On Neuropsychology and Shamanism in Rock Art

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Disappointed with Lewis-Williams and Dowson’s (CA 29:322–38) response to the valid criticism and the many valuable points advanced, such as those on the possible involvement of children in rock-art production, by Turner [p. 229] and Consens [p. 221] of their paper “The Signs of All Times,” I would like to raise several pertinent issues in the hope of providing this debate with a fresh impetus.

The authors claim Wylie’s support of their ethno-"graphic analogy" when she in fact suggests two strategies for assessing the analogical claims of relevance (p. 232, emphasis added): “One is to press the demonstration of tightness of fit between source and subject (showing that it is unique to this model), and the other is to investigate the persistence and uniqueness of the connection between entoptic-like images in art and their experience in trancelike states in a range of source contexts.” It would be relevant, then, to ask whether phosphene forms occur in arts other than those of shamans and whether they are associated primarily with “trance states.” Lewis-Williams and Dowson’s theory is clearly invalidated by Wylie’s test. Phosphene forms are or were most commonly used by two groups of people: children 3–4 years of age [Kellogg, Knoll, and Kugler 1965] and hominids or humans of the pre-iconic era [Bednarik 1984, 1987, 1988a]. The motifs of the art of both groups are largely if not exclusively phosphene forms (the substitution of the term “entoptics” for “phosphenes” has caused such confusion that I shall only use the latter term for “normal-state,” noniconic entoptics). The evidence concerning the role of phosphenes in the cognitive development of very young children is particularly compelling. Peter van Sommers, a professor of psychology at Macquarie University, recently considered the role of graphic universals in the drawings of infants and isolated basic geometric motifs which he called “primitives”: they match Knoll’s phosphene types. Since van Sommers (1984) never mentions Knoll or any of the other writers on phosphenes, his results provide independent corroboration, and the notion that very fundamental universals are involved in early art formation becomes even more persuasive.

Extensive controlled phosphene experiments have been conducted with various groups, including Japanese...
students, American children, and German air force pilots, but never with shamans. All humans, even some blind people, experience phosphenes, but susceptibility to spontaneous phosphenic experiences is by far greatest in infancy. Hallucinogen or trance-induced phosphenes account for only a tiny fraction of such experiences. Hallucinations, in contrast, are not physiologically normal phenomena, and Lewis-Williams and Dowson’s model of the “three progressive stages of mental imagery” is unconvincing. The iconic images occurring in altered states of consciousness are not conjured up at will [Naranjo 1967]; they may even be as “hard-wired” as phosphenes. One can look at original recordings of phosphenes for hours without ever “seeing” a single object in them. How could Lower Palaeolithic hominids, who probably lacked a concept of iconicity [Davis 1986], have managed to do this?

Lewis-Williams and Dowson “solve” this problem by proposing that “the projection of geometric and iconic imagery was part of humankind’s experience throughout the Palaeolithic and in all parts of the world” (p. 234, emphasis added). This is postulated without supporting evidence by researchers who subsequently argue that “a call for proof is inappropriate in rock-art research” (p. 234) and that “observation statements are fallible” and “cannot conclusively falsify a hypothesis” (p. 235). While not conceding that they cannot, upon reflection, sustain their bold claim, they are sufficiently alarmed by my objections to modify their position: “In fact, we claim only that [the] association of [iconic and non-iconic phenomena] is remarkably widespread in rock art” (p. 233). I believe that they owe it to the readers to clarify this key issue by either retracting or reaffirming their claim that Lower and Middle Palaeolithic hominids experienced iconic imagery.

Lewis-Williams and Dowson not only erroneously equate phosphenes with shamanism and altered states but also equate the trances of the San with those of shamans, although the former are communal experiences and not experiences of a shamanistic elite. I am intrigued why—being so interested in shamanism—they concentrate their attention on the San, ignoring the true shamanistic cultures of southern Africa. For instance, what can they tell us about the incidence of phosphenic motifs in the art of Zulu shamans [Callaway 1884; Boshier 1974]? More germane than the Coso Range petroglyphs [which are ethnographically irrelevant because, according to Whitley, they are often of late Pleistocene/early Holocene age] would, it seems, be known shamanistic practices from Siberia, Tibet, the Arctic, West Africa [Gorer 1935], the Caribbean [Long 1977], Brazil [Giesler 1983], or Peru. The shamanistic status of San art is largely based on Lewis-Williams’s own publications (and is not generally accepted even among South African rock-art specialists), and Reichel-Dolmatoff provides the authors’ only ethnographic references for drug-induced hallucinations. As the world’s experts in the use of alkaloids, the South American Indians deserve more than cursory treatment, and the wealth of relevant literature from South America provides ample challenges for the model of “progressive stages of mental imagery.” Lewis-Williams and Dowson conveniently accept the concept of the immutability of phosphenic form constants, while postulating that these flickering, ephemeral forms are consciously transformed into iconic motifs during trance. Naranjo’s [1967, 1968, 1973] finding that the contents of yage [or yage] visions are not the result of conscious elaboration of phosphenes squarely contradicts their model: specific visions are spontaneously elicited by harmaline in controlled experiments with subjects lacking the expectations of indigenes, indicating the existence of a collective unconscious of “iconic form constants.” Imagery related to death and flying stands out, as do images of felines, snakes, and birds of prey. This, surely, would be more relevant to identifying underlying universals in drug- or trance-induced hallucinations than the naive explanation that geometric shapes are consciously elaborated into iconic forms [while the subject’s volitional brain functions have succumbed to trance]: a circle becomes an orange, a breast, a cup of water, or a bomb depending on the disposition of the subject. In pondering the possible phylogenetic persistence of “iconic form constants” one could inquire, for example, whether the Upper Palaeolithic art has a high incidence of felines, snakes, and eagles. It does not, of course (the total being well below 1%)—which does not necessarily preclude shamanism but does render its involvement less likely.

The authors have studied firsthand neither the prehistoric art of the Upper Palaeolithic nor that of the American Southwest or South America, yet they readily reject the advice of those who have studied the occurrence of phosphenic forms in all three regions. Similarly, they have made extensive use of Marshack’s data but consistently misconstrued his illustrations, just as they have misinterpreted Reichel-Dolmatoff [Marshack 1989]. Their use of motifs that occur only at a single site to demonstrate a universal mode is, Marshack [1969] observes, “an indication of the subjectivity in the process of selection and construal that Lewis-Williams and Dowson indulged in in order to prove a theory.” Moreover, they have selected 6 of the 15 phosphenic types for consideration [presumably the ones to be found in the rather limited rock-art references cited] but not the most common ones. [The less common types account for only 16% of all electrically and optically induced phosphenes [Eichmeier and Höfer 1974].] They ignore the physiological causes of phosphenes, and since this is particularly important in understanding the phenomenon it is most unfortunate that they do not consider Meier-Koll’s cybernetic model of phosphenic induction [see Eichmeier and Höfer 1974].

They cite my work in three places, managing to misquote me each time: on p. 205, a table dealing entirely with Australian art is implied to relate to European art, and a typographical error distorts the date of the source [1984], on p. 213, I am listed with several others as having suggested that shamanism existed in the Upper
Palaeolithic, when in fact I had never even used the word “shamanism” in print and would not dream of mooring such a notion, and on p. 214 the dates of two papers are again in error. But more constructive than dwelling on errors would be to take up the authors’ belief that some misunderstanding on my part is indicated by my relegating of meaning to the trivial aspects of early marking traditions (p. 233). I can only repeat that the semantics of prehistoric art is inaccessible to us, while origins or derivation are not. Some of the scholars sharing my view on meaning are cited by Lewis-Williams and Dawson in their introductory paragraph. The phoshene theory is not about meaning; even the role of phosphenes is rather peripheral to it. It is essentially an epistemological theory which, among other things, explores the origins of cognition. I am not aware that this has been preempted, as the authors claim, but perhaps I could be enlightened.

Of course it is highly possible that Upper Palaeolithic people used phosphene forms in their arts—all humans have used them in their image systems since humans evolved from the hominids. We all use them daily, but that does not make us shamans? Nor did such use of recycled motifs make the Aurignacians shamans. Not only is there no unique relationship between shamans and phosphenes, but the latter are cons older than the Aurignacian and phosphene forms must have been used in art and communication long before that culture. The phosphene experiences that Upper Palaeolithic people may have had [irrespective of context] would have had the effect of dramatically reinforcing beliefs in the supernatural qualities of these already enucleated motifs and would thus have validated the metaphysical concepts held, whatever these were. It was in fact this validation principle that initially encouraged me to postulate the phylogenetic antiquity of phosphene forms [Bednarik 1984, 1987]—although neurophysiology certainly provides corroborative evidence for it. This is a far cry from the simplistic model of Lewis-Williams and Dawson, a model which can be resolved thus: Everyone who use phosphene forms is a shaman; hence every human is a shaman; hence there are no shamans (since one term becomes superfluous); hence there can be no shamanistic art.

There is a huge corpus of ethnographic evidence from all parts of the world indicating that body painting, cicatrices, tattoos, decorative paraphernalia, and garments all provided significant information about their wearers, and it seems plausible that such enucleated information found its way into rock art. It may well be correct that the geometric markings on anthropomorphs in rock art were ultimately derived from phosphene forms, but at the level at which Lewis-Williams and Dawson proceed this is not relevant: for example, their “meaning” or function may have been emblemic. The form content becomes relevant only at the next level, at which we might consider, for instance, the potential of phosphenes to validate the potency of such recycled motifs; and it becomes especially pertinent when we look at the question of ultimate derivation by examining the art traditions predating the introduction of two-dimensional iconicity [Bednarik 1988d].

The authors’ intransigence in the face of the serious objections raised by nearly all commentators is inexplicable. Having long admired Lewis-Williams’s dedication in exploring non-positivistic aspects of prehistoric arts and his rejection of naive empiricism or scientism, I regret having to conclude that his desire to find a shortcut to a universal model has led him on. Through its omissions, misconstruals, and selectivity Lewis-Williams and Dawson’s paper actually presents a better case against the involvement of shamans in rock-art production than for it. No art of true shamanistic traditions is considered, while the arts that are richest in phosphene forms [e.g., in Australia] are conspicuously non-shamanistic. Phosphene forms constitute less than 5% of the rock arts of the Upper Palaeolithic of Europe, of the Coso Range, and of the San. Prehistoric arts significantly richer in phosphene types than these occur in all continents, and a reasonably comprehensive table listing their phosphene motif types would need to be about 40–50 times as large as that provided by the authors. It would include, among others, the archaic petroglyphs of Piaui, Brazil [Bednarik 1989a], those of Bolivia [Bednarik 1988b], those of the U.S.A. [Bednarik 1988c], various rock arts in Africa [e.g., dos Santos 1974], the earliest paintings of India and other Asian art, various bodies of European rock art, the largely phosphene art of New Caledonia [Frimigacci and Monnin 1980], and the several extensive pre-iconic petroglyph traditions of Australia [e.g., Bednarik 1987]. In a recent response to the Lewis-Williams and Dawson paper, Bradley [CA 30:68–75] considers the incidence of phosphene types in the megalithic art of Europe. His observations only confirm the ubiquity of these motif types: they are indeed the “signs of all times” rather than the signs of shamanism. But Bradley’s attempt to extend Lewis-Williams and Dawson’s table [fig. 4] also exposes yet another problem with that model: Type VI [filigrees or thin meandering lines] is misunderstood by Bradley as referring to vortices (which are in fact another phosphene motif, Type 10 of Kellogg, Knoll, and Kugler 1965), and IIIE, clearly a radial design in a circle [and thus a combination of phosphene Types 2 and 6], is grouped with dots [Type 7]. The confusion is attributable to Lewis-Williams and Dawson’s arbitrary selection of types and selective utilization of different sources to achieve “fits.” Future attempts of this kind should always refer to the original sources—the work of Knoll, Kugler, Eichmeier, Höfer, and colleagues.

It shamanism were indicated by the frequency of phosphene motifs, which is Lewis-Williams and Dawson’s central postulate, that frequency would provide an indication of shamanistic influence in an art. According to the antithetical but earlier phosphene theory, in the oldest, pre-iconic art traditions phosphene motifs may dominate to the point of exclusiveness [Bednarik 1984], while often being less common in more recent traditions: their frequency thus provides a rough indication
of "archaicness." The two theories appear to be mutually exclusive.

Reply

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Rock-art research has never enjoyed much respect or sustained attention in Anglophone archaeology. The reasons for this include its methodological and theoretical poverty, simplistic ethnographic analogies, obsession with trivia, factual errors, ad hominem jibes, failure to attend closely to what others have written, and unsupported assertions. A number of these problems are evident in Bednarik's comment.

Many of the points he makes could have been cleared up by attentive reading of our paper, the comments, and our reply [CA 29:101-45]. This is evident right at the start when he claims that our argument is "invalidated" by procedures proposed by Wylic (pp. 231-32). In fact she writes:

"Far from calling into question claims made for [the neuropsychological model's] plausibility or security, my aim is to elaborate the suggestion that reliance on analogical inference should not automatically give way to "ethnographic despair." . . . the strength of the model . . . derives not so much from an elimination of inference as from an effective use of two quite different, mutually reinforcing sources which underwrite strong claims for the relevance of specific similarities in visual imagery manifested in diverse contexts."

She concludes, "It is a significant strength of the present model . . . that it not only provides an initially compelling account of puzzling, seemingly 'archaic' phenomena but so clearly opens up new lines of enquiry." All this is vastly different from the impression Bednarik conveys.

To strengthen the "analogical claims of relevance" Wylic suggests the two strategies Bednarik mentions. Here we must emphasise a point we made in our paper and reply: at the present stage of research and with the model still in what we believe to be an initial stage of development, a tight fit between it [in all its components] and an art merely suggests that states of altered consciousness played some part. These states may have included, as we allow (p. 213), migraines (Sacks 1970, Richards 1971, Siegel and Jarvik 1975), schizophrenia (Siegel and Jarvik 1975), and infantile perception [Kello, Knoll, and Kugler 1965]. Bednarik fails to mention that our argument does not end here: it proceeds from this still equivocal point as a "best-fit" hypothesis. For instance, although it is possible that some Upper Palaeolithic visions derived from migraine attacks or schizophrenia, it would be ludicrous to argue that Upper Palaeolithic art can be explained by migraine attacks or schizophrenia. If some of the imagery did derive from such pathological conditions, those conditions were clearly part of a ritual complex (see, for example, Eliade 1973). Similarly, whatever role children may have played [see below], "children's art" cannot explain Upper Palaeolithic art as a whole or the geometric forms as a category. If we bear in mind [1] the very widespread occurrence of some form of shamanism in hunter-gatherer societies, [2] the symbolic role of animals in these societies and the prominent role of animals in Upper Palaeolithic art, and [3] the other points we made in our "Implications" section (pp. 213-17), our explanation is clearly a "best fit." As we put it (p. 213), "All that the parallels exhibited here allow us to conclude at the moment is an association with altered states and, further, . . . that in the Upper Palaeolithic this association probably had at least some features in common with what we understand by shamanism." This cautious claim is ignored by Bednarik, instead he presents a travesty of our argument so that he may the better attack it.

Bednarik wonders why we "concentrate" on San shamanism and ignore the "true shamanistic cultures of southern Africa." Shamanism is, of course, a very diverse phenomenon, and it is hard to say exactly what constitutes "true" shamanism—if there is such a thing. In a passage Bednarik evidently missed, we discussed our use of "shaman" for the San (p. 204):

"We use "shaman" rather than the more usual "medicine man," but we do not thereby imply anything about the social position of the person, his or her mental health, or, indeed, many of the other characteristics often associated with the very heterogeneous phenomenon called shamanism. Instead we emphasise what we believe to be the most important and overriding feature of shamanism and the one with which this paper is principally concerned—altered states of consciousness."

We went on to discuss various San words for "shaman" to show that San shamans are closely associated with concepts of supernatural power. In the light of this discussion it is patently absurd to argue that the San do not practise a form of shamanism. [Interesting as their altered-state experiences are, the Zulus do not make rock art and are therefore irrelevant to our argument.]

It is also absurd to claim that the shamanistic status of San art "is not generally accepted even among South African rock-art specialists." On the contrary, it is hard to think of any practising professional southern African archaeologist or anthropologist who does not now accept it, and we cited numerous arguments for this position (p. 204). [That list can be extended: see, for example, Hufman 1983; Maggs and Scally 1983; Yates, Golson, and Hall 1985; Manhure et al. 1986; Mazel 1989; Parkington et al. 1986; Campbell 1986; Deacon 1986, 1988; S. Hall 1986, 1987; Hammond-Tooke 1983; Morris 1988; Garlake 1987a, b, c; Guenther 1988; Leslie Brooker 1989; Loubser and Dowson 1987; Dowson 1988, 1998; Dowson and Holliday 1989; Sampson 1988; Parkington 1989; Vinnicombe 1986; Wadley 1987].
Never recognising that our model is based on research conducted by neuropsychologists or addressing those primary sources, Bednarik gives two simplistic and erroneous versions of it in order to lampoon it. First he asserts that the model “can be resolved thus: Everyone who uses phosphene forms is a shaman; hence every human is a shaman, hence there are no shamans (since one term becomes superfluous); hence there can be no shamanistic art.” He then compounds his error: “If shamanism were indicated by the frequency of phosphene motifs, which is Lewis-Williams and Dowson’s central postulate, that frequency would provide an indication of shamanistic influence in an art.” Even a superficial reading of our paper shows that the model is not concerned exclusively with entoptic phenomena. It comprises three stages, the last of which includes fully iconic hallucinations, and, furthermore, seven principles governing the perception of mental imagery. Some of these principles clarify the relationships between entoptic and iconic imagery and the ways in which they are combined. In the face of the neuropsychological evidence we cited, it is hard to see how the three stages of our model can be challenged. Certainly, in referring to them as “unconvincing” Bednarik does not attempt to show that the neuropsychologists are wrong.

We tested the utility of this complex model against San rock art because it is known to be associated with altered states and, quite simply, because we know it best. We also tested the model against Coso rock art. On this subject Bednarik says that we “ignore the solid objections of the American commentators,” but when we turn to the comments by North Americans we find that only Vastokas, Faustlitch, and Halverson have anything to say about it. Vastokas merely says that we have carried our argument “too far” (p. 230). Faustlitch says that “the patterns integrated into the Coso anthropomorphs . . . may depict both internal power and omatic garments.” Here he seems to be accepting at least part of our case, but later he advocates caution by adding that there is “no conclusive ethnographic evidence” (we believe he is mistaken) and that we should not “prematurely attribute a shamanistic origin to this art” (p. 225). By contrast, Halverson (p. 225) says that “the heuristic use of ethnography [is] admirable in respect to San and Coso depictions.” In any event, the shamanistic status of Coso art is established by the cogency of the arguments and the evidence; one writer may be right and all the others wrong.

Bednarik also claims that the “relevant literature from South America provides ample challenges for the model of ‘progressive stages of mental imagery,’” but, as we have said, according to Reichel-Dolmatoff (1978:12–13) the Tukano themselves speak of three progressive stages in their yage-induced visions. The evidence, as we reported it (p. 204), is clear enough: for the Tukano there are stages, and those stages are very like the ones we derived from neuropsychology and built into our model. We have not “misinterpreted Reichel-Dolmatoff.”

Focusing on the first stage of our model, Bednarik says we claim that a high frequency of entoptic motifs in an art indicates “shamanistic influence.” We made it clear, however, that some arts seem to dwell on Stage 1 entoptics while others emphasize iconic hallucinations. In southern Africa, for example, some rock-art sites have a high proportion of geometric depictions while others have very few, yet we know that both kinds of site were associated with shamanism. Why are these differences a matter for research. Our position on this point thus makes nonsense of Bednarik’s statement that “phosphene forms constitute less than 5% of the rock arts of the Upper Palaeolithic of Europe, of the Coso Range, and of the San.” Certainly for southern Africa, we know of no research that could possibly confirm or contradict a figure of 5%. Moreover, in Europe, as in southern Africa, some sites have principally entoptics while others have very few. There is little point in combining them and assigning them a percentage of the whole.

More interesting is the argument that children experience Stage 1 entoptic phenomena and that this in some way invalidates our position. It may be true that children see entoptic phenomena. In replying to Martindale (pp. 237–28) on this issue, we did not deny that some geometric rock-art motifs may have been made by children, but we challenge the notion that this could have had much to do with Upper Palaeolithic art (p. 236):

We believe that the long association of entoptic forms with often remarkably “realistic” iconic depictions, various kinds of combinations of entoptic and iconic forms, and combinations of iconic images (theran-thropes), together with evidence for complex Upper Palaeolithic social forms, are better explained by the more extreme varieties of altered consciousness associated with shamanism than by infantile perception.

Moreover, even though there is some evidence that children went into Upper Palaeolithic caves, the locations of many of the signs make it highly unlikely that children made them. In any event, are we talking about very young children making entoptic “doodles” deep underground or about somewhat older children involved in rituals? Some footprints suggest that children may have been part of whatever rituals were performed in the caves. If Bednarik and others are suggesting the second of these two possibilities, we have no problem with the hypothesis.

Referring to the second stage, Bednarik says, “One can look at original recordings of phosphenes for hours without ever ‘seeing’ a single object in them.” We never claimed that people, ancient or modern, try to see things in “original recordings of phosphenes.” The point is that, in an altered state of consciousness, people sometimes do elaborate entoptic mental imagery, and we cited neuropsychological references to show this. He argues that Stage 3 “iconic images . . . are not conjured up at will.” Once again, we made no such claim. He goes on to ask how Lower Palaeolithic hominids, “who probably lacked a concept of iconicity,” saw objects in entoptic forms, and he challenges our statement that “the projection of geometric and iconic imagery was part of
humankind's experience throughout the Palaeolithic and in all parts of the world." It seems to us clear from the context of this last quotation that we were referring to the Upper Palaeolithic, but we nevertheless think it highly probably that Lower and Middle Palaeolithic people [we don't mind the word] experienced both kinds of hallucination. Bednarik is confused about the concept of iconicity. We were clearly writing about hallucinations of things such as animals, which are surely possible independently of a capacity for understanding two-dimensional pictures. Neuropsychological research strongly suggests that animals such as cats and apes can hallucinate (Siegel and Jarvik 1975:87-104). This implies that the capacity to hallucinate is built into the mammalian, not just the human, nervous system. If this reasonable assumption is valid, then it seems highly probable that Lower and Middle Palaeolithic people did hallucinate. But, as Davis [1986] points out, they probably lacked a capacity for iconicity and did not make pictures of their hallucinations.

In suggesting that some iconic hallucinations may be as "hard-wired" as entoptic phenomena, Bednarik raises an intriguing issue that we intend to address in a future publication. For the present we simply note that he overlooks the fact that the Westerners who took part in laboratory experiments had well-formed concepts of felines, snakes, and birds of prey and that some shamanic and Westerners can be expected to share at least some ideas about these creatures. The experiments show not, as he supposes, that images of these creatures are "wired" into the human brain but that even Westerners experience the stresses of certain altered states of consciousness in terms of animal imagery: in all societies animals are probably the most powerful and pregnant symbols. The threatening nature of some of these altered states is embodied in frightening animals that are associated with death [a very widespread way of speaking about entry into altered states]. At the same time it must be said that people who have no knowledge at all of, say, polar bears will clearly not hallucinate them. Bednarik goes on to make the extraordinary suggestion that a low incidence of felines, snakes, and birds of prey in an art renders shamanism as a factor in its production less likely. He is here assuming that shamans automatically depict the images they "see" in the same proportions as they see them, as if depiction were some sort of mechanical photocopying process operating on mental imagery. On the contrary, it is abundantly clear that depiction, entoptic and iconic, is culturally controlled. That is why it is unnecessary for our hypothesis to show a high incidence of any particular species or the occurrence of distinctive entoptic forms in a shamanistic art in the same proportions as in laboratory experiments.

Cultural control must be taken into account when addressing another point as well. Whether certain geometric forms, especially those associated with human figures, are "directly fixed" entoptic phenomena or whether they derived "ultimately" from entoptic shapes is, contrary to what Bednarik implies, a question we did address [pp. 215-16]. This is not to say that all Upper Palaeolithic depictions are images fixed by people in altered states or experiencing afterimages. Once that initial step had been taken, the development of Upper Palaeolithic art probably followed three courses. One stream remained mental imagery fixed while it was being experienced. A second stream derived from recollected mental imagery processed in the ways we have suggested. A third stream derived from contemplation of the graphic products of the first two streams and the realization that they could be duplicated even by someone who had never experienced an altered state of consciousness.

The images that will be "fixed," recollected, processed, contemplated, and duplicated will be culturally controlled, but there is more to it than that. In southern Africa entoptic forms are sometimes integrated into clearly hallucinatory "monsters" and therianthropes (Lewis-Williams and Dowson 1989, Dowson 1989). This is in fact what neuropsychology predicts, and we cited references to support this. We believe that such San, Coso, and Upper Palaeolithic depictions represent visions in which the two kinds of hallucination (entoptic and iconic) were combined. Whether some depictions represent "third-stream," derived forms is another question; clearly, we allowed that this may well be the case.

Although Bednarik says that he has "long admired Lewis-Williams's... rejection of naive empiricism," he fails to see that he himself follows that fallacious methodology and mind-set. Echoing a point made by Marshack (1989), he claims that we "have studied firsthand neither the prehistoric art of the Upper Palaeolithic nor that of the American Southwest" [Coso is actually in the Great Basin]. As it happens, one of us (DLW) has studied Upper Palaeolithic art through the kindness of the late André Leroi-Gourhan and the staff of the Louvre Museum and North American rock art through the good offices of David Whitley, Larry Loendorf, Stuart Conner, and others. More recently both of us have been able to study a large number of caves in the company of Paul Bahn and many French researchers. Borrowing again from Marshack, Bednarik exhibits the empiricist mind-set when he refers to the "selection" we engaged in "in order to prove a theory." Elsewhere he sarcastically [but correctly] quotes us as claiming that a call for "proof is inappropriate in rock-art research" and that "observation statements are fallible" and "cannot conclusively falsify a hypothesis." Clearly, Bednarik puts his faith in "proof" and inductive arguments from theory-free data [Marshack's "analytical data"]. As Conkey [1987, 1988] has emphasised, this kind of empiricism is one of the most debilitating features of rock-art research. The empiricist programme envisages four stages of research: the unbiased collection of theory-free data, "objective" categorization and analysis of these data, induction of an explanation from this analysis, and, finally, a test of the explanation against "new" data. Philosophers of science have amply demonstrated the impossibility of this alluring approach. It is impossible to
collect data without making discriminations according to preconceived ideas; it is impossible to categorize and analyse these data without selecting discriminating features according to a preconceived classification; it is impossible to induce inescapable conclusions; and it is impossible to test those conclusions without an element of circularity. One of us has discussed empiricism in rock-art research (Lewis-Williams 1983, Lewis-Williams and Loubser 1986), and more recently we have collaborated in a critique of Marshack's empiricism (Lewis-Williams and Dowson 1989); we recommend the references we have cited in our publications (e.g., Chalmers 1978, Hempel 1966). At the same time, we emphasise that careful empirical work is as important in rock-art research as in any other field. Unfortunately, the distinction between an empiricist research programme and the empirical work that links theory and data is not fully appreciated, and criticisms of empiricism are often taken to mean that accuracy does not matter.

Bednarik considers us to have been "sufficiently alarmed" by his earlier objections to modify our position. Alarm is not the emotion we experience when we read his observations, and we have shown that no modification of our position was necessary. He says that he hopes to provide the debate "with a fresh impetus," but his misreadings and errors do not accomplish that end. For fresh impetus we must turn to Bradley (CA 30:69-75; see also Lewis-Williams and Dowson n.d.).

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