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On Neuropsychology and Shamanism in Rock Art

ROBERT G. BEDNARIK
Australian Rock Art Research Association, P.O. Box 216, Caulfield South, Victoria 3162, Australia. 21 IV 89

Disappointed with Lewis-Williams and Dowson's (CA 29:232–38) 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. 228] 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 ethnographic 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, 1988*a*). 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 phosphene 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. 216, 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 phosphene 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 final 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 phosphene 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 *yajé*) 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 enquire, 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 phosphene 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 (1989) 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 phosphene 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 phosphene 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 mooting 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 Dowson in their introductory paragraph. The phosphene 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 eons 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 enculturated 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 Dowson, 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 enculturated 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 1988*a*).

The authors' intransigence in the face of the serious objections raised by nearly all commentators is inexpedient. 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 Dowson'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 Piauí, Brazil (Bednarik 1989), those of Bolivia (Bednarik 1988*b*), those of the U.S.A. (Bednarik 1988*c*), 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 phosphenic 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 Dowson 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 Dowson'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 III*E*, 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 Dowson'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.

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. 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.

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