to look further into the role of the figurines. Therefore Schwabe decided to resort to the technique recognised by European archaeology (German in particular) as identifying the '*geschlossene Funde*', which may be translated as 'find associations'. He concentrated on the inventories, tomb by tomb, of the cemeteries at Snaketown, Hodges and La Ciudad. Then he posed for each context the questions: are there different contexts with specific combinations of objects? Are the contexts of tombs with figurines different? Are there common elements in the tombs with figurines? How many isolated figurines have been found as compared to groups of them? Are the groups of figurines also present in the 'Pioneer period'? Does the representation (the figurine) provide any indication of its special meaning? Schwabe also considers other types of data: the kind and number of furnishings in the tomb, location of the tomb within the cemetery, and the sex and age of the dead person.

He came to the following conclusions: the figurines composed a scene and probably represented the dead person's family; they occur with other artefacts (pots, pestles and mortars) which indicated food and fertility; the pregnant figurines or those with initiation marks served to strengthen family spirit. Thus the dead were probably accompanied to the other world by their family group, and figurines were broken and burnt with the

corpse. The cremation could be a way to free the soul so that it should not return to the body. The deceased may even be represented by the largest figurine found in the tomb. Only the tombs of important individuals contain figurines.

Schwabe also devoted some paragraphs to the artisans who manufactured the figurines. They were not specialists, but most likely members of a ritual group. They were not potters as their workmanship was not refined and they did not wait for the figure to dry before firing, and did not care if it broke in the process. The Hohokam manufactured their figurines only to last for the ceremony.

The Hohokam figurines depicted in plates and drawings render this work interesting even for readers who are not too fluent in German. For German researchers Schwabe's investigation is of particular value as he demonstrates that it is possible in America to find a large series (of cemeteries, in this case) which is susceptible to the method of contextual archaeology. Moreover, he shows that even without an absolute chronology (radiocarbon dating, dendrochronology etc.) such series as those found in the Hohokam cemeteries may lead to a very precise order by periods.

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RAR 8-223

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Signs from the ancestors: Zuni cultural symbolism and perceptions of rock art, by M. JANE YOUNG. 1990. University of New Mexico Press, Albuquerque. 308 pages, 89 figures (including 60 plates), 2 maps, bibliography, index. Cloth (1988), US$24.95, ISBN 0 8263 1039 7, or paperback ISBN 0 8263 1203 9.

The focus of this book is the cultural relevance of Zuni rock art to modern Zuni people. It places this art within the general framework of Zuni cosmology and cultural symbolism. The book is clearly written and perceptive: it succeeds in the author's aim of making her academic research into Zuni rock art accessible to a lay audience.

Young links contemporary interpretations of rock art images to an organisational principle of directionality underlying Zuni culture. This principle includes 'everything', including both time and space. The metaphoric base that connects all facets of Zuni cultural symbolism is a concept of multiplicity or accumulation, which itself is connected to the cosmos. Increase or prosperity are possible only if the sun continues on its appropriate journey along the horizon and if the winds emanating from the solstice positions bring sufficient snow and rain. Contemporary Zuni responses to graphic images, including those on rock art, are informed by these structuring principles.

Complex Zuni cultural concepts are presented simply and clearly. For example, the spatial and temporal perceptions underlying a dialectic relationship between present time and myth time are explained clearly both in the text and in the schematic presentation of these concepts (Figure 46). Though Young's interviews with Zunis were conducted primarily in English, it is likely that her knowledge of Zuni language allowed her to obtain some of the insights which she describes.

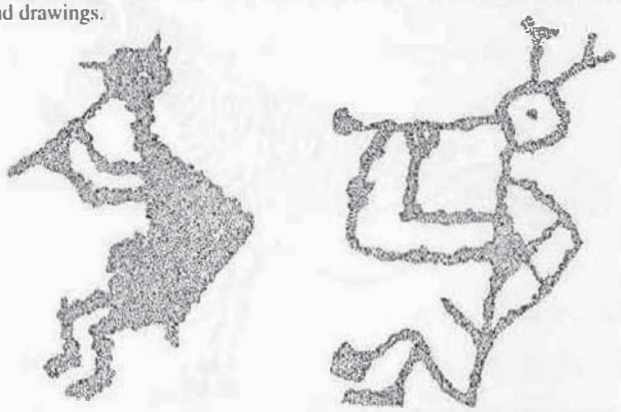
The chapter on continuity and change provides background to contact between Zuni and non-Zuni people, including Spaniards and Anglo-Americans. Young's analysis considers not just those cultural elements which Zunis chose to adopt but also the manner in which they were incorporated into Zuni culture. Evaluation of this process is an on-going concern of the book and she concludes that cultural elements adopted from other societies are superimposed onto a stable Zuni cultural core. In terms of rock art this means that the multivalent and multireferential quality of the graphic images allows recent interpretations influenced by Anglo-American culture to be juxtaposed with those already attributed to a particular symbol. The cluster of meanings surrounding the symbol is merely given one extra dimension.

In this book Young probes the Zuni aesthetic dichotomy previously identified by Tedlock which encompasses the dangerous (powerful, plain, dark, indistinct) and the beautiful (dynamic, varied, multi-coloured, distinct). The connection drawn by Young between an aesthetic principle which conceives of beautiful as distinct or bright, and the stark and demanding Zuni landscape can be compared cross-culturally with Western Desert Australian Aboriginal society. It may be that the environment one inhabits, and is part of, shapes a particular form of aesthetic appreciation.

The issue of gender is dealt with sensibly, putting women into the picture without taking men out of it. While it appears that rock art production was predominantly a male activity, Zuni women were readily able to identify many rock art images, including those of *kachina* figures. The author concludes that Zuni women's knowledge of religious matters formally associated with the male domain is more extensive than non-Zunis normally realise. However, while the book treats gender relations as a central component of human interactions it would have been enhanced by a more explicit consideration of the manner in which gender can be negotiated through material culture.

Young shows a laudable concern with the social relevance of rock art studies and is clearly very willing to meet with an academic's responsibility to the indigenous people under study and, indeed, to the wider society. She presents her research conclusions in such a way that they do not undermine the integrity of indigenous teachings and she often explicitly proffers both 'scientific' and Zuni interpretations of particular phenomena: what academics regard as myths, Zunis regard as history; what academics regard as depictions *by* an earlier group of people, Zunis regard as signs *from* the ancestors *to* contemporary Zuni people. As part of her field work Young produced a photographic exhibit, brochure, slide show, and pamphlet as well as school project material: her intention here was to stimulate young people's interest in Zuni cultural material and to decrease the high incidence of vandalism of the rock art in the region. She also established a rock art recording system that could be continued by Zuni people. It is likely that this type of approach will become a normal part of archaeological/anthropological field work.

Young provides a clear and comprehensive description of her field and documentation procedures. This facilitates academic assessment of her work and allows the more general reader to learn from these procedures. For example, her concern with the conservation of rock art includes a section explaining the destructive capability of techniques such as chalking or tracing. She suggests that future recordings of the art should be restricted to non-destructive methods such as photographs and free-hand drawings.



Petroglyphs of humpbacked flute players (drawn by M. Callahan).

Technically, the quality of the book is good. All figures are clear and some of the line drawings by Murray Callahan are quite elegant. All plates are monochrome. Unfortunately, many do not have scales and there is no list of figures, or of maps. Chalking to highlight engraved figures occurs in two plates (Figures 59 and 84), though in both cases this was done by previous researchers.

My only reservation with this book concerns the chronological sequence proposed. Chapter 2 includes a section on the dating of rock art and it is clear that the author is aware of the problems associated with establishing a regional chronology. However, this is not emphasised throughout the text and I doubt if the chronology of the rock art in this region is as secure as Young often implies. The majority of Southwestern rock art chronologies are based on perceived stylistic similarities with artefacts (usually ceramics) found in habitation sites and this technique is also integral to the sequences proposed by Young. And the comprehensive ceramic typologies published by Colton and Hargrave in 1937, which are themselves largely based on decorative rather than technological features, still underlie the dating of many sites in this region. However, the utility of style as a chronological marker is currently under serious theoretical reassessment. It may have been better for Young to take a more cautious approach to this issue - particularly when the targeted readership of the book is the general public, who may not be well informed on dating techniques, and associated technical problems, and who may well take assertion for fact.

Overall, however, this book is to be commended: it is an important contribution to Zuni ethnography and to rock art research in general. It facilitates understanding of, and respect for, the complexities of Zuni culture by both an academic and a lay audience. And this is certain to enhance our understandings of other indigenous cultures.

Insightful, informative and interesting reading.

Claire Smith
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RAR 8-224

RECENT ROCK ART JOURNALS

Pictogram. Tri-annual newsletter of the Southern African Rock Art Research Association (SARARA), edited by SHIRLEY-ANN PAGER. Recent issues include the following research papers:

Volume 2 (1989), Numbers 2-3:

C. G. SAMPSON and J. C. VOGEL: A painted pebble and associated C-14 date from the Upper Karoo.
WOODHOUSE, H. C.: 'Trumpets' and arrows.
GARLAKE, P. S.: Associated images at one Mashonaland site.
STEEL, R. H.: Drover's whips in the rock art of southern Africa.
PAGER, S.-A.: Extraordinary portraits by San artist.
MORRIS, D.: Conservation of rock engravings in the Northern Cape: getting an act together.
LEE, G.: The Puako Petroglyph Project, Hawaii: problems of conservation.
BEDNARIK, R. G.: World's oldest stone sculpture found.
KUMAR, G.: A unique ritualistic dance composition in Indian rock art.
STEEL, R. H.: How photography helps record deterioration of rock paintings.
VACCARO, V.: New approaches to recording and archiving rock paintings.

Rock painting, Amis 56 (after H. Pager, The rock paintings of the upper Brandberg. Part 1: Amis Gorge, reviewed in this column).

Volume 3 (1990), Numbers 1-3:

LOUBSER, J.: Case hardening, silica skins and some ethical considerations in the conservation of rock paintings.
WOODHOUSE, H. C.: The 'Trailing Streamers'.
BEDNARIK, R. G.: Rock art researchers as rock art vandals.
JACOBSON, L.: Some local developments in cation ratio dating.
LANTEIGNE, M. P. and J. STEINBRING: Rock art research: directions in methodology.
LEE, N.: Spirit forms and thinking strings.
GENGE, P.: The strange paintings in Mahololo Cave, Matopos Hills, Zimbabwe.
WOODHOUSE, H. C.: Wagons in rock art.
GENGE, P.: Bravo Bednarik.
BEDNARIK, R. G.: Latex and ethics: about a rubber fetish of archaeologists.
LOUBSER, J.: Removal and in situ conservation: strategies and problems in rock art conservation at the National Museum, Bloemfontein.
BOCK, F. and A. J. BOCK: A re-examination of an attempt to restore petroglyphs using artificial desert varnish at Petrified Forest.
WOODHOUSE, H. C.: Carrying bags in the rock art of southern Africa.
BEDNARIK, R. G.: Vandalism and academic elitism: some attitudinal problems in rock art studies.

Enquiries to SARARA, P.O. Box 81292, Parkhurst 2120, South Africa.

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Survey. Bollettino del Centro Studi e Museo d'Arte Preistorica di Pinerolo - annual journal of the CeSMAP. Edited by DARIO SEGLIE. High quality print on art paper, perfect-bound paperback, 24 x 17 cm, excellent illustrations throughout.

Volume 1 (1985/86), 38 pages:

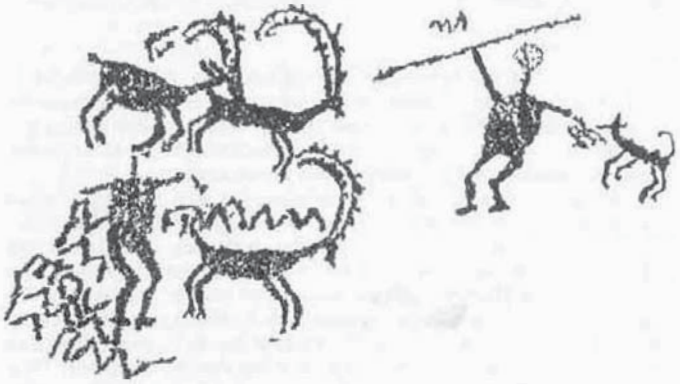
CINQUETTI, M.: Survey sulla Rocca di Cavour.
CINQUETTI, M.: Segnalazioni d'arte rupestre.

There are also reports of the General Meetings of 1984 and 1985 and a book review.

Volume 2 (1987/88), 79 pages:

CINQUETTI, M.: Preistoria nel pinerolese: progetto per la carta archeologica.
SEGLIE, D., P. RICCHIARDI and M. CINQUETTI: Pitture rupestri preistoriche nel Parco Regionale Rocca di Cavour.
SEGLIE, D.: La Peira Eicrita di San Germano Chisone.
ALESSIO, C. and F. MAGRI: Nuovo laboratorio del museo.
BOSIO, R.: La mostra 'Arte rupestre nelle Alpi Occidentali'.
BOSIO, R.: Fotografia in fosforescenza per la riproduzione di incisioni rupestri.
COISSON, O.: Repertorio delle incisioni rupestri della Val Pellice.
GUIOT, T.: L'utilizzazione del computer a fini archeologici.
NISBET, R.: Ricerche archeologiche nell'Oman.

There are also several brief reports in addition to the above research papers.



Petroglyphs, 'Roccia degli Stambecchi', Zona del Moncenisio, Italy.

Volume 3 (1989), 127 pages:

BEDNARIK, R. G.: Darwin '88 - Uno spartiacque negli studi sull'arte rupestre.
SEGLIE, D., P. RICCHIARDI and M. CINQUETTI: An archaeo-anthropological pattern for European post-Palaeolithic rock art.
BOCQUET, A.: L'uomo, la foresta, il legno 5000 ani fa.
COMBA, E.: Simboli del passato: arte rupestre e ricerche antropologiche in Nord America.
BOSIO, R.: La metascienza contemporanea e le teorie sulle incisioni rupestri.
CINQUETTI,: Preistoria nelle Alpi Cozie - Progetto per la carta archeologica.
CINQUETTI, M. and G. BIANCIOTTO: Siti d'arte rupestre.
NISBET, R.: Ricerche territoriali di superficie in alta Val Chisone.
COISSON, O.: Repertorio delle incisioni rupestri della Val Pellice.
BALDI, R.: Incisioni rupestri nella Collina di Verzuolo.
NISBET, R.: Appunti di archeologia dell'ambiente: l'archeobotanica.
BOCKSBERGER, O. J.: Vers de nouvelles méthodes pour l'étude des monuments megalithiques et notamment des pierres à cupules.
SEGLIE, D.: Incisioni rupestri nel Parco della Vanoise in Savoia.
SEGLIE, D.: Dalla ricerca scientifica all'attività didattica.
GUIOT, M. L.: Modem - floppy disk: Scambi di informazioni fra centri di raccolta dati.

Volume 4 (1990), 251 pages:
BEDNARIK, R. G.: About professional rock art vandals.
BEDNARIK, R. G.: Priorità nella conservazione dell'arte rupestre.
SEGLIE, D., P. RICCHIARDI and M. CINQUETTI: Field work: tridimensional survey of the rock engravings.
SEGLIE, D., P. RICCHIARDI, T. GUIOT and M. CINQUETTI: Scheda d'inventario e tipologia - Commissione Internazionale Arte Rupestre Alpi Occidentali (CIARAO).
SWARTZ, B. K. Jr: Standards for the recording of petroglyphs and pictographs.
LANTEIGNE, M. P.: The Precambrian Shield rock paintings: general applications in cognitive perceptual modeling.
COMBA, E.: Some examples of mythological content in American north-west coast rock art.
BEDNARIK, R. G.: The world's oldest stone sculpture.
SEGLIE, D. and P. RICCHIARDI: The megalithic monument at Pinhal da Pedra da Escrita, Portugal - a contribution to the study of post-Palaeolithic religions.
SEGLIE, D.: Monte Bego - arte schematica lineare, convergenze formali e cronologie.
NISBET, R.: Appunti di archeologia dell'ambiente: l'archeobotanica (seconda parte).
DORO, A.: Un ripostiglio di bronzi a Pinerolo.
BALDI, R.: Alcune tipologie cruciformi nelle incisioni rupestri e loro indirizzo simbolico.
CAVALLERA, A.: Arte rupestre in Valle Po.
SEGLIE, D., P. RICCHIARDI, M. CINQUETTI, O. COISSON and R. BOSIO: La stele antropomorfa della Miandassa in Val Chisone e l'orizzonte culturale eneolitico di Balm'Chanto nelle Alpi Cozie (Italia).
CINQUETTI, M.: Nuovi ritrovamenti di arte rupestre in Val Germanasca.
SEGLIE, D.: Parco Regionale Orsiera-Rocciavrè, Coazze, Valle Rio Balma, Roc dl'Ursi.
ARCA, A.: Arte rupestre in Valle di Susa e Alta Moriana: recenti scoperte e sviluppo delle ricerche.
DRAGO, A. and P. PARENTE: Stage 1990: nuove scoperte di arte rupestre in Val Chisone.
BECCARIS, G., O. COISSON, L. GIBELLI, E. GIULIANO, T. GRINDATTO, M. LUSSIANA, G. C. PAVESIO, C. RU, D. SEGLIE and C. TRON: Segnalazioni di arte rupestre.
RICCHIARDI, P. and D. SEGLIE: Museo - territorio - scuola: progetto di utilizzo del Palazzo del Senato quale sede del Centro Studi e Museo Civico d'Arte Preistorica.
GUIOT, M. L.: Settimana Pinerolese dei Musei 1989: Mostra 'L'Uomo la Foresta, il Legno 5.000 anni fa'.
There are also several brief reports, the IFRAO Reports 2-4, and book reviews by J. Clottes, O. Coïsson, P. Ricchiardi and D. Seglie.
Enquiries to CeSMAP, Viale Giolitti 1, 10064 Pinerolo (To), Italy.

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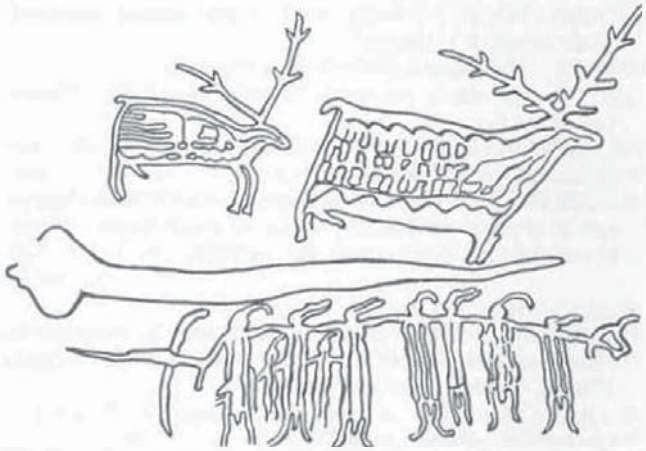
Purakala. Bi-annual journal of the Rock Art Society of India (RASI). Edited by GIRIRAJ KUMAR. Well-produced, refereed journal concentrating on Indian rock art, but of international standard and comprising contributions from abroad. International subscriptions US$15.00.

Volume 1 (1990), Number 1, 20 pages:
BEDNARIK, R. G.: Global perspectives of rock art.
KUMAR, G.: New dimensions in purakala study.
BEDNARIK, R. G.: IFRAO Reports 2-4.
The issue also includes a list of specialists in purakala (palaeoart) studies, and a draft constitution for RASI.

Volume 2 (1991), Number 1, 40 pages:
ODAK, O.: Towards the teaching of rock art in academic institutions. With comments by V. H. Bedekar and G. Kumar.
BEDNARIK, R. G.: Rock art researchers as rock art vandals. With comments by Y. Mathpal and K. K. Chakravarty.
MATHPAL, Y.: Rock art sites in India and problems of their preservation and management.
BEDNARIK, R. G., G. KUMAR and G. S. TYAGI: Petroglyphs from central India.
There are also a debate of the paper on global perspectives, previous issue; a copy of IFRAO Report No. 5; a progress report of RASI; and a membership list.
Enquiries to the Editor, Faculty of Arts, Dayalbagh Educational Institute, Dayalbagh, Agra - 282 005, India.



Rock paintings from Chattarevhwar, Alania, District Kota, Rajasthan, India.

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Jahrbuch der GE.FE.BI. Jahrbuch der Gesellschaft für vergleichende Felsbildforschung. Edited by LOTHAR WANKE. Profusely illustrated, A4 format.

Volume 6 (1987/88), 71 pages:
WANKE, L.: Abstraktion als Ausdruck des Erwachens einer imaginären Welt.
FUCHS, G.: Die anthropomorphe Stele in der Nekropole von Hirebenkal, Raichur-Distrikt, Karnataka, Indien.
TILLNER, E.-O.: Kurzbericht über die GE.FE.BI. Expedition 1986/87 nach Nordpakistan, Zentral- und Südindien.
KUMAR, G., G. NARVARE and R. PANCHOLI: Ostrich eggshell - a new medium for creative art exploited by Upper Palaeolithic man in India.

Volume 7 (1989/90), 95 pages:
WANKE, L.: Nordische Felsbilder: Deutungsversuche anhand verschiedener Abguss- und Abreibungsmethoden, ausgehend von den älteren Abformungen des Herman Wirth-Museums.
ANDERSEN, H. J.: Stellungnahme zum Felsbild von Bräcke: das Sanduhr-Symbol.
WANKE, L.: Die nordischen Felsbilder der Bronzezeit und ihre Deutungen.
TILLNER, E.-O.: Jahrestreffen: Monte Bego; La Valle des Merveilles, September 87.
EVERS, D.: Himmelsboot und Geisterfallen: Merkwürdigkeiten unter den Felsbildern in der Höll am Warscheneck.
KOLMER, H.: Scheiben mit radförmiger Struktur (Felsbilder des Fezzan).
WANKE, L.: Einbindung der Symbolik der Felsbilder des Fezzan in die archetypischen Urvorstellungen der Menschheit.
HESS, G.: Der heilige Baum von Lökeberget.
HESS, G.: Erdgottheit und Weltkreiskreuz: Nyköpinger Felsbilder enträtselt?
KSICA, M.: Der grösste Felsbilderkongress der Welt: Rückblick auf den AURA Kongress.
There are also book reviews, an obituary for Prof. V. S. Wakankar and a membership list.
Enquiries to Dr Lothar Wanke, Geidorfgürtel 40, A-8010 Graz, Austria.

Sahara. Prehistory and history of the Sahara. Annual journal edited by ALFRED MUZZOLINI, produced by Edizioni Pyramids, with the support of the Centro Studi Luigi Negro. Contributions are in Italian, English and French, a considerable number deal with rock art and related subjects. Exceptionally high standard of graphic presentation, production and editorial direction. The first three volumes contain the following contributions relevant here:

Volume 1 (1988), 109 pages:
SOLEILHAVOUP, F.: Découvertes archéologiques exceptionnelles au sud de l'Ahaggar.
CHAIX, L.: Le monde animal à Kerma (Soudan).
KUNZ, J.: Un site à peintures pariétales de l'Oued Tirehart-Tassili n'Ajjer.
BOCCAZZI, A. and D. BOCCAZZI: Alcuni graffiti della parete occidentale del Gilf Kebir.
MUZZOLINI, A.: Figurations rupestres de chars avec attelage, de part et d'autre du Ténéré (Arkana et Oued Taguei, Niger). L'extension des 'Libyco-berbères' au Djado.

Volume 2 (1989), 135 pages:
MUZZOLINI, A.: Les peintures rupestres de Ti-n-Moussa (Tassili-n-Ajjer, Algérie). Têtes Rondes tardifs, groupe d'Iheren-Tahilahi, groupe d'Abaniora.
LE QUELLEC, J.-L.: Les gravures rupestres de Shormet el-Greibât Fezzan Septentrional (Libye).
VERMEERSCH, P. M., E. PAULISSEN and P. VAN PEER: Palaeolithic chert quarries and mines in Egypt.
SOZZANI, M. and G. NEGRO: Due interessanti incisioni del Tadrart algerino.
GALAND, L.: Inscriptions sahariennes.
SOLEILHAVOUP, F.: Un objet énigmatique en terre modelée au SSW de l'Ahaggar.
BOCCAZZI, A.: Ikadnouchere, Tassili di Nord-Ovest.
PETIT-MAIRE, N. and P. CARBONEL: Première découverte de gravures rupestres dans la région de Taoudenni.
WAGNEUR, C.: First AURA Congress, Darwin (1988).

Volume 3 (1990), 135 pages:
SOLEILHAVOUP, F.: Nouvelles stations rupestres à l'Ouest de l'Ahaggar.
VAN ALBADA, A. and A.-M. VAN ALBADA: Gravures du Messak Settafet (Fezzan Libyen).
KRZYZANIAK, L.: Petroglyphs and the research on the development of the cultural attitude towards animals in the Dakhleh Oasis (Egypt).
TROST, F.: Egig: un site important de gravures rupestres et de monuments funéraires préislamiques dans l'Ahaggar.
BOCCAZZI, A.: Ti Leh-Leh - Tassili n'Adjer.
SOZZANI, M.: Due carri al galoppo volante e un dipinto insolito nel Tadrart Acacus.
DUHARD, J.-P., J. GAUSSEN and B. MOUTRON: Une meule ornée saharienne.
LE QUELLEC, J.-L.: Deux *idebnân* en forme de V du Shâti (Fezzân septentrional).
FALESCHINI, G. and G. PALMENTOLA: Costruzioni 'preislamiche' nel Sahara algerino meridionale.
GAUTHIER, Y.: Découverte d'inscriptions énigmatiques dans le Grand Erg Occidental.
BEDNARIK, R. G.: About Pleistocene chert mining.
MUZZOLINI, A.: La datation des chars au 'galop volant'.

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Rock Art Papers. Occasional monograph published by the San Diego Museum of Man. Edited by KEN HEDGES. Perfect-bound, paperback.

Volume 5 (1987), ISBN 0 937808 45 8, 176 pages:
BENSON, A. and L. SEHGAL: The light at the end of the tunnel.
HEDGES, K.: Patterned body anthropomorphs and the concept of phosphenes in rock art.
RAFTER, J.: Shelter Rock of the Providence Mountains.
BENTON, J. S.: Draped snakes.
WERLHOF, J. VON: Construction of earthen art.
HYDER, W. D.: Two little known sites in the Santa Barbara back country.
GOUGH, G. R.: The Indian Hill ceremonial fertility site complex.
STRANGE, W. C.: Hogback, nave, and choir: reporting architectures of holiness.
SILVER, C. S.: Rock art at Mesa Verde National Park: recent discoveries.
BERTSCH, H.: Rock art in the vicinity of Loy Butte, Arizona, with special reference to Honanki.
BALLEREAU, D.: A complete survey of petroglyphs from Cerros la Proveedora and Calera, Sonora.
EWING, E. and M. ROBIN: Sunlight and shadow.
SMITH, R.: Rock feature incorporation.
VIÑAS, R., E. SARRIA, A. RUBIO and V. DEL CASTILLO: Cueva de la Serpiente and its painted murals.
COVER, D.: Agua Grande.
MUSCOTT, K.: A trophy head pictograph from the Peruvian Amazon.
McGOWAN, C.: The rock art of Taiwan.
WEIDLER, J. B.: Pict symbol stones are history.

Volume 6 (1989), ISBN 0 937808 48 2, 119 pages:
BURTON, J. F. and M. M. FARRELL: An introduction to the rock art of southeast Arizona.
MARTYNEC, R. J.: Hohokam, Patayan, or ? Rock art at two sites near Gila Bend, Arizona.
KRISS, V.: Mimbres-Mogollon archaeoastronomy: another connection between Mesoamerica and the Southwest.
ROBIN, M. and E. EWING: The Sun is in his house: summer solstice at San Carlos Mesa, Baja California Norte.
MOORE, E. A.: Serpiente: a new recording.
EWING, E.: Serpiente: new discoveries.
RITTER, E. W., L. A. PAYEN and C. H. RECTOR: Rock art representation and symbolism on the Concepción Peninsula, Baja California Sur, Mexico.
RAFTER, J.: Archaeoastronomical possibility at CA-Riv-984.
JONES, B. M. Jr: The Sun arrives at his home.
SEHGAL, L.: Climbing Jacob's Ladder: symbolism of a fantastic journey along the Milky Way.
HEDGES, K.: Sketches and scratches.
BUCKSKIN, F.: The Cassel intaglios.
VUNCANNON, D. H.: Petroglyphic travel diaries on Hawaii.
MUSCUTT, K.: Cueva de Osiris: a pictograph site in the Peruvian Amazon.
STRANGE, W. C.: Tsagiglálal and her wicked sister: a study in contexts.

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RECENT BOOKS OF INTEREST

Many of the volumes listed here will be reviewed in one of the next issues of *RAR*:

El arte rupestre aragonés. Aportaciones de las pinturas prehistóricas de Albalate del Arzobispo y Estadilla, by ANTONIO BELTRAN. 1989. Editado iberCaja, Zaragoza. 178 pages, illustrated with text drawings and numerous colour plates. ISBN 84 87007 05 8.

A field guide to Aboriginal engravings with special reference to those around Sydney, by PETER STANBURY and JOHN CLEGG. With poems by David Campbell. 1990. Sydney University Press in association with Oxford University Press Australia. 163 pages, richly illustrated with recordings, maps, colour and monochrome plates. ISBN 0 424 00147 0.

Felsbilder. Wiege der Kunst und des Geistes, by EMMANUEL ANATI. 1991. U. Bär Verlag, Zürich. Translated from the Italian by Brigitte Fleischmann-Calabrese, originally published in 1989. With Foreword by Yves Coppens. Profusely illustrated in colour and monochrome, 256 pages. ISBN 3 905137 33 X.

Rock paintings of Mirzapur, by RAKESH TEWARI. 1990. Eureka Printers, Lucknow, India, published by the U.P. State Archaeological Organisation. 59 pages, 24 colour plates, plus drawings and monochrome plates.

Listy kamennoi knigi ulug-khema, by M. A. DEVLET. 1990. Tuvinskoe khizhnoe izdatel'stvo. 120 pages, illustrated. ISBN 5 7655 0135 4.

The riches of ancient Australia. A journey into prehistory, by JOSEPHINE FLOOD. 1990. University of Queensland Press, St Lucia. 373 pages, illustrated throughout. This guidebook deals largely with rock art sites. ISBN 0 7022 2259 3.

The great art of the early Australians, by JAN JELINEK. 1989. Moravian Museum - Anthropos Institute, Brno. 527 pages, profusely illustrated with monochrome and colour plates, as well as exquisite drawings. ISBN 80 7028 001 8.

Petroglyphs and Pueblo myths of the Rio Grande, by CAROL PATTERSON-RUDOLPH. 1990. Avanyu Publishing, Albuquerque. 132 pages, profusely illustrated with line drawings, colour and monochrome plates. ISBN 0 936755 13 X.

The uses of style in archaeology, edited by MARGARET W. CONKEY and CHRISTINE A. HASTORF. 1990. Cambridge University Press, Cambridge. 124 pages, illustrated. Collection of ten conference papers which includes contributions by W. Davis, I. Hodder and P. Wiessner. ISBN 0 521 35061 1.

The Drakensberg Bushmen and their art, by A. R. WILLCOX. 1990 (revised edition). Drakensberg Publications, Winterton, Natal. 120 pages, with colour and monochrome plates as well as 27 figures. ISBN 0 9583113 1 5.

Arqueologia de Cuba: métodos y sistemas, by JOSÉ M. GUARCH. 1987. Editorial de Ciencias Sociales, La Habana. 103 pages, illustrated.

Bolivia prehispanica, by ROY QUEREJAZU LEWIS. 1989. Liberia Editorial 'Juventud', La Paz. 416 pages, drawings and monochrome plates.

Les gravures rupestres de la Quebrada las Pintadas de Marquesa (Norte Chico, Chili), by D. BALLEREAU, H. NIEMEYER F. and E. PIZARRO W. 1986. Editions Recherche sur les Civilisations, Paris. 236 pages, profusely illustrated. ISBN 2 86538 146 3.

Arqueoligía aborigen de Cuba, by R. DACAL MOURE and M. RIVERO DE LA CALLE. 1984. Editorial Gente Nueva, Ciudad de la Habana. 174 pages, illustrated.

Antropomorfnye izobrazheniya, edited by R. S. VASIL'EVSKII. 1987. Izdatel'stvo 'Nauka', Sibirskoe Otdelenie, Novosibirsk. 224 pages, illustrated.

Problemy izucheniya naskal'nykh izobrazhenii b SSSR, edited by M. A. DEVLET. 1990. Institut Arkheologii, Akademii Nauk SSSR, Moscow. 240 pages with line drawings. Proceedings of the first rock art conference held in the U.S.S.R., comprising 42 contributions.

Petroglyphs of the Wulanchabu grassland, by GAI SHANLIN. 1989. Cultural Relics Publishing House, Beijing. 335 pages, line drawings in text, plus 32 pages of colour and monochrome plates. Published in Chinese, with English summary. ISBN 7 5010 0317 3/K 124.

Petroglyphs of the Yinshan Mountains, by GAI SHANLIN. 1986. Cultural Relics Publishing House, Beijing. 441 pages, line drawings in text, plus 48 pages of plates, some in colour. Published in Chinese, with English summary.

Kalliotaidetta - Tutkimusta ja tulkintaa, edited by AINO NISSINAHO. 1990. Suomen Antropologisen Seuran Julkaisuja 1. 99 pages, illustrated. ISBN 951 95435 7 0.

Journées internationales d'étude sur la conservation de l'art rupestre. Actes, 50 ans après la découverte de Lascaux. 1990. Groupe Art Rupestre de l'ICOM pour la Conservation. 183 pages, illustrated. With contributions in English, French and Spanish. ISBN 2 9504621 0 3.

Preserving our rock art heritage. Proceedings from the Symposium on Rock Art Conservation and Protection, ARARA Conference 1987. Edited by HELEN K. CROTTY. 1989. Occasional Paper 1, American Rock Art Research Association, San Miguel, California. 105 pages, illustrated. The volume contains 12 papers.

Die ältesten Wagenbilder Europas, by DIETRICH EVERS. 1988. Druckerei Gutenberg, Meisungen. 36 pages, 46 illustrations.

Arte rupestre nelle Alpi Occidentali dalla Valle Po alla Valchiusella, edited by DARIO SEGLIE and a committee of the Centro Studi e Museo d'Arte Prteistorica. 1988. Museo Nazionale della Montagna 'Duca Degli Abruzzi', Torino. 131 pages, with numerous colour and monochrome plates.

Kamennaya letopis' karelii, by Y. A. SAVVATEEV. 1990. 118 pages, profusely illustrated. ISBN 5 7545 0316 4.

Ensayo sobre el rigen y significacion del arte prehistorico, by ANTONIO BELTRAN MARTINEZ. 1989. Universidad de Zaragoza. 199 pages, numerous line drawings and monochrome plates. ISBN 84 7733 136 7.

La pintura prehistórica, by M. A. GARCIA GUINEA. 1989. Ediciones vicens-vives, Barcelona. 216 pages, of which 128 are full-page colour plates. ISBN 84 316 2708 5.

Art in the mirror of ages, by JAN JELINEK. 1990. Moravian Museum, Brno. 64 pages, 93 illustrations, including some colour plates. ISBN 80 7028 011 5.

Rock art of the Southern Black Hills. A contextual approach, by LINEA SUNDSTROM. 1990. Garland Publishing, New York and London. 422 pages, text drawings. ISBN 0 8240 6833 5.

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RECENT PAPERS OF INTEREST

Radiocarbon dating of prehistoric rock paintings by selective oxidation of organic carbon, by J. RUSS, M. HYMAN, H. J. SHAFER and M. W. ROWE. *Nature*, 1990. Volume 348, Number 6303, pp. 710-1.

The dating of a pictograph in the Lower Pecos region of south-west Texas is reported. The method used allows the separation of inorganic carbon from the organic material in the paint, and the dating of the latter with the AMS method.

Masques et théromorphes dans l'art rupestre du Sahara central, by ALFRED MUZZOLINI. *Archéo-Nil*, 1991. Volume 1, pp. 17-42.

Saharan rock art of masks or therianthropes are described. They are found mostly in the 'naturalistic bubaline school' and among the Tassilian 'round heads'.

Southern-Bushman descendants in the Transkei - rock art and rainmaking, by F. E. PRINS. *South African Journal of Ethnology*, 1990, Volume 13, Number 3, pp. 110-6.

Southern-San descendants interviewed in the Transkei apparently support some aspects of identified concepts in what has become known as the 'trance hypothesis'. However, their testimonies also seem to uphold older Western ideas concerning the interpretation of San rock art.

Stone Age psychedelia, by ROGER LEWIN. *New Scientist*, 1991, Number 1772, pp. 24-8.

This paper provides a popular account of the trance and shaman hypotheses emanating from South Africa.

Symbolism and modern human origins, by J. M. LINDLY and G. A. CLARK. *Current Anthropology*, 1990, Volume 31, Number 3, pp. 233-61. With comments by O. Bar-Yosef, D. Lieberman, J. Shea, H. L. Dibble, P. G. Chase, C. Gamble, R. H. Gargett, K. Jacobs, P. Mellars, A. Pike-Tay, Y. Smirnov, L. G. Straus, C. B. Stringer, E. Trinkaus and R. White.

The authors examine evidence from all sites that have yielded pre-Upper Palaeolithic modern humans, and demonstrate that evidence for symbolic behaviour cannot be correlated with hominid taxa as has sometimes been claimed. There is no evidence that the transitions from the Middle to the Upper Palaeolithic and from archaic *H. sapiens* to morphologically modern humans coincided in time.

Rock art and inter-regional interaction in northeastern Australian prehistory, by BRUNO DAVID and NOELENE COLE. *Antiquity*, 1990, Volume 64, pp. 788-806.

The Cape York Peninsula shows distinct regional pattern in its recent rock art. There is also regional pattern in the exchange networks of ethnohistorical times. The possibilities of relationships are explored.

The parietal art of the Late Magdalenian, by JEAN CLOTTES. *Antiquity*, 1990, Volume 64, pp. 527-48.

The chronology of Leroi-Gourhan's Style IV of the Magdalenian is reviewed. The author discusses the present dating criteria for that style, before considering them in the context of the problems of placing Late Magdalenian sites into the traditional chronology, especially in the Pyrenees.

The evolutionary emergence of modern human behaviour: language and its archaeology, by WILLIAM NOBLE and IAIN DAVIDSON. *Man* (N.S.), 1991, Volume 26, pp. 223-53.

The origin of modern human behaviour is traced to a point at which language emerged. The authors argue that any approach to explaining the emergence of modern human behaviour needs both to pay closer attention to the archaeological record and to be informed by appropriate theory. Language began when the symbolic property of signs was first discovered, and there are claimed to be no signs of symbols in the archaeological record prior to about 32 000 years ago.

Why are so many ancient rock paintings red? By N. COOK, I. DAVIDSON and S. SUTTON. *Australian Aboriginal Studies*, 1990, Number 1, pp. 30-2.

Cave art studies and the Aboriginal community in the Southeast, by ROBERT G. BEDNARIK. *Aboriginal Heritage Newsletter*, 1989, Number 29, pp. 3-4.

Le corps féminin et son langage dans l'art paléolithique, by JEAN-PIERRE DUHARD. *Oxford Journal of Archaeology*, 1990, Volume 9, Number 3, pp. 241-55.

It is proposed that figurative Palaeolithic art obeys the same laws and had the same aims as speech, and that it should be analysed in the same way as language. Palaeolithic artists intended to show women in their various functions, especially that of mother, and if it is possible to speak of a 'language of art', then we are dealing with a physiological language of the female body.

Izobrazheniya mamontov v paleoliticheskom iskusstve, by V. P. LYUBIN. *Sovetskaya Arkheologiya*, 1991, pp. 20-42.

A number of mammoth images in Palaeolithic art are analysed according to palaeontological criteria. Some specific local features of the mammoth paintings in Kapova Cave in the Urals are characterised. Questions are raised about the differences in the Franco-Cantabrian and eastern arts, and the factors which influenced the subjects of figurative art.

Cation-ratio dating of rock-engravings: a critical appraisal, by MAURICE P. LANTEIGNE. *Antiquity*, 1991, Volume 65, pp. 292-5.

Issue is taken with one of the new methods of dating petroglyphs, the cation-ratio analysis of rock varnish. It is shown that several error factors severely constrain the efficacy of the method for the minimum dating and chronological sequencing of petroglyph.

Cation-ratio varnish dating and petroglyph chronology in southeastern Colorado, by LAWRENCE L. LOENDORF. *Antiquity*, 1991, Volume 65, pp. 246-55.

Where petroglyphs are covered by rock varnish, the opportunity for absolute dating by the cation-ratio method exists. The dates reported here seem to run alongside the chronological pattern inferred by conventional means.

Homo erectus skapade konst i mellanistiden, by ROBERT G. BEDNARIK. *Populär Arkeologi*, 1991, Volume 9, Number 1, p. 18.

Pour des approches rationelles et differenciées dans l'étude des divers centres mondiaux d'art rupestre préhistorique, by A. MUZZOLINI. *Espacio, Tiempo y Forma*, 1989, Volume 2, pp. 15-47.

A propos des masques et visages rupestres du Sahara, by FRANÇOIS SOLEILHAVOUP. *Archéo-Nil*, 1991, Volume 1, pp. 43-58.

The author considers masks in Saharan rock art to refer to hunting, sexuality or divine transcendence. Personalised faces of individuals are rarely seen. Some speculations are offered about the different types and their possible meanings.

Die Felsbilder Neukaledoniens, by GOTTFRIED KREUZER and CHRISTINE KREUZER. *Ur- und frühzeit*, 1990, Volume 17, Number 3, pp. 9-14.

A brief description of the petroglyphs of New Caledonia is presented, together with illustrations of the more frequent motifs and some comparisons with other art bodies.

Matériaux pour servir à l'histoire de Montespan, by ROBERT BEGOUEN and JEAN CLOTTES. *Bulletin de la Société Préhistorique Ariège-Pyrénées*, 1988, Volume 43, pp. 13-33.

La protection de l'art rupestre, by JEAN CLOTTES. *Jornados sobre Parques con Arte Rupestre*, 1990, pp. 67-88.

The cave petroglyphs of Australia, by ROBERT BEDNARIK. *Australian Aboriginal Studies*, 1990, Number 2, pp. 64-8.

River Mizque basin rock art, Bolivia, by ROY QUEREJAZU LEWIS. *Latin American Indian Literatures Journal*, 1990, Volume 6, Number 1, pp. 65-73.

Several Bolivian key sites of rock art are described and discussed, including the small cave of Toro Muerto with very ancient petroglyphs, and the large painting site El Buey.

The time clock of aged patinas, by C. A. PINEDA, M. PEISACH and L. JACOBSON. *Nuclear Active*, 1989, Volume 41, pp. 17-24.

Petroglyphs in the U.S. Virgin Islands: a survey, by C. N. DUBELAAR. *Proceedings of the Thirteenth International Congress for Caribbean Archaeology*, 1991, Part 2, pp. 944-73.

ORIENTATION

Cairns '92

THE SECOND AURA CONGRESS
Cairns, 30 August - 4 September 1992
THIRD ANNOUNCEMENT

The second congress of the Australian Rock Art Research Association (AURA) will be held in conjunction with the 1992 meeting of the International Federation of Rock Art Organisations (IFRAO) in Cairns, in close proximity of the major rock art regions of north Queensland. Around 500 specialists are expected to attend the 12 academic symposia, where almost 200 papers will be presented, preceded and followed by several weeks of field trips. This is the principal international event in palaeoart studies and cognitive archaeology. Abstracts for papers are called for now, and registrations are invited.

ACADEMIC PROGRAM

The Second AURA Congress will comprise the following symposia:

A) ***Rock art studies: the post-stylistic era. Where do we go from here?*** Chaired by Dr Michel Lorblanchet (France) and Dr Paul Bahn (United Kingdom).
B) ***Rock art and information exchange.*** Claire Smith (Australia).
C) ***Rock art studies as a curriculum for teaching.*** Dr Giriraj Kumar (India) and Professor Osaga Odak (Kenya).
D) ***The rock art of the Sahara.*** Alfred Muzzolini (France).
E) ***Spatial considerations in rock art.*** Dr Paul Faulstich (U.S.A.) and Dr Paul Taçon (Australia).
F) ***Dating of rock art.*** Alan Watchman (Australia) and Professor Jack Steinbring (Canada).
G) ***Preservation of rock art.*** Andrew Thorn (Australia) and Dr Nicholas Stanley Price (U.S.A.).
H) ***Management of rock art.*** Dr Graeme Ward (Australia) and Bruce Ford (Australia).
I) ***Rock art of north Queensland.*** Dr Mike Morwood (Australia) and Noelene Cole (Australia).
J) ***The ethics of rock art research.*** Robert G. Bednarik (Australia) and Mario Consens (Uruguay).
K) ***General session.*** Chair to be nominated.
L) ***Workshops on management: indigenous experiences and perceptions.*** Natalie Franklin (Australia) and Michael Rowland (Australia).

Rationales for the first five symposia have appeared in the May 1991 issue of *RAR*, and are appearing in a number of other journals abroad. The rationales for Symposia F, G, I and J appear in the current issue of *RAR*. Abstracts (not exceeding 200 words) of papers on any of the subjects covered by the above symposia, or on any other palaeoart-related subject, are invited now. Presenters must be prepared to respond to questions from the audience, and papers must therefore be presented by the author (or one of the authors), unless the symposium chair permits otherwise.

The official language of the congress is English, but a small number of papers in French, German, Russian and Spanish can be accepted. A strict time limit of 20 minutes applies and papers will be followed by question times of 10 minutes. The pre-congress program will contain abstracts of all papers presented. AURA will have first publication rights of all papers.

ADDITIONAL EVENTS

Several additional events will be attached to the Second AURA Congress:

1) The Second General Meeting of AURA. An agenda will be published before the event. Chair: AURA President George Chaloupka, A.O. Attendance is restricted to full members of AURA. An agenda will be in the congress program.
2) The 1992 IFRAO Meeting. An agenda will be published in *IFRAO Report No. 8* (May 1992). Chair: Convener Robert G. Bednarik.
3) CAR Meeting. The official meeting of the Comité International pour l'Art Rupestre will be chaired by CAR President Dr Jean Clottes.
4) A meeting of the ICOM-CC Rock Art Conservation Working Group will be chaired by Jacques Brunet and Ivan Haskovec (see detailed announcement below).
5) A meeting of parties interested in the AURA Exhibition expected to open in 1993 at the Australian Museum. This will be convened by curators Dr Paul S. C. Taçon and Robert G. Bednarik.

As in Darwin in 1988, there will be rock art ***films and videos*** shown at the congress. Videos submitted must be VHS type.

An exhibition area on the floor below the conference lecture halls will be utilised for a ***poster and photographic exhibition*** on rock art. This is also expected to include a mock-up and information stand of the AURA/IFRAO/Australian Museum exhibition project, the most ambitious exhibition of palaeoart ever attempted, which will be attended by staff of that project.

The ***plenary session*** and opening of the congress will be on the afternoon of Sunday, 30 August 1992, at the Cairns Civic Centre. It will be preceded by a public performance of the Tjapukai Aboriginal Dance Company. Key note addresses by the Prime Minister of Australia, Aboriginal Congress Patrons, and AURA Vice-President Professor Jack Steinbring are scheduled.

Cairns '92 is expected to be well covered by the printed and electronic media. Congress participants who intend to present papers that are of media interest are requested to submit suitable ***press releases*** to the Congress Chairpersons or to the Congress Media Officer, Noel Burman.

FIELD TRIPS

There is more rock art in northern Australia than in any other world region. The principal concentrations are, from west, the Pilbara, the Kimberley, the Victoria River District, Arnhem Land, and north Queensland. Australian rock art represents the oldest continuous cultural record in the world, beginning before the celebrated Upper Palaeolithic art of Europe, and providing much of the basis of contemporary Aboriginal cosmology. The congress field trip program can only include a tiny sample of this enormous wealth of rock art, but it does include some of the most important and most spectacular sites. In many cases the experience of the site visit will be greatly enhanced by the guid-

ance of traditional site custodians and trained Aboriginal rangers, such as those provided by the Quinkan Reserves at Laura. Here is the program of field trips:

Pre-congress field trips (ex Cairns, during week 25-29 August)
1-day trips from Cairns (costs to be advised)
1-day trip to Chillagoe rock art
1-day to Bare Hill site

Overview of north Queensland rock art (organised and led by Bruno David):
5-day self drive (4WD only), approximately $A200 per person.
This self-drive (or shared 4WD rental vehicle) tour takes in Chillagoe, Mitchell and Palmer sites and terminates in Laura. Participants provide camping equipment and food, guide/camp fees.

Self-drive tour to Laura (Quinkan Reserves): 1)
Car rental companies permit only 4WD vehicles to travel to Laura due to road conditions, although Laura is accessible by conventional vehicles. The driving time from Cairns to Laura is about 5 hours. Laura is a very small town with limited services. Camping and guide fees payable at Laura.

Bus tour to Laura (Quinkan Reserves): 1)
4 days, $A200, guide fee extra, camp ground fee included, provide own camping gear and food.

Jowalbinna Bush Camp, Laura (S. Trezise): 2)
5-day 4WD safari, $A750.
4-day 4WD safari, $A600.
2-day fly/fly, $A595.
1-day fly/fly, $A275.

Deighton River Bush Camp, Laura (S. Trezise): 2)
5-day rock art safari, drive/fly, $A625 (fit participants).

Rock art in Townsville area (Elizabeth Hatte):
1-day self-drive, 4WD not necessary.

Grand Tour 1992 (Dr Hugh Cairns and Howard P. McNickle):
See details below.

Grand Tour of New South Wales (Caryll Sefton):
See details below.

Post-congress field trips (ex Cairns, during week 5-11 September)
Self-drive tour to Laura (Quinkan Reserves): 1)
Car rental companies permit only 4WD vehicles to travel to Laura due to road conditions, although Laura is accessible by conventional vehicles. The driving time from Cairns to Laura is about 5 hours. Laura is a very small town with limited services. Camping and guide fees payable at Laura.

Bus tour to Laura (Quinkan Reserves): 1)
4 days, $A200, guide fee extra, camp ground fee included, provide own camping gear and food.

4WD safari to Laura (Quinkan Reserves): 1)
5 days, $A550 all inclusive, guide fee extra, supply own sleeping bag.

Jowalbinna Bush Camp, Laura (S. Trezise): 2)
5-day 4WD safari, $A750.
7-day 4WD safari, $A950, Jowalbinna/Deighton combined.
2-day fly/fly, $A595.
1-day fly/fly, $A275.

Deighton River Bush Camp, Laura (S. Trezise): 2)
4-day rock art safari, drive/fly $A500 (fit participants).

Queensland Tour (Robert G. Bednarik):
See details below.

The current exchange rate is about $A1.00 = US$0.78. Participants may combine any field trip modules. Except for the tours by commercial operators, tour leaders will have no liability for any accidents or mishaps.

REGISTRATION AND ACCOMMODATION

Registration fees are identical to those of the First AURA Congress in 1988: prior to 30 April 1992, $A80 for AURA members, $A40 for student members, $A130 for non-members (US$62, 31 and 101 respectively). Non-members are welcome to join AURA before registering for the congress.

After April 1992: $A100 for AURA members, $A50 for student members, $A150 for non-members (US$78, 39 and 117 respectively). Registration forms are enclosed in this issue.

The registration fees cover morning and afternoon teas for five days ($A32.50), the cost of venues and facilities, participation in all sessions, functions and meetings of the congress, the *Congress program and manual* and any other pre-congress publications. The reduced registration for student members is thus subsidised by the full registrations, which also meet the cost of registrations of subsidised participants from abroad. Any surplus receipts will flow into the consolidated AURA publications fund. No official or committee member of the AURA Congress receives any payment or other form of compensation for any form of service, the entire event is organised by volunteers (except for the field trips by tour operators).

No cancellation charges apply to registration fees paid; such fees will be refunded in full upon request. Registrations will be accepted right up to 30 August 1992, and day registrations will be available from the registration desk.

The registration form lists the ten recommended hotels, which were selected by the congress committee in Cairns from among the many inspected. The rates given on the registration form are all reduced rates, negotiated for congress participants. In addition, the congress has reserved block bookings for low-cost backpackers' accommodation. It is imperative that accommodation be booked in advance, because it may be difficult to obtain accommodation in Cairns at that time of the year.

FURTHER INFORMATION

For conditions of ***subsidisation*** of congress participants, please refer to page 152, November 1990 issue of *RAR*.

Ansett Airlines is the ***official domestic airline*** of the congress, and Coles Myer Ansett Travel Pty Ltd is the ***official travel agent*** of the congress.

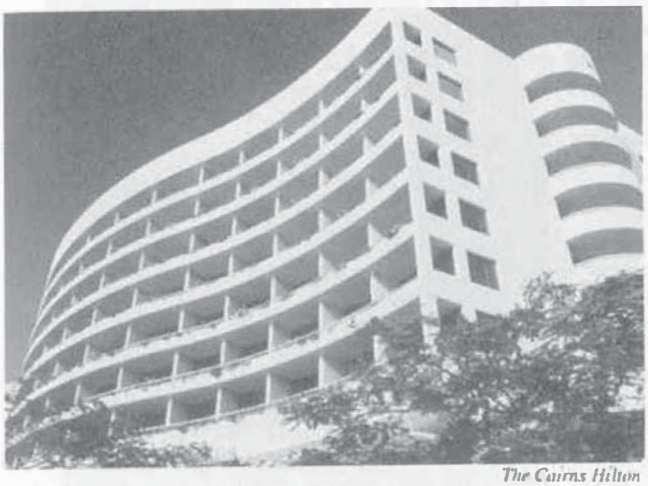
The congress committee has accepted the bid of the Cairns Hilton to be the ***venue of the Second AURA Congress***. Several luxury hotels were in the running, and the Hilton is one of the two best venues in Cairns. It has an almost new building (see below) with a well-designed conference centre providing three identically sized lecture halls separated only by a corridor. Located on the waterfront, it is in the hub of the city, next to the major tourist malls: there are dozens of restaurants within a radius of 5-10 walking minutes, and of course the Hilton has all services in-house. There are numerous other hotels within easy walking distance, including backpackers' accommodation just 3 minutes away.

Cairns has a compact central business district which is only a few kilometres from the international airport. The town is hemmed in by rainforest-clad, steep mountains, it has a superb

1) The logistics of field trips marked 'Quinkan Reserves' will be arranged by tour operators where applicable, and the guide services will be provided by Laura Aboriginal rangers.

2) Field trips marked 'S. Trezise' are arranged by Trezise Bush Guide Service. Costs listed are approximate and all inclusive, except sleeping bag.

tropical but not oppressive climate. The Barrier Reef is close, and the coast of north Queensland is widely regarded as the most picturesque part of Australia.



The Cairns Hilton

I take this opportunity to cordially thank all those who have contributed to the planning of this significant and most worthwhile event, including Percy Trezise, Lindsay Roughsey, George Musgrave, Noelene Cole, Mary Haginikitas, Beverly Trezise, Matthew Trezise, Stephen Trezise, Elaine Godden, Noel Burman, Elfriede Bednarik, Michel Lorblanchet, Paul Bahn, Claire Smith, Giriraj Kumar, Osaga Odak, Alfred Muzzolini, Paul Faulstich, Paul Taçon, Alan Watchman, Jack Steinbring, Andrew Thorn, Nicholas Stanley Price, Graeme Ward, Bruce Ford, Mike Morwood, Mario Consens, Nicky Horsfall, Natalie Franklin, Caryll Sefton, Hugh Cairns, Howard P. McNickle and Elizabeth Hatte. Preparations are proceeding on all fronts, excellent abstracts are flooding in, and there have been numerous enquiries concerning registration. As rock art conferences go, Cairns '92 will be a blockbuster par excellence!

R. G. Bednarik (Congress Co-chair)

RAR 8-225

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The Grand Tour 1992

This pre-congress field trip will commence in the Pilbara (Western Australia) and end at Cairns (Queensland). It is intended to leave the Dampier-Karratha region on 1 August 1992 and arrive in Cairns on 24 August. This will allow travellers to traverse Australia's north, yet arrive in time to join one of the Laura field trips before the congress, or alternatively to visit Darwin and Kakadu National Park before flying on to Cairns.

Transport will be either by own vehicle, self-drive rented vehicle, or in a safari-company vehicle (accompanied).

Visitors to Australia can book vehicles directly with Avis/Ansett Airlines (the official domestic airline of the AURA Congress), or through own travel agent. As a rough guide, the basic cost of a rented vehicle will be about $A2000 for a car, $A3500 for a Landcruiser, to be divided over the number of people travelling in the vehicle. For the safari company 6-wheel vehicle group, individual all-up cost will be around $A3000. Participants hiring or taking own vehicles will be responsible for all food, fuel and camping gear required.

Sites to be visited on this tour include many of the outstanding rock art sites of the Pilbara (see reports by H. P. McNickle, *RAR* 1: 5-24; 2: 48-64), Kimberley (cf. D. Welsh, *RAR* 7: 110-24) and Victoria River District (cf. H. P. McNickle, *RAR* 8: 36-46). The terrain for the vehicles and for walking and camping will often be rough, but the heat is not expected to be excessive.

If desired by participants, the field trip could commence earlier, and other participants could possibly join at Broome and Kununurra en route. In view of the distances to be traversed the expedition cannot leave Dampier later than 1 August.

Some members might choose to leave the party at Katherine on 20 August to visit Kakadu, before flying to Cairns from Darwin. Others (including the safari vehicle passengers if a minimum number so choose) will travel the 2000 km to the east coast overland. Members of both groups will be free to join the Laura field trips. Still others might stay longer in the Victoria River area. The overall program will be flexible.

For further information, including details of the safari company offer, and for registration, please write directly to the Tour Leader, Dr Hugh Cairns, 23 Wallaroy Road, Double Bay, NSW 2028, Australia - Telephone (02) 327 1488.

The Grand Tour of New South Wales

The Grand Tour of New South Wales Rock Art Sites will be led by Caryll Sefton and organised by naturalist Richard Jordan of Emu Tours. This pre-congress tour is from Sunday, 9 August to Sunday, 23 August 1992 (15 days), it commences and finishes in Sydney, fully catered with a cook, four accommodated nights included, the balance is camping, for which bus transport and all equipment except sleeping bags is provided. The cost will be $A1400 and there will be a limit of 16 participants.

The Grand Tour of NSW will visit major rock art complexes in the Sydney Basin, including the central, northern and southern sections of the Basin. There will also be comprehensive coverage of sites in the New England Tableland and in far-western New South Wales. Major sites proposed to be visited include Canoelands, Devils Rock, Sevintons, Milbrodale, Moonbi, Graman, Mount Grenfell, Sturts Meadows, Mootwingee, Euriowie, Mount Manora, Coles Creek, Woronora and others. Sites visited will include paintings, drawings, stencils and petroglyphs, and will include classical site types. Aboriginal participation will be invited.

For further information please contact the Tour Leader, Caryll Sefton, 12 Chenhall Street, Woonona, NSW 2517, Australia.

The Queensland Tour

This post-congress tour is expected to commence on 6 September at Cairns. Its first stop will be Laura, where several of the principal sites are to be visited. From there to Chillagoe, to see several decorated limestone caves and other sites (Walkunder Arch Cave, Walkunder Galleries, Pillar Cave, Fern Cave, Castle Rock, Tower of London site, sites at Mungana; see B. David and M. David, *RAR* 5: 147-56; B. David, *RAR* 7: 74-5). The rock art of this region is visually less spectacular than that of Laura but is of considerable scientific value. On to Bare Hill, where John Clegg produced the work for his M.A. thesis. From Mareeba south to Hughenden, several sites there and at Porcupine Gorge. Then to Townsville at the coast, where one of the party has to deliver a lecture at the university, while the group has the option of seeing the 'shield' art area. Along the coast to Rockhampton, then to the Carnarvon Ranges, to see some of the numerous rock art sites in Carnarvon Gorge with their spectacular stencil art.

The group will arrive at Carnarvon on 19 or 20 September, and can then spend several days in the region. From there, participants could travel to Brisbane (1 day), or back north, or south to Armidale (University of New England). The itinerary is most flexible, participants can discontinue, and fly out from Cairns, Townsville or Rockhampton as the group passes through these centres.

Logistics will be by private or hire 4WD vehicles (there would be alternative routes for conventional cars). Participants would supply and share transport and camping gear, supplies would be shared by arrangement. Costs - in addition to travel, camping and subsistence - will be limited to guiding or camping fees where these apply.

For further information and registration, please contact the Tour Leader, Robert G. Bednarik, AURA, P.O. Box 216, Caulfield South, Vic. 3162, Australia - Telephone (03) 523 0549.

Meeting of ICOM-CC Rock Art Conservation Working Group at AURA Congress 1992

Jacques Brunet and Ivan P. Haskovec (Joint Co-ordinators) invite all congress participants to an informal meeting of the ICOM-CC Rock Art Working Group during the Second AURA Congress, for the purpose of advancement of the organisation and proper functioning of the Rock Art Conservation Working Group. Details of the meeting will appear in the congress program. We propose the following items for the agenda of this meeting:

(1) Assessment of the *Groupe de Travail Art Rupestre* in terms of its past performance.
(2) Discussion of the Co-ordinators' proposals.
(3) Proposals for future directions.
(4) Preparation of Washington ICOM-CC 1993 meeting.
(5) Other items.

The meeting will be open to all people interested in rock art conservation. Should anyone wish to add other items to the agenda, one of the two Co-ordinators should be contacted. Agenda items will be accepted immediately prior to the meeting as well.

To be a voting member of the ICOM-CC Rock Art Working Group, one becomes a member of one's national ICOM, indicating on the application form a preference for the Conservation Committee, Rock Art Conservation Group. We would welcome you.

Jacques Brunet
L.R.M.H.
29 Rue de Paris
77420 Champs sur Marne
France

Ivan Haskovec
A.N.P.W.S.
P.O. Box 71
Jabiru, N.T. 0886
Australia

Notices

The 1991 AURA CHINA TOUR was a great success. The eight AURA members listed in the August *AURA Newsletter* were joined by AURA President George Chaloupka and Caryll E. Sefton, as well as by several overseas AURA members: Dr Paul Bahn (England), Shirley Chesney (U.S.A.), Professors Dario Seglie and Emmanuel Anati (Italy) and Professor Arild Hvidtfeldt (Denmark). Since there were 29 non-Chinese conference participants at the Yinchuan rock art conference, AURA provided one half of the international contingent! A full report of this memorable tour will appear in the next issue of the *AURA Newsletter*.

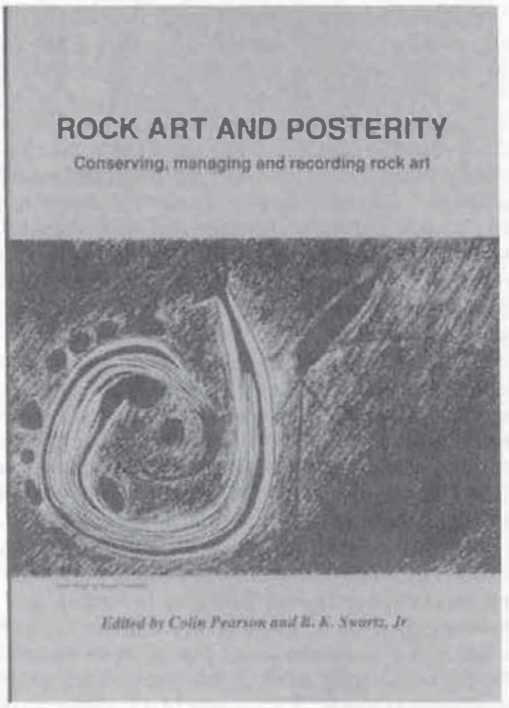
Emeritus Professor Derek John MULVANEY, A.O., has been appointed an Officer of the Order of Australia, for his service to prehistory and anthropology, particularly through the study of Aboriginal cultural heritage.

Alexander MARSHACK has been elected to the rank of Fellow of the American Association for the Advancement of Science, for innovative research on Upper Palaeolithic art and symbolic systems.

Professor Josephine FLOOD has been awarded the C. J. Dennis Literary Award for Natural History by the Victorian Fellowship of Australian Writers, on the basis of the literary merit of her book, *The riches of ancient Australia: a journey into prehistory*.

New AURA members

We welcome the following new members:
Ms Miriel Lenore, Summertown, South Australia
Professor Marvin W. Rowe, College Station, Texas, U.S.A.
Dr Martin Magne, Edmonton, Alberta, Canada
Dr Ann Sieveking, Darsham, United Kingdom
Scandinavian Society for Prehistoric Art, Tanumshede, Sweden
Drs Jan-Kees A. Hagers, Voorburg, Netherlands
Mr John Suter, Mona Vale, New South Wales
The Israel Museum, Jerusalem, Israel
Campbelltown City Council, Campbelltown, New South Wales
Q.NPWS Library, Rockhampton, Queensland
McMaster University Library, Hamilton, Ontario, Canada
Charles Sturt University Library, Albury, New South Wales
Universidad de Valencia, Valencia, Spain
Mr Robert Tickner, MP, Canberra, A.C.T.
Mrs Joan Binnion, Belair, South Australia
Aboriginal and Islander Affairs Library, Brisbane, Queensland
Mr Paul Molino, Maroubra, New South Wales
Akademie der Wissenschaften, Heidelberg, Germany
Universidad de Santiago de Compostela, Santiago, Spain
Mr Paul Finn, Hampton, Victoria
Dr John L. Bradshaw, Clayton, Victoria
B. N. Burnell, Frances, South Australia
Christine Lovell-Jones, Armidale, New South Wales
Ms Rhonda K. Baker, Burrapine, New South Wales
Mr Peter J. Pilles, Jr, Flagstaff, Arizona, U.S.A.
State Museum Library, Windhoek, Namibia
Ms Amanda Lovell, Rasmussen, Queensland
Mr Robert G. Martin, Box Hill, Victoria
General Editor, Origini, Rome, Italy
Dr Christopher Doutney, London, United Kingdom
Rock Art Research Association of Canada, Winnipeg, Canada
Mr Edward A. Stasack, Prescott, Arizona, U.S.A.
New Zealand Historic Places Trust, Wellington, New Zealand
Dr Ralph J. Hartley, Lincoln, Nebraska, U.S.A.
Ms Joan Snyder, Elsternwick, Victoria
June Ross, Wollomombi, New South Wales
Mrs Ruth Jansyn, Highland Park, New Jersey, U.S.A.
Mr Tim Hawkes, Cairns, Queensland
Dr Mila Simoes de Abreu, Oeiras, Portugal
Dr Gaudenzio Ragazzi, Esine, Italy
Dr Guiseppe Brunod, Savigliano, Italy
Ms Patricia A. Kolln, Oyster Bay, New South Wales
Ms Jane Kessy, Dar es Salaam, Tanzania
Mr Ian Hemingway, Ballarat, Victoria
Museo Chileno de Arte Precolombino, Santiago, Chile
P. W. Purcell, Currumbin, Queensland
Dr Steven J. Waller, Newark, Delaware, U.S.A.
Professor Arild Hvidtfeldt, Rude, Denmark
Dr Maria Pia Falchi, Capital Federal, Argentina
Laraine Nelson, Belmont North, New South Wales
Mrs Gaynor Cuthbert, Eltham, Victoria
Miss Deborah E. Stone, West Killara, New South Wales
Mr Ken O'Gower, Scotts Head, New South Wales
F. H. Jansen, Condon, Queensland
Mr Mossa Alivand, Aghajari, Iran
Mr Amir Fatoorechi, Tehran, Iran
Dr Khosro Janani, Gheytarie, Iran
Mr Noel Burman, Ravenshoe, Queensland
Mr Hadobás Sándor, Rudabánya, Hungary
Dr James Keyser, Portland, Oregon, U.S.A.
Professor Joachim Hahn, Tübingen, Germany
Library of the Musée de l'Homme, Paris, France



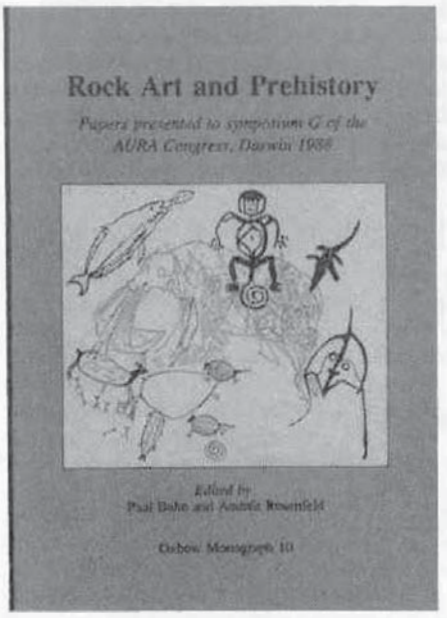
Rock art and posterity:
Conserving, managing and recording rock art
DARWIN CONGRESS PROCEEDINGS,
SYMPOSIA M AND E

BIBLIOGRAPHICAL DETAILS
Rock art and posterity: Conserving, managing and recording rock art. 1991. Edited by Colin Pearson and B. K. Swartz, Jr. Proceedings of Symposia M and E, First AURA Congress, Darwin 1988. Occasional AURA Publication No. 4. Australian Rock Art Research Association, Melbourne. 160 pages, 40 plates, 22 line drawings, 21 maps, 19 tables, on 100g/m² matt art paper, set on Linotronic 330 - 2500 dpi, laminated paperback cover, full A4 format (297x210 mm). ISBN 0 646 03751 X. RRP $A26.00 (50% discount applies to orders from affiliated specialists and to multiple orders, min. 5 copies).

Comprises contributions by Robert G. Bednarik, Tony Blanks, Roberto Bosio, Steve Brown, J. Brunet, Elizabeth Caldicott, Mauro Cinquetti, J. Clarke, Paul Faulstich, J. Flood, Tommaso Guiot, Ivan P. Haskovec, P. Haydock, William D. Hyder, D. Lambert, Colin Pearson, N. North, Peter Randolph, Piero Ricchiardi, Eric W. Ritter, Dario Seglie, Nicholas P. Stanley Price, Hilary Sullivan, B. K. Swartz, Jr, Andrew Thorn, Gajendra S. Tyagi, P. Vidal, J. Vouvé, Joseph Wallam, Grahame L. Walsh, John K. Zancanella.

The list price of *Rock art and posterity* is $A26.00, but the volume is available to members of any of the 17 organisations affiliated with IFRAO (practically all the major rock art organisations in the world) at *50% discount* ($A13.00, or US$10.00, plus packing in sturdy cardboard boxes, $A1.50, and surface postage of $6.20 within Australia, $A9.40 to anywhere in the world, *only one copy per member*). This price represents the actual printing cost (allowing for review and complimentary copies), plus packing and postage. Printing costs were underwritten by the Australian Institute of Aboriginal and Torres Strait Islander Studies and by the Australian Commission for Unesco. **All money recovered from sales will be paid into the AURA Publications Fund,** which will meet the costs of printing future quality work in palaeoart studies. Thus by purchasing this volume you are contributing to the establishment of a perpetual publishing base for rock art studies.

To order at the special rate, please state that you are a member of a rock art organisation, and send a bank draft or postal money order for **$A20.70 within Australia**, or $A23.90 **(US$18.40) anywhere else** in the world, or simply provide your Visa or MasterCard number and expiry date, to:
AURA, P.O. Box 216, Caulfield South, Vic. 3162, Australia



Rock art and prehistory
DARWIN CONGRESS PROCEEDINGS, SYMPOSIUM G

BIBLIOGRAPHICAL DETAILS
Rock art and prehistory. 1991. Edited by Paul Bahn and Andrée Rosenfeld. Proceedings of Symposium G, First AURA Congress, Darwin 1988. Oxbow Monograph 10, Oxbow Books, Oxford. 147 pages, 15 plates, 45 line drawings, 10 maps, 7 tables, on 100g/m² matt art paper, laminated paperback cover, full A4 format (297x210 mm). ISBN 0 946897 32 8. £15.00, US$30.00, $A38.50.

Comprises contributions by Paul Bahn, Steve Brown, Margaret Bullen, Shirley Chesney, John Clegg, Anthony Forge, Natalie Franklin, Susan Johnston, Engkos Kosasih, Robert Layton, Josephine McDonald, Kelvin Officer, André Prous, Andrée Rosenfeld, Claire Smith.

Rock art and prehistory, edited by Paul Bahn and Andrée Rosenfeld. Fifteen papers presented in Symposium G, First AURA Congress.
Order from: Oxbow Books, Park End Place, Oxford OX1 1HN, United Kingdom.
Or: Oxbow Books, P.O. Box 5605, Bloomington, IN 47407, U.S.A.

IFRAO Report No. 7

IIIrd INTERNATIONAL ROCK ART SYMPOSIUM IN BOLIVIA

MATTHIAS STRECKER (SIARB)

This symposium was held in the city of Santa Cruz, Bolivia, from 25 to 28 June 1991, organised by the Sociedad de Investigación del Arte Rupestre de Bolivia (SIARB), the Bolivian Culture Institute, the National Archaeological Institute, and the Casa de Cultura (cultural community centre) of Santa Cruz. The 100 participants came from various South American countries, from North America and from Europe. For most of the Latin American researchers attendance involved a considerable sacrifice, as for those whose costs were not subsidised, the travel expenses amounted to more than a month' salary. Yet in spite of these economic difficulties the symposium was well attended by colleagues from Argentina, Brazil, Peru, Chile and Colombia.

The copious program consisted of three sections of paper presentations, various expositions of rock art documentation, an exhibition of publications, and presentation of video films.

Section 1 of the academic proceedings, ***Management and conservation of rock art sites***, was co-ordinated by Nicholas Stanley Price of the Getty Conservation Institute, U.S.A., who gave a brief introduction stressing that measures for protection and conservation of any archaeological site must form part of a more general plan of management of the site and its surrounding area. Papers presented by Ian N. Wainwright (Canada) and Antoinette Padgett (U.S.A.) also reflected the viewpoints of North American professionals whose work practices are beyond the means of the average Latin American researcher, who has to manage without sophisticated equipment and lacks access to suitable laboratories. But both talks served their purpose of outlining the principal objectives and methods of site protection and conservation. Wainwright gave a summary of the work of the Canadian Conservation Institute since 1972, including stereophotogrammetry and digital image processing. Padgett, a graduate from the diploma course in rock art conservation held in Canberra, Australia, in 1989, reported about a follow-up course she directed in California in April/May 1991. Fellow students of the Australian course were instructed in the subjects of rock weathering, archaeology and anthropology of rock art, research planning, analytical techniques, conservation procedures and rock art recording.

Erica Bolle (Argentina) presented the rock art conservation project at Cerro Colorado, Córdoba, conducted in collaboration with A. E. Charola (ICCROM Argentina), Carlos Weber (Chile) and Mark Wypyski (Metropolitan Museum of Art, New York). Finally, Omar Claure, Leo Ticlla and Fernando Huaranca explained Bolivian projects. While Claure outlined the protection of the sculptured rock of Samaipata (Dept of Santa Cruz) by the construction of a fence and custodial supervision, Ticlla concentrated on the technical aspects of conservation of Samaipata. Huaranca presented a plan to protect rock paintings in the region of Torotoro (Dept of Potosí), by impeding access to the site through filling in holes in a vertical cliff with natural stones of local origin. This low-cost proposal does not interfere with the natural setting of the site and Huaranca, an architect with experience in ecological work, proposes to use plants to conceal the remedial work. His project shows that with ingenuity the integrity of a site can be maintained even if funds are lacking.

During the symposium a comprehensive bibliography on rock art protection and conservation was presented, compiled by N. Stanley Price, D. McCarthy, M. Strecker and L. Aramayo [a copy has been deposited in the AURA Archive].

Section 2 of the Symposium, ***New studies of Bolivian rock art***, was co-ordinated by Roy Querejazu Lewis. The eight presentations dealt with sites in different Departments (provinces). Rock art in the Dept of Santa Cruz was discussed by Matthias Strecker (*The study of the sculptured rock of Samaipata by Hermann Trimborn*, published in 1967 in Germany, was recently translated into Spanish by Strecker). Omar Claure addressed sites in the Andean zone of Santa Cruz, and Erica Pia spoke about the *Stylistic sequence of rock art in the east of Bolivia*. Carlos Kaifler's investigation, *Pre-Hispanic petroglyphs of Capinsal*, consists of careful documentation and analysis of geological characteristics, carving techniques, petroglyph depth and superimpositions, to elicit a relative chronology. Juan Carlos Jemio and Roberto Mantilla explained their documentation of rock art in the Dept of Tarija, an initial phase of a project by the Bolivian Culture Institute and SIARB. Freddy Taboada gave an update of his long-term investigation of Colonial rock art of Chirapaca, Dept of La Paz, where he recently found a location where ritual offerings are still being performed by present-day Aymara Indians. M. Strecker discussed rock art in the region of Betanzos, Dept of Potosí, where a great stylistic range exists among paintings and petroglyphs which apparently span several millennia. They begin with hunting scenes, followed by repre-

El Sol. La Aguada-Pasorapa region, Depto. de Cochabamba, Bolivia (recording by Roy Querejazu Lewis, SIARB Boletín *4).*

sentations of the ceramic-agricultural period, up to Colonial times. Finally, Querejazu Lewis talked about rock art as part of the popular Andean religion. He demonstrated that many sites in the Bolivian highlands, which in pre-Hispanic times formed part of the *wak'as* (sacred places and objects) of the Andean pantheon, maintain their ritual character. At Korini 3, a site with pre-Hispanic and Colonial rock paintings in the Dept of Oruro, he was shown a ritual offering (*misa*) in a buried wooden box, below a huge blood stain on the centre of the rockshelter, where according to informants a bull had been sacrificed.

In the extensive Section 3 (co-ordinated by Carlos J. Gradin, Argentina), 23 papers were given, dealing with new studies of rock art in other South American countries (Colombia, Peru, Brazil, Argentina and Chile). An address by Juan Schobinger (Argentina) covered North, Central and South America, providing a vision of the most ancient American art (a summary of a book Schobinger is currently writing). For lack of space, only a few of the other talks are mentioned here, which showed that rock art research is on a solid footing in Brazil, Argentina and Chile.

Luis Briones Morales (Argentina) presented the investigation of recently discovered geoglyphs and petroglyphs at Cerro Colorado, north Chile. Martha Locks presented the research by various Brazilian investigators (including Maria de Conceiçao de Beltrao, Darlan Pereira Cordeiro and Rosa Maria Mendonça de Magalhaes) in the State of Bahia where rock paintings claimed to depict the deer species *Blastoceros dichotomus* provide indications that enable us to reconstruct former environmental conditions; in the region of Lagoa Santa, Minas Gerais, deer bones were excavated which could possibly be related to rock paintings at the same sites. Alenice Motta Baeta explained the research in Minas Gerais, directed by André Prous (French Archaeological Mission) where 500 rock art sites have been investigated and two phases have been defined: the ancient 'Planalto' tradition, and the earlier 'Ballet' stylistic unity which can be associated with the 'Noreste' tradition of Piauí. This raises the important problem (as pointed out by Juan Schobinger in the discussion following the talk) of how it is possible that the Noreste tradition in Piauí can be one of the oldest rock art styles, as maintained by Niéde Guidon. Motta Baeta reasoned that it took a long time till this style arrived in Minas Gerais.

Carlos J. Gradin detailed his studies of rock art in Patagonia (over a period of 30 years!) which enabled him to define a solid chronology spanning the stylistic developments of 10 000 years. María Mercedes Podestá and María Pia Falchi investigated rock art of the formative period (with ^{14}C dates of excavated pigments dating from 6600-3000 B.C.) and of the late agricultural-ceramic period (A.D. 1000-1480) in the region of Antofagasta de la Sierra, Argentina. María Isabel Hernández Llosas summarised the findings at Humahuaca, Argentina, where rock art sites with pre-Hispanic and Colonial indigenous motifs were inhabited in Colonial times.

A publication containing the symposium program, list of participants and Spanish summaries of talks was distributed (edited by M. Strecker). Some of the papers presented will be published in the forthcoming issues of *SIARB Boletín* and *SIARB Contribuciones*.

After concluding the academic program, two groups guided by Querejazu Lewis and Claure visited rock art sites in the Andean zone of the Dept of Santa Cruz: El Buey, Toro Muerto, Peña Escrita, Almacigal and Samaipata.

SIARB plans to hold its IVth International Rock Art Symposium in 1997 in the historic city of Sucre, Bolivia, and has applied to IFRAO to host the 1997 IFRAO meeting during this symposium. In the meantime, two meetings of rock art specialists are likely to be organised in other South American countries: in 1993 in Salta, Argentina, and in 1995 in Arica, Chile. During the recent symposium, Gradin proposed a closer collaboration among South American colleagues with the setting up of a federation of several institutions and research centres in different countries. María Mercedes Podestá (SIARB representative for Argentina) was nominated as co-ordinator of this project and will also assist in preparing the meeting planned to take place in Salta in 1993.

RAR 8-226

INTERNATIONAL ROCK ART CONGRESS IN U.S.A. - 1994

A major international rock art congress is to be held in the U.S.A. in 1994, under the auspices of ARARA (American Rock Art Research Association), with the support and co-operation of numerous organisations in the United States and abroad, including ACASPP, RAAC and rock art groups in Mexico. Sixty-four ARARA members have volunteered to serve on the 1994 Conference Committee. The Steering Committee for this event has been established and the following sub-committees have been formed, with the Chair names included where established:

Mary Gorden, Conference Chair
Sue Ann Sinay, General Organisation
Steven Stoney, Site Management
Diane Hamann, Program
Kay Sanger, Publications
Joyce Alpert, Tours
Donna Gillette, Registration
A. J. Bock, Finance

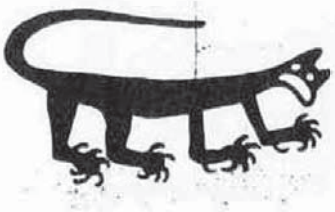
Other sub-committees cover Publicity, Grants and Sponsors, Transportation, Special Events and Leisure Activities, Exhibits and Vendors.

Several sites are under consideration for this important international event, and eleven U.S. cities have been invited to present bids. There are many cities in the United States which offer the appropriate facilities, with access to large concentrations of rock art and other attractions of interest to the international community. The presentation of scientific papers will be the focus of the congress, however. These papers will cover many aspects of rock art research with several symposia running concurrently. ARARA will publish all papers presented which meet a specific pre-congress publication schedule.

The official language will be English, however, translators will be provided. ARARA and supporters will sponsor individuals from economically disadvantaged countries to ensure their attendance. These individuals must demonstrate that all other means available to them for funding have been exhausted. ARARA plans to encourage native peoples to become involved in all aspects of this congress - especially in field trips to rock art sites.

A bid to host the 1994 IFRAO Meeting at this congress has been presented to the IFRAO Convener, and to the present IFRAO Chairperson, Shirley-Ann Pager, during the recent IFRAO Meeting in Natal. This 6-page document provides extensive details about the proposed organisation of this major event, detailing the roles of the various sub-committees and providing a complete list of all Steering Committee members and their contact addresses. Shirley-Ann Pager has called for a postal ballot on the issue, noting that IFRAO members attending the Natal meeting have decided that the American event would be the obvious choice for the 1994 IFRAO Meeting. IFRAO members have now voted to accept ARARA's bid. Consequently the 1994 IFRAO Meeting will be held in the United States, together with the international congress now being planned.

All IFRAO members are urged to publicise and promote this event by whatever means available, and to ensure attendance of their representatives in 1994.

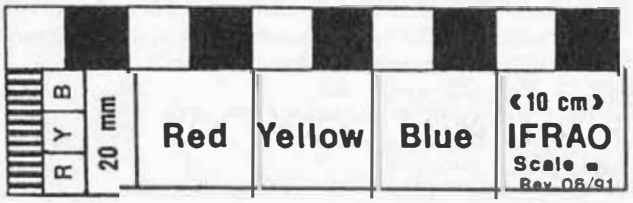


Petrified Forest petroglyph, Arizona.

Notices

The proposal of an IFRAO Standard Scale, by R. G. Bednarik in *IFRAO Report No. 6*, has attracted several supportive comments, but only one detailed response. Paul A. Peterson, of Petaluma, California, provided the following comments:

1. The same three-colour scale could be added parallel to the smaller, millimetre bars so that it would show up in close-up shots (see figure).



2. The printing date could be significant since all dyes, including inks, fade with time. The printing/revision date could be included in one corner of the scale.
3. Plastic scales last longer and are washable, however, they tend to reflect light and would probably be more expensive to produce. If printed on paper, the stock should be thick enough to resist curling, bending, getting blown away etc.
4. Ideally, a scale should have a self-adhesive backing with glue strong enough to stick to rock surfaces and not wear out after repeated uses, along with a convenient carrying case to protect it when not in use.

Response by R. G. Bednarik

It is so self-evident that Points 1 and 2 are improvements of the concept that no further comment is required, and they should be incorporated. Concerning Point 3, the surface must be matt and least-reflective. Finally, the fixing of the scale is an old problem, where researchers must use their good judgement. Two small pads of double-sided adhesive tape, which is commercially available, can be placed on the back of the scale, which can then be used on stable rock. Friable rock, especially weathered sandstone, is not suitable for this, because grains from the rock surface are likely to remain on the adhesive. On very soft or porous rock, particularly in limestone caves, I use two thin steel pins, and this also works on very stable surfaces with fine fissures. In all other cases, or wherever there is any danger of affecting the surface near a motif, the scale will need to be hand held. Under no circumstances should scales be attached over, or very close to, a rock art motif, by whatever means, as they could then create a conservation hazard or a research problem.

*

ETCHED IN TIME -

An exhibition on the petroglyphs of Val Camonica, Italy, conducted by the Società Cooperativa Archeologica Le Orme dell'Uomo.

This exhibition is currently being held in a number of cities. It consists of illustrative panels and casts. There are 35 panels measuring 90 by 80 centimetres that require a display space of roughly 70 metres along walls or partitions. The first panels deal with the environment, research history, engraving techniques and research methods.

The panels following the map of Val Camonica lead the visitor through a cultural and chronological sequence, beginning with the images made by epi-Palaeolithic hunter-gatherers, followed by imagery of Neolithic proto-cultivators and Copper, Bronze and Iron Age metal users, and concluding with Roman and Mediaeval figures.

Casts show the true size and nature of the petroglyphs and portray hunting scenes, symbols, zoomorphs and anthropomorphs.

Two publications by the Società Cooperativa Archeologica Le Orme dell'Uomo serve as appropriate reading material: *Etched in time*, and *Rupestrian archaeology, techniques and terminology - A methodological approach: petroglyphs.*

Ludwig Jaffe

Petroglyphs on Naquane Rock, Val Camonica, Italy (recording by M. de Abreu, A. Fossati and L. Jaffe).

NOTES FOR CONTRIBUTORS

Manuscripts of major research papers should preferably be from 4000 to 8000 words. Longer articles will be considered on the basis of merit. Submissions should comprise the original together with one copy, typed in double-space, with a wide margin on one side of each page. Underline words to be italicised and identify each page by number and author's surname. The preferred method of submission is on a 5.25 inch double-sided, double density (DS-DD) diskette written in *MS Word*, together with a hard copy. The content of the paper should be outlined by three to five keywords (e.g. 'Petroglyphs - patination - ethnography - Pilbara') placed above the title. The manuscript must include an abstract of 50 to 100 words, summarising the article.

Spelling and punctuation in this journal follow the *Style manual for authors, editors and printers of Australian government publications* and the *Macquarie dictionary*; where the two disagree the former has precedence. Footnotes should not be used. The bibliography and references in the text should follow the style indicated in this issue.

If line drawings are included they must be larger than the intended published size (preferably by a factor of 1.5 to 2) and line thicknesses, stippling, lettering sizes etc. must be selected accordingly. Photographs should be black and white gloss prints of high contrast. Photographs of rock art which were obtained by physical enhancement or other interference will be categorically rejected. In regions where traditional indigenous rock art custodians exist, their approval must be obtained before submission of any illustrations of rock art, and where copyright applies the author must obtain the appropriate consent. Captions (on a separate sheet) are required for all illustrative material, together with an indication in the text as to where they, and any tables and schedules, are to be placed.

Announcements intended for a specific issue of this journal ought to be available at least two months before the month of intended publication. Galley proofs are issued of all articles and must be returned promptly after correction by the author(s). Each author or group of authors receive thirty free copies of their article, additional reprints are available at cost.

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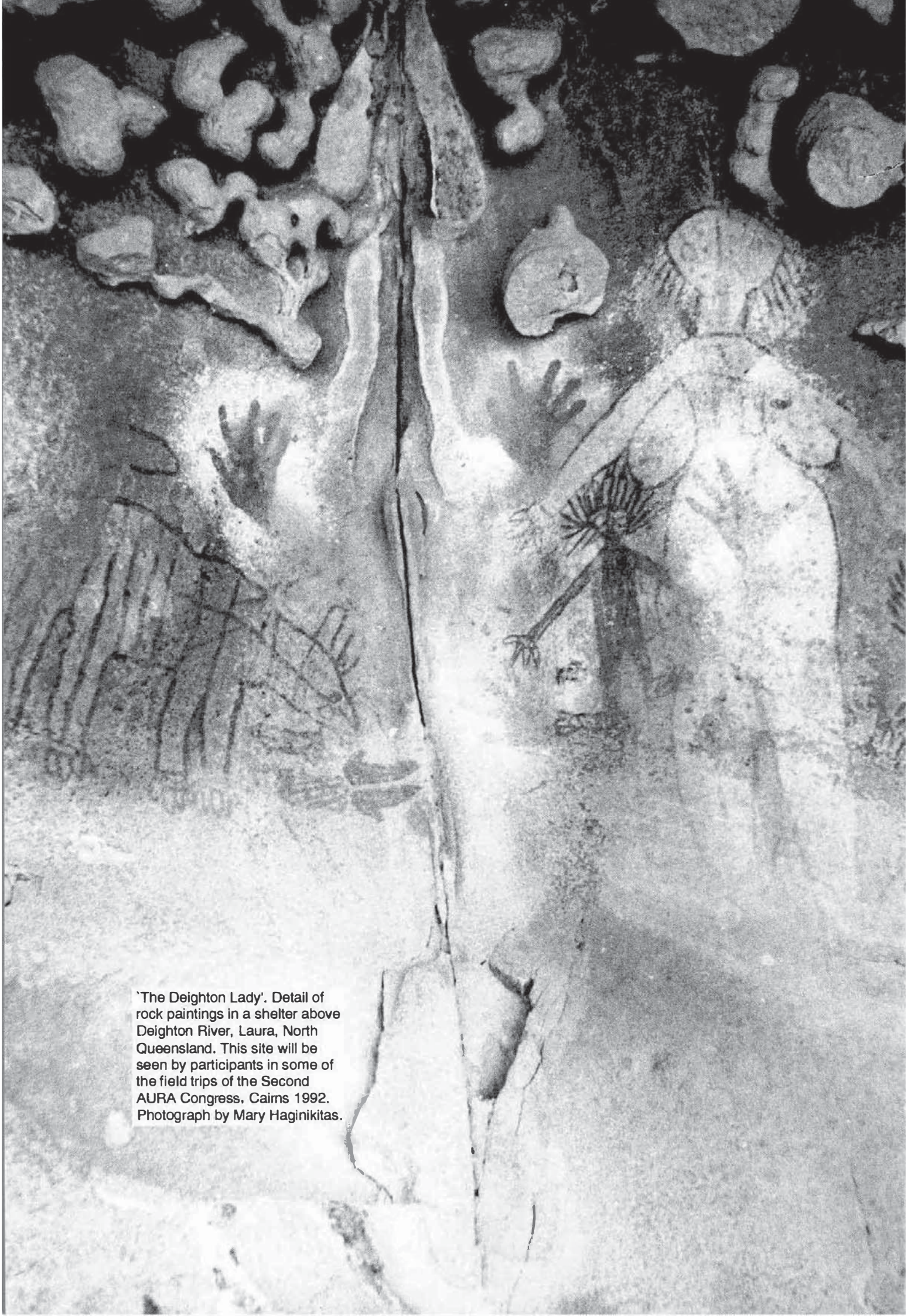

'The Deighton Lady'. Detail of rock paintings in a shelter above Deighton River, Laura, North Queensland. This site will be seen by participants in some of the field trips of the Second AURA Congress, Cairns 1992. Photograph by Mary Haginikitas.