Reaction

Following the publication of the article 'Testing the "Three Stages of Trance" Model' by Patricia Helvenston and Paul Bahn in the October 2003 issue of the Cambridge Archaeological Journal *we have received three responses. These are printed here, followed by a reply from the authors. A related contribution by David Lewis-Williams is published in the 'Shorter Notes' section of this issue.*

Hallucinations in Caves

Jean Clottes

In a recent article, Helvenston & Bahn (2003) challenge what I said about the hallucinogenic character of deep caves in an interview I gave to a magazine in which the title of a book I mentioned (Féniès 1965) had been misreproduced. They add 'on closer examination . . . this book devotes not a chapter but less than two of its 158 pages (pp. 41–2) to brief anecdotal accounts . . . including only two reports of "visual hallucinations".' So that they conclude that 'clearly, these descriptions are of no importance or frequency in this domain, as has been confirmed to Bahn by speleologist friends' (Helvenston & Bahn 2003, 221).

These comments are deliberately misleading on several accounts. First, the authors choose to quote an oral interview to a magazine and not the published scientific studies in which the citation was given earlier (Lewis-Williams & Clottes 1998, 48) and later (Clottes & Lewis-Williams 2001, 210) where no such mistake occurs. This is a cheap way of scoring a point. Second, the part of the cited book about hallucinations is indeed a short chapter[1] with an unequivocal title ('Errements sensoriels'), of five pages and not two (Féniès 1965, 39–43). Third, in addition to 'innumerable instances of auditory hallucinations' (p. 42) which are extremely frequent from all accounts and may have played an important part in the perceptions of people visiting the deep caves in the Palaeolithic, Féniès cites eleven examples (and not two) of visual hallucinations, both inside the cave and after coming out of it. One of them deserves to be quoted in full because it is a case in point directly relevant to the argument about the Three Stages of trance: the caver saw 'luminous dots moving like comets, slowly, for 5 to 10 minutes' (p. 41) which is a clear case of 'entoptics' without drug stimulation. Fourth, we had also quoted another study (Lewis-Williams & Clottes 1998, 48) published by a geologist/caver who specializes in the study of caves (Renault 1995–96). In his study, Renault mentioned several other examples of visual and auditory hallucinations and gave other references on the subject, such as Saumande 1973. Along with the references to Féniès and Renault, we also cited another testimony (Simonnet 1996, quoted in Clottes & Lewis-Williams 2001, 210). Simonnet, who is an archaeologist, described repeated visual hallucinations after remaining and working underground for long periods (Simonnet 1996, 343).

Omitting a number of testimonies by selective quotation in order to play down the importance of visual and auditory hallucinations caused by a prolonged stay in caves is obviously nothing but a sleight-of-hand trick which should be exposed as such.

Note

1. It is so called by the author : 'In this chapter, we shall essentially deal with vision and audition' (Féniès 1965, 39). '*Dans ce chapitre, nous aurons essentiellement en vue la vision et l'audition*'.

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Author biography

Until his retirement in 1999, ***Jean Clottes***, was Scientific Adviser to the French Ministry of Culture. He is also a former Chairman of the International Committee for Rock Art (ICOMOS) and the editor of *INORA* (*International Newsletter on Rock Art*). His research interests focus on prehistoric rock art: its preservation and recording, dating problems, study of the archaeological context, problems of epistemology, and the search for meaning. Recent books include *World Rock Art*, 2002, Getty Foundation Press and his edited volume *Return to Chauvet Cave*, 2003, Thames & Hudson.

'Testing' and Altered States of Consciousness in Upper Palaeolithic Art Research

David Pearce

It is some comment on the importance of an idea that fifteen years after its inception it is still being vigorously debated. In most cases, such debate is to be welcomed: it indicates a healthy climate of critically-minded researchers. In the case of the so-called 'Three Stages of Trance model', however, the debate has become stale. Instead of arguing the heuristic and methodological merits of the model, researchers who oppose it criticize their own misconception (or misrepresentation?) of what was originally presented. Helvenston and Bahn's (2003) recent contribution to the debate unfortunately falls into this regrettable category.

The idea in question is that altered states of consciousness were drawn upon by people in the European Upper Palaeolithic as a source of imagery for the famous cave paintings. In their 1988 paper, Lewis-Williams and Dowson argue the position on the basis of neuropsychological accounts of the types of imagery that are 'seen' by people in altered states of consciousness induced by a wide range of techniques (Lewis-Williams & Dowson 1988). In the ensuing fifteen years they have much elaborated and substantiated their argument using a variety of sources. I divide my comments on Helvenston and Bahn's criticism into two parts: the blatant errors of fact perpetuated by Helvenston and Bahn, and epistemological issues relating to the study of rock art. A number of additional comments could be made, but I have tried to limit my commentary to errors of fact and the methodology of studying rock art.

The facts of the matter

In their attack, Helvenston and Bahn focus on one aspect of the original paper: Lewis-Williams and Dowson's proposition that endogenous mental imagery can be divided into three stages — what Helvenston and Bahn (not Lewis-Williams and Dowson) call the Three Stages of Trance (TST) model. At no point in their summary do they tell us what the 'TST model' is. They nevertheless propose to 'test' it. On the basis of their examination of some of Ludwig's (1968) seventy types of altered states of consciousness they conclude that a progression through the three stages of trance is experienced only under the influence of mescaline, psilocybine or lysergic acid diethylamid (LSD) (p. 214). The evidence for this is presented in their unfortunately titled pamphlet, *Desperately Seeking Trance Plants: Testing the 'Three Stages of Trance' Model* (Helvenston & Bahn 2002). The test they suggest is simple: are any of these substances, in botanical form, found in the European Upper Palaeolithic, in rock painting sites in particular? Their answer is an emphatic 'no'. They use their conclusion to dismiss the use of neuropsychology in Upper Palaeolithic research in its entirety.

A neat test? Not so. The entire proposition is based on a fallacy. Lewis-Williams and Dowson never argued that the three stages were experienced as an ineluctable progression. On the contrary, they explicitly state,

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> These three stages are not necessarily sequential. Some subjects appear to move directly into the third stage, while others do not progress beyond the first. . . . Nor should the stages be considered discrete. . . . The three stages we propose should therefore be seen as cumulative rather than sequential. (Lewis-Williams & Dowson 1988, 204)

Furthermore, it is difficult to see how the presence or absence of a progression of stages influences the basic proposition that altered states of consciousness were involved in some of the practices that led to the production of some of the art.

The 'three stages' were suggested as classes of experience-types that may be experienced by people in altered states of consciousness. The three levels of trance act as a heuristic device to help researchers recognize imagery that may have been generated in altered states (see e.g. Dronfield 1995; 1996). That individuals experience 'entoptic' and iconic hallucinations for a variety of reasons is indisputable. Whether or not any given mechanism of hallucination leads to the experience of all three levels is irrelevant to Lewis-Williams and Dowson's central argument.

Lewis-Williams and Dowson's original insistence on finding evidence of all three levels in a corpus of art was an attempt to add rigour to their arguments: they did not want every zig-zag image to be taken as evidence of altered states of consciousness in the art. Other gross misunderstandings by Helvenston and Bahn of Lewis-Williams and Dowson's original text could be cited.

The study of rock art

Some comments are also necessary on how one studies and interprets rock art. The approach and 'test' outlined by Helvenston and Bahn betrays grave errors of logic and lack of familiarity with contemporary thought about the generation of archaeological knowledge. The neuropsychological model was an early attempt to raise the study of Upper Palaeolithic parietal art from the empiricist quagmire into which it was sinking. A substantial body of corroborative evidence has since been adduced to support the original position (Lewis-Williams and Dowson elaborate their argument in Lewis-Williams & Dowson 1989; 1992; 1993; Lewis-Williams 1991; 1997a,b,c; 2002; Clottes & Lewis-Williams 1996; Lewis-Williams & Clottes 1998; Dowson 1989; a number of other writers have profitably employed the model in other areas, e.g. Bradley 1989; Patton 1990; Sherratt 1990; 1991; Dronfield 1992; 1993; 1994; 1995; 1996). These positions explicitly follow those forms of argument proposed as appropriate for archaeology by philosophers of science (see Lewis-Williams 1991 for an explicit example; for discussions of the types of argument see e.g. Wylie 1982; 1985; 1989; 1993). Helvenston and Bahn (p. 213) and Bradshaw (p. 216) should note that simplistic Popperian falsificationism of the sort they advocate, besides its general flaws, is particularly unsuitable for use with archaeological data.

In its general application, falsificationism suffers from two serious, and related, problems (the arguments against falsificationism are accessibly summarized in e.g. Hacking 1983; Chalmers 1999). In naïve falsificationism, of the type Helvenston, Bahn and Bradshaw suggest, a falsifiable hypothesis is proposed and then concerted efforts are made to show that it is false in light of observational or experimental data. The first problem with this is that observations are themselves theory based (a point fundamental to falsificationism). In short, this means that when there is a clash between an observation and a hypothesis, all that logically can be said is that *one* of the two is false. The hypothesis is therefore not *necessarily* falsified. The second, related problem is that complex sets of premises and webs of assumptions are involved in testing hypotheses; in an apparent inconsistency between an observation statement and a hypothesis any one of these assumptions and premises could be at fault (Duhem/Quine thesis). Once again, the hypothesis is not necessarily falsified.

The general difficulties of falsificationism are well illustrated by Chippindale's imaginative scenario. It also illustrates further, specific deficiencies in Helvenston and Bahn's 'test'. Designing a test is not easy. It needs to be constructed in such a way as to test the relevant hypothesis, not one of the assumptions implicit in the make-up of the design. It is here that Helvenston and Bahn's 'test' falls down. It is a morass of unspecified assumptions, any one of which could be false (and several of which are). They include, *inter alia*:

1. The three stages of trance are *ineluctably* sequential.
2. The three stages are *only* generated by the consumption of certain, specified, plant hallucinogens.
3. The hallucinogens would be consumed in the same place that the art was made.
4. There would be material residues of the hallucinogen.
5. Material residues would be discarded.
6. Material residues would be discarded in the same place as the art was produced.
7. The residues would be in a form recognizable to archaeologists.

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8. The residues would survive to the time of excavation.

If any of these assumptions are false the test would prove negative, but it would not falsify the hypothesis. Doubt can be cast on a number of these assumptions; I have already argued that the first two are incorrect. Chippindale's suggestions (p. 218) cast doubts on others: What if the hallucinogen was consumed at a location remote from the painting site? What if the left-over hallucinogen was ritually disposed of?

These are both perfectly plausible possibilities, yet they, and others, are not taken into account by the test. The reason for this is that the implications that are deduced (they are not deductions in the logical sense in this instance) from the hypothesis are subjective, and based on the theoretical perspective of the deducers. In this case, that theory happens to be at odds with the hypothesis being tested.

The points I have made about falsificationism are not new, either in the philosophy of science or in archaeology. Indeed, the criticism of falsificationism and other aspects of positivism was a major component of the post-processual critique in archaeology (e.g. Wylie 1989; Shanks & Tilley 1992) and has now been accepted as standard in introductory texts on archaeological theory (e.g. Whitley 1998; Johnson 1999). In light of these criticisms, and more importantly the general acceptance of the criticisms by the larger archaeological community, it is disturbing to note that such naïve appeals to 'science' continue to be made.

The 'meaning' of rock art

On a more general level, Bradshaw's dismissal of rock art as 'something to do on a rainy day' (p. 216) needs some comment. Even if some groups of Australian aboriginals did paint for purely recreational reasons, there is substantial ethnographic evidence that many cultural groups, from around the world, make rock art for religious or ritual reasons — including, of course, many Australian groups (e.g. Elkin 1930; Crawford 1972; Mowaljarlai & Vinnicombe 1995). Such a simplistic and naïve use of ethnographic analogy (*one* recorded ethnographic instance applied to the rock art of the world) ignores a substantial literature on the subject. Bradshaw, no doubt, favours a 'doodling and graffiti' explanation for the art because it is the explanation most readily understood within his Western worldview. It should not be necessary to point out that one cannot interpret the material remains of other cultures in terms of Western cosmology. It is this same ethnocentric perspective that leads Helvenston and Bahn to describe a trance-related explanation as 'startling' (p. 222). It is startling only in a twenty-first century view that apportions low value to the less rational aspects of consciousness and condemns outright the use of mind-altering drugs. A brief survey of world ethnography would reveal a long list of cultures that *do* value and cultivate these very states. It is doubtful that they would find the proposition that some rock art was related to altered states of consciousness 'startling'.

On the general topic of analogy, it is important to note that San and Coso ethnography were *not* used to derive (pp. 213, 222) the neuropsychological model, or as analogies for the Upper Palaeolithic (Lewis-Williams 1991). The model was derived from the neuropsychological literature; the San and Coso arts were cited as examples of ethnographically known arts that incorporated altered states of consciousness in the rituals that led to their production.

The alternative options that Bradshaw suggests (p. 216) have long been considered by researchers — and dismissed for lack of evidence (this is not to say that various of them may not be relevant to other traditions of art in other parts of the world).

Finally, Chippindale's 'social context' should perhaps not be so lightly dismissed. It could be enlightening to question why papers that criticize misrepresentations of early research continue to be published. Is it not now time to engage with current research and move the study of the Upper Palaeolithic forward?

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'People Talk About Heaven . . .'

David Wilson

As I have some personal experience which may be relevant, I hope that my remarks on the series of articles in *CAJ* October 2003 relating to 'Testing the "Three Stages of Trance" model' will be of interest.

In 1999–2000 I wrote and produced a BBC film about this issue (*ape-man*, BBC2, 2000). There are some points in Helvenston & Bahn's paper which are directly contradicted by what I learnt in making the film. Firstly, they assert (p. 214, col. 2 para. 3) that:

> *Desperately Seeking Trance Plants* discussed six representative, naturally-induced trance states, including: hypnosis (both heterohypnosis which is a trance induced by another party, and autohypnosis which is a trance induced by the subject), meditation, relaxation states, peak experiences, psychoanalytic free-associative trance, and ritual dance-induced trance. None of the subjective reports of altered consciousness reported in these trance states is consistent with the TST model! Moreover, the experiences reported for other trance states cited by Ludwig do not conform to the TST model insofar as the authors have been able to determine.

Furthermore, in *Desperately Seeking Trance Plants* (shame about the title) they remark that Patricia Helvenston 'had never had a patient who had ever described anything remotely similar to the three stages'. If so, that may be attributable to the clinical arena and purpose within which she was working.

Certainly the experience of one of the contributors to our film, Dr Etzel Cardeña (then at the United States' Services' University of the Health Sciences, now at the University of Texas) was very different. I interviewed him because he had conducted a study with a number of highly hypnotizable subjects, having screened hundreds of people 'to find out the top one to two per cent, that is people who are very responsive', auto-hypnotics as it were. The purpose of the study was to discover what happened to them *in the absence of any suggestions from outside*; to try and eliminate the cultural components of their experience and discover what their experiences had in common. The result was as follows, and I am quoting from the interview I conducted with him in 1999:

> What I found was that across individuals — and again please bear in mind that these people were not in contact with each other, they were not in the same class, they did not know each other — I found that there seemed to be more or less a general pattern.
>
> In the first stage, if you will — what you might call light hypnosis — again in a very highly hypnotizable people, what they would experience might be a number of body sensations, such as spinning, beginning a floating, relaxation, feeling perhaps that their limbs were changing in size, and there might be some geometric figures as well that were happening, seeing tunnels, grids, things of that sort.
>
> From that place they would typically then go on to having a sensation that they were floating out of their bodies, and they could float out and fly . . . that's what they were experiencing, sometimes they would both float and fall down a tunnel. It was not at the same time, but you would have both types of experiences. Now this is different from the first one, in that the person is no longer in his or her physical body, the experience is done in some other realm.
>
> After this going out of the body, coming out of the body, they would go into a place where they may see a number of unusual images. If you have heard sometimes, like surrealistic type of landscapes, where you might see something that was just a vast sea of darkness, or they may see just colours, bright colours, kaleidoscopes, music going on with the kaleidoscope, something that was very rich in terms of vivid imagery. This was more going into the realm of deep hypnosis. Associated with the deep hypnosis sometimes people felt that there was something spiritually very rich and important to what they were having . . .
>
> . . . Some of the most striking and for me actually touching type of experiences and interesting experiences were exactly those in which people not only felt that they were becoming part of, but very literally would say 'I am merging with the energy, I am merging with the colours, I am merging with the light, I am it, I cannot differentiate myself from it', very clearly stated, so you have a kind of state where there was no separation between them and what they were experiencing, they were it. At times you might say well, 'I'm no longer matter, I am just energy, I am just energy'. And again, you know, bearing in mind that we are not talking about anything that has been suggested to them, but something that happens to them and that surprised them. One of the very clear things about this study is that I would have people with some of the most striking experiences come to me afterwards and some of them would ask me 'am I crazy?, where is this coming from?', and I say to them no, it's a striking experience, I cannot tell you very much about it, because I don't want to guide what experience you

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have, but no you are not crazy (see Lewis-Williams & Dowson 1988).[1]

This sounds to me *exactly* like the 'Three Stages' described by Dowson and Lewis-Williams, elicited not by drumming, or dancing or even any particular process, but by deep relaxation. And certainly not by drugs.[2]

As an illustration — rather than evidence — we filmed a hypnosis session with one of Cardeña's subjects, who did indeed experience this kind of pattern. 'People talk about heaven', she said on emerging from her trance, 'and I think that's what it's like'.

Experiences like this have suggested to Cardeña and others that these effects may be produced by the *absence* of sensory information arriving at the optical cortex; so for the film I then turned to Dr Dominic ffytche of the Institute of Psychiatry in London, who coincidentally had just published a paper about such 'automatic' hallucinations (ffytche & Howard 1999).

The starting point of this paper was a study of hallucinations occurring spontaneously to patients with eye disease. 37 per cent of them experienced what the study called 'tessellopsia', that is grid patterns, interpreted by patients variously as 'very fine netting', 'brickwork', or 'diamond-shaped fences'. Other less prevalent styles of hallucination included 'hyperchromatopsia' ('shapes in vivid colours that wiggle', 'angular patterns in vivid colour') and 'dendropsia' ('irregular branching forms described as trees, branches or maps'). A brief scan on the Lascaux website turns up examples not only of tessellopsia, as Lewis-Williams and Dowson have reported, but also hyperchromatopsia and dendropsia (see pictures, below).

ffytche and Howard were the first investigators to recognize these categories of hallucination as occurring with patients with eye-disease, which was their main concern. So when I turned up in ffytche's office at the Institute of Psychiatry, it was quite a surprise. We filmed an interview with him, and a session with one of his patients, who, when asked on camera to draw his hallucinations, drew spontaneously three of the *Signs of All Times'* 'form-constants': parallel lines, grids and nested curves.

ffytche and Howard's paper went on to consider the literature of such hallucinations and found equivalent patterns across a wide range of other non-optical conditions: cerebral pathology, sensory deprivation, ingestion of LSD/mescaline, and migraine. Please note that only two of these conditions would normally be considered as inducing 'altered states of consciousness'.

Other research by ffytche and others has shown

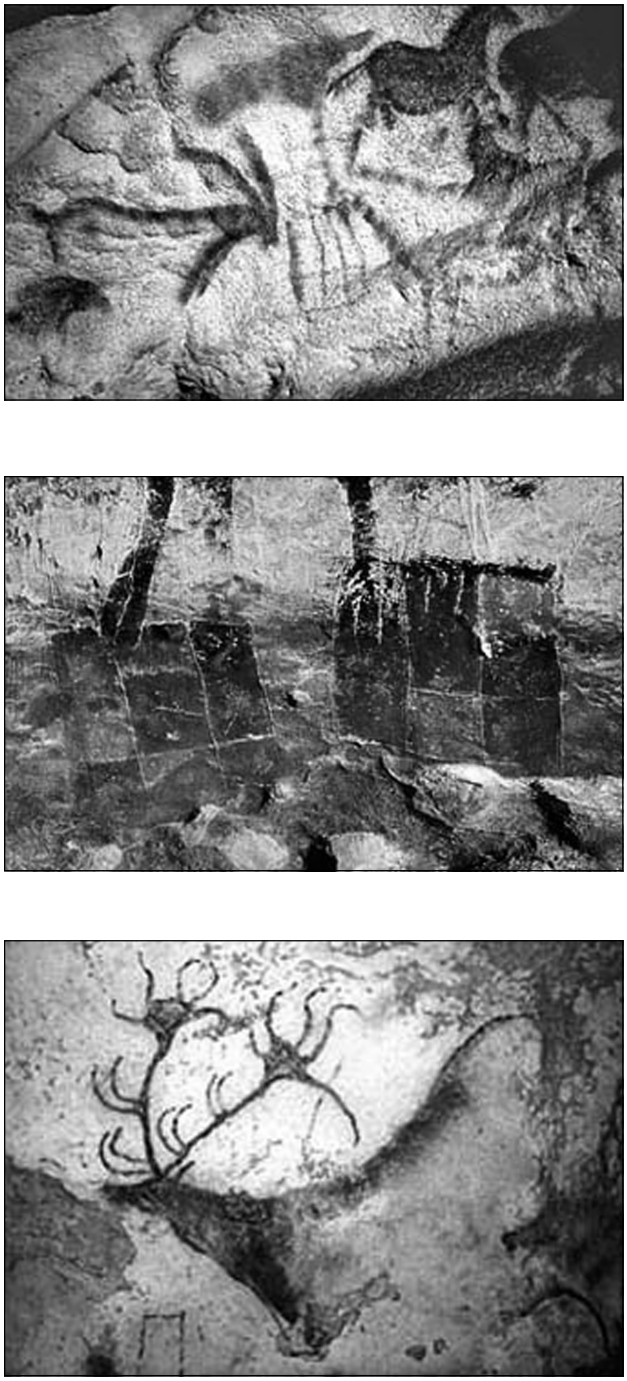


Figure 1. *a) Tessellopsia; b) hyperchromatopsia/ tessellopsia; c) dendropsia: the paired curves extending from the stems of the antlers are also motifs common both to the* Signs of All Times *and to the experience of ffytche and Howard's macular blindness patients. (Rock art from Lascaux Cave. Courtesy of Norbert Aujoulat, Responsable du Département d'Art Pariétal, Centre National de Préhistoire, Périgueux, France.)*

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through fMRI scanning that the hallucinations are indeed caused by the spontaneous firing of neural networks in the optical cortex of the brain in the absence of stimuli from outside (see ffytche *et al.* 1998; Burke 2002).

Taken together, the two lines of research show

1. That the 'TST model' does exist in nature in the absence of psychedelics.
2. That the neurological mechanisms which produce the sensations associated with 'Stage One' — what Dowson and Lewis-Williams called 'entoptic hallucinations' (erroneously, as we now know, as they come not from the eye but from optic neural networks) — derive from the inherent architecture of the human brain.
3. That such Stage One hallucinations are, because of point 2 (above), likely to occur in almost any deep form of altered consciousness, as indeed ffytche and his colleagues discovered, when they reviewed the literature.

A further point to be made regarding Helvenston and Bahn's paper is the nature of what they call the 'TST model'. When talking about hallucinations one must always bear in mind that because they are so intense and subjective, and because they usually involve some interference with a subject's sense of time and space, as well as an important emotional component, one is very unlikely to get detailed and accurate reports of what a person has experienced and in what order. Yet their paper insists upon a very rigid interpretation of what was anyway originally only an explanatory scheme. The wonder is that what one might prefer to call the three modes of trance are reported so consistently from so many different visionary *milieux*. For example, images of tunnels and vortexes are common to near-death experiences, Cardeña's auto-hypnotics, *Alice in Wonderland,* and in a most spectacular example Hieronymus Bosch's *Ascent to the Empyrean* (Fig. 2) in the Doge's Palace, Venice.

Of course, what exercises archaeologists is not the intrinsic nature of hallucination, but whether the Palaeolithic paintings and markings to be found on the cave walls of France, Spain — and now it seems England — are to be understood in terms of such hallucinations. The proponents of such a view have adduced arguments that extend far wider than merely the presence of 'entoptic', or more accurately, perhaps, 'Stage One' imagery. Those arguments will stand or fall on their own merits. But there is a general point which ought to be made — or remade, as Richard Bradley has already raised it very cogently in the last issue but one of this journal (Bradley 2003). And that is that we should never forget that for most of human history, religion has been perhaps a more potent motivating force than any other. It infused the whole of life, colouring even the most 'secular' activities. So even if the Palaeolithic pictures had not been painted in remote, difficult, and useless locations, one would suspect that they were religious in inspiration, and indeed, most interpretations tend to assume this.

But the connection between religion and what today we call art is deeper than a mere question of iconology. There is a matter of process. In discussing analogues for 'religious knowing' — that sense religious people have that their experiences have led them to know more not only about this world, but about the spiritual world too — cognitive psychologists Fraser Watts and Mark Williams have this to say about 'aesthetic "knowing"':

> Though there is no exact consensus on the nature of aesthetic cognition, one widely accepted tenet is that it involves a kind of distancing . . . In aesthetic perception, we separate the object from ourself . . . This contemplative absorption needs to be in some degree an emotional one. Feelings of affection and reverence are necessarily involved. Without them, all that is possible is a critical analysis of the work of art, not aesthetic perception of it.
>
> However, emotional restraint is also required . . . Seeing a work of art may set various emotional impulses in train . . . However, impulses at the centre of consciousness have to be restrained if the web of associations at the fringes of consciousness are to elaborate themselves. There also has to be restraint of any direct striving for results while viewing the work of art . . .
>
> The ability to perceive art in this way is by no means universal. It is a specialised perceptual skill that some people acquire more easily than others. Even when the general skill has been acquired, it cannot invariably be brought into operation. There are times when we are too tired or preoccupied to respond to art in this way. Usually repeated attention is necessary before a work of art is properly seen. The moment when this occurs, though somewhat unpredictable, can be sudden and dramatic . . .
>
> In all these ways, it seems that aesthetic cognition is a relatively good analogue of religious cognition. The religious person needs to acquire a steady contemplation of the divine that is in some ways like the aesthetic contemplation of a work of art. This is very different from discursive theological thought. The religious person also needs to put self-preoccupation aside. Further, the kind of contemplation of God which is cultivated in prayer,

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> because it is sustained by love of God, is necessarily an emotional response. Efforts to contemplate God are sometimes rewarded only with the experience of 'aridity' that contemplatives over the centuries have described so vividly and bitterly. However, with repetition and persistence, moments of illumination are reported to follow in which the caul is removed, and God is discerned with a directness and certainty that is like the moment when the work of art is suddenly 'seen' (Watts & Williams 1988).

The two modes of thought — 'religion' and 'aesthetics' — engender similar mental experiences in the minds and hearts of adepts. So it is no surprise, really, that religion relies so heavily on elaborate artistic production or that so much art is religious: they are almost like two sides of the same mental coin.

This leads on of course to the consideration of what religion is, and in particular, how people 'do' religion. And here it is obvious that altered states of consciousness are key. Religious *experience*, as opposed to religious *belief*, inevitably does involve altered states of consciousness, as we see every day in evangelical churches, Hindu ashrams and Buddhist monasteries. The formal correspondences between Palaeolithic art, 'shamanic' art in the American West and the San/bushman art and mythology of Southern Africa would seem to suggest that the religious experiences, and possibly even beliefs, may have been similar, *although obviously not the same.* Look a bit further and you find the same vocabulary of motifs extending to classical mythology and beyond. Medievalists and ancient historians, with their knowledge of sibyls and oracles, saints and prophets, will not be surprised to hear that religion involves altered states of consciousness. Not for everyone, indeed — and religious authorities once they develop have a notorious reputation for fierce protection of the 'mysteries' of the faith (that is, the core experiences which validate it) — but in every case at the heart of religion is an all-embracing *experience,* a spiritual experience, of altered consciousness; Pentecost, if you like. Much religious activity aims to reproduce that experience in controlled conditions, even where, as with Buddhism, it eschews the concept of a supernatural deity.

So in contemplating the Palaeolithic paintings, one has to ask, as Colonel Rainborough asked at Putney in 1649, 'what is the reasonableness of it?' The answer seems obvious. These are religious paintings and they reflect religious experience, that is, what people of a sceptical and scientific bent used to call 'hallucinations'. How they got there is the subject of this controversy. It must be said that one of the characteristics of Drs Bahn and Helvenston's writing is a failure of imagination in this area. They may not like the 'shamaniac' approach, but they offer no alternative. So they can only argue for a negative, which as I hope I have shown is illusory. Were they to offer their own suggestions, then at least one could engage in a real debate about real history.

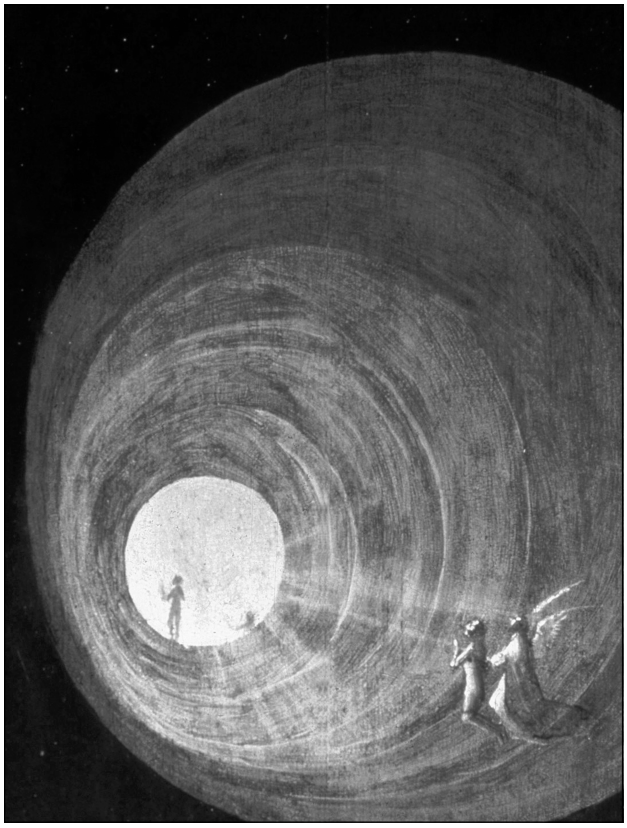


Figure 2. *Detail from* The Ascent into the Empyrean *or* Highest Heaven*; panel from an altarpiece thought to be of the Last Judgement (oil on panel) 99 by Hieronymus Bosch* (c. *1450–1516). (©Palazzo Ducale, Venice, Italy / Bridgeman Art Library.)*

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Notes

1. BBC transcript of interview with author, 1999.
2. For the details of Cardeña's research see Cardeña (1996).

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Author biography

David Wilson is a television producer and writer. He was series producer of the archaeological magazine ***Down to Earth*** [Channel 4] and has since produced a wide range of historical documentaries, including ***Ruling Passions***, ***apeman***, ***Battle Fields*** [BBC 2], ***Armada***, ***Empire*** and ***Monarchy*** [C4]. *Monarchy*, a history of the English monarchy from 450 to 1450 BC co-written with David Starkey to accompany the Channel 4 series, will be published by Chatto & Windus in the summer of 2004.

Waking the Trance-Fixed

Reply from Helvenston & Bahn

We must start by pointing out that David Wilson is not a disinterested commentator, but a strong proponent of the Three Stages of Trance (TST) model. Indeed, the TV programme to which he refers was outrageously one-sided, with not even a whisper of skepticism about the ill-informed claims being promulgated. Nevertheless, Wilson has raised some important points, many of which are somewhat tangential to our papers, so let us clarify that we have focused our attention and criticism upon the TST model specifically. We have not discussed the broader question of whether or not naturally-induced trance might have been an important feature in the lived experience of Upper Palaeolithic peoples, nor the general issue of whether or not 'religious' experiences (James 1902) as we understand that term today, which have often included some forms of what are contemporarily referred to as 'altered states of consciousness', might have been motivating factors in Palaeolithic cave art. We have concentrated specifically on the type of trance predicted by the TST model. Indeed, we don't dispute Wilson's discussion of religion or the idea that 'religious' thought and experience have been dominant in human affairs for eons, nor do we dispute the deep affiliation between aesthetic sensibilities and religious phenomena, a relationship whose biological depth has been beautifully explored by Dissanayake (1995).

We would remind Wilson that of all the 70 altered states of consciousness cited by Ludwig (1968) shamanism was only one. And it is the specific emphasis on shamanism that we have criticized (Bahn 2001) with respect to the TST model, not 'religion' or spirituality in the broadest sense of signifying belief in supernatural spirits or forces, nor the possible role of altered states of consciousness in the context of spiritual rituals. One of many reasons we have criticized the shamanic aspect of the TST model is because the South African data base upon which it was formulated has been misrepresented by Lewis-Williams. For example, neither of the two investigators who studied contemporary !Kung the most intensively believed that shamanism was an aspect of their healing dances (Lee 1966; Katz 1982).

In recent years a new interdisciplinary field that terms itself 'neurotheology' has focused upon the role that brain structures and functions play in spiritual experiences, and numerous studies have confirmed (Gloor 1960; 1986; Gross *et al.* 1972; Rolls 1984; Persinger 2003) and elaborated upon the investigations of Penfield (1958) who found that electrical stimulation of the temporal lobes (which include deep limbic structures such as the septum, amygdala and hippocampus) resulted in illusions, hallucinations, and experiences of ecstasy or odd sensations that are characteristic of altered states of consciousness, or temporal lobe seizure disorders involving the same neural substrates (Blumer 1975; Gloor 1990). Winkelman (2004) goes so far as to argue that shamanism is the foundation of human

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cognitive evolution and spiritual experience. This assumes that studies of shamanism for which we have historical records only dating back two millennia (Eliade 1964; but see Bahn 2001) have something to say about spiritual expression 30,000 years ago and completely fails to consider the possibility that spiritual expression has evolved, adapted, and codified into a huge variety of 'religious practices', including many varieties of shamanism (Lorblanchet 2001, 95–115). By analogy, what informed person would suggest that Christianity is the same now as it was 2000 years ago following the death of Christ? Although numerous authors have assumed that the 'sorcerer' figure found in the cave of Les Trois Frères in the French Pyrenees represents a 'shaman', some have criticized this approach (Bahn 1998) and it seems much more likely that the figure is a 'horned god', as Breuil called it, or that he was wearing animal accoutrements with which he was either hunting or reenacting some myth (Thackeray 1993).

Moreover, the structures mediating emotional and spiritual experiences (the limbic system), are phylogenetically extremely ancient and well developed in the great apes. Evolutionarily the neocortex of the temporal lobes predated the expansion of the frontal lobes and inferior parietal lobules of modern *Homo sapiens* (Joseph 1996). Based upon this information, it seems certain that the evolution of these limbic structures predated 'shamanism' by hundreds of thousands of years. For example, Donald (1991) suggested that the evolution of 'mythic culture' began some 500,000 years ago. Donald hypothesizes that this phase in the evolution of the human mind was characterized by narrative thought as opposed to paradigmatic thought which involves the logical-scientific skills that emerge only in literate cultures after years of systematic education. According to Donald's theories of mind, mythic culture was fully developed, at least in its pattern of daily use, by the Upper Palaeolithic.

Mythic culture would have been characterized by the telling of stories and creation myths, reenactments of myth through ritual, and singing, dancing and shaking rattles and/or beating drums around the communal hearth. All of these activities are known to facilitate a hypnotic trance. The 'spiritual' experiential dimensions of all of these activities are mediated, in large part, by the limbic system and temporal lobes, thus, trance phenomena of the hypnotic type may have been common from $^1/_2$ million years ago, or earlier, to the present. Over time, complexity of ritual practices and specialization of socio-religious functions may have gradually evolved until an individual was selected to fulfill a role such as that of the shaman, and this person apparently became the primary possessor of techniques of trance induction. More than likely such a role only became fully developed during the past few thousand years. A mythic culture might have used plant substances to induce and/or enhance trance (Siberian shamanism is clearly associated with the hallucinogenic mushroom, *Amanita muscaria*) although in Europe, especially where caves containing Upper Palaeolithic cave art are located, the evidence for hallucinogenic substances is very sparse. Mythic culture was an oral culture and we can gain a glimpse of it through the Homeric epics, whose origins developed prior to Greek cultural Paideia (Jaeger 1945).

Ong (1982) describes the profound differences in the psychology and cognition of individuals living in a completely oral culture, and those living in a literate culture. In the former, the stories, dancing, and ritual all tend to produce trance states in some individuals during the activity, as mentioned above. We emphasize that 'the effects of oral states of consciousness are bizarre to the literate mind', an observation that is consistent with the common experience of trance episodes in such cultures (Ong 1982, 30). Indeed, Ong emphasizes that without studying the differences between oral and literate cultures in depth, it is virtually impossible for the literate mind to comprehend the mind of the non-literate individual (and by this we mean the individual who has never been exposed to any form of writing). Such a mind as this was that of Palaeolithic individuals. Literate cultures significantly alter the cognitive capacities of their members and this can be seen even in chimpanzees. For example, bonobos who when raised in an artificial culture especially designed to facilitate their production, modification, and purposeful use of tools; their understanding of sentences of naturally-spoken English; and their acquisition of a large lexicon of visual symbols (Savage-Rumbaugh *et al.* 1993) 'do not act, think or communicate like the same species' (Donald 1998), thus revealing latent cognitive potential that is not apparent when observing them in their natural surroundings.

Donald believes the same is true of humans and concludes that humans would not have the same fundamental characteristics of mind that are manifested in contemporary, highly literate Western culture, without our very specific cultural context, including extensive training in literate cognitive skills. It is our almost complete immersion in such enculturation processes that renders 'altered states of consciousness' so different from 'ordinary' con-

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sciousness for us, and distances us from the more affective aspects of experience so richly dominant in oral cultures (Huizinga 1996). And, let us hasten to emphasize, we are not stressing these differences between oral and literate cultures because we are Eurocentric, but because it is the highly literate scientist who is proposing the theories under discussion in the first place. Given this situation, the nature of her/his mind is of definite importance in trying to fully understand these matters because of the innumerable assumptions, mostly unrealized and unexamined, that the literate mind projects upon the non-literate mind.

Finally, literate cultures rely upon and enhance scientific cognitive abilities primarily mediated by the left cerebral hemisphere, thus, it is probable that 30,000 years ago, the right hemisphere may have been more dominant than it is today and visual artistic tasks may have had a critical significance which is difficult for us to appreciate (Jaynes 1977) because of the powerful affective component associated with the perception and production of the art (Hodgson 2003a). Some of the major functions associated with the right cerebral hemisphere include social and emotional intelligence; nonlinguistic environmental awareness; visual-spatial perceptual functioning, including analysis of depth, figure-ground, and stereopsis; perceptual-manipulative functions; holistic apprehension; intuitive thinking; musical abilities; creativity; visual imagery; visual language as the basis of visual symbolism and metaphoric thought, and the mediation, elaboration and expression of affective states (Springer & Deutsch 1993; Hodgson 2003b). Mythic culture would have been not only dependent upon these abilities but would have contributed to the enhanced evolutionary development of them. Certainly, there would have been no need for specialized 'shamans' as the activities in which the entire community was involved were sufficient to produce spiritual experiences consistently in many participants without requiring any special mediator. But, having said all this, and even if all of the above speculations are correct, we still have no way of knowing *how* any form of trance was tied into the art (Bahn 1998, 237), nor *how* we could determine from the artistic images whether or not trance had been more involved in its inspiration than any other form of consciousness (Bahn 2001).

Unfortunately, Wilson conflates visual images and other 'unusual' experiences characteristic of naturally-induced trance states, such as hypnosis, meditation and relaxation, with the highly specific pattern of images that characterizes the TST model (which is based upon the ingestion of mescaline). We have previously pointed out the tendency of proponents of the model to make this error (Helvenston & Bahn 2002). The experiences reported by subjects of Etzel Cardeña (1996) are typical of hypnosis, meditation and deep states of relaxation (Holroyd 2003; Otani 2003) and include such events as imaging colours or a bright light, experiencing a sense of profound and unexplainable transcendental knowledge, feelings of floating, out-of-body experiences, perceptions of music, descending down a spiral staircase, entering a dark cave, etc. It is also important to note that Cardeña did not refer to these images as 'hallucinations', an important distinction that proponents of the TST model ignore, a topic beyond the scope of our response. Let us simply declare that not all vivid imagery experiences are hallucinations!

In the 1996 paper Cardeña did not report that his subjects described a few geometric figures. The experiences related by his subjects, while having some of the characteristics compatible with the TST model, were essentially different in pattern. For example, the TST model emphasizes the experience of imaging geometric figures, followed by images of more complex animal, human and therianthropic figures but none of Cardeña's subjects reported images of animals — whereas the TST model emphasizes such figures. The emphasis in the TST model is on vivid imagery experiences and while some hypnotic subjects do report vivid images, more commonly bodily sensations, floating sensations, fear, joy, ecstasy, dissociation from the physical body and transcendental experiences predominate.

In *Desperately Seeking Trance Plants* (Helvenston & Bahn 2002) Helvenston reported that she had never had any reports of hypnotic subjects describing geometric figures, nor did she know of any other reports in the literature to that effect. Wilson suggested that perhaps her use of hypnosis in a clinical setting may have been a determining factor. This is quite possible, as the general setting of the hypnotic induction forms an implicit suggestion, in often unidentifiable ways, as to what the hypnotic experience might entail, an idea that Cardeña had suggested in 1996. Most reports in the literature have typically described hypnosis within clinical settings where patients may be so preoccupied with physical and psychological health problems that these issues dominate the hypnotic experience. Such preoccupations could be one reason why geometric figures are not described in the clinical hypnotic literature.

Wilson's quotations from his 1999 interview with Cardeña indicated that some subjects had re-

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ported imaging geometric figures, and Cardeña was therefore contacted for further details of his methods and findings. He indicated that his comments in 1996 had only represented a very brief version of some of the data (an abstract of the results was published in 1988, and a paper reporting all the relevant details is in press) but that some of his subjects described 'geometric shapes, like a grid thing', 'a spiral staircase', 'a tunnel'. While Helvenston's subjects have reported the latter two (which, by the way, we don't consider to be geometric figures), no one ever described a grid or any geometric perceptions. Cardeña pointed out that there were a few other reports of images of geometric figures (Feldman 1976), including circles of light, a crescent, and stars spinning out from a vortex 'like spokes going out from a wheel', and we are very much indebted to him for acquainting us with this source. Geometric images have also been reported in hypnagogic and hypnopompic states (Mavromatis 1987).

Cardeña specified that his subjects were students in college courses. He never mentioned shamanism to them, and the data were gathered in 1987, before the burgeoning popularity of shamanic themes, although one of his participants, who had been disturbed by her strange experiences, reported the day after her hypnosis that she had begun to read a book on shamanism. One has to wonder what factors were involved in her choice of that subject at that particular time and whether these factors had influenced her hypnotic experience. The only suggestions supplied to Cardeña's subjects were that they were to experience as deep a hypnotic trance as possible and that he had no specific expectations as to what they might experience. He asked them prior to the induction as to what, if any, expectations they had of hypnosis and no one mentioned geometric figures.

None of Cardeña's subjects reported any current use of psychedelic drugs but one subject reported previous experience with LSD. This may be an important factor because ingestion of LSD, mescaline, or psilocybine, and the smoking of high doses of marijuana do produce geometric images. If subjects had previously had such experiences, the association between drug-induced trance and geometric figures could well influence the nature of a subsequent, naturally-induced trance. Seven of ten subjects in Feldman's experiment reported use of LSD or marijuana during the course of the experiment and eight subjects reported seeing some geometric designs, i.e. circles or a crescent moon. Since this study was carried out at University of California at Berkeley, Feldman notes that most of the students on campus were interested in psychology, hypnosis, altered states of consciousness, and psychedelic drugs. These interests certainly formed part of the 'mind set' which each subject brought to the hypnotic experiment. Apparently Feldman did not inquire about use of drugs prior to the inception of the experiment, so it is possible that most of his subjects had experience with them either before or during the study. Moreover, since geometric figures are reported in hypnagogic states, individuals who are prone to such experiences may be primed to image geometric figures in trance.

We still question why college students, in a non-clinical setting, would experience geometric images in hypnosis whereas clinical subjects do not. Cardeña believes the reports of geometric figures during hypnosis are rare because many investigators are not working with 'hypnotic virtuosos' (i.e. subjects with very high scores on standard hypnotic questionnaires) and they provide more specific instructions to subjects and limit the time of the hypnotic experience. He also suggests that most investigators do not attempt a micro-analysis of the experience as he and a few others do. While this may be the case with many investigators, Helvenston did conduct micro-analyses, with no instances of geometric figures reported by highly hypnotizable subjects who were also suggestion-free in all of their initial hypnotic evaluations. We do not know the content of Cardeña's subjects' course of studies, of their exposure to a variety of subjects in assorted media, or of their particular 'mind-set' in approaching the hypnotic task.

We wonder if those who volunteer for such experiments might not have a deep interest in shamanism, altered states of consciousness or paranormal experiences, and have actively sought out information about such phenomena that they did not report prior to hypnosis. Such knowledge would certainly form a stored experiential base that could be drawn upon during hypnosis, and as we pointed out (2002), subjects would undoubtedly experience geometric figures if they had any suggestion, either sometime prior to the experiment or during the hypnosis, inspiring them to do so. At any rate, there is some evidence of geometric imaging in naturally-induced trance states, but we think the factors facilitating the perception of such images require further investigation. Our main point is that Cardeña's subjects, while reporting some images and experiences consistent with the TST model, did not experience the over-all pattern of imagery that is predicted by

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it. Rather, their dominant experiences are consistent with well-known hypnotic phenomena.

Pearce criticizes our paper because we fail to 'argue the heuristic and methodological merits of the TST model'. Time and again when the data fail to support the TST model, its proponents argue methodology. Although we have a great deal to say about comments which Lewis-Williams has made in emphasizing the superiority of his methods, space limitations prevent us from elaborating on those issues here, except to say that Lewis-Williams devoted considerable effort to defending his use of analogical methodologies based upon ethnological research and documentation. We recognize the importance of ethnological research when it is interpreted accurately, as we believe that San historical documents are treated by Solomon (1997; 1999; 2001; pers. comm.). Like Pager (1994) and Hromnik (1991) before her, she has clearly demonstrated that Lewis-Williams has simply projected his shamanistic TST theory onto that body of data. We also believe that arguments from analogy are an acceptable method, as long as they are supported by some historical and/or ethnographic sources. In the absence of those sources, such a method becomes essentially an informed speculation and should be identified as such, particularly when attempting to posit mental and sociocultural attributes to *Homo sapiens sapiens* some 15,000–30,000 years before the present.

We are tempted to reject the remaining criticisms because Pearce has either failed to read our relevant papers or he has deliberately misquoted from them. We will simply comment briefly upon the most egregious of Pearce's misattributions. In claiming that we have ignored a 'morass of unspecified assumptions' Pearce says that we presuppose the three stages of trance 'were experienced as an ineluctable progression'. We do not assume such, have never said we did and have been aware of Lewis-Williams' disclaimer as quoted by Pearce and discussed previously by us (Helvenston & Bahn 2002, 17). Nevertheless, all proponents of the TST model have stressed the importance of geometric figures early in the drug-induced trance state, followed subsequently by more complex visual imagery experiences. All of the papers written by proponents of the TST model continue to cite three stages of trance as the *typical* trance experience for all altered states of consciousness, across all time and geographical locations. They have emphasized all three stages as a *sine qua non* of trance experience *per se.* Therefore, whether or not there is an experience of all three levels of trance for the majority of individuals is clearly **not** irrelevant to Lewis-Williams & Dowson's central argument, as Pearce would wish us to believe and as Chippindale attempted to argue previously (2003).

As we demonstrated in our 2002 booklet, the TST model was derived *in toto* from the work of Henrich Kluver (1928) and was based entirely upon his subjects' descriptions of geometric and highly complex visual imagery experiences following ingestion of mescaline. That pattern of drug-induced vivid imagery experiences is only produced by three substances: mescaline, psilocybin and LSD. This is an empirical fact. We explored in some detail the evidence of many other substances popularly believed to induce hallucinations and six naturally-induced trance states in our previous publications and found that none produced a pattern of vivid imagery experience similar to that of mescaline. We discussed the fact that Ludwig had listed some 70 different forms of altered states of consciousness, and that none of these produced a pattern of images consistent with the TST model (except mescaline, psilocybin and LSD) as far as we had been able to ascertain. If the proponents of the TST model have evidence to the contrary, it is their obligation to publish it, and not spin spider-webs of irrelevant digressions that clearly waste the readers' time. Neither mescaline nor psilocybin has ever been found in Europe and there has never been a culture that deliberately ingested LSD until modern times. Incidentally, none of these plants has ever been shown to grow in South Africa, and therefore they could never have been used by the San. This is another major factual error of the TST model.

Whether or not plants containing these substances ever grew in Europe is another empirical question. Since palaeobotany has little difficulty identifying pollens, seeds, spores, plant forms, etc. from thousands of years ago (Mercuri 1999) it is only a matter of time before evidence of plants containing mescaline and psilocybin will be found in Europe *if they ever grew there*. In fact, there are numerous worldwide data bases containing evidence of assorted plant remains from the remote past, and there is a centre for the study of palaeobotany, palynology and palaeoecology in France that provides lists of plants known to have grown in that region during previous millennia.

We have no personal investment in whether or not plants containing mescaline and psilocybin are found in Europe. Although plants containing these substances are frequently found in South America, particularly, and North America also, there is no

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evidence that any such plants ever grew in Europe and we have simply pointed this out as a serious problem for the empirical basis of the TST model. In our view, this fact refutes the model. Lewis-Williams (2001) pointed out that 'normal' science, as discussed by Kuhn (1970, 5) 'often suppresses fundamental novelties' as if critics of the TST model would attempt to conceal evidence for it. Such a position would imply that confirmation of European mescaline and psilocybin-containing plants would be ignored by critics of the TST model or simply 'not perceived'. This is quite preposterous, and we are willing to bet that if such plants ever grew in Europe, the odds are they will be discovered and widely reported.

Pearce asserts that we *assume* plants alleged to have produced images consistent with the TST model should be found in the area of the rock-art site. We base our discussion, however, upon the empirical findings of Lewis-Williams who found that most of the San rock-art sites at Giant's Castle and Barkley East, South Africa, could have been living sites and the presence of the artefacts in many tended to confirm that conclusion (1981). We state very explicitly that there would be material residues of the plants in the general region in which the cave art is found and the findings by Boyd & Dering (1996) support this contention.

Pearce states that we dismiss the use of 'neuropsychology' in Upper Palaeolithic research in its entirety. This is absurd. One of us is a neuropsychologist who has been interested in the evolution of the human brain and mind since adolescence. Indeed, we take neuropsychology so seriously that we have been forced to criticize a simplistic, unsophisticated and trivial resort to neuropsychology by the proponents of the TST model. These individuals assume that the human brain and mind of Upper Palaeolithic peoples who produced the rock-art images were identical to the modern brain and mind. They also assume that alleged rituals of shamanism occurring 15–30,000 years ago were similar to the sociocultural patterns in societies who have practised shamanism over the past few thousand years. We have addressed these topics already and can 'neurologize' (speculate about mind/brain functions) endlessly, as well as others, but in the final analysis there is simply no way of knowing with a high degree of certainty what the mind of people living 30,000 years ago was like. At best, our attempts are more or less well-informed speculations.

To assume that the artists required a trance experience in order to produce the rock art, as proponents of the TST model do, is startling to us because, again, it depends upon the assumption of identical minds for Palaeolithic peoples and contemporary Western peoples. Such an assumption ignores some 5000 years of literate cultural and cognitive development that incorporates Sumero-Egyptian, Greco-Roman, Judeo-Christian, medieval-Renaissance, Indo-Arabic, Western-European and American technology and science. To imply, as Pearce does, that we do not value 'less rational aspects of consciousness' is quite preposterous since Helvenston is a neuropsychologist whose professional education and practice involved explorations of the unconscious motivations of behaviour and the study of altered states of consciousness. In this regard, Pearce assumes that we are ignorant of and/or have some prejudice against the fact that drug-induced trance has been and is a critical aspect of the mythic, ritual, and religious practices of various traditional cultures around the world. In fact, Helvenston has studied such cultures and the psychoactive substances they utilize for thirty years. It is the actual familiarity with this vast body of work that led her to criticize the lack of such a data base as far as the application of the TST model to Palaeolithic cave art was concerned.

Pearce says that it is time for research to move forward. We agree. Wilson claims that we have put forward no alternative, and merely argue for a negative, without offering our own suggestions. Such comments merely expose a profound ignorance of the literature concerning Ice Age art; we have indeed put forward convincing evidence (e.g. Bahn 2003) that some Palaeolithic cave art is strongly religious in motivation, based entirely on solid data such as its location, rather than on spurious speculations, outdated or erroneous neuropsychology and distorted ethnography. Pearce wonders why a model that is 15 years old is still being debated, and why 'papers that criticize misrepresentations of early research continue to be published'. The answer is simple. As explained elsewhere (Helvenston & Bahn 2002, 8), it took years for true specialists in shamanism, and subsequently neuropsychologists, to become aware of the distortions of their data that had been perpetrated in a completely different domain; and at the same time, the simplistic but erroneous picture of the past presented by the model's adherents has inevitably appealed greatly to the media and to uncritical minds. That is why it is still regrettably necessary to expose the fundamental errors in this approach.

Finally, we turn to Clottes, whose comment is

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simply disappointing. He did eventually correct his earlier error regarding the title of the Féniès book, but not, alas, his misunderstanding of that book's contents. And far from taking a 'cheap shot', we were in fact being charitable to him by focusing on the Féniès reference, by far the most serious source on which he has drawn. But now he has drawn attention to other, weaker sources, we shall examine them in full. For a start, it is essential to go back and review Féniès (1965, 41–2) in order to describe accurately what has been cited by Clottes as evidence for caves as a 'hallucinogenic milieu'. Again, we would caution proponents of the TST model that not all *vivid imagery experiences* are considered to be 'hallucinations', and part of Clottes's critique of our paper may lie in semantic misunderstandings which we will attempt to clarify here. The chapter in question is entitled 'Wanderings of the Senses' and describes a number of unusual visual phenomena, occurring in response to prolonged near-darkness. Only *two* of these are referred to by Féniès as *visual hallucinations*, as we noted in our *CAJ* paper (Helvenston & Bahn 2003, 221). The visual images reported in this section were described by a subject as luminous dots, moving like comets, slowly. Two other people described a light that became brighter and brighter and then disappeared. These phenomena would be referred to by most neuropsychologists as phosphenes, or very simple visual hallucinations of a non-veridical nature. Féniès stresses that the perception of these visual images is strictly a cortical function, as has been demonstrated by numerous empirical studies. Lewis-Williams & Dowson (1988, 202) made a clear distinction between 'entoptics' (form constants and phosphenes) and hallucinations, but their concepts and terminology are not consistent with accepted neuropsychological practice. Their 'entoptics' are actually non-veridical, simple hallucinations mediated by the primary visual cortex, not the eyeball or other sub-cortical optic structures.

The other unusual visual sensations that Clottes refers to as 'hallucinations' are actually discussed by Féniès as representing the well-known disappearance of morphoscopic (sense of shapes and contrasts) and chromatic (colour) perception in a very low-light environment where vision is mediated primarily by mesoptic vision. (Photopic vision is commonly referred to as day vision, mediated by cones. Scotopic vision is night vision, mediated by the rods. Mesoptic vision is mediated by both rods and cones in an environment that provides low levels of light.) In this section, Féniès describes an individual who was sleep-deprived for 50 hours, who kept bending down to pick up satchels of fossils that were actually rubble. Clearly his morphoscopic and chromatic perceptual abilities had disappeared. This particular example is not, however, a simple demonstration of the loss of these perceptual abilities in a darkened setting, because he had also been deprived of sleep for 50 hours, a factor that can produce dream-like, hallucinatory phenomena (Tyler 1955). Whether or not the remaining examples in this section also referred to individuals who were sleep-deprived in addition to having lost morphoscopic and chromatic perceptual abilities is unclear, although the nature of their visions suggests that sleep deprivation was involved. They certainly did report some bizarre visual experiences. For example one individual reported seeing a souk filled with carpet vendors, dunes and skulls, but Féniès maintains that these phenomena are not hallucinations. He says that similar perceptions occur when an individual emerges from a cave after a long stay, where the sky may appear to be pink. Such images appear to be the result of the fact that prolonged darkness produces certain specific alterations in the excitability of receptors at the retinal level, and in neurons in the visual cortex to subsequent light stimulation (Boroojerdi *et al.* 2000; Yang *et al.* 1988).

In this regard, Féniès discusses what he refers to as after-images, and he provides some anecdotal examples whereby visual imagery is reported after emerging from a cave. In one instance a man reported seeing some coloured concretions between his eyes and the wall of his room some seven or eight hours after exiting the cave. In another example, a man saw black patches of moving geometric forms that began about one hour after he emerged from the cave. Again, these after-images are not considered to be hallucinations by Féniès. Moreover, since they occur upon emerging from a cave they would not appear to provide support for the TST model, because according to that hypothesis, the paintings would have been inspired by the darkness of the cave and been in progress or completed prior to emerging into daylight.

Let us now examine the Palaeolithic cave art for examples of 'spots' of light. There are numerous 'dots' in Palaeolithic cave art, but they are rarely nicely circular (many of those in Chauvet were made by slapping the painted palm of the hand against the wall). Others in Chauvet are laid out to form animal shapes. The other good examples of dots in cave art are at El Castillo (Spain), where they are strung out in a single or double line along one wall of a corridor; and in Pech Merle where there are some inside

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and around two horse figures. In the Combel gallery in Pech Merle, there are lots of red dots on the ceiling, in clusters. At the recently discovered cave of Frayssinet, there are many dots (primarily black) in the upper cave, in groups or rows; in the lower cave, there is a little 'cloud' of small black dots, made with a fingertip (Lorblanchet pers. comm.). While some of these dots ('of light'?) may depict the experience of phosphenes or simple hallucinations, it is difficult to see how one would distinguish the recording of that experience from numerous other explanations of such dots. Moreover, these are only a few examples of the possible depiction of phosphenes out of hundreds of caves where no such depictions exist at all. The above-mentioned dots are more difficult to explain from the TST perspective, in that the phosphenes in the literature cited by Clottes are white in colour, not red or black.

The only examples we can think of, in the entire body of Palaeolithic rock art, that could possibly depict the experience of a gradually increasing source of light as reported by one of Féniès' subjects, would have to be three caves, Altexrri, Covaciella and Chauvet. In those caves a few walls have been scraped to provide a lighter background for new figures to stand out against. We do not consider this an example of back lighting (though others may), but this is only three caves out of hundreds. If the cave environment is viewed as stimulating the experience of phosphenes, it would seem that there should be hundreds of depictions of such figures, rather than only a tiny handful.

We mentioned in our *CAJ* paper that other speleological sources reported auditory hallucinations which upon reflection, they realized were non-veridical and we have never disputed the fact that some speleologists have experienced auditory illusions and hallucinations. Still, we wonder how proponents of the TST model believe that auditory hallucinations affected the depictions of visual hallucinations in the cave art.

Turning now to Clottes's other sources, let us note that after-images, loss of morphoscopic and chromatic perception, sleep deprivation, and severe exhaustion play a major role in the production of unusual visual experiences in these sources just as they did in Féniès's reports. Simonnet (1996, 343) does *not*, contrary to Clottes's claim, mention 'repeated visual hallucinations'. What he says is that long stays underground could have had, among other consequences, effects of a hallucinatory kind. For example, when he was young, during excavations in 1947–50, he underwent sessions in Labastide cave of 15 hours, sometimes staying awake for 36 hours. In the feeble light, there was attenuation of colour vision, and he began to see landscape lines evoking the outside world in the sinuosities of a white calcite thread on the ceiling. This may only have been one incident rather than many — we are not told — but in any case it is clear that prolonged darkness and sleep deprivation were to blame.

The article by Renault (1995–96), which appeared in a magazine of the paranormal, contains a number of anecdotal accounts of 'speleological hallucinations', all clearly tied to 'limits of resistance' and exhaustion. For example, two boys trapped in a cave for two days and two nights often saw a light that grew bright and then disappeared. A man saw lights everywhere, especially small brightly-lit houses, when he was exhausted after several days. Again, these perceptions seem to be a result of sleep deprivation. The great pioneer of speleology, Norbert Casteret, reported seeing varied 'lights' and very bright colours after several days underground. Renault himself saw his first 'underground hallucination' after 48 hours of exploration.

Clottes also cites the work of Saumande (1973), as quoted by Renault, but (unlike us) has apparently not read it himself. In this thesis of more than 200 pages, devoted to 'human behaviour in an exceptional milieu, the underground milieu', the late Pierre Saumande devoted three pages to hallucinations — an even tinier percentage than in the Féniès thesis, thus further underlining the lack of importance of this phenomenon. He begins by stressing that when one analyzes tales of underground hallucinations, they always coincide with a lowering of vigilance, a disturbance of the circadian rhythm, or a very advanced state of exhaustion, and often a combination of these (Saumande 1973, 94). He also emphasizes that there are not many such reports. After repeating some examples from the Féniès book, he mentions someone who — above a deafening waterfall — had auditory hallucinations while exhausted after 16 hours of difficult progress. He also quotes a tale by C. Queffelec, who saw a giant bug, several metres across, on a cave wall; once again, Queffelec adds that 'I was very cold, I was also very tired, and I attribute it to this exhaustion and the fact that I was completely frozen'.

Saumande's principal example (1973, 95) is an experience of Renault himself, as reported by R. Algiboust (Renault's article also gives this same account, and identifies Angiboust [spelled differently] as a colonel and a medic). In the course of a particularly difficult speleological expedition, a luminous

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zone was seen above a waterfall, and eventually turned into a cafe terrace with the upper rocks as hanging vegetation, and the lower rocks as tables and chairs. Renault (through the narrator) emphasizes that 'I was close to exhaustion with at least two to three days of rest needed to recuperate', and he says that the best comparison he can think of is becoming extremely sleepy during a long drive at night (Saumande 1973, 96), when the eyes start to close and one's attention wanders. Renault himself (1995–96) continues the tale by revealing that, having left the cave the extreme fatigue continued, and, while motoring slowly on his Vespa, he underwent frequent hallucinations (after-images in Féniès's terminology) with strange interpretations of the shadows — and he only just missed a dog, which was a patch of oil on the road. One would expect that, for Clottes's logic to be in any way consistent, he must therefore consider a vehicle at night to be a hallucinogenic milieu, and thus, presumably, night-time drivers who see things must be shamans?

We should also point out that a number of prehistorians have spent very long hours in dark deep caves, concentrating furiously on tracing images that are often very hard to see, and thus obviously putting their tired eyes under great strain. Breuil, as is well known, reckoned that he had spent more than 700 days of his life making underground tracings (Bahn & Vertut 1997, 49) but, as far as we are aware, he never mentioned any kind of hallucinations. Glory, during his campaigns of tracing engravings in Lascaux, often worked from dusk till the early hours of the morning (Delluc & Delluc 2003, 22) — and from 1952 to 1963 this work demanded thousands of hours of effort — 5000 by some estimates (Delluc & Delluc 2003, 25). And Lemozi (1929, 41) reports spending 12 or 13 consecutive hours tracing the art of Pech Merle, only interrupting the work for a few minutes of food. When one includes the arduous process of getting to and from the decorated galleries at that time, and setting up the work, this must indicate sessions lasting the best part of a full day. Yet none of these pioneers mentions any kind of hallucination.

Clottes accuses us of 'sleight of hand'. We believe that such an accusation might more accurately be applied to those who cite carefully selected and highly obscure references, some of which they have not even read, and which in any case do not support their claims. This is by no means the first time that this tactic has been adopted by the proponents of the TST model; indeed it has almost become a hallmark of their approach.

As a conclusion, we feel it necessary to spell out some indisputable facts in the simplest terms so that nobody can misunderstand them in the future:

1. The evidence cited from Clottes's sources reveals that caves are *not* a hallucinogenic milieu, with actual hallucinations occurring only very rarely, although other visual abnormalities such as achromatica, after-images, etc., do occur.
2. Virtually all the experiences of visual hallucinations in caves are directly linked to extreme exhaustion, and not to the caves *per se*.
3. Even under conditions of extreme fatigue, such experiences in caves are extremely rare.

In a recent review of yet another utterly uncritical book about the shamanic obsession in archaeology, Lewis-Williams (2003b, 95) declared that 'with a bit of luck, [the book] may be a prelude to some more cries of "mea culpa"'. We fervently share the hope that some of those who have been promulgating serious errors of fact and interpretation will indeed now acknowledge that they have been mistaken on some points, and issue some 'mea culpas' — for example, about caves being a hallucinogenic milieu. In view of their long record of shifting goalposts and ducking and weaving, however, we sadly doubt that they will ever do the honourable thing, preferring instead to remain stubbornly and ineluctably 'trance-fixed'.

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