Comment on
SOUND REFLECTION AS AN EXPLANATION FOR THE CONTENT AND CONTEXT OF ROCK ART

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FURTHER COMMENT

Epistemology and Palaeolithic rock art

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A number of speculations about the meaning or significance of rock art have been based on specific variables of the sites in question. Rock art sites offer an inexhaustible supply of characteristics that could be quantified and interpreted in any way one chooses: morphology, geology, mineralogy, biology, hydrology and acoustics can provide some of them. For instance, a typical example of an interpretative speculation predicated on hydrology is the 'abnormal water' hypothesis (Bahn 1978) which suggests a connection between rock art occurring in a limited series of Franco-Cantabrian caves, and springs in these same caves. All of these hypotheses focus on some perceived common characteristic of the sites, and if it seems to agree with the distribution pattern of the art concerned, it is proposed that there is a correlation. The potential for such hypotheses is practically unlimited, because the supply of variables available for consideration seems limitless.

The question of the 'crucial common denominator of a phenomenon category' (CCD) needs to be satisfied in some form before the empirical evidence of such correlations can be accepted as relevant (Bednarik 1990/91). This question is intrinsically connected with the taphonomy of the art and of its support (Bednarik 1994a). The CCD of the corpus of rock art one *seeks to* examine (not necessarily the same as the corpus of rock art one *does* examine) is in this case the cultural motivation for the art — the reason why it was produced, why in the location where it occurs, and why in the form in which it occurs. As in all determinations of common denominators of taxonomic systems, this should not be a simple process of latching onto the first correlation one happens to perceive: common denominator variables often seem to correspond to a set of data or a mental construct to which they are in fact not related, and this fundamental truth has led to more erroneous interpretations in science than any other factor. The problem is simply that it is not obvious which of a phenomenon's variables the CCD is; it can be extremely difficult to determine, and the two methods that should guide us in this process are refutation and the theory of taphonomic logic (Bednarik 1992, 1994a).

Dr Waller's hypothesis of acoustic motivation for rock art is scientific because it can be rendered refutable, and in addition it is susceptible to various procedures of taphonomic logic. The idea of many archaeologists, that the meaning or significance of rock art can be gleaned by simple interpretation of its empirical indices (e.g. those of distribution, composition and statistics) is a massive fallacy, and it is so at two levels: firstly, at the logical level, because these indices are almost entirely taphonomic characteristics of the evidence, and not archaeological or cultural indices, as archaeologists are often tempted to think; secondly, at the epistemological level, due to the significant limitations of anthropocentric empiricism.

Waller's bold and imaginative hypothesis is taphonomically unsophisticated. He writes of 'the European cave art tradition, the canyon art of the Americas, the Australian shelter art', as if these were taxonomic definitions of archaeological significance. They are not even descriptively sound (*South* American 'canyon art'?), but if taken through the taphonomic filter they emerge meaningless. Upper Palaeolithic rock art was never endemic to caves, its occurrence in them is itself a largely taphonomic phenomenon and thus has almost no value for its interpretation: almost certainly, a non-crucial common denominator of the phenomenon category was interpreted as being crucial, when in fact the spatiality of this art is merely a simple geomorphological phenomenon. So if Waller wishes to determine whether Upper Palaeolithic rock art had an acoustic motivation, he would need to study the acoustic properties also of sites of the 99.9% or so of the art that has not survived, principally because it was not located in caves. This is impossible, hence any statistical findings from his work are fundamentally irrelevant: they cannot refer to the hypothetical art tradition.

This does not refute his hypothesis, however, it merely shows that the same characteristics that enabled a tiny remnant of the art to survive may also be related to acoustics — such as concavity of the decorated rock panel — in which case the acoustics of the sites would be exactly as relevant to the motivation of the art as the geomorphology of the sites. This is not only logical, it seems also sensible: rock art tends to survive best in sheltered locations, and they are the ones most likely to possess good acoustic reflectivity. As Waller notes, 'a general feature that [the sites] have in common is the rock surfaces'. This tautology (there is no rock art without rock) brings us to a number of practical weaknesses of Waller's hypothesis: the vast majority of the rock art sites of the world cannot be expected to possess acoustic properties of the sort he tries to locate: they have no echo, or they have no images that could conceivably relate to sound. More than one half of the world's rock art sites consist of rock pavements, low outcrops or boulders, or other sites that have no noticeable characteristics of sound reflection. Many sites comprise depictions of both carnivores and herbivores on the same panels; and at some we have the interpretations of the traditional custodians of the art, sometimes even of the artists themselves, and there is no hint of acoustic significance.

A major weakness of Waller's hypothesis is that it is derived from Upper Palaeolithic European rock art, one of the most 'untypical' rock arts of the world (Bednarik 1993a), which is also one of its smallest: I once described it as representing perhaps 0.03% of world rock art (Bednarik 1990a). I have never understood why this tiny corpus, found at less than 300 sites across an entire continent, has attracted such an exorbitant interest from scholars. It is not the oldest rock art, nor the most complete body, and even from a Eurocentric perspective it is not the most beautiful rock art we have in the world (consider specific traditions of Africa, for example). So, apart from being the most promoted rock art, what is it about the Upper Palaeolithic rock art of Europe that not only attracts the interest of so many researchers, but prompts them to extend their 'explanations' of this untypical corpus to the remaining 99.97% or so of world rock art?

The next pertinent question we need to ask to place Waller's hypothesis into context is: what actually is Upper Palaeolithic rock art? The Upper Palaeolithic is a chronological pigeonhole that is not a real historical period, it does not define a culture, ethnic entity, and probably not a language group, religion or distinctive way of life. Archaeology uses many such arbitrary definitions, based on such subjectively perceived markers as the use of certain materials in artefact production. We must remember that these are not real, historically factual designations, and that we cannot validly attribute cultural meanings to them: they could not define art periods or styles. Moreover, there is very limited objective evidence as to the true age of these art corpora (Bednarik 1994b; *contra* Lorblanchet 1993), and we know that fakes are not uncommon in that of the 'Upper Palaeolithic'. This arbitrarily defined period persisted for many times as long as the Neolithic, or the entire Historical periods. To lump the tiny remnants of art which somehow managed to survive from these tens of thousands of years into *one single tradition*, and to attribute them to a single motivation, be it shamanism, echoes or whatever else, is quite unacceptable (as Bahn points out in the present debate, and also implied in 1978). Even a most superficial examination of these remnants, distorted as they are by countless

taphonomic processes, shows that this is indefensible. We know that a large proportion of world rock art was produced by juveniles, and this also applies to the Palaeolithic tradition (how many of the human foot prints found on the floors of the caves are of adults? For the positivist archaeologist, the paucity of adult foot prints from the Upper Palaeolithic presumably proves that human populations consisted exclusively of juveniles!). We can also deduce several other reliable observations from the collective evidence, and what they indicate together is that any uniform explanation can only be the result of un-rigorous reasoning of the sort I have considered in connection with supposedly female figurines of that same period (Bednarik 1990b).

In addition to these major problems with Waller's hypothesis, there are a few minor points inviting comment. His model predicts a spatial separation of carnivore and herbivore (particularly ungulate) depiction. Throughout world rock art, apparent juxtapositions of these animals are common, which are often considered to be scenes (cf. Lenssen-Erz 1991 for a thorough discussion of 'scenes' in rock art). I do not know what species they depict, being very ignorant about rock art, but more enlightened researchers who do know this claim that there are wolves with sheep, wolves with deer, tigers with deer, tigers with cattle, lions with antelopes, dingoes with kangaroos, panthers with deer, jaguars with camelids and many others. Occurring in all continents except Antarctica, these combinations are antithetical to Waller's version of the acoustic hypothesis. His proposal that the artists depicted what the echo reminded them of does not really explain why they depicted the carnivores, it merely offers a negative argument.

Waller mentions that certain hoof-like motifs occur only early in the development of art. They might be in the order of 30 000 years old, but remain essentially undated. Some rock art may be as much as ten times as old as that (Bednarik 1993b), so they are not quite as early as he thinks. Also, the model of increasing depiction of movement in the development of Palaeolithic art he attributes to Leroi-Gourhan is not valid for Upper Palaeolithic art (Bednarik 1989), and even less so for other palaeoarts: the most dynamic arts are often among the early art forms of a given region (consider central India, north-eastern Brazil, Arnhem Land, Kimberley, central Europe). It must also be mentioned that Waller provides no ethnographic support for his hypothesis,

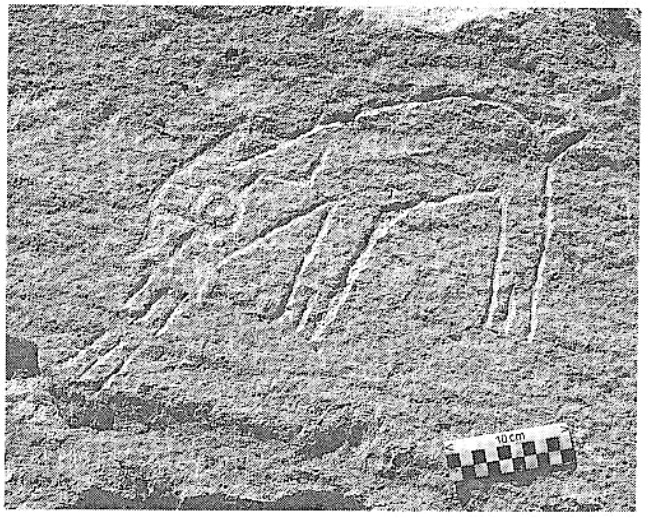

Figure 1. Petroglyph of a quadruped, with arrangement of dash marks apparently emanating from mouth, as if to indicate sound. Toro Muerto, southern Peru — the largest assemblage of petroglyphs of the Americas.

except to mention that the ancient Greeks attributed echoes to a supernatural being. More specifically, he mentions no relevant ethnographic information on rock art production, and I suggest he might find it difficult to provide any that supports his model.

Waller raises the question of how sound may have been depicted by rock artists. I reproduce two rock art motifs here of animals with 'emanations' which some rock art connoisseurs are likely to interpret as sound (Figures 1 and 2). Perhaps that is what they do depict, but I have no idea how Westernised researchers would propose to determine this. Their own interpretations of such markings are of great interest to the study of the visual but enculturated perception and cognitive processing of sensory input by Westernised researchers; they are of no interest in any other scientific field, most certainly not in rock art studies. I do not

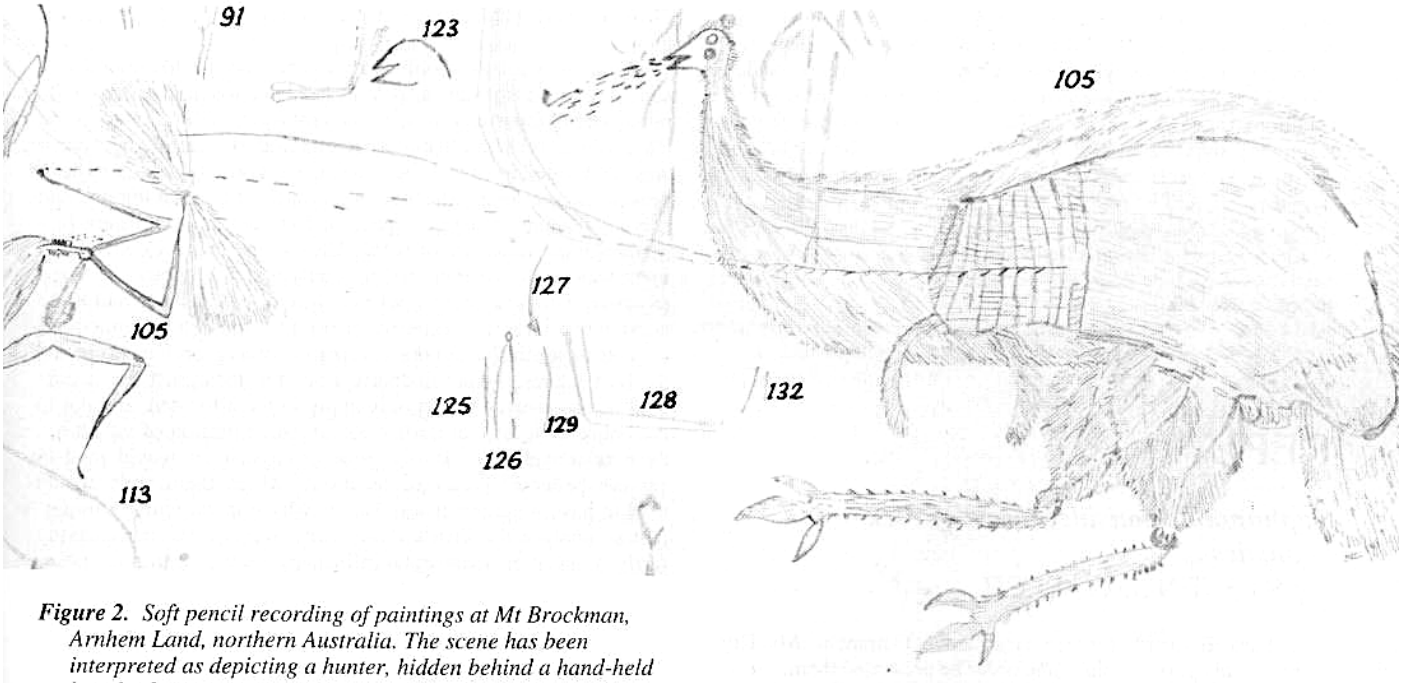

Figure 2. Soft pencil recording of paintings at Mt Brockman, Arnhem Land, northern Australia. The scene has been interpreted as depicting a hunter, hidden behind a hand-held bunch of grass, spearing an emu. Both the dashed line from the hand to the bird, and the dashes 'emanating' from the bird's beak suggest that non-visual aspects of the event are depicted. (Recording from Chippindale and Taçon 1993.)

believe that rock art research should be concerned with the study of the cognition of rock art researchers (except in a critical sense). In other words, if we were interested in what rock art students think is depicted in pre-Historic rock arts, then we need to found a new discipline and call it the study of rock art researchers, and attach it to cognitive psychology.

The main strength of Waller's hypothesis is that it emphasises the possibility of world views that are vastly different from ours, of spatial concepts and definitions which may have determined the reality of artists of past eras. People of the Pleistocene possessed constructs of reality which would have been conceptual artefacts, just as our own reality is. It is entirely possible that acoustic space played a role in their world: points in space do possess definite acoustical characteristics, as Waller says. It would be hasty to assume that we know whether Palaeolithic humans dealt with such phenomena in some way, or ignored them. Whilst they apparently had the same basic set of sensory capacities we possess, this does not at all mean that they applied them in quite the same way, just as there is no proof that verbal communication was always the dominant form of communication for hominids. Even though it is not likely that Waller's hypothesis is valid for a large proportion of rock art, it does present a refreshing attempt to see Palaeolithic humans as real people who interacted with their physical environment. Perhaps his hypothesis does apply to a small part of world rock art; it is a valid working hypothesis. The key question is: how would one test such a proposition?

Since Waller seems determined to pursue the matter further, I suggest a procedure that would provide us with more comprehensive information. One could conduct a detailed survey of some dozens of decorated sites one attributes to a single tradition, and produce 'acoustic maps' for their entire topographies, on which the intensity of sound reflection is spatially recorded as an 'acoustic topography'. Then one would overlay this map with one showing the density and type of the art. One could then check those locations where art should occur but is absent, to see if there could have been art in the past that has been lost through speleothem growth, abrasion (cave bears, bats), exfoliation or solution; or alternatively, if the site's acoustics have been altered (by sedimentation, tectonic change, flowstone growth, anthropogenic structures etc.). Finally, one could check if there are any recurrent patterns in the distribution of specific motifs relative to acoustic topography. If there is significant correspondence in a number of cases, one could try to establish whether these might all belong to a narrowly definable art tradition (but this would be difficult and tenuous: scientifically secure attribution to specific traditions remains a tricky business). This would not prove or refute the hypothesis, but it would show whether it is worth pursuing further. If there is poor correspondence at most sites the hypothesis would be refuted, and could be either discarded or reformulated. This would be a scientific procedure to develop Waller's ideas further, and I encourage him to follow it.

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