

COMMENTS

In science, falsifiability rules

By ROBERT G. BEDNARIK

Derek Hodgson's paper is one of the most worthwhile additions of recent years to the discussion of art origins. Like the recent paper by Feliks (1998a), also in this journal, it enriches a debate that has become bogged down in unfalsifiable, often idiosyncratic hypotheses about the meanings of palaeoart whose principal feature it is that they are assumed to appeal to the public. Serious work such as that of Feliks and Hodgson receives scant attention in this scramble for public favour. Indeed, Hodgson's paper illustrates the issue well. It is based largely on a model presented almost twenty years ago, the phosphene theory. While that theory is perfectly falsifiable and thus scientific, it has been ignored for all these years, but the competing shamanistic model, which its originator has freely admitted is profoundly unfalsifiable, has been widely preferred since it was proposed in 1988. This prompts a simple question: why do untestable (and, one might add, *unlikely*) hypotheses receive this favourable treatment in archaeology, while hypotheses that are fully testable, and that have never been falsified, are consistently ignored?

Hodgson reminds us that all the hypotheses trying to explain 'abstract' marks before and during the U. P. will not lead to a satisfactory model because they cannot be substantiated. Or to rephrase this prediction as a scientific proposition: none of the various 'interpretations' we have for this corpus are falsifiable. The phosphene theory, by contrast, does not 'explain' these marks, it merely suggests derivation, but it is falsifiable and thus scientific. It can be significantly weakened by the discovery of a major art body of non-phosphenic marks predating the introduction of two-dimensional iconic art. Until such evidence is presented, the phosphene theory remains unrefuted. In fact I regard Hodgson's paper as a testing of my phosphene theory. He quite correctly points out that that theory made certain predictions: 'the model [allows] us to predict that the present gaps in the record ... will be made up of further combines as well as subsequent aggregates similar to those produced by infants'. In the present paper he fills some of these gaps. A second way to test the phosphene theory is, as I have proposed many years ago, to look for disconfirming evidence, in the form of very early palaeoart that is clearly not phosphene inspired. The phosphene theory is preferable to the untestable alternatives we have in the literature, and yet Hodgson's paper is the first discussion of it to appear in print. As an epistemologist who is profoundly interested in the heuristic dynamics of the discipline I welcome the opportunity to comment.

I have bemoaned for a good twenty years the fact that non-iconic palaeoart has been so severely neglected. The preference for 'nice pictures', which has distorted Pleistocene palaeoart research for a century, is biased,

and it should be self-evident that iconic art is semiotically less sophisticated than non-iconic. In iconic art iconicity provides an obvious referent, whereas any referent in non-iconic art must be culturally negotiated and transferred.

Hodgson's proposition that palaeoart developed as a means of expanding human cognitive capacities beyond the limits biologically imposed by brain size, functioning as a kind of surrogate cortex, seems to be a novel and original idea. It likens palaeoart to back-up storage of computer data: the ceiling in brain size imposed by natal limitations led to external means of storing data. This is the most exciting twist in Hodgson's paper, and it adds a new facet to the phosphene theory. Until now I favoured the explanation that the discovery of shared phosphenes, through their externalisation, led to the realisation of a shared consciously experienced reality. Hodgson's idea of a 'surrogate cortex' offers attractive features and could be explored further, and it does not exclude the mechanism I had favoured; the two could both be valid components of an interpretation of art origins tied to the 'catalyst' role of the phosphenes.

An interesting point made by Hodgson is that repetition is appealing to us because it is an integral feature of the brain, it signifies 'safe-ness'. This is also worth pursuing further, because there are 'elements of repetition' evident in much if not most early palaeoart: consider, for instance, multiple parallel lines, finger flutings, concentric circles, multiple arcs (Quneitra), multiple cupules, paired markings generally, series of notches, multiple zigzags or meanders, or objects made in large numbers to a single pattern (e.g. disc beads). This concept offers considerable prospects for further development: the establishment of synaptic pathways is contingent upon repetition, as is the production of many early marks. There is a good deal in this paper to prompt some stagnant synapses in my own cortex to begin firing again.

I am not convinced that Hodgson's separation of (U. P. and later) 'art' from (L./M. P.) 'proto-art' serves any useful purpose, it reinforces the artificial discrimination between non-representational and representational art (are depictions of phosphenes representational?). Worse still, it emphasises the even more artificial separation of the M. P. and U. P. (Fiedler 1999; Bednarik and Kuckenburg 1999). Also, it begs the question, 'What is art?', a question archaeologists and art historians have not dealt with in a scientifically meaningful way (cf. Rosenfeld 1999). I therefore prefer the collective term *palaeo-art* to describe all art-like phenomena of the past, including beads, pendants, engraved marks and 'non-utilitarian' objects generally.

Concerning an epistemologically anchored definition of art, Hodgson's preferred version seems to correspond closely to mine: art is the medium conveying the artist's awareness of perceived reality to human sensory perception; it externalises human concepts of reality. In an epistemologically more satisfying definition, art is the one phenomenon in human experience of which there can be no crucial common denominators of phenomenon

categories (CCDs) that are inaccessible to humans; art thus consists of the only collective phenomena which humans can experience 'objectively' (Bednarik 1994b).

Hodgson cites Bahn as stating that the 40 000-year boundary is exaggerated due to more recent rock art having survived better. While this is, in an over-simplistic way, a valid statement, the role of taphonomic logic requires much more careful consideration, and 40 000 years is not a taphonomic threshold in rock art. Besides, it is evident from Hodgson's mention of an 'assumed upsurge' in cultural activity at the M./U. P. division (there is quite probably more M. P. rock art in the world than U. P., conversely) that he ignores the concept of the taphonomic threshold. We need to be reminded here that archaeology does not deal with cultural events or developments or anything of the kind, it deals exclusively with material residues of events, and unless one understands how the characteristics of these residues can be translated into scientific propositions about events in the past our speculations are scientifically irrelevant. The formulae for such translation are encoded in taphonomic logic, which decrees that the statistical or other quantitative, and often even qualitative, characteristics of the material record are frequently irrelevant to its interpretation. This is the most fundamental law in all of archaeology (Bednarik 1995b).

Hodgson sees the cave art in Altamira and Lascaux as being about 16 000 years old. I think some or all of the Altamira art might be somewhat younger, perhaps 14 000 years, while some of the Lascaux art is probably not of the Pleistocene at all (Bahn 1994).

The marked bovine rib from Pech de l'Azé should not be cited in the context of discussing secure palaeoart. It has always been controversial, and d'Errico and Villa (1997) have persuasively shown that judgment should be reserved on this specimen. Similarly, Hodgson cites radiocarbon-derived datings of petroglyphs from the Olary region in South Australia. It must be emphasised that these dates are no longer regarded as valid, the analyst who produced them has himself withdrawn them by casting severe doubts on the method he used to calibrate these 'dates'.

I fail to see the significance of the proposed 'hyper-reality experience'. If the making of certain markings had induced a trance-like state, that in itself would not constitute an 'explanation for the appearance of L./M. P. phosphene-like motifs', as Hodgson suggests. I have drawn many phosphene motifs but have never experienced the slightest 'altered state' as a result. Nor do people who draw nothing but phosphenes today (such as infants) enter such a state.

Concerning Lewin's 'shrinking' human brain, I doubt that the rather slight difference in average cranial volume between Neanderthaloid sapiens and later sapienoids is of any great significance. It is one thing to accept that encephalisation was essentially complete by the time archaic *Homo sapiens* forms appeared, but such minor differences in brain size are negligible, and they are well within the range of modern human cranial sizes.

Moreover, I understand that the *size* of a human organ is a poor indicator of its *performance*.

Concerning the issue of symboling before the Aurignacian, speech or language are forms of symbol use. Colonisation by seafaring undeniably requires complex communication (Noble and Davidson 1996), forward planning and abstract thought of an order of magnitude not significantly different from that available to present-day humans. Since we have known for some time that seafaring hominids navigated the sea many hundreds of millennia ago, this record demonstrates the use of symbolism as well as technological sophistication (Bednarik 1999).

Concerning phosphenes, particularly their physiological aspects, the principal resource remains the work of Eichmaier and Höfer (1974), which Hodgson has not cited. The published works he cites on this subject are eclipsed by Eichmaier and Höfer's more comprehensive work.

Concerning the Bilzingsleben markings, I refer readers to the recent laser-microscopic study of the principal specimens from that site by Steguweit (1999), according to which the engraved grooves were made intentionally and are not incidental utilitarian marks. The method used for this study is technically superior to the microscopy used elsewhere by Marshack, d'Errico and myself. For instance it eliminates subjective aspects of judging groove profiles and replaces visual judgments with hard metric data.

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