

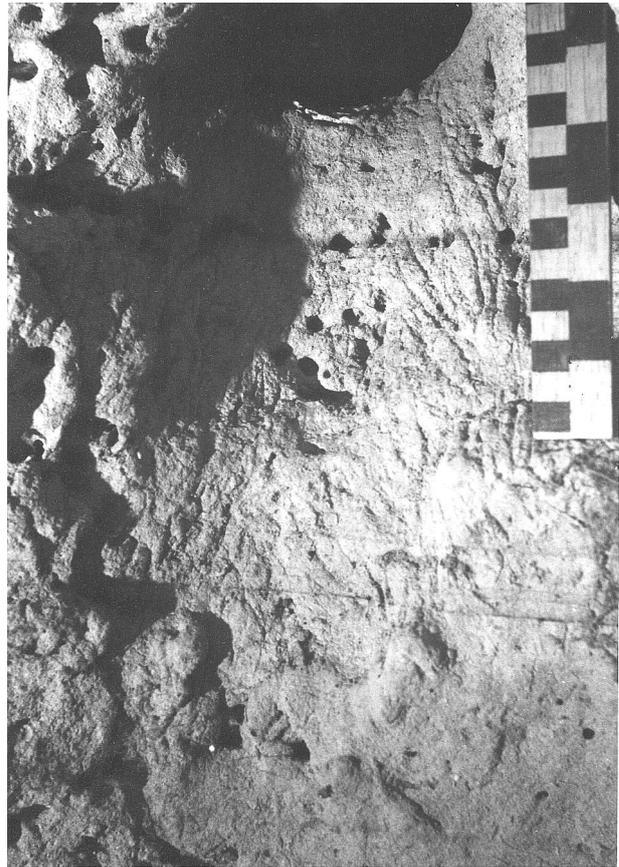
## *Rock markings of humans and other animals*

By ROBERT G. BEDNARIK

Sharpe's primary concern here is that my assessment of the Koonalda boulder markings he has many years ago mistakenly described as petroglyphs (Sharpe and Sharpe 1976) 'ought not to stand merely on rhetoric or subjective interpretation'. This assessment, however, is neither rhetoric nor subjective interpretation, but is the result of several decades of intensive study of animal markings in about 1000 caves, well over 300 in Australia alone. As part of these studies I have conducted numerous experiments, for instance with the paws of live animals as well as dead. I have undertaken extensive field microscopy and morphological surveys of countless numbers of such markings (e.g. measuring claw spacings or incision depths, defining groove sections). I have even studied, measured and photographed hundreds of similar mammalian markings on masonry, painted clay bricks, wood, tree trunks and rock, for instance those occurring at possum dreys or on building walls (Figure 1). The purpose of this work was to secure generic knowledge about nonhuman animal marks before applying it to the wall markings in caves to better understand them. Sharpe considers this a subjective approach, while at the same time belatedly recommending that it be done. Let us see what he offers in its place.

Without previous experience in the study of either cave art or animal markings in caves, Sharpe has in 1973 briefly ('In the short time we had'; Sharpe and Sharpe 1976: 128) examined 'stream-like' markings on many of the boulders of what he calls Rock-fall C in Koonalda Cave. He thought them to be human engravings, without considering any alternative interpretation, and apparently unaware of the trillions of animal claw marks that exist in the caves of the world. In the decades since then he has not returned to the site to re-examine these markings in the light of developments since, and he has failed to absorb the relevant literature (for instance, he still cites Marshack's 1977 ideas of meander streams). Nor has he seen natural markings at more than a few sites, for instance he has not been in any of the other Australian caves he mentions, except Tanatanoola Cave where he spent about one hour. Moreover, he has never conducted analytical or experimental work with claw markings, be it of types he would presumably not dispute (e.g. those found at occupied dreys) or of types that he might dispute.

Before responding to this paper I need to clarify an is-

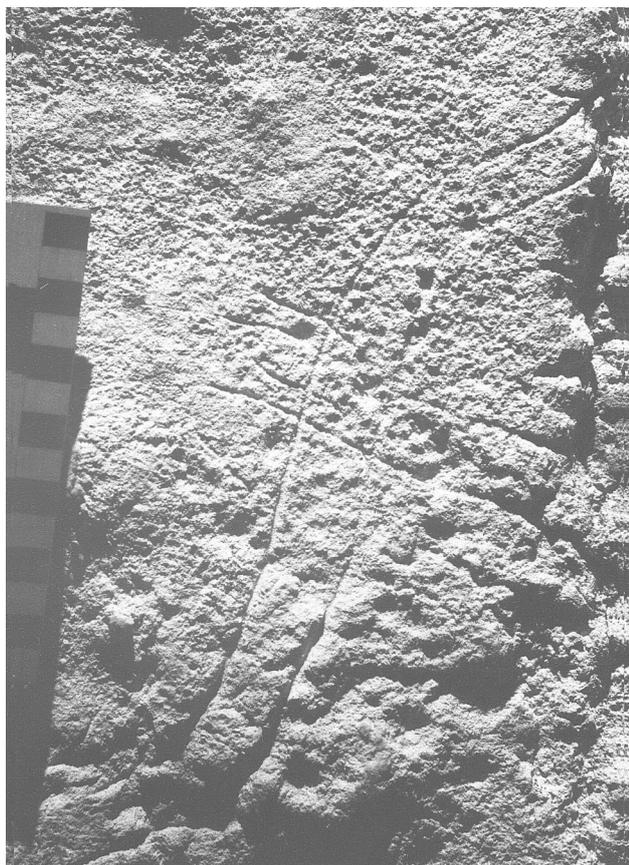


**Figure 1.** *One of several possum dreys in limestone I have examined. This one is located in a rockshelter near Blanchetown, Murray River; that is heavily decorated with petroglyphs. However, the marks visible just below the opening of the natural rock hole, which at the time was occupied by *Trichosurus vulpecula*, were made by animal claws and are of recent age.*

sue of ideology: Dr Sharpe is primarily a theologian, which may account for the contradiction contained within the title of his paper. For me, a scientist, all cave markings by creatures capable of locomotion are animal markings. That includes humans, Martians, or anything else that crawls, walks, flies or swims. Hence, I write of 'natural markings' and 'nonhuman animals' (Bednarik 1991a). This is not an issue of semantics, but one of fundamental differences of ideology.

Sharpe's lack of familiarity with the material in question and the literature about it is illustrated throughout the paper, for instance when he writes that there are sets of markings on Koonalda boulders comprising up to eight lines. I have seen no sets of more than four lines that can safely be attributed to one single 'tool' application. In addition, there are no sets that 'branch' from others; there may be the rare, occasional case of a single line apparently branching from another single line, but that is to be expected in any random accumulation of hundreds of lines on a single panel. Similarly, sets that cross over other sets will often occur in such panels, and to describe them as 'grids' falsely ascribes intentionality (Figure 2). Sharpe mistakenly claims that only what he calls Rock-fall C exhibits

line markings. Certainly, their density varies, but in principle they can be found in all parts of the massive talus formations (i.e. in his Rock-falls A to E). They are equally common along the walls of the cave, a point he failed to notice in 1973.



**Figure 2.** One of thousands of grid-like sets of animal claw marks I have examined. This one is from Wombat Cave, South Australia.

Nonhuman markings are thousands of times more common in caves than are linear incisions made by humans, therefore if we are confronted by a wall marking we find hard to identify, the initial probability that it was made by a nonhuman animal is thousands of times greater than the probability that it is rock art. This is the brutal truth, and for this reason alone, the onus is on the researcher to show that *this is not a nonhuman mark*. Sharpe, who has long been trying to interpret Koonalda markings as a form of writing, will need to falsify the proposition that his 'streams' on the boulders in that cave were made by animal claws before he can validly think about their epigraphy. There is no value in reading as script rock markings that were made by animal claws.

Sharpe mistakenly thinks that I omitted to list the engravings of Koonalda in my 1990 summary of Australian cave art. If he consults Table 1 in that paper he will see that I listed the cave as containing numerous 'tool marks' (see also Bednarik et al. 2003, listing 46 sites). Most certainly there are engravings in Koonalda, but I found none among the boulders with their multiple claw mark sets, which were made either by nonhuman animals, or by Aborigines with

an animal paw who were very determined to trick us into believing that these are animal claw marks. Sharpe misinterprets his own 1976 paper when he characterises it as stating that *some* of his Koonalda boulder markings are human. This is not what his paper conveys, where in fact an alternative explanation is not considered at all, and where the makers of the 'engravings' are consistently defined as 'artists'. Sharpe also asks, 'where are Bednarik's clear criteria', which illustrates his superficial reading of my work as well. For instance, he cites Bednarik (1998), but he has not noticed that this paper provides such detailed criteria. Sharpe continues: '[t]he field needs communicable criteria, reliable guidelines, and more objectivity. So, in an ideal world, what distinguishes markings of a human origin from those of an animal origin?' So many of the queries he has are anticipated and considered in that 1998 paper that it deserves to be cited at length:

To recognise individual animal scratch marks requires considerable experience, because their morphologies differ greatly according to the species' climbing ability and method, 'speleo-behaviour', mobility, relative length of extremities, shape of claws or talons and their mechanics of application, and according to the shape of the claw points. The latter, for instance, can vary according to local conditions, the specimen's age, and so forth. Claw marks of Chiroptera (the most common of all animal scratch marks) may be quite different from those of similar-sized animals that are unable to fly. Moreover, great variations can be caused by the lithology of the support rock (relative hardness, moisture content, relative air humidity etc.), and most particularly by modification processes (weathering, including speleo-weathering, the deposition of speleothems, and the deforming action of some precipitates, notably certain types of carbonate deposits). It is therefore necessary to appreciate that there are no simple, ready-made rules for discriminating between animal scratch marks and other, similar rock marks. Rather this is a process of elimination in which many factors need to be taken into account, and in which alternatives have to be discounted systematically.

The empirical basis of this discrimination process consists of two bodies of evidence: the study of markings that can safely be attributed to animal species (e.g. megafaunal marks, which have been most thoroughly studied in Europe, such as those of *Ursus spelaeus*, and in Australia), and the study of 'experimental' animal markings. The latter have involved a number of species, and in Australia especially possums. I have documented several instances of occupied possum dreys in limestone cavities (including in actual caves), and have microscopically surveyed the fresh climbing marks in the immediate vicinity of the occupied lairs of *Trichosurus vulpecula*. Marking experiments have also been conducted with live specimens, and their claws and claw spacings examined as part of this project. I have not conducted such experimental work with *Sarcophilus*, but have studied the very numerous claw markings in known lairs of the Tasmanian Devil, e.g. in Koonalda and Tantanoola Caves.

In Australia, four broad categories of animal scratch marks on rock surfaces have been distinguished. Large accumulations of claw marks are usually much easier to identify than isolated or single marks. One of the distinguishing characteristics refers to the relative positions of multiple marks constituting a 'set', and the relative course

of individual grooves of a multi-pronged instrument such as an animal paw. Small rock fragments may only bear one or two grooves of a set (either exfoliated from a wall, or marked in situ within the sediment), which renders discrimination more difficult than on a cave wall. Nevertheless, other variables remain and can be consulted quite effectively in such cases, referring to the experience gained from parietal markings. These include:

1. Longitudinal striations are very frequently present in lines engraved with stone tools, and several distinctive forms are recognised by researchers as being characteristic. Animal scratches typically bear no striations.
2. Even if a claw did bear some irregularities which would produce striations, as may conceivably be the case, these would significantly differ from those occasioned by stone points. In the latter, the point is usually slightly turned over the course of a groove, which results in significant changes in the longitudinal striations. This is particularly clear at changes of direction. A claw point, forming part of a multi-pronged instrument, cannot be rotated in the same way.
3. Claw points are always rounded and comparatively symmetrical, stone points are rarely so.
4. In cross-section, a claw-caused groove is rather U-shaped, with the sidewalls steep, and *striae parasites* are never present.
5. Morphologically, claw marks are frequently of slightly 'cuneiform' appearance, i.e. with one end deeper and abrupt, and the other shallow and 'fading'. This applies especially to short marks.
6. Where such a mark is well preserved, the deeper and wider end can provide a fairly good impression of the shape of the claw point.

If the limestone is very soft, lines may have been incised by a material such as bone or even wood. This would be much harder to distinguish from claw markings than are stone marks, but there is no evidence of such tool materials having been used, either in Australian cave art or portable engravings on stone. I have conducted experimental work with dry and 'green' wood, bone, and other materials, including in south-western Western Australia. Tool marks occur very frequently in about a dozen Australian caves we know of, and they have been studied in some detail in Nung-kol, Mooraa, Paroong, Ngrang, Orchestra Shell and Mandurah Caves. In all cases they could be demonstrated to have been made with stone tools, and the stone types of these tools were convincingly determined at two sites, from their distinctive striation patterns.

Is it possible to comply with Sharpe's key demand that the 'field needs the formalisation of the experience of researchers so that decisions can be made on lines of uncertain origin, so that inexperienced researchers can learn, and so that debate and decision can be made on potentially incorrect guesses or experience-based interpretations'? This is not possible within the academic context he probably has in mind: it cannot be learnt from a book. The ability of the Aboriginal tracker to detect the near-invisible cannot be abstracted in simplistic empiricist terms; it cannot be deconstructed into its components and then re-assembled without considerable loss in resolution or integrity. If this

were attempted it would result only in a parody of the ability of tracking. In much the same way it would be extremely difficult to write a book that could convey how nonhuman markings are distinguished from human.

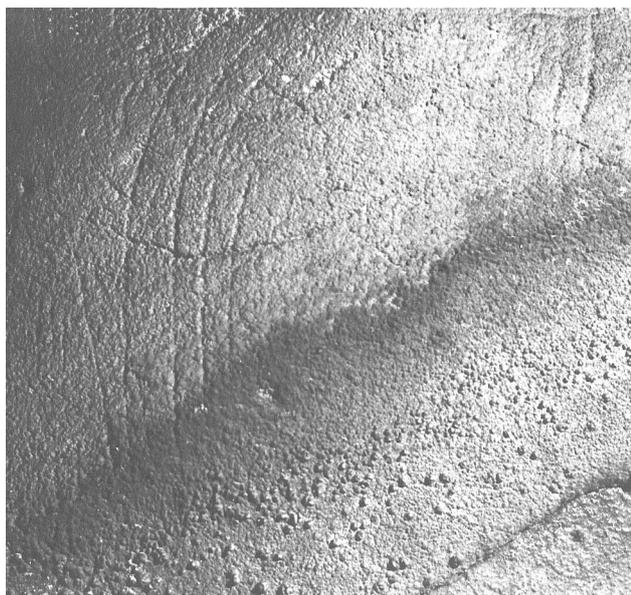
Sharpe's demands are those of naive empiricism, confusing science with scientism. 'Given Rock-fall C's location in pitch-blackness, its difficulty of access, its depth within the cave, and the freely available rock on the surface of the Nullarbor Plain, however, it seems most unlikely that a non-trogloxene would go there to perform some daily activity that requires clawing', he muses. He seems to think that the reason why there are so many animal scratch marks in caves is that certain species seek out such sites to perform some 'daily activity'. The reason for such concentrations is of course taphonomic, and the same as for cave art: limestone dissolves rapidly where it is exposed to rain, especially in an acidic environment. Hence, markings out of caves survive only for a short time, whereas in caves they accumulate over tens of millennia. Similarly, Sharpe still does not seem to appreciate that if some of the Koonalda finger flutings precede the most recent rock-fall events, then the boulders on top of these rock-falls must be more recent than the finger flutings that are concealed by them. The same applies to any animal scratches on these boulders, which must necessarily postdate the event of the rock-fall. Sharpe also seems to assume that the cave's entrance accessibility was always as it is today, which is almost certainly false. Caves that developed along the upper zone of an aquifer are often tectonically unstable, because their structural stability is subjected to the effects of watertable fluctuations. Moreover, the description of three 'cliffs' is an exaggeration: his second 'cliff' is a talus slope averaging not much more than 30°, which I can descend by running, and his third 'cliff' is a boulder slope of similar steepness, perhaps 15 m high and still reached by daylight, and certainly not a major barrier to many animals. Conversely, the entrance of Koonalda Cave is not a doline, but a sinkhole. A doline is a closed depression in a karst region. I also reject many of his descriptive details of Koonalda, for instance his 'Directional Stele' (Sharpe and Sharpe 1976) is a figment of his imagination. I have seen no stone arrangements in the Upper Chamber, and his description of the 'stele' illustrates his approach:

When one of us was working alone, deep in the wall gallery area, the presence of this stone glowing some distance away in reflected light was strongly felt. We named it the Directional Stele because we used it to find our way into and out of the wall-marking area. One only needs to walk towards it, and when reached the rest of the path is clear (Sharpe and Sharpe 1976: 129).

Pleistocene visitors lacked not only Sharpe's disposition, they also lacked electric lights and can safely be presumed to have been unmoved by this glowing light of direction. The 'paths' often referred to by Sharpe, too, are a result of his subjective perception, yet he hesitates not to describe vastly more comprehensive and objective work as 'subjective'.

Sharpe mentions that I listed Princess Margaret Rose Cave as one of those sites that could not have been accessed by Indigenes, so the scratch markings in it are clearly

by nonhuman species. We have in fact studied very numerous such caves that exclude human markings, either because their entrances were opened only recently (e.g. Tantanoola Cave; see Figure 3), or entering them required mechanical means not available in the past. Sharpe's question, could macropods access the Upper Chamber, is easily answered: I have seen the tracks and skeletal remains of non-trogloxenes up to two kilometres from the entrance of some caves, and have discussed their ethology when trapped in a cave in some detail since first recording such data in the Gläserkogelschacht in June 1963 (see Bednarik 1991a: Fig. 1). Thus my experience with this material, acquired over more than four decades, exceeds Sharpe's many times over.



**Figure 3.** *Animal claw marks in Tantanoola Cave. Those of two species are visible on the left.*

Sharpe has no difficulty accepting the identification of animal scratches in French caves; in fact he advocates the identification of cave bear scratches even where they occur immediately next to human markings. His discussion and illustration of apparent human reactions to bear scratches in Rouffignac is an important contribution to the general subject. He is certainly right that at least some of the configurations, such as he depicts in his Figure 4, seem to record such reactions.

Of course it would be possible with a hand-held animal foot to produce markings that would be almost indistinguishable from genuine animal scratches, particularly if the person had a good understanding of animal behaviour in caves. Similarly, we cannot guarantee that stone tools or shards or whatever we excavate from an archaeological layer have not been planted there by a skilled person trying to mislead us. However, it must be remembered that the hand-held animal paw hypothesis was first proposed by Hallam at Orchestra Shell Cave, where she thought that the finger flutings were made with severed paws attached to long sticks. She was doubly wrong in this instance: first because the steep floor of that cave was much closer to the ceiling when the marks were made (the floor subsidence is

clearly evident), and secondly because they were made with human fingers. However, there is another aspect to the severed paws hypothesis: assuming that some of the marks were made in this way, ostensibly to copy natural marks, what would it tell us? Would it imply that the marks were some kind of writing system?

Sharpe presents a critique of the simple triangulation method I once offered, suggesting that it needs to be more developed. He would prefer to see 'location' included as a further dimension, but in a cave, present location often does not equate with past location. Caves are regularly filled with or emptied of solid or liquid materials, and their morphology is notoriously impermanent. Both the evacuation and convacuation volumes of caves expand and contract over time, and it would be naive to suggest that the relative location of anything in them is somehow fixed. It is necessarily in a state of flux, when seen in the long-term time frame demanded by ancient wall markings. The same applies to such aspects as hardness of rock or rock-like speleothems: it is in a constant state of flux, depending on speleoclimatic and hydrological circumstances. Therefore location in relation to some boulder or floor or cave space, or relative hardness of wall, or which side of a boulder is up are all variables that can and do vary over time.

In seeking to expand the idea of using triangulation to determine details of natural cave markings I think Sharpe needs to appreciate that the purpose of this model was not to discriminate between human and other animal markings. He also needs to note that I specifically stated that this model should be

improved from further input as the range of identifiable markings grows. If the model were perfect and complete (which it is of course not even remotely) it would be possible to determine with it one of the factors (or the range eligible for consideration) by ascertaining the two other factors (Bednarik 1991a: 39).

I also anticipated that several other variables were involved, such as, for instance, 'softness of the medium or subsequent speleothem growth', but stated that none of these are 'essential' as they are variable over time. I stand by that statement, and add location to those ineligible variables.

A key issue raised by Sharpe is his suggestion that the components of sets of multiple lines may have been drawn individually, in which case they can fairly be assumed to be human marks. However, the discrimination between such markings presents no difficulty to the specialist. First, if the individual lines were drawn with stone, they would be readily recognisable micro-morphologically. Second, it is surprisingly difficult to draw on a coarse rock surface several single lines in such a way that they appear to have been made in unison, in a single sweep of a multi-pronged tool.

In his conclusions, Sharpe poses six questions, which can be answered from a perusal of the literature on the subject:

- 'Do animal scratches contain internal striations?' See Bednarik (1998) for answer. There is an extensive literature available on the incidence and study of groove

striations (d'Errico 1988, 1989, 1991, 1994; Bednarik 1988, 1991b, 1992a, 1992c, 1994, 2001, 2004).

- 'Do any animal scratches include internal branches or cross-overs?' Yes.
- 'What would the result of frantic scratching behaviour look like?' Place one live wombat on floor of deep shaft, with no possibility of escape, light visible above. The claw marks it will produce in its puny attempts to climb a vertical wall before it starves to death might satisfy Sharpe's requirements, and they have been studied in considerable detail, and published.
- 'Do climbing scratches differ from stretching scratches?' I have never listed 'stretching scratches'. Sharpe has, and will need to answer this question.
- 'In general, do the scratches differ that a species makes for different reasons?' Yes; see relevant literature.
- 'Did humans use the paws of dead animals to make lines and, if so, are there any observables that distinguish them from lines that living animals make?' If ethnographic or archaeological evidence (e.g. a severed animal paw with signs of having been used) supporting the use of animal paws to copy animal scratches in caves were produced, the proposition would be worth pursuing. If its sole purpose is to save Sharpe's hypothesis that the boulder markings in Koonalda were made by humans it is merely frivolous. Concerning the second part of the question, I think that unless the copies were made by a person well versed in the relevant ethology I might be able to discriminate between animal scratches and marks made by people holding a severed animal paw. Animal scratches contain more information than the mere fact that they were made with animal paws.

There is a simple way to resolve the issue Sharpe canvasses here, and if he is interested in achieving this, all he needs to do is to set up a blind test. He could produce a number of markings, some with animal paws (or even live animals if he is so inclined), and some with stone tools, in the presence of an independent referee who records the way each marking was made. As a medium he could use plaster of Paris, a medium he has very usefully employed in the past to experiment with finger flutings. I would gladly subject myself to a blind test, something I have done before and for very similar exercises. Indeed, I would go so far as to suggest that the other principal researchers of the Parietal Markings Project (i.e. G. D. Aslin and E. Bednarik) might be also willing to take the same test. We would, separately, examine each marking and our pronouncements would then be recorded and checked against the notes of the referee. I predict that our success rate would be above 90%; in fact I would venture to suggest that we might be right in 100% of cases. This would merely demonstrate the point, it would not answer Sharpe's request for knowledge of how to achieve such discrimination. For this there are no shortcuts, *he will have to do the same amount of field-work others have done* if he is unwilling to accept their judgment.

In summary, Sharpe ignores data and research of sub-

stance, especially on how human markings can be recognised among natural cave markings, and instead offers 'pure speculation', to use his own term, and a lot of disjointed ideas of 'what should be done'. The main issue he seems to be concerned with, to show that the boulder markings in Koonalda's Upper Chamber could have been made by humans, is not served by re-stating his superseded views of three decades ago. At that time he stated that what he wanted to do 'is to analyse the way [the boulder markings] are built up, to draw and photograph them, and to describe them in relation to form'. To embark on such a re-appraisal of the 'preliminary survey' he commenced in 1973 might be more productive than disparaging other, *much* more comprehensive work by defining it as 'rhetoric and subjective interpretation'.

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