RAR DEBATE

Comment on
THE EMERGENCE OF THE REPRESENTATION OF ANIMALS IN PALAEOART
by Derek Hodgson and Patricia A. Helvenston, *RAR* 23: 3–40.

Against deceit

By LIVIO DOBREZ

Yet another reaction to the stimulating discussion (*RAR* 20: 3–22; 23: 3–40) initiated by Hodgson, then Hodgson and Helvenston, this time a reaction introducing views which have not been canvassed so far. It should be taken in a context of sympathy with H&H's evolutionary approach, with their appeal to ethology and neuroscience and their proposed longer time scale for representational and associated developments. My focus, however, is the deceit model as a key to animal, including human, behaviour, particularly as regards perception and image-making. Of course we do practise deceit and in ways integral to social behaviour. Of course deceit features in other animals, so much so we may allow ourselves a rhetorical flourish in saying it appears to be the norm rather than the exception. But there are difficulties in giving deceit logical priority. Knowledge defined negatively as the elimination of error (an extreme broadening of Popper's falsification logic) results in a highly cumbersome model of cognition. Naturally insofar as this may be a matter of empirics we await the findings of neuroscience. In the meantime it is worth noting that there is evidence for a positive, i.e. truth-first model of behaviour in animals. Following Goodall, H&H refer to the example of Figan, the chimp who cheats the group and in so doing evinces signs of something like guilt. Colleagues at my university (published in Dobrez et al. 2004) have observed that choughs on occasion ostentatiously pretend to feed the group's chicks, only to swallow the food themselves when they are sure the others are not looking. If caught in the act, these birds will be subjected to aggressive displays of group disapproval and be required to make clear signs of submissive remorse. It was Aquinas, in the context of a consideration of lying, who said that society functions through relations of truthfulness, i.e. mutual reliance. Studies of animal society supply this with a biological basis. For example, my colleagues have argued for inbuilt limits to deceit. The blue of the wren confers dominance and attracts females. But there is a limit to blue, as blue is tied to immune-suppressing testosterone. Thus excess of show reduces performance and the show-cheat is eliminated from the race.

On the matter of deceit in relation to representation: though no longer a major influence on art history pedagogy, Gombrich remains a significant art theorist and *Art and illusion* remains a significant text. H&H acknowledge a debt in this respect. I suspect others in the field on whom they rely also owe something to Gombrich. There are, however, serious problems with Gombrich's theories — and I am not simply referring to his mechanism of 'projection', of whose limitations H&H are aware. Gombrich sees art as based not on nature but on other art. This view has its place to the extent that it rightly stresses the importance of mental schemata, which generate visual art conventions. Contra Ruskin, there is no such thing as an 'innocent' eye. Taken too far, though, this constitutes a worryingly subjectivist position, not to mention a logical regression (art copies art copies art). In fact we interact with our world, not least in processes of representation. Gombrich allows for this in an unsatisfactory post-factum way by appealing to what he calls 'making' and 'matching'. You make rather than copy — then match with reality. This has a certain reasonableness if we do not ask too many questions about how the whole business started and precisely what it is that keeps it going. The other problem is that Gombrich's implicit definition of 'matching' is entirely geared to a particular European tradition of mimesis for which, unsurprisingly, Gombrich is a passionate apologist. It is the tradition illustrated in a repertoire of anecdotes which, as an Italian, I have known since childhood, such as the tale of Giotto and the fly (taken from Vasari). Working in Cimabue's studio, the young Giotto mischievously paints a fly on the nose of one of his master's figures — which Cimabue tries to brush off. Here mimesis means creating an illusion of reality so good it will be taken as real. Gombrich thinks of it as the progressive discovery of representational modes such as foreshortening, a value-laden project which (in Berenson's terminology) takes us from representing what we 'know' (read 'conceptually') to representing

what we 'see' (read 'perceptually'). It is a liberating historical trajectory, the great tradition in world art. *Art and illusion* promotes the idea and it structures Gombrich's *The story of art,* whose chapter titles tell it all: Franco-Cantabria, Africa and Pre-Columbian America constitute 'strange beginnings'; Asia gets one short chapter; Greece is 'the great awakening'; the Italian Renaissance is 'the conquest of reality'. All this up to and including nineteenth-century realism/naturalism and impressionism — after which things start to unravel and the great tradition collapses, thanks to cubism, expressionism and the rest. The tradition was one of making schemata/illusions to match not what we know about the real but what we see of it.

But we need to be clear that Gombrich's matching is of this culture-specific kind and not to be confused with iconic likeness (as defined by Peirce). The Gombrich model is characteristically modern-subjectivist: what the eye sees of the object from a given position *X* — from which I might 'see' a profile body with one leg when I 'know' it to possess two. If what the eye sees from *X* were a criterion of iconicity then all representations of the object would look modern-European-realist, which they do not. The fact is that iconicity or iconic likeness (non-culture-specific in the sense that it is recognisable by a chimp) is not the same as Gombrich's mimetic matching. Rather it is a correlate of the object, processed or 'known' over time, which is why likeness can take varied culture-specific forms and why a horse by Dürer is not more iconic, i.e. more a likeness, than one by Picasso. Gombrich thinks that his Greeks-to-Giotto mimetic tradition is more accurate, i.e. more geared to seeing, than iconic representations by other traditions. But more accurate according to what norm of seeing? That of seeing from position *X*.

The contradictions are obvious, since the eye at *X* is now proposed as an 'innocent' eye, not because it is untutored, but because its seeing is superior to knowing. One-leg profile marks a representational advance. But why give absolute, ahistorical value to foreshortening? Why prioritise seeing when you started out by prioritising making, i.e. the schema? Critically for the present discussion, Gombrich's theory is utterly culture-bound, Eurocentric, biased in favour of historically recent art developments. Not too recent, however, since Gombrich draws the line at that early twentieth-century 'return to the primitive' which he reads as a crisis of representation. But it was the mimetic project which was historically eccentric. The moderns simply returned to the non-mimetic-illusionist representational norm. Thanks to which we read e.g. world rock art as neither 'strange' nor a 'beginning'.

From all of which I conclude that the Gombrich model of art as illusion brings with it problematical baggage. Quite simply: why think of representation as a form of deceit? Is *trompe l'oeil* to be its characteristic manifestation — instead of a curiosity exploited by Italian mannerists? Historically and culture-diversely, most makers of images have not thought they were creating illusions; rather they thought they were making something real, however variously they defined reality. Moreover we do not think of our mental representations as illusions, unless it is in the context of drug experience or under the influence of (untenable) philosophical scepticism. At the same time we do acknowledge phenomena of ambiguity. If our mental schemata were fundamentally illusory, i.e. a deceptive version of the real in which one thing is really another, the evolution reality-principle would make short work of us. So why overemphasise the role of mimetic illusion in representation? For Gombrich the answer is plain: because he values a particular kind of art. But we need not follow Gombrich in this, and in the context of rock art we would be well advised not to. An addendum: Gombrich's argument is premised on the empiricist learning model. This cannot sit comfortably with H&H's model of evolutionary hard-wiring.

The 'representation as deceit' model in H&H's argument links hunting, meat-consumption and increased encephalisation. There is probably something in this. Then there is the hunting/disguise/mime/representation nexus. With help from Donald's Episodic, Mimetic and Mythic phases and an extension of his chronology, H&H unfold a narrative which connects hunting with the representation of animals. Without repeating previous *RAR* commentary, I want to note that H&H confuse iconicity with mimetic realism or naturalism. In fact their argument has no need of the latter: it suffices for their purpose that animal likeness of *any* sort be involved. The deceit model in general and its Gombrich version in particular are likewise inessential. I favour a different model, one which would modify but not negate that proposed by H&H. This other model would help answer the question still left open by H&H's model, viz. why should hunting disguise, even with the Donald intermediary of campfire mime, prompt representation? As it stands, this is an unexplained leap. We could begin not, as H&H do, with a sudden introduction of the deceit principle (geared to lead to representation via Gombrich), but with a view of representation as truth-oriented, i.e. as the making not of an illusion of the real but, neutrally and retaining one of Gombrich's terms, an 'equivalent' of reality. This at once clarifies the relation between representation and language. Like language (and regardless of whether it preceded or postdated it), representation reads one thing for another. It is the logic of metaphor and, in extended form, narrative. Since no symbol — or image — can exhaust the real, its mechanism is synecdochic, part-for-whole. Ambiguity has a role to play in this model, but the key is thinking by correspondence. Thus for example, and taking our cue from Deregowski's stress on animal contour, we may explain recognition of animals in terms of

synecdoche rather than disambiguation.

One advantage of the equivalence model as here defined is that it helps us to imagine how developmental changes might have occurred. Following Donald we can see the prototype in pre-linguistic representation and the inner logic of an eventual shift to representation. By 'inner' I mean nothing mysterious, quite the opposite. If, for whatever unrelated evolutionary reasons, the human mind has increased reflexive capacities, it will — other things being equal — articulate these, that is, externalise, i.e. represent, perhaps initially in the form of theatrical mime, then as language and visual representation. This would be in response to a social imperative. The equivalence model is a model of communication, social relations, and relations understood as truth-based. In this model the leap from mime to representation evident in H&H seems less extreme. It follows, however, that representation is over-determined. It is unlikely to have any single source such as hunting disguise, though the hunt/disguise nexus would still be involved. In which connection I suggest that the neuromodel may serve to provide a substrate for any human communicative behaviour: what makes deceit possible makes much else possible. Were H&H to reduce their reliance on the single factor (deceit as fundamental premise, hunt as empirical trigger) their argument about our feelings towards animals and the animal representation thesis could be put forward less exclusively, so avoiding the criticism that there is no conclusive evidence for animal representation as preceding other forms of iconicity — and that even if there were, taphonomic considerations would nullify it.

Equally significantly, the equivalence/communication model provides an alternative to external (in the sense of accidental or mechanistic) explanations for representation, an alternative which does more justice to mythic correspondence thinking — or, putting it another way, which is less damagingly modern culture-specific. You represent because you have the brain to do it and the social stimulus to articulate. Thus when representation happens it does so because it already makes sense, not because of a fortuitous event such as a mis-recognition. You do not 'find' (another Gombrich term) representation 'out there'.[1] It is a mental construct and no amount of outside prompting will generate it because it is the idea of representation which structures our reading of things 'out there', not the reverse. External stimulus misses the point that if you can grasp the principle, you have already been there, it is an argument that is always 'too late'. This is one difficulty of Bednarik's 'root mistaken for snake' explanation, taken (indirectly or directly) from Gombrich who takes it from Alberti (Gombrich 1972: 89–90), and it seems symptomatic not of possible proto-human logic but of the alienated modern logic which stands outside its situation, 'seeing' (the epistemological subject-object model) rather than 'knowing', i.e. being-in-the-world, as Heidegger would put it. The same problem exists with the hunt/disguise argument when it postulates disguise as prompting identity-confusion. Thus therianthropy becomes a failure to grasp things as they really are and religious ritual an extended failure to disambiguate. It is still there, if you follow this logic, in the contemporary ritual of eating the body and drinking the blood of a 2000-year-old deceased. *Verum corpus*: the real thing. Of course Kant had a valid point to make about superstition in *Was ist Aufklärung?* — valid in its day and ours. But not when applied ahistorically to cultures very unlike ours, a category which must include our proto-ancestors. Whatever else they may be, therianthropy in particular and mythic religion in general are not the product of simple error, mistaking one thing for another. Moderns still connected with mythic logic (e.g. in Australia) think they are of a piece with animals, and so did Darwin, and so do I. It is not a question of 'realising' one has made a 'mistake' (H&H's terminology) but of knowing by correspondence: I experience a storm and link it to my feelings, not as a projection of my feelings onto a storm with which they are in reality unconnected, but as a way of thinking relations — which as it happens are real and not imagined. We moderns cannot ask the ancients to get real because, in their situation, they always were: it is we who (rightly) worry about reality and our relation to it. In any case metaphor and stories still constitute our cognitive mode. It is useless to label it 'primitive', as Gombrich does, and post-Enlightenment secularism is naïve in its efforts to demythologise it. It is also violent, seeking to replace 'their' myth with 'ours'. Someone will object that our science works and their myths do not. But here I would follow Lévi-Strauss: *la pensée sauvage* is just as logical as ours, but directed to solving quite different, indeed for us often unimaginably different, problems. And in this connection I think Donald gets it wrong: 'theoretic' logic is no recent invention. All stories, myths, hypotheses last because they work, though cultures interpret ways of working very differently, not least in that some of these ways are not reducible to modern functionalism.

The problem with the root/snake idea and its development in H&H's account may be put another way, in terms of the definition of representation. The ambiguity in question involves the subject's confusion on the basis of a recognition of iconic resemblance/difference (root or snake?). But it is important to clarify the relation of representation both to such resemblance and to ambiguity. While representation has some connection with the recognition of iconic

1 Here I must express disagreement with the central point of Feliks' finely-detailed argument for the importance of fossils in pre-History. I accept that humans have long had an aesthetic and intellectual interest in fossils but not that they were influenced to read these as representations unless they already possessed the concept of representation.

resemblance, its structure is different in that it involves a substitution of 'stand-in' for 'real'. This is what I mean by 'equivalence'. That such substitution is not dependent on iconic likeness is evident from the fact that it may operate, in Peirce's terminology, iconically *or* symbolically. Unlike the recognition of resemblance, which simply perceives X (though in connection with Y), substitution is a function of evolved reflexivity and of a piece with analogic thought and with metaphoric logic (Gr. *metapherein*: 'to transfer'). All this takes discussion of representation back to linguistic communication and removes it from the assumptions of the deceit model. Unlike ambiguity, representation has nothing to do with perceptual — or conceptual — confusion.

What the present argument hopes to bring to the discussion is an examination of certain fundamental premises, and if some of its own assumptions about representation and the equivalence model as applied to evolution are speculative, they are not more speculative than the deceit model applied to evolution, but probably better suit the logic of representation and therefore its genesis and the nature of the mind that thought it. The H&H thesis is not ruled out, with the proviso that, even as they practise disguise, hunters *communicate*, that is to say represent, first mimetically, then linguistically, and at some stage via visual marks. These last may well begin as minimal modifications of existing objects, in line with the argument for the Berekhat Ram and Tan-Tan pieces — but not via the external logic envisaged by Alberti. The very postulate of a deep-time scale calls for a model of the ancient mind as intelligent, the point of the exercise being our belated realisation that our remote ancestors were more like us than we have imagined. If neuroscience disproves my suggestion that perception, while involving resolution of ambiguities, is not primarily a process of disambiguation or negative recognition, it will not, by the same token, support theses identified above as modern culture-specific and therefore suspect.

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REPLY

The evolution of animal representation: response to Dobrez

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We welcome Livio Dobrez' comments and appreciate his sympathy with our model. Indeed, his remarks give us an opportunity to clarify some critical misunderstandings that may be shared by others in regard to our paper, 'The emergence of the representation of animals in palaeoart: insights from evolution and the cognitive, limbic and visual systems of the brain' (Hodgson and Helvenston 2006), published in this journal. Helvenston will begin our discourse with a rather broad overview of the interlocking hypotheses we presented (this will not include the extensive sources we cited in our paper, thus we refer the reader to that paper). Hodgson will then continue with an in-depth response to more specific details raised by Dobrez.

The following is but a condensed summary of our interlocking hypotheses and in no way includes the myriad of details presented in our original paper. In that paper we noted in the 'Introduction' that we were proposing *one* scenario in the evolution of the production of the earliest visual depictions — that did not imply that we thought it was the only one. We first dealt with hominins' hunting prowess, specifically the ability to use some form of disguise to approach and kill prey or to avoid predators. The deep-time evolution of this skill was supported by evidence from the evolution of primate behaviour, comparative ethology and comparative primate neuroanatomy as well as the archaeological record. Over the course of millions of years some primates became effective hunters and given data from observations of chimpanzees in the wild we suggested that early hominins would have already possessed hunting skills which continued to evolve in effectiveness after the early human-early chimpanzee split occurring some 5–7 million years ago (according to most palaeontologists, evolutionary biologists and archaeologists today).

Additionally, primates were both predator and prey and they would have evolved effective mechanisms attempting to fool both their own prey animals and the predators who stalked them. Given the crucial nature of such skills, primates also developed neural mechanisms to categorise animals — perhaps into prey, predator, neutral animals, dangerous animals, inanimate objects, conspecifics, strangers etc. We cited the neurobiological evidence attesting to the inbuilt nature of the process of categorisation, which exists in many species other than primates, but was almost certainly highly developed prior to the human-

chimpanzee split. Effective hunting and gathering behaviours were crucial for survival and are mediated by ever-evolving neural substrates. As is the case with reproductive behaviour and courting rituals, hunting behaviour is accompanied by powerful experienced emotions mediated by the evolving limbic system, which has increased in size and complexity during primate evolution along with the development of effective systems of visual perception and fine motor skills, communication, and other related systems.

We certainly agree with Dobrez that 'truth' behaviours (concerned with objective reality) are probably more dominant than 'deceit' behaviours, but most animals studied, especially primates, possess the neural flexibility to express either, according to the situational requirements. In this regard, we would not characterise our hypotheses as a 'deceit model' and it is a misunderstanding of our work to suggest it is the single factor leading to artistic representation. Dobrez places far too much emphasis on the one example of deceitful behaviour we cite, i.e. hunting disguises. Deceit is simply one behaviour out of many effective behavioural strategies tested by evolution for survival value in specific situations. In one specific case of deceitful behaviour, we see the evolution of a hunting strategy (disguise), whose consequence was a more successful hunter. For example, it seemed to us that early hominins could have become more effective hunters by employing hunting disguises to fool their prey, just as hunting-gathering cultures have done for thousands of years of recorded history. Indeed, animal skins, perhaps daubed with the prey's dung, constitute not only a visual disguise but also an olfactory one and are likely to have been accompanied by calls imitating the prey. There are some direct consequences to using a disguise which we believe eventually became exapted into ritual behaviours, religious beliefs, and artistic representations. For example, with a disguise the hunter can approach very close to large prey or predator and the emotional response of the hunter to such proximity would be intense, varied and mediated by an ever-evolving limbic system which enabled the differentiation of a multiple number of emotional states as well as communicative abilities to advise conspecifics of these affective experiences. We cited extensive evidence supporting the evolution of these capacities.

We are *not* suggesting that hunting disguises directly prompt representation, rather, they form a basis for its development because hunting itself, as well as its re-creation during mimetic and mythic culture, is accompanied by powerful emotional reactions and invested with great meaning as demonstrated by hunting and gathering cultures throughout the written record — we see no evidence for assuming that earlier hominins did not also have similar emotional reactions, especially based upon what we know of comparative primate neuroanatomy and behaviour. We clearly agree with Dobrez that visual artistic representation is over determined. We are simply suggesting one series of interlocking hypotheses that we think is highly significant in the evolution of such depictions because hunting and all the activities associated with it likely constituted one of the most important and meaningful aspects of daily lived experience of small bands of hominins.

Given this highly developed suite of behaviours which requires and is made possible by increased corticalisation, and also mediated by increased development of the visual system, sensori-motor system, limbic system, communicative systems, all representing millions of years of evolution, early hominins had the capacity to expand their teaching, learning and communicative abilities through mimesis. Mimesis is a highly developed skill of primates such as the chimpanzee and there is every reason to suppose early hominins were already adept at imitation. Thus, as important as hunting would have been it is safe to assume that hunters would attempt to communicate the details of a successful or perhaps disastrous hunt through imitating the behaviour of their quarry and their own hunting skills.[2] In most hunting and gathering societies meat is considered a special treat and observations of chimpanzees confirm its importance in chimpanzee society, in spite of the fact that gathered food may constitute a larger percentage of the diet.

Likely imitations of the hunt, including animal skins and masks, and imitations of animal calls and behaviour, began to involve scratching the hunter's footprint and that of prey in sand, later in more permanent mediums such as cave walls and/or rocky outcroppings. We know that chimpanzees will spend hours scrawling with pencils or crayons on paper, although their 'drawings' do not *appear* to represent any specific objects. Moreover, re-presentations of the hunt, accompanied by assorted regalia, could easily become ritualised, and involve dancing or imitation of animal behaviour by the entire group, probably in the evening around a fire. We know that frenzied behaviour of this type is often accompanied by a form of hypnotic trance during which the imagination of all is enhanced, social cohesion is achieved and important details of lived experience are communicated and saved in a type of retrieval system which requires enhanced working memory, as well as an increasing development of long-term memory. The form of trance engendered by dancing and/or animal imitation is not characterised by hallucinations, although illusions are frequent because of the heightened state of imagination and the enhanced emotional reactions common to assorted forms of hypnotic trance (Lex 1979). Experiences accompanied by such strong

2 Since gathering was a crucial aspect of survival, perhaps ceremonies involving plants were re-presented also, although few cultures presented carvings or drawings of plants. Some evidence for this is found in Australia about 10 000 BP (Helvenston and Hodgson 2006).

emotions are enhanced as compared to experiences accompanied by more neutral emotions; thus both the original hunt and its re-creation will be remembered clearly (Anderson et al. 2006).

Given the enhanced imagination and emotions accompanying ritual behaviour that recapitulates the hunt discussed above, and given the extreme importance of animals to early hominins, we believe that a behavioural and a subjective experiential substrate was then in place to serve as a foundation for the creation of mythic culture which perhaps included animal worship and stories of great, deified, ancestor achievements. Given the paucity of European Upper Palaeolithic images of humans or therianthropes, it *seems likely* that animal worship may have been the original spiritual development, although the many 'Venus' figures suggest females, especially pregnant females, might have had divine status also, perhaps at a later time period as early myths became more complex. There are certainly a number of paradigms where great goddesses are associated with the God hunter in his animal or human form from recorded history. That this worship may have been associated with big game hunting is evidenced by the fact that certain groups of African ancestors were routinely hunting the hippopotamus from 600 000 BP. We cited other examples of big game hunting in other areas of the world in our paper. The emotions associated with getting close to huge animals, killing them or being killed and wounded, were likely communicated via mimesis during re-presentations of the hunt. The emotions of fear, admiration, and awe were likely experienced during close encounters with this huge prey and are certainly emotions typically experienced during the worship of assorted Gods as described in written records of many worshippers over thousands of years of recorded history (James 1902).

We have numerous examples from around the world of the belief in the divinity of certain admired animals or human/animal hybrids (Klingender 1971). It seems to us that these hybrids arose first from purely hunting activities where the human wore an animal disguise. Later, probably during the repetition of the hunt with enhanced imagination and emotion, these hunted animals were endowed with supernatural powers and became the first divinities. Perhaps certain great hunters were ascribed with both human and animal powers, were viewed as brothers of the prey and were later depicted as therianthropes. In this connection, it is important to note that the hunter, wearing an animal head, skin, or other disguise, could appear to be a therianthrope during the ritual repetition of the hunt — he could be seen as both animal and human by his companions because the hypnotic trance states associated with dance are characterised by a high level of imaginative flexibility leading to the perception of illusions and/or pseudohallucinations, and, while they are linked with reality, they are not dominated by it (see Hodgson 2006 for a detailed discussion of visual perceptual and processing mechanisms).

For example, the myths of the South African Bushmen show before the historical time period there was a mythic world where animals and humans were believed to be one in the same. Later, they were distinguished as two different creatures (Solomon 1997). But, during their dances the Bushmen often imagine themselves to be animals. We are not suggesting that disguise automatically leads to identity confusion for of course the hunter needs to have a highly realistic understanding of his environment and prey. But during the ritualised representations of the hunt, a rather specific condition, imagination has free play and it is likely during this activity that the participants experienced emotions and illusions, imaginings, pseudohallucinations, and ideas that began to form a basis for myth and belief in divine beings. Also, it seems likely that participants attached considerable meaning to these experiences during reflective periods that followed. Whether the hunter ever completely lost his own identity during the ritual is questionable so it is inaccurate to refer to 'identity confusion'. In reports from Bushmen of South Africa, during trance the hunter actually believes he is an animal but whether or not he is aware of his own identity *at the same time* is unclear because it does not appear that question has been asked (Katz 1982). However, from a psychological perspective, in order to 'know' you have become an animal presupposes you 'know' you were formerly something else — in other words the human self is still an observer of the now-animal self. Many psychotherapists refer to this aspect of the self as 'the observing ego'.

From 4th millennium B.C.E. we have a Sumerian myth (Sanders 1960) celebrating Gilgamesh, mighty hunter before the lord — and his therianthropic companion Enkidu, meaning creation of the God of the water and fertile earth. References to the fertile earth raise questions about the presence of 'mother goddesses' and there are female goddesses, including a mother goddess, in the Gilgamesh epic. This story includes both a God and a therianthropic beast/man (Gilgamesh who appears to represent the deified form of an actual 'larger than life' historic figure and Enkidu who is animal/becoming human and beloved companion of Gilgamesh). 'The Epic of Gilgamesh' includes spiritual beings, Gods in the form of men and women, Gods in the form of animals and Gods in the form of therianthropic hybrids.

Our written record of this story probably reflects a long process whereby some form of this myth was transmitted orally for untold thousands of years. While mythic culture may have included similar myths, they were originally not as highly developed but it seems certain that a type of myth that was a prototype of Gilgamesh-like stories existed during the evolution of Donald's mythic culture, especially given the extreme importance of the paradigm of the hunt which includes a great hunter and his brother/animal

companion and/or prey. Thus, at the beginning of mythic culture the neural and behavioural substrate was now in place to form a basis for the development of myths, spiritual beliefs and 'religion' from ritual practices that had *originally* evolved to communicate specific hunts to other individuals in the social group. Later, as the abstracted or 'iconic' hunt became more ritualised it is likely 'iconic' hunters and 'iconic' prey, as well as 'iconic' animal/human mothers, evolved into divine beings. We believe that this evolutionary progression of behaviours is a good example of exaptation, i.e. the substitution of one evolutionarily adaptive behaviour by a more highly evolved, similar behaviour that has a secondary purpose: i.e. from the hunt to re-presentation of the hunt, to religious ritual and mythic beliefs about animals, therianthropes and humans to re-presentation of these beliefs in preserved 'artistic' drawings and objects.

Accompanying the evolution of these behavioural and belief systems as elaborated above, were attempts at the re-presentation of events through a visual-motor expression of important objects — i.e. 'prehistoric art'. We discussed our idea that some of the earliest re-presentations of animals and the hunt were likely depictions of human and animal tracks, scratched into the sand and subsequently engraved or painted onto rock surfaces. As re-presentations of the hunt became rituals repeating the hunt, and subsequently rituals preceding the hunt as a form of 'hunting magic', oral stories emerged to accompany imitations of the hunt and artistic depictions became more sophisticated. Hunting magic refers to rituals designed to insure an abundance of prey, and it is perhaps at this point that pregnant female animals and humans began to take on new significance in myth and ritual. From the foot prints of animals and humans representing actors in the hunt, abstract images of animals and humans may have evolved — something like stick figures. From stick figures more elaborate representations of the animals hunted and/ or admired/ and/or considered dangerous predators involved increasingly realistic depictions of animals — likely in sand originally, or perhaps in body paint, masks, hides etc., but eventually on preservable media like rock. We view the development of artistic representation of animals, therianthropes, hunters and the hunt to have been a very long evolutionary path and earlier depicted in media and objects that are scantily, if at all, represented in the archaeological record. For example, body paintings, masks, animal disguises, scratches in the sand, are seldom preserved from those distant times, but at the stage of artistic depictions of animals in media like rock, we see the accumulation and material consequence of millions of years of evolutionary development that we may never find in the material record, the early and intermediate stages of which we can only know by logical inference from a few scattered artefacts, subsequent data, and supposition.

We want to emphasise that the process elaborated above is not strictly cause and effect — 'Deceit did not trigger representation' in a linear fashion. Rather certain adaptive developments which included a highly specific hunting strategy involving deceit, represented a necessary and sufficient condition for further later developments that included a mimetic re-presentation of the hunt, that again became a plateau from which ritual behaviour, animal worship and supernatural belief systems became possible which formed a substrate upon which increasingly sophisticated artistic representations became possible, and so on. Each plateau represents a different level of neural and behavioural adaptation, which is necessary for each subsequent higher, re-worked variation. We hope that the reader will understand we are postulating complex neural and behavioural mechanisms, which have to be in place before the next complex mechanism can develop. Thus we can speak of each behavioural plateau of capabilities which are related to one another in a hierarchical fashion only in the sense that each level of development presupposes a plateau of prior developments, and it represents a base from which further evolutions may occur, but these connections do not represent a linear cause and effect relationship. Our model is much more complex than that and depends upon sophisticated neural substrates and behavioural repertoires, i.e. complex *systems* building upon and intimately interconnected to one another and eventually serving purposes other than those for which they initially evolved, but not supplanting the neural mechanisms and behaviour of the earlier stages. Imagining a spiral would be the best analogy to represent our linked hypotheses and thus, 'The Spiral Model' would be a far more accurate characterisation of our work than the 'Deceit' model.

Across mimetic and mythic stages there is an increasing acceleration in the development of learned and transmitted skills such that the speed of cultural developments during the theoretic stage is much more rapid than that of the mythic phase, which in turn is more rapid than that of the mimetic stage. Thus, culture in the form of stored long-term memories and external records on media such as stone, bone, wood, hides, pottery, cloth, paper, and eventually cyberspace, becomes intimately entwined with the evolving neurobiological substrates and culture also facilitates the expanding ability to communicate using learned oral, and then written language, as well as more innate systems involved with facial expressions, body language, mimesis, and emotional call signals.

Helvenston has given an overall summary of our original paper and commented on some of Dobrez' broader substantive points where appropriate. Hodgson will now endeavour to proceed with a more detailed response to Dobrez' criticisms. In seeking to refine the substance of our target paper, Dobrez suggests that the roots of representation need to be seen in a positive rather than negative light. However,

the question of whether representation arose out of a positive or negative valence is tangential to the main thrust of our argument. It is an accepted fact that evolution is neutral in so far as value is concerned; in other words it works on the basis of any course that may help promote survival. One of the means by which this is furthered is through mimicry where one organism simulates another creature or thing that might advance the transmission of that organism's genes into the next generation. This relationship can be interpreted as positive in the sense that mimicry can be mutually beneficial to both parties involved in this dynamic — the example of the orchids that simulate female insects so that male insects are induced to mate with the impostor is relevant here. The insect obtains the reward of nectar for its exertions and the orchid has its pollen distributed. Thus both parties receive something positive from the interaction. Mimicry as expressed in nature performs many functions of which the orchid/insect liaison is just one example, as an aid to camouflage being the one more often quoted. As we pointed out, mimicry in nature is not under the control of the perpetrators although some creatures such as squid and cuttlefish give this impression and show some flexibility in range of displays, but these are thought to be mediated by instrumental conditioning. The important question to be addressed is when did such ploys as manifest in nature come to be subject to intentional manipulation for purposes other than that for which they were originally devised by evolution?

Although non-human primates are capable of seeing objects in pictures these are often mistaken for the real thing (infants before two years age make the same error in attempting to physically put on a picture of shoes). This suggests somewhere between the capabilities of chimps and the emergence of hominins a propensity arose that enabled hominins to understand the trick contained in mimicry and employ this purposively to meet immediate needs. The crucial part in all this is that when hominins came to realise the significance of mimicry they did not act as if the object represented was real but were able to inhibit this response. In this respect, but for exceptional circumstances, one or two which Dobrez mentions, humans do not mistake a representation for the real object, rather the real thing is suggested by the representation. We might say that it was not so much that a mistake was made by those that discovered the significance of representational ambiguity but rather the fact that they came to realise the mistake in the mistake of reacting to something as if it were the real object when it was merely a question of resemblance (Dobrez prefers to use the term equivalence but this is more a quibble about terminology than anything else). Sometime later they were able to use this realisation, at first for the purpose of survival, and later as a means of auto-stimulating corresponding brain areas somewhat detached from the real situation as in depictions of animals. In this sense, hominins began to exploit the mechanisms of deceit as found in nature to their mutual benefit to the detriment of other species. It is true that cheating in humans, for reasons to do with group dynamics and evolutionary constraints, tends to be weeded out leading to the pre-eminence of truth in this regard. This, however, applies to intra-species rather than the cross-species dynamics, the latter of which was the main focus of our concern in relation to competition with fauna populating the same ecological niche as hominins. From an intra-species perspective, however, representational expertise (e.g. disguise) provided shared advantages for proto-humans allowing them to survive more effectively as part of a process of what Dobrez terms mutual reliance.

The supposed unexplained leap from disguise to representation that Dobrez mentions is not such a leap when seen in the context of cognitive evolution, where being able to make connections separating previously unrelated phenomena would have increasingly become the norm, of which the hunter in disguise was one example that would have helped promote the significance of representational equivalence/ambiguity. As already stipulated, this propensity has to be seen in the context of a coming together of a complex suite of interrelated events, including sophisticated disguises involving masks, as well as scratches in soft earth, and the realisation of the significance of tracks etc.

Gombrich's view on art are indeed paradoxical and, in fact, we alluded only in passing to one aspect of this commentator's ideas with respect to the role of projection; that does not imply that we accept wholesale what he has to say — in fact we take quite the opposite position on many of these issues. As an empiricist and associationist, Gombrich regards perception as mediated by learning that is mediated by the culture into which one is born. One of us has pointed out elsewhere (Hodgson 2003) that despite this theoretical position Gombrich was still willing to admit the need for some starting point where the process of making and matching had to begin, but the significance of these factors were either played down or not followed up in *Art and illusion*. In this regard, Gombrich pays lip service to Gibson's (Gibson 1979; see also Hodgson 2002 and 2004) ideas on perception, which have become far more productive for understanding the processes of seeing. One of Gibson's central ideas is the concept of invariance and the rejection of the idea of images as pictures in the brain. Invariance (what remains the same despite change) is a process whereby, through engagement with the affordances available in the optic array, consistencies or patterns are directly available to the visual system. From the perspective of neuroscience and evolution, and although Gibson regarded the visual system as able to pick up invariances without much processing, there are compelling reasons why this approach is the more apposite. Because the visual system is able to recognise objects in less than 150 ms

it might appear that it is encoding invariances with immediate effect but this ignores the complicated and subtle processes that occur in the brain even during this short time span. Briefly, the human brain does not come unprepared into the world, as the empiricists tend to argue, rather it comes with a host of predispositions that, all things being equal, set the agenda for responding appropriately to the affordances implicit in the optical array. These predispositions have been moulded by the evolutionary history of *Homo sapiens* that, for example, enable infants to quickly tune into the stable patterns issuing from environmental input. The same invariances are stimulated by depictive representation but because the latter are not as rich and redundant as the actual optical array they tend to be seen as just that, representations. In terms of neural network theory, this implies that the visual world as such stimulates neurons with greater authority than a representation might.

Invariances can also be actual objects — those enduring forms that have proved particularly significant for the survival of a species. These observations suggest that there are neural substrates for tuning into particular aspects of the world, such as animals and the human form. Hodgson has proposed that the fact animals in rock art are invariably portrayed in side-ways outline view during an enormously long period suggests that this is mediated by such a system. This is supported by the fact that recent research into how neural networks encode form propose that the sideways outline view, particularly of animals, is the most efficient and economical way for storing information (interested readers are referred to Hodgson 2002, 2003 and 2004 for a more detailed account of this). This accords with Deregowski's (1989) proposition that there are certain aspects of graphic representation that are isomorphic with how the brain encodes visual information, whereas others are conventions with some overlap between the two. This is reflected in the debate on the role of representation in various cultural groups. Some authorities take the view that the actual form of the representation is immaterial to its understanding, as it is how meaning is accorded that is crucial (see Layton 1991 for a discussion on this and specifically Goodman 1968 and Wolheim 1970). Gombrich, being an empiricist, seems more in sympathy with this outlook. However, it is a mistake to regard the various theoretical positions in this debate as mutually exclusive. It has been established that the ability to see two-dimensional representations, when proper controls are in place, is universal (Halverson 1992). This universality is guaranteed so long as such representations meet the criteria for invariance already discussed — which Hodgson suggests corresponds with Dobrez' iconic images. The problem comes when one attempts to produce, rather than simply view, depictions. From the perspective of neuroscience, and in agreement with Gombrich, it is much easier to produce iconic images, in other words the crucial invariances, because there is a preponderance of these encoded in neural networks in what are called view-dependent co-ordinates (Hodgson 2002, 2004). That is, there is a hierarchy of separate but overlapping neural co-ordinates (with different tuning curves) that encode different sides of an object with one, such as the side view, usually being dominant. These reflect the actual invariances as they exist in neural centres that give rise to what one knows about the object. The predominance of these networks leads also to what is termed regression to the real object. Importantly, it is such neural settings that affect how two-dimensional representation is produced. Even the most practiced artists have been shown to be constrained by these factors (Taylor and Mitchel 1997). Hodgson would propose that the invariances as manifest in graphic representation constitute a template which a culture can choose to add or distort depending on the meaning accorded thereby giving rise to conventions. He does not think, however, as Goodman and others have postulated, that the meaning apportioned to a depiction is ever totally detached from what is immediately represented.

These concerns can be related to Peirce's (1982; 1868a; 1868b) theory of semiotics that is reflected in the following statement by Cheyne (n.d.) (and addresses Dobrez' thoughts on these issues):

> The sign for Peirce, in contrast to Saussure, is part of a triad serving as a constrained mediation between objects of the world and the interpretants of consciousness. The sign is constrained by non-arbitrary physical structures of the world and of the interpreter. The world is unlabelled but not amorphous. There are real world constraints that preclude strict arbitrariness of interpretation and guide our segmenting. There are also physiological and cultural-historical constraints on the categorization and selection of signs (e.g. Edelman 1989).

The icon is the most basic sign since the iconic sign has a direct connection with its object by being a part, component or aspect of the object itself. Although any aspect of an object might serve as an iconic sign those aspects that are selected are typically diagnostic of the object in that they serve to differentiate that object, in some respect or practical way, from other objects (e.g. Tversky 1977). The iconic sign is based then on a direct physical connection of the sign and object. An indexical sign has a less direct connection, that of association. Since the indexical sign stands for its object associatively we may describe that association as one of metonomy, as the crown might stand for the monarch or the hoof prints may stand for the deer. Finally, the symbol stands for an object by virtue of some convention or, likely more often, by historical evolution of earlier iconic and indexical signs.

In this respect, 'lower' organisms are almost completely dependent on the presenting stimulus on a one to one basis whereas, at the opposite extreme, 'higher' organisms, as more sophisticated cognitive interpretants, have the added flexibility to decide the

nature of the symbol concerned. The hierarchical and embedded nature of how interpretation proceeds, where higher levels depend on lower ones, is not only reflected in cognitive processes but also how representation seems to occur in palaeoart. This is further echoed in how we have described the course by which materially embodied representation unfolds during the Pleistocene in relation to disguise and naturalistic images of animals/faces/human form (iconic), hand-prints/tracks/stick figures (indexical sign), to later systems that are no longer obviously tied to the presenting stimulus, e.g. writing (symbols). Interestingly, these categories coincide with Anati's pictograms (iconic) and ideograms (indexical) in relation to rock art (and this commentators broad survey of world rock art and the universal theme of animals connected to hunting groups seems to confirm the trajectory we propose).

These observations are relevant to Dobrez' rather convoluted argument as to the relationship between representation and language. He seems to want to have it both ways in suggesting that what one sees is already given as part of the ability to be able to recognise objects but this is subject to refashioning by way of language. In fact, as neuroscience/psychology has increasingly found, the visual pathways are relatively independent of language. Hodgson suspects the confusion is to be found in the assumption that the visual world is thought to be an infinitely malleable commodity subject to the experience of a particular group, of which language is deemed to play the leading role. This ignores the existence of one visual reality common to humans as a species that has been shaped by the demands of evolution. This is obvious in that being able to reshape visual reality according to ongoing circumstances would lead to individuals unable to agree on what is real and the rapid demise of those so disposed through a reduced ability to attend to the dangers of the world. The best that language can hope to do here is accentuate different aspects of this perceived reality. Having said this, the representations that we find in depictions may be more prone to the demands of language because they are more loosely coupled to evolutionary imperatives yet are still dependent on the visual channel — which is in agreement with Dobrez' idea that one represents because this already makes sense (this reading also accounts for the overlap mentioned earlier in relation to conventions). But this does not answer the question as to when the trick of representation first began to make sense — something we endeavoured to address in alluding to the relationship between animals and disguises and the significance of ambiguity (of the roots and snakes kind). Dobrez' analysis therefore seems to apply to a much later phase of cognitive evolution than our investigation is concerned with.

The preference in European art towards real appearances can be regarded as something exceedingly difficult to achieve. This is because such an undertaking involves the inclusion of much redundancy, especially with reference to the co-ordinated 3D spatial layout of objects represented on a flat surface. The Chinese actually came up with aspects of linear perspective before the Renaissance but regarded this as an inferior means of portrayal as they were more concerned with the object itself and how this related to other objects based on different concerns (see Costall 1993). In terms of how the brain works, in order to be able to produce pictures of this order, it is certainly true that more areas of the brain need to be brought into the equation than, say, if one simply wished to portray a single object in its typical profile. In this sense, the former is a more complex ability that needed the considerable scaffolding of earlier generations of artists, including various scientific insights, for this to be achieved. This brings us back to Gombrich's Eurocentric view that progress in art has to be measured by how well appearances are represented which the above analysis, in relation to the number of brain areas involved, seems to confirm. However, these considerations take us into the thorny issue of post-modernism and postprocessual archaeology with the wish to treat all manifestations of art as equally valid. While there is merit in these more recent interpretive schools, this does not imply that all art should be regarded by the same yardstick, rather that the message therein has to be given due consideration according to context. From this perspective, art seems to perform a different function according to cultural factors and the 'prehistoric' and historic period concerned, so it is not always useful and can, indeed, lead to many errors to compare one with the other. As we have stressed, in the case of how representation may have originally arisen, the function of the first 'art' was radically different from that which followed around half a million years later. In essence, then, art from any culture can be complex for any number of reasons depending on what criteria are being used by way of assessment and it is what a culture chooses to prioritise and how this is translated into representational form that is all important.

As to the point about all cultures employing logic in response to prevailing circumstances, there are different ways whereby logic can be applied and one will come up with more powerful explanations to account for complex situations when the right questions are framed in the appropriate way. And the principles of logic, as a system for assessing truth, also differ according to the criteria used for validation. In this sense, logic is not a static system but one that has evolved according to demands. Of course myths, legends, and storytelling apply some form of logic, otherwise they would not make sense. But many myths and rituals are rife with symbolism and the logic used often satisfies emotional needs, not intellectual needs, indeed some aspects of the myth may even seem contradictory to intellectual reasoning (Bloch 1991; de Heusch 1985). As Deacon (1997) similarly

specifies, Donald was not saying theoretical thinking developed as out of nowhere but rather more complex ways of thinking are predicated on earlier modes of thought and these earlier modes are still very much with us today. Unfortunately, these questions have become mired in debates relating to the social sciences that are somewhat separate from those that form the basis for determining 'truth', i.e. reality. Nevertheless, it seems likely that it was possible to attain some degree of detachment during Donald's mythic stage, allowing access to true knowledge of the world. Of course, the greatest degree of detachment results from the process known as the Scientific Method, which was based upon not only 'prehistoric' developments, but several thousand years of cultural evolution made possible by written language. So, we find ourselves in the peculiar situation of being consumed by the world in which we live while at the same time being potentially able to examine it in more dispassionate ways. Donald's notion of a theoretical stage of cognitive expertise may therefore not be far from the truth except that We would add that this stage contains within it other more complex and sophisticated modes of thinking that depend on various modes of analysis ranging from induction to abduction to hypothetico-deductive reasoning giving rise to knowledge that is not only counter intuitive but defies common-sense.

In conclusion, we think that Dobrez is on the whole sympathetic to our main thesis and that his criticisms derive either from a misreading of the target article or a misplaced concern for terminology that we have tried to address in this reply.

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Comment on
WHAT'S IN A WORD, WHAT'S IN A HYPHEN?
by Christopher Chippindale and Paul S. C. Taçon,
RAR 23: 254–257.

FURTHER COMMENTS

On 'rock art' history and terminology

By B. K. SWARTZ, Jr

In reading the '*RAR* Debate' of Chippendale and Taçon's paper (2006) I was struck by (1) how little United States research has been published on 'rock art' in the journal and (2) how little has been published on the intellectual history of the field in general. This has stimulated me into preparing this statement in order to give a perspective on these matters. First I will present a brief intellectual history of 'rock art' study in a region of the United States that I sense is not well known to the rest of the world and even in recent time forgotten in the U.S. I feel that two important aspects of what I am about to write about are (1) the relation of 'rock art' to a body of theory, and (2) considering this history finding a suitable term for the discipline. You may notice that I have used quotes around the term rock art, *since much of it is NOT functional ART!*

The first and most prominent student of Franz Boas, the founder of American anthropology, was Alfred L. Kroeber. Upon receiving his doctorate (the second awarded in the discipline) he became a member of the faculty at the University of California in 1901, an institution which was soon to become the premier university of the west coast of the United States. It was here he established a thriving anthropology program. What he encountered on his arrival was a vast region populated by virtually unknown native inhabitants. Kroeber was concerned with saving what was later called 'vanishing data', of the elaborate but disappearing culture of the native inhabitants of California. His first priority was to see that the recording and analysis of ethnographic and linguistic data was undertaken. Later he turned to archaeological research. The compiling of the massive 995-page volume *Handbook of the Indians of California*, published in 1925, attests to his interest and concern in preserving the disappearing ethnographic data.

The method of 'natural history' was extended to archaeological remains of past human activity. In the United States this area of study was included in the field of anthropology and was originally historical in the same sense as the term history was used in 'natural history'. This was a synthesising approach, rather than the particularistic approach of the established academic field of history. In the United States this field of study became known as the 'cultural historical approach' and the focus of this school of scholarship was called 'culture history', the anthropological sequel of the biological and geolgical fields of natural history.

> The distinctive feature of the [culture] historical approach ... [is] an endeavor at descriptive integration. By 'descriptive' I mean that phenomena are preserved intact as phenomena ... in distinction ... [to] the ... nonhistorical sciences which set out to decompose phenomena in order to determine processes (Kroeber 1935).

Starting about 1925 one of Kroeber's students, Julian H. Steward, set out to compile the 'rock art' of the western United States and in 1929 published *Petroglyphs of California and adjoining states* and described 130 localities. Steward used the term 'petroglyph' for rock markings and 'pictograph' for rock paintings, following the terminology employed by Garrick Mallery in his original Bureau of American Ethnology study in 1881. Mallery states that the term 'petroglyph' was used by Richard Andree in 1878 for representational rendering that was carved, pecked or otherwise incised and 'whether figured only by coloration and incision together is upon a rock' (Mallery 1893: 31). Franklin Fenenga, another University of California scholar, proposed that the word 'petroglyph' be applied to both (Fenenga 1949: 1), i.e. the term 'petroglyph' came to have two meanings, (1) rock markings and (2) rock markings *and* rock paintings. I find the term 'pictograph' to be unfortunate in that it is an invented word combining the Latin root (*picto*) and Greek root (*graph*). A more suitable term might be 'eidograph,' using the Greek root *eido* (figure or image). Christy G. Turner II (1963:

2) suggests that the word 'petrograph' be substituted for 'petroglyph' in the composite meaning proposed by Fenenga. Turner also has ties to the University of California, at one time being a graduate student in the Department of Anthropology.

From 1949 to 1962 the use of the terms petroglyph and pictograph was unchallenged in the American west and elsewhere. It was in this interregnum that I became involved in *petroglyph* research in the Klamath basin of southern Oregon and northern California. In 1962 Robert F. Heizer, an archaeologist at the University of California, and his student Martin A. Baumhoff published a comprehensive 412-page volume on the petroglyphs of Nevada and eastern California. I have learned by hearsay that the University of California Press would not publish the volume unless (1) California data were included and (2) a 'jazzier' term be used for 'petroglyphs' in the title of the study. The work, now entitled *Prehistoric* ***rock art*** *of Nevada and eastern California,* was then published. In 1963 I wrote a review of this study for *The American Journal of Archaeology*, commending the study but strongly opposing the use of the word 'rock art' as a synonym for 'petroglyph' (Swartz 1963). It is interesting to note that through the text within the volume the term 'petroglyph' is used!

In 1963 Campbell Grant published *The rock art of the North American Indians.* Grant's academic training was in art rather than anthropology. He initially became involved with 'rock art' in studying the elaborate Chumash rock paintings in the Santa Barbara region of southern California. This experience may have led him to focus on paintings (pictographs). Initially Grant used 'rock art' as a descriptive term (note it was not capitalised in the title above). His books were quite popular and well illustrated. The term 'rock art' was used in their titles and text. These two factors essentially led to the replacement of the term 'petroglyph' with 'rock art', or, more likely, introduced the term 'rock art' to the general American public for the first time.

In 1973 the *magnum opus* of California 'rock art' was published, entitled *Prehistoric rock art of California,* by Robert F. Heizer and C. W. Clewlow, Jr. It was issued in two volumes, Vol. 1, 149 pp. plus 23 plates, and Vol. 2, 384 figs. Again Heizer uses the terms petroglyph and pictograph in the text.

On 10 May 1974 nearly a hundred persons, primarily from the American Southwest, attended what was called a symposium at Farmington, NM. From this assemblage the American Rock Art Research Association (ARARA) was eventually formed. The members of this association had few if any ties to the University of California. They were primarily avocationalists that were familiar with Southwest archaeological remains. Professional Southwest archaeological research was done by scholars from the east coast, especially Harvard University. Two fundamental shifts occurred: (1) the field of petroglyph study established at the University of California was eclipsed and (2) activity, now termed rock art research, was taken over by avocationalists in the United States. Regional rock art organisations then developed in the United States. Two in the far west are the Nevada Rock Art Foundation and the Bay Area Rock Art Research Association.

To counter this shift I and Joseph J. Snyder in 1978 founded the American Committee to Advance the Study of *Petroglyphs* and *Pictographs*, Inc. or ACASPP. This was to be an organisation of professional scholars committed to research on petroglyphs and pictographs. After a positive initial response we discovered that few people had protracted interest. Why is this so? Petroglyph research does not fit the established rubric. Professional archaeologists think in terms of recovered portable artefacts that can be taken to a laboratory for analysis and dating. This cannot be effectively done by petroglyph researchers.

We now come to the issues of terminology. What do we call our field of study? The problem is our subject matter is defined not by a segment of knowledge, but by method of research. Archaeologists tend to reject our field because the subject matter cannot be effectively removed from the field to the laboratory for protracted analysis. There are no neat units of analysis. Also, at this time, our evidence cannot be effectively dated. I feel we should amalgamate our field with (cultural) anthropology, an established academic discipline. Remember, Kroeber found it important to record the culture of California natives. The purpose of Steward's study was to record an aspect of California native culture. Our studies should be integrated with cultural anthropology — not art, at least in the formal sense.

The term 'rock art' should be abandoned. Much of the evidence we study had utilitarian, not aesthetic, function to its makers, e.g. maps, trail markers, property lines, warning of danger, group identification etc. The term 'rock art' gained currency in that it was a cover term for both painted and pecked markings. The term 'rock markings' seems pedestrian, but is more precise. It gives our phantom field a rather prosaic and 'down to earth' name. The use of Turner's term of 'petrograph' is excellent, but has been ignored.

A purpose of this discussion is to bring out the problem of terminology. When petroglyph study is termed rock art study it is 'de-anthropologised' and removed as a legitimate academic field. It no longer has a body of theory that would integrate it as an autonomous discipline or sub-discipline. A brief holistic account of a rich, but not well known, field of petroglyph research accomplished by anthropologists in the American west is provided for scholars elsewhere, many being subscribers to *Rock Art Research.* In fact I wish to propose a formal title for this development — *The Berkeley Anthropological*

School of Petroglyph Research. I feel that to this day Kroeber's original approach to the subject was correct, but now is corrupted. The term 'rock art' as a name for our academic discipline should be modified.

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Contesting the incontestable

By JACK STEINBRING

It is no longer true that archaeologists shy away from rock art because 'they cannot take it into their labs'. Numerous archaeologists throughout the United States have 'rediscovered' rock art and are both excavating rock art sites and are applying clinical techniques to the investigation of petroglyphs and paintings in the field. And, despite their empirical training and perspectives, they still call it 'rock art'. This might be because they are busy doing something with it, and not getting entangled in revisionistic adventures in the full face of monumental precedence.

It might be seen as perfectly normal that people trained in the west would see the west as the centre of things. The east, however, is not barren in the 'intellectual history' of rock art research. The late James Swauger, for one, recorded hundreds of sites in the Ohio valley and constantly looked abroad in his synthetic efforts. The late Klaus Wellmann, from New York, published in 1979 an unchallenged compendium of rock art in North America in which the east was not stinted. The recent publication of *The rock art of eastern North America* (2004), an award-winning volume edited by Carol Diaz-Granados and James Duncan should, if read, settle the hash about the east. Many of the contributions to this volume are, in fact, by archaeologists.

Probably few realise that the late Selwyn Dewdney, the founding father of Canadian rock art research, did his earliest work in Minnesota. In the 1950s, Dewdney meticulously recorded the large Spirit Island Site on the Nett Lake Indian Reservation. The Eastern States Rock Art Research Association is a vigorous scientific organisation, loaded with Ph.D. archaeologists. It has always published a good newsletter, as well as special publications on rock art research. It has no aim to change its name. If *RAR* changed its name to satisfy the miniscule elements that delight in contesting the incontestable, it would be seen as weak and unworthy of the universal respect it now enjoys.

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Rock-writing, picture-writing, petroglyphs, rock-art; and the importance of the hyphen

By PAUL S. C. TAÇON and CHRISTOPHER CHIPPINDALE

We thank B. K. Swartz, Jr. for his excellent contribution to the debate we recently generated. He makes a number of valid points and enlightens us with important American developments in a field we all are most passionate about. As Swartz emphasises, it was Garrick Mallery's 1893 classic, *Picture-writing of the American Indians*, that was pivotal in launching interest in what we call 'rock-art' or 'rock art' of western North America. But earlier, in 1886, Mallery also published *Pictographs of the North American Indians*, another meticulous volume that focuses on different forms of drawing but includes 'petroglyphs'. Swartz refers to Mallery's attribution of the use of 'petroglyph' by Richard Andree in 1878. Given that some readers may wish to pursue this we provide the full reference details here (see Andree 1878) but also reproduce the full text of the pertinent section within Mallery 1893, page 31:

> In the plan of this work a distinction has been made between a petroglyph, as Andree names the class, or rock-writing, as Ewbank called it, and all other descriptions of picture-writing. The criterion for the former is that the picture, whether carved or pecked, or otherwise incised, and whether figured only by coloration or by coloration and incision together, is upon a rock either in situ or sufficiently large for inference that the picture was imposed upon it where it was found. This criterion allows geographic classification.

It is interesting to note that Mallery placed a hyphen in 'rock-writing' and 'picture-writing' and that Thomas Ewbank's publication was in 1866. This predates European publications, with serious European interest in 'cave art' beginning in the 1860s with Félix Garrigou (see Bahn and Vertut 1997: 16). Indeed, as far as we can tell, the first published term for what most refer to as 'rock art' had a hyphen in it, Ewbank's rock-writing! In Australia, interest in petroglyphs began soon after Europeans arrived in many parts of the country (Taçon 2001). For instance, the first documented European encounters with rock art occurred during the initial months of settlement

at Port Jackson (Sydney) in 1788. When Governor Arthur Phillip and his men began exploring the land surrounding the harbour, petroglyph sites impressed them enough to describe them in their diaries:

> In all the excursions of Governor Phillip, and in the neighbourhood of Botany Bay and Port Jackson, the figures of animals, of shields, and weapons, and even of men have been carved upon the rocks, roughly indeed, but sufficiently well to ascertain very fully what was the object intended. Fish were often represented, and in one place the form of a large lizard was sketched out with tolerable accuracy. On the top of one of the hills the figure of a man, in the attitude usually assumed by them when they begin to dance, was executed in a still superior style (Phillip 1970[1789]: 58).
>
> They consisted chiefly of representations of the natives in different attitudes; of their canoes; of several sorts of fish and animals … they seemed to exhibit tolerably strong likenesses (White 1790[1962]: 141).

In other parts of Australia 'rock-art' was discovered in the early 1800s but serious study did not commence until the late 1800s, with many publications by R. H. Matthews (see Taçon 2001 for a brief history of Australian rock-art research). In central Asia petroglyphs have been studied since at least the seventeenth century (Francfort 1998: 304) and in many parts of the world, such as Arnhem Land, Australia, or Valcamonica, Italy, it is obvious indigenous peoples noted, interacted with, interpreted and responded to earlier rock-art.

Our discipline, sub-discipline or whatever we might classify it as, has a lengthy history (Bednarik 2001: 7–12) with roots that predate both archaeology and anthropology as academic pursuits. Swartz argues that 'When petroglyph study is termed rock art study it is 'de-anthropologised' and removed as a legitimate academic field'. This leads him to believe 'it no longer has a body of theory that would integrate it as an autonomous discipline or sub-discipline'. We argue to the contrary, that the nature of rock-art research is such that it will always be multidisciplinary, drawing on theory from a number of sources: archaeology, anthropology, indigenous studies and traditions, psychology, chemistry, geology, neuroscience, art history, philosophy, aesthetics, semiotics and elsewhere. Our *The archaeology of rock-art* (Chippindale and Taçon 1998) was a first comprehensive attempt to encapsulate method and theory for rock-art studies from a primarily archaeological point of view but we and others have also published books and articles emphasising landscape approaches (e.g. Chippindale and Nash 2004). Readers of *Rock Art Research* will no doubt also be familiar with other ways of studying and theorising rock-art. But Swartz is perhaps right, that it is time to ground our research in some coherent theoretical framework.

Swartz concludes that the term 'rock art' as a name for our discipline 'should be modified'. This is precisely the point we made in our original paper. He recommends 'rock-markings' or Turner's 'petrograph'. But imposing yet another new word or phrase will not actually help. Briefly, it provides a clean sheet of paper and a crisp definition but at a very substantial cost — for a new word or phrase is a mystery, which the community of writers and readers has to learn the meaning of. And then the pristine new word or phrase suffers the same blurring that all words experience as long as they are in active use. For instance, in 1865 Sir John Lubbock invented what he saw as the unambiguous words 'Palaeolithic' and 'Neolithic', each with a clear definition. They soon came into common use — but through their very success their meanings were blurred. 'Neolithic' remains a useful word, but it long ago lost its Lubbocky crispness. Often, one now has to explain just what aspect of a once united and now diverse 'Neolithic' is relevant to a certain context.

So we do think the best solution really is not to essay another novelty, but to add the hyphen between 'rock' and 'art', following the good hyphenated convention first established by Ewbank in 1866 with his 'rock-writing'.

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Rock art history and use of the term: reply to Swartz

By MAVIS GREER and JOHN GREER

We welcome this opportunity to respond to Professor Swartz's article from the perspective of professional archaeologists who have written archaeology-based dissertations in anthropology on rock art, as archaeologists who have owned and operated a private archaeological consulting business in the western United States since 1977. During this time we have dealt with rock art sites from a compliance perspective, and as long-time rock art researchers who have travelled the world to seek comparative rock art data and the views of researchers who deal with problems much different from those

we encounter. We have been active members of the American Rock Art Research Association (ARARA) since 1992, and Mavis is currently president of the organisation. Rock art research today is a multi-disciplinary topic. As much as we might like it to remain the purview of archaeology or Swartz would like it to be conducted or controlled by cultural anthropologists and reserved for academics, rock art research has passed that time. Rock art studies are conducted by at least archaeologists, cultural anthropologists, art historians, artists, chemists, photographers, computer scientists and linguistics. Each discipline contributes a different perspective to our knowledge of the people who made these eye-catching remains.

ARARA and rock art research

ARARA was founded in 1974 in the south-western state of New Mexico, and the papers from the symposium were published in the first issue of *American Indian Rock Art*. The organisation was incorporated in the west-coast state of California because many of its organisers were from there. The organisation was never meant to be tied to the University of California system but instead was established as a private, non-profit research group of professionals and avocationalists with strong interests in formal recording and analysis of pictograms and petroglyphs. At the time ARARA was formed, the entire cultural resources system within the United States, which included pre-Historic and Historic remains of any culture, was undergoing a change as attitudes shifted regarding who the watchdogs of these resources were to be. Until that time universities considered archaeological research, and the sites, to be primarily their domain. As government agencies and professional private contractors entered the picture with the onset of new regulatory legislation, the recording, study and publication of the newly gained knowledge moved away from its primary association with academic institutions. The establishment of an organisation dealing with one aspect of cultural resources, rock art, in an independent setting outside of a university setting was not unusual, and organisations such as the Oregon Trail Society already existed to study and publish on other site types.

ARARA was designed to be like other archaeological associations — a mixture of professional and avocational people interested in a particular subject, but not approaching it narrowly. It was meant to reach beyond universities and become a field of study, such as has become common for such topics as Maya studies, translation of Mesoamerican texts, peopling of the New World, and historical archaeology. The Society for American Archaeology (SAA) was organised this way, and long before the 1970s most states and geographic regions in North America had similar archaeological societies. The difference with ARARA was that its membership included professionals from many fields, in addition to archaeology, making it more like historic trail organisations.

One of the main purposes of ARARA was and is to provide a publication outlet for current research associated with the extended site-type rock art, with all of its ties to physical sciences, cultural anthropological theory, ethnography, and all manner of archaeological applications and approaches. The membership continues to represent that same diversity but with an emphasis on archaeologists. Five of the ten presidents of ARARA have had an archaeological background.

Swartz states that prior to ARARA, archaeological research in the American Southwest was conducted by east coast scholars. This statement, whether referring to all of south-western archaeological research (as it states) or to the more narrow rock art research (which is implied) insults the tremendous amount of south-western archaeology done by numerous western base scholars. John began work in the Southwest in the 1950s, with training from specialists who had been working there for decades, and at the Museum of New Mexico in the mid-1970s was heavily influenced by Polly Schaafsma, regional rock art specialist continuing her ground-breaking technical and theoretical publications in Southwestern rock art.

Thus, to summarise our comments on Swartz's statements regarding ARARA, it was not formed with the intention of having ties to the University of California and does not have a formal association today, although past and present members have individual associations with that and many other institutions across the United States and beyond. It is interesting that Swartz credits ARARA with causing a shift away from rock art studies at the University of California (we assume he means the entire system here and not just Berkeley), since many, if not more, studies are being conducted by University of California students, professors and research associates today than there were pre-1974. This can be seen in the ever-increasing number of papers published by affiliates from these institutions. The number of people working in the Southwest continues to grow steadily, as with any region of the world, and ARARA members have been in a position to foster communication between researchers of different levels and intensities in matters pertaining to rock art.

Rock art research — professionals and avocationals

The statement that rock art research has been taken over by avocationals is often made but not usually supported by facts, such as an examination of qualifications of authors of rock art site forms, reports and publications. Instead Swartz erroneously cites the coming of organisations such as ARARA, the Nevada Rock Art Foundation, and the Bay Area Rock Art Research Association, as evidence of a lack of professional interest or participation. As discussed above, ARARA was organised by a variety of professionals (the primary lead at the time,

Frank Bock, had a Ph.D. in anthropology), and if he had investigated the Nevada Rock Art Foundation, he would have found that the primary founding member, Alanah Woody, has a doctorate with a rock art dissertation. In all cases we know of in the United States, including the Society for American Archaeology Rock Art Interest Group, Eastern States Rock Art Research Association, Upper Midwest Rock Art Association, Utah Rock Art Research Association and Texas Archaeological Society Rock Art Recording Group, to name a few more, there is a mixture of professional and avocational rock art researchers.

A few years ago, we examined the rock art research history of the state of Montana (Greer and Greer 1999) as an example of how much rock art research was conducted by professionals versus avocationals. The details of this work can be found in the original article, which can be downloaded from the web (*www.GreerServices.com*), but a brief review will serve as an example of our point.

Montana is the fourth largest state in the United States in area. Its northern border is the international boundary with Canada. It encompasses two main topographic regions. The eastern area is a mixture of plains interspersed with mountain island chains, and the western part of the state is dominated by the Rocky Mountains. Sandstone formations of the plains contain mainly petroglyphs, and limestone outcroppings of the mountains and foothills have mostly paintings. Today about 800 rock art sites have been recorded during very limited work, and most of the state has not been surveyed for archaeological remains.

The earliest professional article on a Montana rock art site, a painted bluff, appeared in 1908, by biologist John Elrod in the *Biological Series Bulletin* of the University of Montana. Before that, dating back to the mid-1800s, was the usual cadre of newspaper and magazine articles void of scientific scrutiny. Fifty years later came the archaeological dissertation of William Mulloy on excavations and rock art of Pictograph Cave (Mulloy 1958). From this point forward, rock art is a constant in the history of Montana archaeology. In the 1950s rock art occurs in reports on Smithsonian River Basin Survey projects, and it was during this decade that Professor Carling Malouf, a general anthropologist, began studying and writing on the rock art of Montana and the Plains, with particular emphasis on the western part of the state. His work ultimately led to the development of a classification system for pictograms in the region (Malouf 1956, 1961). In the late 1950s and into the early 1960s, members of the Montana Archaeological Society conducted a state-wide rock art survey that resulted in the recording of many sites. In the 1960s Stuart Conner, an attorney by trade, and other members of the Billings Archaeological Society made significant contributions to the state's rock art data base. Conner's influence on the recording, professional quality research and interpretation, and the preservation of Montana rock art has received honors from the Society for American Archaeology and the American Rock Art Research Association. In 1971 he published the first book dealing exclusively with Montana rock art.

By the 1970s, publications and site recording forms on rock art were increasing for Montana primarily because of the increase in professional archaeological surveys being conducted in compliance with federal environmental laws. During this decade, Dr James Keyser, a professional archaeologist, began extensive recording and writing on the rock art of Montana and the Plains, with emphasis on regionalism, stylistic classifications and interpretation through ethnographic comparisons, which morphed into a specialty on robe and ledger drawings and their relation to rock art. Dr Larry Loendorf, another professional archaeologist, first recorded Montana rock art in the 1960s, and in the 1980s he became one of the most active rock art researchers in Montana and the western United States, also serving as president of the American Rock Art Research Association. Among his contributions are excavations that have linked rock art with buried material remains, experimental projects to test characteristics of paint, and pollen analysis to help interpret rock art site function.

Professional contract archaeologists and government archaeological specialists have contributed significantly to recording and preserving rock art, and thousands of sites have been found during that work, as well as tests for all manner of new approaches and technology, such as laser recording. Contract projects have been the impetus for development, testing and refining of specialised recording and preservation methods. The late Dr Lynn Fredlund, another trained archaeologist who spent most of her career as a private contractor, was particularly instrumental in such studies, especially in her approaches to detailed recording and information preservation. Rock art recording has accelerated throughout Montana since the 1990s, and almost all of the new recording has been conducted by government archaeologists and private contractors.

Archaeologists and rock art

The Montana example discussed above is just one case of the significant role professionals have played and continue to play in rock art research in the United States. Swartz lists three reasons he believes archaeologists have not been involved in rock art research, and we address these concerns below.

1. *The subject matter cannot be effectively removed from the field to the laboratory for protracted analysis*

Rock art sites are photographed, drawn to scale, and today even reconstructed via laser technology, all of which are non-impacting (as opposed to older methods using highly destructive latex moulds). Pigments, varnish and related rock surfaces are

sampled for laboratory analyses. Current methods allow researchers to take data from the field to the lab in the same fashion for rock art sites as they would for any other site type. The difference is that in most rock art cases it is not necessary to destroy the site in order to collect the materials to be analysed. Thus, samples, drawings, photographs and notes that would come in from an excavation also come in from a rock art project placing them on equal footing, and at the same time abiding by current non-collection policies of management agencies. Photographs and laser scans can be manipulated in ways that past visual and metric inspections of artefacts could never approach.

2. *There are no 'neat units of analysis'*

Again analysis is not dependent on being able to hold a rock in one's hand. Advanced artefact recording and analysis are now being done primarily by computer, with emphasis on shape recognition, component analysis, spatial relationships and context recognition. Experimental archaeology for interpretation and understanding of natural processes, and the use of ethnographic information and comparisons continue to be parts of standard analyses. The same is being done for rock art, with much the same theory, approaches, and constraints on interpretation. The most obvious analytical units in rock art are figure shapes, sizes, physical components, intrasite distribution and context, landscape distribution and context, and extensive use of ethnographic information requiring the same degree of scrutiny and constant evaluation of information as any other kind of archaeological research. Of course, graphics analysis includes not only rock art, but also decorated ceramics, painted sticks, carved trees, body paint, tattoos, decorated canoes, wall painting, and many other elements of material culture and its non-material parallels. Rock art figures can be dealt with in the same ways as stone tools, with the same approaches to typological studies, physical analyses, seriation and direct dating.

3. *Evidence cannot be effectively dated*

Dating in rock art research runs the gamut of relative to absolute as with any other archaeological remains. Paintings and petroglyphs can be dated absolutely by small samples of paint or from carbon trapped in mineral accretions (and by other methods), and relatively by the usual analysis techniques of style, superposition, various forms of seriation and context. Paint from a panel, exfoliated pieces of figures, or tools used to produce the rock art are sometimes found below panels in dated deposits that securely date the rock art. Cross-dating and stylistic dating are sometimes remarkably effective, with the obvious principle that the more detailed and complex the two similar figures, the more likely the cultural relation — the same as with any cultural elements. Thus, rock art figures can be dated directly or by a variety of archaeological contexts. With any kind of archaeology there are always dating difficulties — such as with rock cairns, medicine wheels, small lithic sites, empty tombs, isolated artefacts, features with no associated charcoal — and rock art must deal with exactly these same challenges.

Terminology

Swartz believes the problem with rock art terminology stems from a problem of subject matter, which he identifies as 'method of research' rather than a 'segment of knowledge'. We do not see either of these as an identifier for the subject of rock art, and indeed, there seems to be no purpose in trying to limit what rock art research does, why, or how. This subject is instead identified by a generalised and highly diverse site type — just as interest in stone circles, bison jumps or Paleoindian sites are identified by a site type and not a method of research. In all cases the site type defines a discipline with established and ever-changing methods and theories to draw upon. We happen to believe that rock art is best subsumed under archaeology, in its broadest sense, within the study of past material culture. But archaeology must be understood as subsumed under general cultural anthropology as a non-isolated part of general anthropology. Archaeology has at its disposal a wide range of tools and theories to deal with material items, and the study and synthesis of those items, and rock art, is perhaps best viewed within those approaches. Today it is not useful to consider that any form of study, of whatever subject, can be divorced from other topics, and rock art is the same. Thus, while we place rock art studies squarely within archaeology and not cultural anthropology, it is with the understanding that there can be no useful limiting separation between these two fields, or several others.

Archaeology must often take a synchronic, one-time approach to consider adequately the diachronic, extended-time necessities of problems like chronological ordering and cultural changes through time and space. These concepts certainly are not unique to archaeology, as an isolated discipline, but they are the daily bread of archaeological thought. Likewise, style recognition and distribution are instrumental in any kind of archaeological work, including rock art. Like most of the rest of the world, most rock art in the United States was made by people of the past and must be dealt with as other kinds of pre-Historic resources, albeit with the possibility of careful use of additional ethnographic information. Thus, we have interactive fields of archaeology, cultural anthropology and ethnography, with obvious considerations of aesthetics, art history and art (with input from artists). Chemistry, physics and geology are instrumental to paint mixtures, detailed component analysis, and organics trapped in varnish or other kinds of rock coating.

Thus, rock art has not been removed as legitimate

academic study. Instead, it has been recognised globally since at least the 1970s as a discipline that interactively incorporates multiple fields. This has expanded our knowledge about these kinds of sites, increased public awareness of their importance, and has led to more active conservation and preservation efforts and successes relative to these fragile archaeological remains.

The term *rock art* is well established in scientific and popular literature and public vernacular. Changing the name will not change the subject matter, and acceptance of that name as the study of pictograms and petroglyphs will never be the driving force in whether someone, professional archaeologist or other interested person, becomes a researcher. People study rock art because the subject is of interest to them personally. Terminology is not the basis for the discipline, and it does nothing to invent new terms that even the inventor will soon forget. What is important is that terminology be used that is understandable across the worldwide professional community, popular literature and people in general. We need to be able to communicate not only with each other, but with knowledgeable people on the street.

Too much time and thought have been expended on the term *rock art* that would be better spent on furthering our analyses of the subject matter. It is irrelevant if it is called *rock art*, *cultural graphics*, or some other catchy phrase. What is important is that the name is recognised and designates the field of study, and the sites and subject matter that pertain to that study.

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Reflections on North American archaeology and rock art

By ALANAH WOODY
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B. K. Swartz's thoughts on the practice of North American rock art research are thought provoking and reflect his long, and considered, interest in the field. We hope that his comments are the starting point for a reflexive debate about the relationship between the archaeological study of rock art in North America and culture heritage management.

We disagree with Swartz about the necessity of finding an alternative to 'rock art' as a name for our object of study. We believe that the term 'rock art' is an entirely satisfactory one for a number of reasons. First, and not least, the term has come to be generally accepted and, more importantly, to be widely understood by archaeologists and avocationals alike as an inclusive term for all forms of culturally meaningful human-made markings in the physical environment. From the more commonly known petroglyphs, pictograms and cupules, to less well-known types, such as incised stones, intaglios and rock alignments, 'rock art' is understood to refer to all these cultural phenomena. It is, perhaps, one of the reasons the title of Heizer and Baumhoff's (1962) *Prehistoric rock art of Nevada and eastern California* uses it, as they discuss more forms of rock art than just petroglyphs. In important ways, these diverse cultural objects share significant characteristics; they are all the product of human creative expression, imbued with symbolic meaning, rich in interpretive potential, and constitute a system of visual representation that is embedded in social communication.

Archaeologists and rock art researchers should get over their unease with the term 'art' and stop misunderstanding art as something that is solely aesthetic and non-pragmatic. Social and cultural anthropologists have had fewer problems in using the term 'art' to describe the anthropology of visual representation (as well as other forms of cultural behaviour through which social and cultural meanings are expressed) (e.g. Layton 1991, 1992; Ucko and Rosenfeld 1967). Even Franz Boas, Alfred Kroeber and Julian Steward understood that symbolic culture should be considered as a fundamental part of society, so much so that they focused on the topic in their own research. Of course, it should be recognised that not all rock art functioned as art — and indeed, the same body of rock art, during its use-life, may sometimes function as art (when it is incorporated in social practices and theories of being) and sometimes not (when it is either ignored or it is not used as a vehicle of symbolic communication).

Swartz's second point, that North American rock art research would benefit from greater involvement and interest by professional archaeologists, is an important point and one that we agree with. Unlike other parts of the world, it is rare in North America for universities to have programs that teach rock art, or encourage students to conduct research on it. North American archaeologists are generally more comfortable dealing with archaeology that is readily quantifiable and classifiable, such as debitage scatters and historic can dumps, than something seemingly as intractable as rock art. Because rock art theory in North America emphasises religious and ritual explanations, to professional archaeologists it seems as if its study requires specialised theory that has little or no purchase on general archaeological theory. Yet, such theory has raised considerable public interest in the field, something that many North American archaeologists are uncomfortable with. All these factors have contri-

buted to a separation between rock art research and professional and academic archaeology in North America, making rock art research a study domain where avocational archaeologists and organisations play a prominent role (Quinlan 2007: 1–2).

In the face of general professional disinterest since the 1970s, avocational archaeologists have taken up the challenge of understanding North American rock art with great enthusiasm and dedication. Organisations such as the American Rock Art Research Association have provided an important forum for debate, and have attempted to foster communication between avocational and professional archaeologists. As Swartz notes, greater academic and professional interest in North American rock art research is essential for the discipline's future. One important benefit would be to promote involvement in rock art preservation and research by Native American communities. Another benefit would be to expose the public to a wider range of archaeological thinking on rock art research than is currently the case. Public interest in the field should be welcomed and harnessed to the important task of protecting rock art sites from the various conservation threats that they face. Academic research is important, but needs to be supplemented by culture resource management practices that include Native American communities and the public as part of a strategy of long-term site conservation.

As an aside, the Nevada Rock Art Foundation was founded in 2002, so it is unclear how it could be part of the reason that Swartz's *American Committee to Advance the Study of Petroglyphs and Pictographs* was founded in 1978! NRAF is a non-profit organisation with a staff of professional archaeologists and a Board and Advisory Council that include anthropologists, Native American elders and community leaders. Its mission is to document and protect rock art sites in Nevada and surrounding areas, and lead programs of public interpretation. In all its activities, public support is essential and is one reason we wish to see the development of a better relationship between professional archaeologists and the public in North America.

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Reply to R. G. Bednarik's *RAR* Comment on FINGER FLUTINGS IN CHAMBER A1 OF ROUFFIGNAC CAVE, FRANCE by Kevin Sharpe and Leslie Van Gelder, *RAR* 23: 179–198.

More about 'More about finger flutings'

By KEVIN SHARPE and LESLIE VAN GELDER

We are grateful to Robert Bednarik for this opportunity to publish in the journal he edits and to reply to his response (2006) to our paper. He has played a significant role in the development of the study of finger flutings and we very much seek to carry this work further in the empirical spirit he seeks to uphold. Thus, while we appreciate the positive things he says of our work, we also open the various points we try to establish in the paper to qualification, alteration, or even abandonment according to the evidence available. The points below go through those Bednarik makes, one by one.

The first point for us to respond to concerns the cave floor and its alteration over time. A good portion of the current floor does not resemble the Pleistocene floor, let alone its levels, and obviously so because it has been extensively altered since the 1950s to construct a train line through it. The train and its accompanying destruction of the floor does not venture into Chamber A and its sub-chambers, however, and thus the most obvious floor changes, those related to the train, do not apply to Chamber A1. Further, some of the train line along the section of Chamber G leading up to the 'Great Ceiling' passes by cave bear hibernation pits, suggesting that the floor here is still much the same (apart from that altered for the train) as during the Paleolithic. (Also of note are the several instances of bear scratches over human markings, including a broad clawing [suggesting it may have been done by a cave bear] over a mammoth drawn in the ceiling [Barrière 1982: 36, Fig. 74, the three lines either side of the mammoth's belly; or Plassard 1999: 48]. This information, when juxtaposed to the more common scene of human markings over bear claw marks, suggests that the relationship between the human and bear occupation of the cave is not mutually exclusive. Hence the floors of the cave that each species used may have a complex relationship.)

The chief issue at this point is the level of the floor in the fluted sub-chamber of Chamber A1 at the time the ceiling was fluted. We have suggested it was much the same as at present. Bednarik disagrees, his first point involving the bear pits and scratches, or at least the lack of them in the fluted sub-chamber. On the other hand, numerous chambers in Rouffignac do not have cave bear pits and (intensely) scratched walls, which suggests that either they were not accessible to the bears or else that the bears were selective in where they hibernated and left some chambers untouched. Without evidence that the bears would use any and all available space in a cave for hibernation, it is premature for Bednarik to claim (2006: 196) that the lack of scratches and pits in Chamber A1 indicates 'the floor was higher in the past, and that access to A1 may not have been possible to cave bears'. The bears may not have ventured into A1 for reasons other than its inaccessibility.

The only positive evidence that we can think of that places some limit on the floor height is a fluting that runs down a wall in the fluted sub-chamber to approximately 60 cm above the current floor. This could of course mean the floor was up to 60 cm higher than at present.

Bednarik wishes to employ Occam's Razor to say the floor level has changed markedly since the flutings were made, since this offers a simpler explanation than that it has remained much the same in height. To say this he has to believe that the floor could have been markedly lower than at present just as readily as it could have been markedly higher. His statement (2006: 196) that 'the alternative explanation, that the floor was higher at the time the flutings were made', is incorrect unless he can show that floor levels only ever fall, which we are sure he would not suggest. Some chambers (e.g. Chamber L) in the cave show currently active clay deposits and may indeed be rising. If the floor were lower, then the same suggestion as we made, namely that the children had to be held up, would again make sense.

Whether the children were held up or not, the point that children made the flutings still holds and the interesting question as to what was going on in

Chamber A1 still rises to the fore. That is the real issue to discuss.

What Bednarik says about the mining of clay for agriculture rather than ceramics is informative. We discussed the latter use because we have been told several times by a researcher of Rouffignac that the clay was mined in the fluted sub-chamber of Chamber A1, and that pottery is what it was used for. As far as we know, this opinion has not been published. No one has pointed us to hard evidence for this claim either. In part, the insistence may be prompted by the discovery of pottery shards in various chambers in the cave, including in Chambers A and E (Barrière 1975; 1982), the latter being another place we have been told mining took place. We accept that the case against mining has not been made irrefutably. On the other hand, if, as Bednarik thinks, the floor level was markedly higher when the flutings were made and when any mining took place, the proximity of the floor to the ceiling makes it even more possible that mining there might have led to marks on the ceiling.

The mining of chert from the ceiling — Bednarik's suggestion — would leave obvious marks whereas, in the fluted section of Chamber A1, very little of the ceiling shows any marks apart from the flutings, modern graffiti and modern scrapes; only a few fossil shells are absent and a couple of chert nodules are missing. Despite what Bednarik thinks, we can say that there was probably no mining of the chert from the fluted ceiling.

Our point about the making of zigzags was to contrast the making of them by wrist movement (obviously involving the arms and fingers as well) with a more whole-body movement that involves the hips (and therefore obviously also involving, as Bednarik writes (2006: 196), 'wrist, elbow, shoulder, … and legs'. What we wrote (Sharpe and Van Gelder 2006: 185: 'The curves of zigzag made by wrist movement differ from zizag curves made by hip movement') is a simplification but is not, as Bednarik writes, 'oversimplification in the pursuit of explanation'. Of course, a greater study could be made of what is needed to create the various flutings found here or anywhere else.

Of course we do not suggest that 'cave water as such … dissolve[s] limestone', as Bednarik implies (2006: 196) we do. The flutings in the fluted sub-chamber of Chamber A1 were mostly made visible in clay. From much experience with washing overalls used in this sub-chamber, water does remove this clay, thank goodness. The point in this section of our paper concerns the survival of these clay flutings if the sub-chamber were flooded.

We agree with Bednarik (2006: 196) that 'intentionality' can be 'a very rubbery concept' and that its use in the context of the flutings in the fluted sub-chamber of Chamber A1 needs careful definition. To this end, the discussion requested by Bednarik would require far more space and development than this response to his comments allows, but is an important challenge to be followed up. In the meantime, it would be helpful if Bednarik would supply his basis for why the reasons we provide for intentionality, besides that of the sub-chamber's morphology, are inadequate. Just saying they are not does not really help.

Bednarik then asks us to comment on the antiquity of the rock art in Rouffignac. This becomes a much larger issue than our paper hopes to cover, and Bednarik opens the question up even further by asking it for all of the cave and not just the fluted sub-chamber of Chamber A1. The comment below about C14 dating of charcoal associated with flutings in Chamber E offers one approach to this issue. Another is a discussion of the merits of the stylistic approaches (based on Breuil's or other scholars' schemas) and how they now appear inadequate for dating such sites as Rouffignac, given the absolute datings published for Chauvet Cave (Clottes 2003) — besides the many contradictions they present. One could discuss the animals depicted in the cave and their extinction in this part of France. And then one could look at, as Bednarik writes (2006: 196), 'the state of weathering, … the compositional properties of the red "patina" ', and the relationship of the flutings in the fluted sub-chamber of Chamber A1 'to other features, especially other types of speleothems clearly present (and quite possibly datable).' These are important subjects for continuing research.

The next section of Bednarik's critique centres on Figure 6 from Chamber E: 'What I see in it is a series of sub-parallel finger flutings, some of which bear compressed, smeared remains of the red surface deposit'. The information on this chamber provided in our paper was very brief since another paper is being prepared on it, plus some of the research needs confirming in situ. The point of including it in our current paper is of course not to provide a full presentation but to use it as an indicator.

1. Bednarik writes (2006: 196) about this: 'I regard the feature described as "careful re-layering with clay over the flutings" as entirely fortuitous; the fingers of the fluter were simply coated with red sediment'. Yes, in this cluster and in the other similar clusters in Chamber E, much of the clay within the flutings was probably applied unintentionally from the residue on the original fingers doing the fluting. However, at least two of these flutings have then been scratched with a charcoaled stick and then clay has been applied over the charcoal scratch: fluting, then charcoaled scratch, then clay over both. This sequence is clearly obvious on close examination. In another cluster in Chamber E, one fluting has been gone over, as can be seen by their different ending points, and the second has a different-coloured clay on it from that on the other flutings in the cluster, different also from the clay in the immediate locality.
2. The cluster pictured in Figure 6, plus the other

similar clusters in Chamber E, have been analysed internally giving their order of manufacture plus the direction in which they were made. This was reported, with PowerPoint slides, at a conference at which Bednarik attended. There are several motifs or symbols portrayed in these clusters too, making them an even more 'suberb study base to tackle the difficult question of symbolism', as Bednarik writes (2006: 197). To top this all off, a young child (from three-finger widths, probably one of those active in Chamber A1) has fluted above these clusters and probably while on the shoulders of one of the adults.

3. Bednarik then reiterates a point we made in the PowerPoint presentation, that these flutings and scratchings are ripe for dating. To heighten the opportunity here is the fluted clay over the charcoaled scratch over a fluting, meaning the charcoal here is probably from the time of the fluting. What we now need to actualise this first opportunity for an absolute dating of Rouffignac 'art' are permissions and finances.

Concerning our 'repudiation of previous interpretations of the Rouffignac finger flutings,' Bednarik writes (2006: 197) that they are 'simply part of that huge corpus of nonsense that has been written about cave art over the last century. But it was written many decades ago, and we have moved on since then'. This, unfortunately, is not quite so. The 'nonsense' still continues, even unabated, in the popular and scholarly media including on Rouffignac.

We are pleased that Bednarik supports our work in Chamber A1 of Rouffignac Cave; hopefully he also supports it in the many other places in the cave that are revealing a large amount of useful information, some of which he has yet to hear or read. We hope our work there will continue. We also find constructive dialogue about our work, especially suggestions as to subjects and research design very useful.

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RAR 24-828

Progress with rock art protection in Tasmania

In *RAR* 23: 119–122, Peter C. Sims reported the significant and endemic problems with rock art vandalism on the north-western coast of Tasmania. As a result of his submission to the Tasmanian state government, two submissions by the IFRAO Convener and a number of adverse media reports, the government has recently responded by commencing positive steps towards an improvement.

In particular, we have been critical of the great disparity between, on the one hand, maximum fines for vandalising historic heritage and indigenous, and on the other hand between prescribed penalties in the more progressive states of Australia (and other countries) and the parochial state of cultural heritage protection on Tasmania. We have highlighted that the maximum fine for destroying rock art is currently $A1000, whereas the maximum fine for vandalising historical heritage is $A500 000. Several other inadequacies of the relevant legislation and particularly its implementation have been highlighted by us.

The Premier of Tasmania advised us in June 2006 that the relevant legislation would be 'investigated, as will the option of increasing fines', and other measures were also under consideration. The Minister responsible, Hon Paula Wriedt MHA, informed us two months later that she visited the site of the most recent vandalism and conceded that current legal and physical protection are both inadequate, and that she was 'determined to have new legislation developed'.

In January 2007 the state government, through the Department of Tourism, Arts and Environment, advised IFRAO that the new *Tasmanian Aboriginal Heritage Bill* is scheduled for release and consultation in mid-2007. Judging from ongoing discussions it will have provisions for fines in line with most mainland states (the case of Western Australia is of course irrelevant for comparison) and will hopefully modernise the relevant protection provisions for rock art in Tasmania.

R. G. Bednarik

BRIEF REPORTS

The crisis in Lascaux: update March 2007

By MELODY DI PIAZZA

Contrary to statements by French officials, Lascaux remains in extremely critical condition. Since international attention was drawn to the plight of the cave by the 16 May 2006 *Time* cover article, 'Heritage at risk', the French authorities have closed ranks and are issuing *false and misleading statements* (see below) about the condition of the cave. According to their statements in public interviews, the cave is now recovering and the crisis is over. This could not be further from the truth.

The cave is rampaged by a proliferation of black spots as large as human hands, which have now begun to cover some of the paintings. Authorities were woefully slow to act in identifying and treating when the spots first appeared last year. Today, Lascaux and its paintings are suffering from the ineptitude and lack of response of those charged with the care of the cave. The ill-fitted air-conditioning machine is completely shut down. The cave has no means of circulating its natural currents of air. Moisture is building; water can be seen running down the paintings. The once sparkling white calcite canvas of Lascaux is now grey.

Current conditions in the cave

- The bacterial and fungus infection inside Lascaux is NOT under control.
- The proliferation of black spots, which was first reported in 2006, continues at an alarming rate. Biologists have not yet identified the nature of these spots and, consequently, have not prescribed a proper treatment to irradiate the spots.
- The head of the famous 'black cow' is now completely covered by the black spots.
- Even without a diagnosis on the black spots, major disagreements on their treatment exist between those in charge of Lascaux. Some want to begin another round of disastrous antibiotic spraying inside the cave. Others are virulently opposed to this.
- The ill-fitted air-conditioning machine is completely shut down and still in place.
- The cave is very wet and water can be seen running down the walls covered with paintings.
- The temporary roof set up in 2000 (after the removal of a permanent roof) to aid in the installation of the air-conditioning machine remains in place exposing the cave to sudden variations in exterior climate and precipitation.
- The white calcite, which gave the paintings of Lascaux their brilliant canvas, has turned grey.

Critical issues remain

- Authorities refuse to address the major problem which led to the current crisis in Lascaux: the ill-conceived, ill-fitted air-conditioning system.
- Continued lack of expertise to correctly and immediately identify and treat the black spots now rampant in the cave.
- There is still too much human presence in the cave on a regular basis.

Statements by authorities, from le Figaro, *13 December 2006*

- Isabelle Pallot-Frossart, Director, Laboratoires de Recherche des Monuments Historiques (LRMH): Pallot-Frossart said that today's administrators of Lascaux are simply picking up the pieces and cleaning up the mess caused by the crisis in the cave forty years ago. She intimates that this current crisis is nothing more than a continuation of the old crisis.
- It must be noted, however, that the First Scientific Commission was able to bring to resolution that earlier crisis and that the cave of Lascaux and its precious paintings remained safe and in excellent health from the 1960s until 2000 when construction on the new air-conditioning machine began.
- Jean-Michel Geneste, curator of Lascaux: Geneste, referring to the recent crisis, admitted there was panic because no one understood what was happening in the cave. He said they were very afraid that the *fusarium* would crawl across the walls and cover the walls and damage the ancient paintings by creating fissures and breaking down pieces of calcite. To say this, Geneste implies that these things did not happen. Indeed they did.
- The *le Figaro* article, '180 degree ecological turn around for the Lascaux Cave', in which the above comments appeared, also reviewed the Lascaux dossier published in *Monumental* (Nov. 2006). *Monumental* is the official magazine of the Laboratoires de Recherche des Monuments

Historiques (LRMH). The LRMH is under the direction of Isabelle Pallot-Frossart and is one of four administrations in charge of Lascaux.

- The *le Figaro* article reflects the new alarming message of the *Monumental* dossier, which promotes the notion of *preventive conservation* around Lascaux *without addressing* the causes or possible remedies to the *yet unsolved and still critical conservation crisis*. Of note is the refusal of authorities to remove the ill-fitted air machine and replace it with one based on the operation of the old air-circulation machine, and their lack of attention to the black spots now covering the cave's walls and some of its paintings.

The ICPL continues to call for a truly independent, international committee of scientists and experts in cave art and its conservation to monitor and report to the world on Lascaux and its health.

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RAR 24-829

Holocene petroglyphs at Philippi, Greece

By GEORGE DIMITRIADIS

The petroglyphs in the region of Philippi were probably carved by the Hedones, a Thracian tribe that occupied the lands stretching between the Strymon and Nestos rivers and from Mt Pangaion to the Rodopi mountain range. The engraved rock surfaces that are found two kilometres from the historic town of Philippi may narrate unknown events. The archaeological evidence suggests a dynamic culture undergoing a religious transformation process of an organised society in transition from the Late Bronze Age to the Early Iron Age. Following initial research by the author (1998–2000) and financed by the Municipality of Philippi, a joint co-operation between HERAC- IISL-ALA has been initiated in 2004 and the 'Hellenic Rock Art Documentation Project' (HRAD) started a full investigation of the rock art in the area of Philippi. The project has been approved by the Greek Ministry of Culture and is financed by 'Culture Enterprise' of the Municipality of Philippi and by the Ministry of Macedonia/Thrace.

Geomorphological aspects

The area being investigated is located in East Macedonia. The geological substratum is composed of granite, syenite, diorite and marble. During the Holocene the plain of Drama or plain of Philippi was characterised by marshes and alluvial deposits. Since the ninth millennium BP, the climate in the region remained relatively stable, and pollen and pedological analyses prove that the area was covered by woodlands (Davidson-Thomas 1986).

The HRAD research area is delimited by Mt Phalakro (2111 m) and Mt Orvilos (1888 m) to the north; Mt Pangaion (1965 m) and the Philippi Marshes (now drained) to the south-west; and Mt Symbolo (694 m) to the south-east.

Archaeological context

The territory was inhabited at least since the Neolithic (Renfrew 1971; Todorova 1978; Koukouli-Chryssanthaki and Romiopoulou 1992; Treuil 1992; Demoule 1994; Kalogirou 1994), as the settlement of Dikili Tash (c. 5000–3000 B.C.E.) proves, and the area remains settled also during the Bronze Age (3000–1050 B.C.E.) and Early Iron Age (1050–700 B.C.E.), but periodic cycles of desertion and re-population have been recognised (Papadopoulos 2002).

Historical evidences

The presence of Hedones in the plain of Philippi is well documented by early Greek authors: Herodotus (VII, 123), Thucydides (I, 58, 2; II, 99, 4: '[...] και πέραν Αξιού μέχρι Στρυμόνος τήν Μυγδονίαν καλουμένη Ηδωνός εξελάσαντες νέμονται [...]'; IV, 109), Strabo [VII (C329), 11: 'Παίονες δέ (τά) περί τόν Αξιόν ποταμόν καί τήν καλουμένν διά τούτο Αμφαξίτιν, Ήδωνοί δέ καί Βισάλται τήν λοιπήν μέχρι Στρυμόνος [...]' and VIII [C331], 36). Textual discordances on their exact location are partly justified by the nomadic character of the Thracian tribes.

The rock art sites

Rock art sites of Prophet Helias (πρ. Ηλίας Φ/ Β.1-1α, 1β, Β.2, Β.3-Eiv) and Mana (Μάνα Φ/Β.1, Β.2, Β.3, Β.4-Eiv) are located in the Municipality of Philippi, parish of Filippoi in Kavala Province.

Methodology of documentation

Phase I (DRP)

The fieldwork was carried out in three years and focused on the area of Prophet Helias. A survey of the area was carried out to record essential environmental data and signs of ancient anthropic activity. Gallery mines for the extraction of iron minerals were in use until the 18th century and probably were already in use in antiquity. Tracing was carried out on PVC standard sheets (90 × 120 cm). Two engraving techniques were distinguished, 'hard pecking' and 'filiform'.

Three panels were studied:

- πρ. Ηλίας Φ/Β.1 (1.85 × 1.75 m in an excavated area of 6.10 × 5.20 m). It is an emerging cliff in good state of conservation.
- πρ. Ηλίας Φ/Β.2 (13.50 × 6.50 m). It is a flat slab in

a good state of conservation. About 500 m from both rocks there is a stream. Nearby there are two iron ore mines, now out of use.

- πρ. Ηλίας Φ/Β.3 (7.70 × 5.00 m). The rock is in fair state of conservation. At its base was found a small stone flake now kept at the historic archive of Philippi's Cultural Association.

Statistical analysis

Over 300 unique motifs have been recognised on rocks of the Prophet Helias area. Initial calculations indicate that schematic anthropomorphous figures account for 50% of the repertoire. 'Animals' account for a further 45% of it and 'horsemen' account only for 5% of the repertoire.

Phase II (HRAD)

In the second phase, the hypothesis proposed by Dimitriadis (1999b) about the possibility that the Prophet Helias area was an open-air sanctuary of the Hedones has been tested. Exposing more surface of the known engraved rocks and completing the tracing on sheets of all carved rocks has revealed more petroglyphs.

New discoveries

Prophet Helias area: during the accurate cleaning and stratigraphic exploration of the area B1, a pit hole full of darkish sediment was unearthed. C. Prestipino has determined in the laboratory the presence of charcoal. Fragments of dark and red coarse ceramic vessels have been dated to the Late Bronze Age after optical examination by A. Vianello, which collimate with the evaluation of the rock art style examined by the author. Preparation of photographs and drawings as well as the cataloguing of potsherds are being carried out by I. Mailland and A. Vianello. F. Coimbra has discovered a new carving labelled 'Philippi's horse'. It is a small-scale 'horse', in naturalistic style, found during the opening of a new sector on rock πρ.Ηλίας Φ/Β.2 and studied by the author. The 'horsemen' figures are being studied by G. Iliades (HERAC). The author believes that the rocky surface, the pit hole and the engraved 'horseman' figure on rock B.1 may be linked, after considering the archaeoastronomical annotations written by A. Gaspani (Astronomical Observatory of Brera-Milan). The shadow of a wooden stick inserted in the pit hole was projected on the horseman during the summer solstice on Friday, 21 July 2006.

Mana area: three new rocks (Μάνα Φ/Β.2, Β.3, Β.4-Eiv) have been discovered after S. Foustopoulos recognised petroglyphs on a rock. Mana is located in a strategic position along old mountain passages connecting the small valley of Mesorema with the plain of Philippi. The geomorpology of rock Μάνα Φ/Β.1 is under study by D. Cardoso (Museum of Guimães-Portugal) in order to establish the conservation action to be undertaken in the future.

Photogrammetric survey of rocky surface

During the 2006 fieldwork season A. Vianello and D. Delfino have attempted a photogrammetric and GIS survey of the rocks. GIS software packages are also being used. A fixed frame (0.60 × 0.80 m) has been used as reference after the impossibility to use specialist equipment due to the uneven nature of the terrain. Particular care has been taken to maintain the petroglyphs under adequate natural light; the photographs have been therefore taken in batches over the course of a week. The resulting photographs will require substantial post-processing and are intended to become a tool of research and experimentation to further the study of petroglyphs in their original context.

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RAR 24-830

ORIENTATION

Dampier rainwater as acidic as beer: CSIRO

ROBERT G. BEDNARIK

On 25 July 2002, in direct response to a report about the destruction of Dampier rock art (Bednarik 2002), the State Government of Western Australia announced that it would conduct a four-year study of this issue. On 16 October 2002, the then Premier, Dr G. Gallop, announced a committee of nine members, the Rock Art Monitoring Reference Committee (RAMRC), to oversee this project. Exactly four years later, on 17 October 2006, the government released a report on the results of only the first of these four years of study, conducted by a supposedly independent team from CSIRO (The Australian Commonwealth Scientific and Research Organisation).

This raises some very pertinent questions: why did it take four years to present the outcomes of just the first of four years of research? Is the project team that produced this report free of influence from government agencies? Does this report exonerate the government from the accusation that its policies are destroying the Dampier rock art, or from the responsibility of managing this world-class cultural monument? These are the principal questions examined here.

The first two questions are easily answered. The incredible delay is attributable to government procrastination and dithering. Although the RAMRC was established three months after the initial announcement, it took to 16 July 2003 to invite interested parties to conduct this study, i.e. a full year. It took another year, to 12 August 2004, to commence the project by CSIRO, which the government announced then as a unique and advanced pioneering study, 'the first of its kind in the world'. CSIRO (2006) produced a report for the first year of its work (August 2004 to August 2005) on 10 April 2006, but the government delayed its release for another six months. At the same time it became known that the study had run out of funding. Therefore the history of this project indicates a scandalous inefficiency of the government.

The second question, was there government influence in the project, is just as readily clarified. One of the members of the CSIRO team is not a scientist of that organisation, but is none other than Bill Carr, recently of the Department of Industry and Resources, currently the Director of the Conservation Commission (CC 2006) of Western Australia. At the DIR, he was responsible for defending the government from accusations of rock art vandalism, and appointing him Director of the Conservation Commission is like placing the fox in charge of the chicken coop. It refutes the idea that the government lacks a sense of humour or irony. The fact that Carr has served as a member of the CSIRO team, despite his significant conflict of interest, severely questions the independence of this report.

Which brings us to the third question to be considered. The value of the CSIRO report should be judged solely on how well it meets the original objectives of the project. The principal objective, as stated on its page 4, was to 'investigate and report on impacts of proposed industrial developments on the rock art of the Burrup'. The RAMRC formulated three research questions to be investigated by this project:

- Is the natural weathering of the rock art of the Burrup Peninsula being accelerated by industrial emissions?
- Is there a significant and measurable problem?
- If there is a significant issue, what management approaches are recommended?

The project has not clarified the first issue, and has hardly even attempted to do this. The second issue remains unanswered, hence no attempt was made to address questions of management. Moreover, the initial objective, to report on the 'impacts of proposed developments' (such as the Pluto plant and others) was completely ignored in this report. No modelling of any kind was even attempted, and in that sense alone this report is significantly inferior to previous studies of the impact of Dampier industry, such as those by Sinclair Knight Merz just a few years ago. The project has therefore failed to deliver on any of its objectives, and in that sense it is an unmitigated failure.

It has, however, provided excellent basic data on the quantification of some of the many relevant airborne pollutants, and in that sense offers substantial justification for the concerns first expressed five years ago (Bednarik 2002). Most important of all, it provides unequivocal confirmation that acidic precipitation occurs for most of the year. Although the data are highly fragmentary (two of the samplers are

said to have 'experienced problems'; CSIRO 2006: 1), covering only a few weeks at each of only five of the sampling sites, they suffice to show that acid rain occurred in eighteen out of twenty periods checked (CSIRO 2006: Table 13a). Acid rain is precipitation of a pH of <5.6 caused by anthropic agents, especially industrial emissions. At Site 8, the pH was 7.5 and 5.8 respectively in two periods, but in the eighteen other periods it fell between pH 4.3 and 5.3, with a mean of pH 4.597. This represents a ten-fold increase in acidity (reduction of hydrogen ion concentration) from the upper limit of acid rain. It means in practical terms that the rainwater at Dampier has the acidity of beer, but is slightly less acidic than lemon juice.

The rainwater pH of Dampier was mostly in the vicinity of pH 7.0 and 7.2 in the 1960s (Bednarik 2002: 36), peaking at pH 7.6, and has fallen gradually since then, especially after the commissioning of the NW Shelf facility in 1980. On 29 June 2002, Pilbara MLA Fred Riebeling was quoted by The *West Australian Weekend Extra* as saying '[I]f the government produces acid rain [at Dampier] it will be an absolute tragedy. And the first time I see a reputable agency say that, then I'll take it seriously.' Perhaps Mr Riebeling does not regard the CSIRO as a reputable agency, but the CSIRO report does provide substantial evidence of acid rain at Dampier. The granophyre and dolerite rocks of the Archipelago typically lack acid neutralising capacity, and the ferruginous mineral crust covering all rocks is gradually degraded through the mobilisation of its cations, notably iron and manganese. Ford et al. (1994) have shown that a reduction in pH of 2.2 units in the Napier Range, Kimberley, has increased rock solubility by 230%.

This brings us to the most serious omission in the CSIRO study: it completely disregarded the crucial factor in the rock art deterioration, the erosion of the iron-rich rock patina. The percussion petroglyphs are not 'etchings', as they are naively called in the report, they were made by pounding through the mineral accretionary deposit, exposing the light-coloured weathering separating this dark-brown substrate from the unaltered rock beneath. The result of this technique is called a sgraffito. Thus the petroglyphs depend for their continued existence entirely on the preservation of the surface patina, which has provided the necessary colour contrast since they were created. The whole point of their conservation revolves around the need to prevent the mobilisation of this surface crust's cations, especially iron and manganese, caused by a lowering of the ambient environmental pH. The solubility of iron increases about 100 000-fold through the lowering of the pH from 8.5 to 6.0. Much of this change occurs in the pH range of 7 to 6, which represents a ten-fold increase of acidity (the pH scale is a decadial logarithm). Not only does the CSIRO report reveal a decrease of the precipitation pH from around 7.2 to an average of 4.6, its single measurement of pH 7.5 (16–23 March 2005, at sampling site 8) confirms that, under exceptional climatic conditions the pristine values are still achieved — perhaps once in a year. About fifty weeks in the year, precipitation (dew or rain) is in the form of 'acid rain', containing sulphuric, nitric and other acids.

It needs to be appreciated that the distribution of this dark-brown, ferruginous accretionary crust is a feature of the high-pH arid environments of Australia, and all similar environments of the world (e.g. parts of south-western U.S.A., Mexico, Arabia, Sahara). It is absent in any region of low-pH regimes, such as the Kimberley, and of course in the vicinity of any major city. The argument raised in the CSIRO report, that the air quality at Dampier is better than in many polluted cities, is irrelevant. The purpose of the CSIRO project was not to determine the effects of pollution on the human population, it was to establish the processes effecting the deterioration of the ferromanganeous accretion. It should be self-evident that sgraffito petroglyphs on such deposits would never survive in the air pollution of southeast-Asian cities, or even in Perth (cf. Rye et al. 1996). In referring to the conditions in polluted cities, the CSIRO team indicates that it has misunderstood the brief of its project.

There are also significant shortcomings in its design. For instance, it is well known that gaseous air emissions such as nitrogen dioxide travel enormous distances (Wenig et al. 2003), and the 'control site' CSIRO used in this project, at Mardie Station, is only 81 km from Dampier. It is undeniably within the zone affected by the Dampier fallout, as shown by several observations. A minimum distance of 200 or 300 km would be advisable, although even that would not provide true control data. To place the control station so close to the source of emissions was inappropriate. Similarly, the report's frequent comments about sampling sites 1 and 3 providing good background data are misleading. These sites are only 7 and 14 km respectively from the principal pollution source. Hence the report's assumptions made about background levels are false.

There are three basic methods of measuring air pollution, the passive, active and automatic sampling methods. These offer considerable differences in cost and reliability, the cheapest by far being passive sampling, involving no pumping of air. Automatic sampling methods are about 1000 times as costly. CSIRO has chosen to use the passive sampling method, no doubt for economic reasons, at the expense of precision and reliability. The method is unable to show maximum levels or daily variations, it simply provides a rough guide of average level over exposure period (Steinbacher et al. 2005). It is technically no more sophisticated than the methods used by independent environmental advocacy groups (such as the 'Bucket Brigades' in the U.S.A.), and its results are not legally accepted in the European Union. To claim, as the government has, that this study is

groundbreaking, is severely misleading. Vastly more sophisticated studies of the effects of industrial emissions on rock art have been conducted in other countries (cf. Bednarik 2002 for some references), and CSIRO's pollutant measurements at Dampier are low-budget versions of work done by that agency previously. The Melbourne laboratory of CSIRO certainly has the capability of conducting much more sophisticated research on nitrogen oxides (cf. Galbally and Roy 1978).

The quantified data provided by CSIRO does not support the recent claim made by the former corrupt Minister, John Bowler, that pollution is low at Dampier. (In February 2007, Bowler was sacked after the Crime and Corruption Commission of Western Australia sensationally exposed his corruption while a minister.) It shows that modelling predictions severely understated the level of air pollution (SKM 2003). For instance, the Dampier nitrogen dioxide concentrations are now said to hover around 2000 to 3000 ppt (CSIRO 2006: Fig. 10), whereas predictions had been about 200 ppt three years ago (op. cit.: Fig. 25). Compared to those of 4 ppt at Cape Grim in Tasmania (pers. comm. Rob Gillett, member of the CSIRO team), a site with relatively clean air, the Dampier levels are close to a thousand times as high as at a 'clean' site. This is hardly surprising; Dampier industry emits around 15 000 tonnes of nitrogen oxide per year, Woodside being the greatest polluter in Australia (cf. *National Pollutant Inventory*). In some countries such as Sweden, a nitrogen oxide levy is paid by industries emitting large quantities of this pollutant, but Australia lacks such incentive to reduce emissions.

In short, the CSIRO study has confirmed that the petrochemical industry at Dampier produces acid rain nearly all year round, but it has failed to investigate its effects on the rock art or the rock patina. It has therefore failed to address its terms of reference, which were specifically to study the effects of the emissions on the rock art; to assess the impact of future developments on the Burrup; and to advise on appropriate management measures. The project failed in all its terms of reference.

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RAR 24-831

The Burrup Blues

Right next to the coastline
In Ourstralia's North West
Is a whole lot of petroglyphs
Known as the world's best.

The local peoples call them
The witnesses of life itself.
Unfortunately that rock art is
Next to the North West shelf.

When Stonehenge was just a twinkle
In some Druid priest's clear eye
All them rocks up on the Burrup
Were under the Pilbara's blue sky.

And then a company called Woodside
Found gas to sell to the Japanese,
So they asked the late Harry Butler
Help us move them rocks, mate, please.

So them rocks they got plonked
Behind a tall galvanised fence
Desecrating our age old culture
Simply don't make common sense.

And old Harry Butler he passed away
And his list of rocks was went and lost
Now we're moving more of them rocks
And one day we'll surely pay the cost.

Now they have all been shifted
And that place is all erosion
And they'll all be destroyed
By a god almighty explosion.

M. J. McBain and Noel Nannup

Chilean court ruling

Further to the recent report about the proposed destruction of the El Mauro petroglyph complex in Chile (*RAR* 23: 261–263), Patricio Bustamente Díaz reports that there has been a crucial court ruling recently. It concerns the refusal of government agencies to provide rock art protection advocates with information they requested about the Rio Condor project. The court found that the Chilean authorities failed to provide access to four of the seven items of information requested. The requested information was of 'clear public interest'. Relevant parts of the court ruling are presented here.

Violation of Article 13, Right to Information

The court found unanimously a violation of Article 13 of the Convention (Freedom of Thought and Expression): 'With respect to the facts of the present case, the Court concludes that Article 13 of the Convention, which specifically establishes the rights to "seek" and "receive" "information", protects the right of all persons to request access to information held by the State, with the exceptions permitted by the restrictions regime of the Convention. As a result, this article supports the right of persons to receive such information and the positive obligation on the State to supply it, so that the person may have access to the information or receive a reasoned response when, for grounds permitted by the Convention, the State may limit access to it in the specific case. The said information should be provided without a need to demonstrate a direct interest in obtaining it, or a personal interest, except in cases where there applies a legitimate restriction. Disclosure to one person in turn permits it [the information] to circulate in society in such a way that it can be known, obtained and evaluated. In this way, the right to freedom of thought and of expression contemplates protection of the right of access to information under State control ...' (para. 77).

The court noted the connection between freedom of expression and information and rights of democratic participation, concluding that 'access to information held by the State ... permits participation in public governance' (paras. 84–86).

Principles governing restrictions on access

The court notes that exceptions should be allowed but that they should be limited to those permitted by the Convention (respect for the rights and reputations of others, protection of national security, public order, health or public morals) and stresses that these should always be proportionate and minimise restriction of the right being protected (paras. 88–91).

The court ruled that 'in a democratic society it is indispensable that state authorities are governed by the principle of maximum disclosure, which establishes the presumption that all information should be accessible, subject to a restricted system of exceptions'. (para. 92).

The burden is upon the State 'to prove that in setting restrictions on access to information in its possession it complied with the restrictions' laid out by the court (para. 94).

Principles applied to the present case

At the relevant time there was no legal basis in Chile for denying access to the information (para. 94).

By failing to justify their refusal to provide access, the Chilean authorities violated the principles that require restrictions on rights to pursue legitimate goals and are necessary in a democratic society (para. 95).

The failure to adopt precise criteria on exemptions in the domestic legal system 'creates ample room for discretional and arbitrary state actions in classifying information as secret, reserved or confidential' (para 98).

Obligation to take measures necessary to guarantee the right to information (Art. 2)

The court also ruled that the state should adopt means of guaranteeing the rights protected by the Convention — in other words, an access to information law or similar — and should eradicate norms and practices that violate the Convention rights. The court notes that 'in particular, this means a legal framework that regulates restrictions on access to information held by the State that should comply with the Convention standards and may only impose restrictions for reasons permitted by the Convention' (para. 101).

Violations of fair trial rights (Art. 8.1)

The court found that Chile had also violated the applicants' right to a fair trial in the context of the administrative and judicial proceedings brought by the applicants to challenge the denial of information (see para. 174).

What Chile must do

The court ordered the state of Chile to provide the information requested about the Rio Condor project or adopt a reasoned decision as to why it is not providing it (paras. 157–158).

The court ordered Chile to publish key paragraphs of the sentence in the state journal, and, importantly, to adopt the necessary measures to guarantee the right of access to state-held information in the future. These should include measures 'to guarantee the effectiveness of an adequate administrative process for dealing with requests for information, which sets deadlines for providing the information and is handled by properly trained officials' (para. 163). In addition, the court requires the state to train public officials on the right of access to information:

'In this case the administrative authority charged with responding to the request for information … showed an attitude that threatened the right of access to State-held information. In this respect, this Tribunal notes with concern that various elements of proof presented in this case coincide in showing that public officials do not respond effectively to information requests' (para. 164).

'The court considers that the State [Chile] should, in a reasonable time, conduct training for the bodies, authorities, public agents charged with receiving requests for information on the norms that regulate this right, including on the Convention standards that they should respect with regard to restrictions on access to such information' (para. 165).

Rock art studies: a bibliographic database

This valuable resource is a compilation in progress which was begun in March 1993. Currently the database contains over 16 000 citations to the world's

rock art literature, with an emphasis on English language and North American citations. Over 7160 of these citations are held in the compiler's personal library. These and many others were reviewed for annotation. The database is available on the Internet, as a project of the Bay Area Rock Art Research Association Archive, Bancroft Library, University of California, Berkeley.

A few comments regarding the background and objectives of the compiler, and the assembly of the raw data for inclusion in the database are in order. Leigh Marymor has been interested in rock art conservation and the literature of rock art studies for over thirty years. He is the Immediate Past President of the American Rock Art Research Association (2004–2006). He is a co-founder of the Bay Area Rock Art Research Association and has been its Co-chairperson since its inception in 1983. Leigh Marymor received the American Rock Art Research Association's Castleton Award for research excellence in 2002 in recognition of the significance of the compilation of *Rock art studies: a bibliographic database*.

The rock art studies database was initially conceived as a tool useful in cataloguing the rock art literature held in the compiler's personal library, which currently consists of approximately 80 shelf-feet of books, periodicals, grey literature, ephemera and research notes. As the project grew, additional citations were included from over seventy additional sources, including: bibliographies, research archives, library catalogues, on-line search services and other private holdings (these sources are notated in the 'library' field of the database, and are detailed in the text document, key to sources for citations).

The scope of the data included in the database has been limited by the time and energy available to the compiler, as well as by his subjective approach to keyword notations (found in the keywords field). The past emphasis in the database on English language literature has steadily given way to the growing inclusion of the French, Spanish, Italian and Portuguese literature.

Rock art studies: a bibliographic database is found at

http://bancroft.berkeley.edu/collections/rockart.html

WAC Inter-Congress: Archaeological Invisibility and Forgotten Knowledge

University of Lodz, Poland
5–8 September 2007

Vast amounts of cultural customs, traditions, material expressions and cosmological universes escape the trowels of archaeologists. The aim of this conference is to focus on this invisible web of reality that formed the foundation of hunter-gatherer life and on exploring ways of integrating this with the archaeologically visible aspects of pre-Historic culture.

This conference will also focus on forgotten knowledge, including the large amount of important ethnographies from the earlier centuries as well as more recent work. Much of this was written in languages that are not part of the current mainstream and consequently is little known and not included in current theoretical debate.

As globalisation continues apace, fewer and fewer pockets of traditional small-scale societies remain and this has created a sense of urgency in this important field of research. Leading ethnoarchaeologists from across the world will present their research while open discussion sessions will focus on key issues.

The conference will be organised around themed geographical sessions, and ethnoarchaeological film presentations. An excursion, included in the registration fee, will take place on 8 September 2007.

Conveners: Lucyna Domanska, Ole Gron and Karen Hardy
E-mail contact: *ethnoarch@gmail.com*
Web: *http://www.worldarchaeologicalcongress.org/site/invisibility.php*

ARARA news

ARARA is annually seeking nominations for the following awards:

- The Conservation and Preservation Award for excellence in the conservation and protection of rock art.
- The Wellman Award for excellence in service to the field of rock art.
- The Castleton Award for excellence in writing about rock art.
- The Oliver Award for excellence in rock art photography.
- The Frank and A. J. Bock Award for extraordinary achievement in rock art research over a lifetime.

For more information visit *www.arara.org*, e-mail or write to:
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The American Rock Art Research Association has announced the Call for Papers for its 2007 Conference in Billings, Montana, 29 June – 2 July 2007. Follow the link below for the Call for Papers document and instructions, the Basic Application Form for submitting your paper, and the combined Call for Papers

and Application Form in PDF format:
http://www.arara.org/2007_arara_paper_call.html

IFRAO Report No. 38

International Cupule Conference 2007

The Cochabamba Rock Art Research Association (AEARC), a member of IFRAO, is concluding the last details for the International Cupule Conference that will take place in Cochabamba (Bolivia, South America) from 17 July to 23 July 2007.

The academic event will be conducted during the first three days (17–19 July) at the Centro Pedagógico y Cultural Simón I. Patiño, situated in Av. Potosí, Cochabamba.

Registration will take place at that venue from 8 a.m. to 10 a.m. on Tuesday, 17th July, followed by the Inauguration at 10 a.m.

Participants may choose among the following hotels:

Portales ***** Single room US$52 (double US$60)
Aranjuez **** Single room US$45 (double US$50)
Diplomat **** Single room US$47
Regina *** Single room US$18 (double US$26)
Regina Apart. Hotel *** Single room US$25 (double US$33)
Anteus Apart. Hotel *** Single room US$23 (double US$28)

AEARC recommends Hotel Diplomat due to the special agreement between both institutions, in order to provide special treatment to the participants of the International Cupule Conference.

The excursions will take place from Friday 20th July to Monday 23rd. During the main excursion, the first day will include the area of Tarata (close to the city of Cochabamba) where impressive and unique cupule sites have recently been discovered. Arrival at Mizque will occur during the afternoon of the same day. The remaining three days will be dedicated to visiting different sites in Mizque, returning to Cochabamba on Monday 23rd July, during the afternoon. Arrangements will be made for those rock art scholars who wish to carry out one-day excursions to nearby sites (such as those below the Tunari Mountain), or visit other sites within or outside the Department of Cochabamba.

Excursion prices will be announced during the conference. Hotel bookings can be made through AEARC's E-mail (*aearc@hotmail.com*). Cheaper hotels can also be booked through AEARC.

AEARC looks forward to welcoming the world's cupule experts at this first International Cupule Conference.

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The IFRAO homepage (Italy)
http://www.cesmap.it/ifrao/ifrao.html

The Australian homepage of IFRAO
http://mc2.vicnet.net.au/home/ifrao/web/index.html