



KEYWORDS: *Petroglyph – Lithology – Weathering – Patination – Dampier Archipelago*

THE SCIENCE OF DAMPIER ROCK ART — PART 1

Robert G. Bednarik

Abstract. This paper is part of a series attempting to place the massive concentration of petroglyphs and stone arrangements in the Dampier Archipelago of Western Australia into a scientific framework. It therefore focuses on issues other than the subjective responses of researchers to the cultural material, presenting instead falsifiable and testable evidence about it. In preparation for data presentation relating to the antiquity of these cultural manifestations, the lithology, weathering processes and surface accretions at Dampier are examined in detail. This information is also fundamental to the important issues of the deterioration of this monument, aspects of its preservation and of its management. Similarly, a consideration of the size of the corpus and of the proportion destroyed by industrial development is essential to an informed endeavour of managing the monument. The classification of the petroglyphs and the technology of their creation are also canvassed. Finally, the investigation of these various aspects leads to a discussion of the differences between archaeological and scientific approaches to the Dampier rock art. It is shown that an archaeological focus on interpretation-based statistics is in the absence of ethnographic detail inconsequential.

Introduction

A monument of such extraordinary magnitude, antiquity and cultural importance as the Dampier rock art corpus forms an integral part of the common heritage of *all humanity*; it helps define the humanity of us all — of our entire species. Therefore, the determination of its fate should not be the prerogative of an ephemeral entity such as a government. The state's role is merely that of a caretaker, which is in that respect answerable to the international community. Many other stakeholders, however, also have relevant views about the Dampier monument, and they have been listed and considered (Bednarik 2005a). One of these views, the perspective of science, is of importance not only to those few scientists who have studied the monument, or to those who might want to know about their findings; there are also other important aspects to be considered. For instance, scientific understanding of the rock art, and particularly of its context, is crucial to the ability of making informed decisions about the management of this, or any similar 'cultural resource'. In addition, the perceived significance of a heritage property is certainly one of the key factors in establishing its overall socio-political, historical, cultural and heuristic importance. Therefore, the scientific perspective of the monumental concentration of rock art

at Dampier, reputed to be the largest in the world, should not be neglected. It is the subject I will address here.

The immediate prompter of this article, however, is a series of recent media announcements, debates and unpublished reports by various archaeologists and others about specific aspects of the rock art of Dampier, such as its antiquity, quantity, quantity destroyed, and world significance. Some of these comments were no doubt well intended, others are malicious (in particular some of those concerning the quantity of rock art destroyed). For instance, claims of great antiquity are ostensibly intended to raise the importance of the monument, providing further justification for greater efforts to preserve it. As the re-discoverer of the monument and as the initiator of the sustained campaign to save the rock art from destruction (Bednarik 1994a, 2006a), I naturally welcome all measures that might improve the prospects of the campaign. However, there are also points of caution to be considered.

The present phase of the Dampier campaign (which I commenced in early 2002; the campaign was initially begun by me decades earlier, when I lobbied the Western Australian Museum in 1969/70) resembles in many ways the 1995 campaign to save the rock art of the Côa valley in Portugal (Arcà et al. 2001), and

other such campaigns prompted by a relevant state (e.g. Guadiana, also in Portugal; Kunjota in India, or El Mauro in Chile; Bustamente Díaz 2006). There are so many similarities in such campaigns to preserve rock art under threat from development that it would be most requisite to analyse them, especially for the benefit of future campaigners for rock art protection. One I will review here is the tendency of archaeologists to promote claims of great antiquity. In the Côa case, archaeologists decided, on the basis of their 'stylistic' considerations, that the valley's petroglyphs are in the order of 26 000 years old (e.g. Zilhão 1995). They then used this unsupported and untested age claim as the principal reason for the need to preserve the Côa rock art. Initially I shared this belief of the Pleistocene age of the corpus, but after obtaining detailed photographs of it, and especially after a sample of the rock was sent to me for analysis, I began expressing reservations. The rock is rapidly weathering schist, a metamorphic rock that on exposure to water gradually reverts to its original state, mudstone, and then decomposes. I was not prepared to mislead the public into believing a fallacy for the sake of securing better prospects for the art's preservation and demanded that the rock art be preserved irrespective of its antiquity. Not only should the campaign to save the rock art not be compromised by falsities, I also stated practical reasons. For instance, I objected to the proposition that Pleistocene age should be cited as the justification for our demand for preservation of the petroglyphs. It is generally accepted that most rock art in the world is not of the Pleistocene, and any emphasis on having to protect rock art based on its greater antiquity would prejudice future endeavours to have Holocene rock art protected. Additionally, there was the danger of a later backlash, when it might need to be disclosed that the age claim on which demands for preservation had been based were false. To me, the issue of antiquity should not be connected to the issue of preservation. For this I was criticised, defamed and denounced by some archaeologists.

History has a tendency of repeating itself, and in the Australian campaign to save the Dampier rock art, there are many parallels with the Côa campaign in Portugal. I have described several of these in detail (Bednarik 2006a), but have not commented on the dating issue so far. At Dampier, too, we are now beginning to hear the voices of archaeologists making unfounded age claims for the rock art, although fortunately not with quite the same fervour we witnessed in Portugal. Once again I feel obliged to dampen their enthusiasm: if a rock art corpus is to be preserved, it must be on the basis of intrinsic properties independent of unknown or unverified attributes. Archaeologists making such claims need to provide substantial evidence in their favour. In the absence of sound data, spectacular claims can detract from the integrity of the campaign, as well as of future campaigns of its type. At the present time, no-one has

presented credible evidence for Pleistocene antiquity for any Dampier rock art, let alone for ages ranging up to 30 000 years as have been claimed.

Similarly, a variety of other aspects of the Dampier corpus has attracted precipitate public comments. For instance the presence of 'Cleland Hills faces', the 'oldest known depictions of human faces', the presence of images of extinct species, the extent of the damage to the monument and the meaning of the petroglyphs have all been subject to extensive public debate. This debate affects public opinion, and reflects on the credibility of rock art science. I therefore regard it as important to establish unambiguously the scientific status of the Dampier rock art: what do we know about it that can reasonably be expected to stand up to falsification attempts? Here I will endeavour to establish a basis for developing credible knowledge about this corpus, but more generically, my analysis can be applied to any other body of petroglyphs — or indeed to any rock art. I will begin by briefly reviewing previous speculations about the age of Dampier rock art, but initially without commenting on scientific evidence. This is not a subject suitable for consideration until preliminary issues, mostly related to lithology, are adequately clarified. Such preliminaries are equally essential to questions of preservation, which are in such urgent need of review but have so far been largely ignored in archaeological discourse. By the time I review the technology and classification of Dampier petroglyphs it will have become evident to the reader that my perspective is so fundamentally different from that of the numerous archaeologists who have commented on this subject that we seem to exist on different planets. This renders a comparison of the starkly contrasting archaeological and scientific approaches to rock art inevitable. After illuminating these differences the scene will have been set for the second part of this paper.

The Dampier Rock Art Precinct (270 km²), as I named it on 22 March 2004 when I submitted it for National Heritage listing (prompting two similar further submissions from others some months later), occupies most of the Dampier Archipelago on the coast of Western Australia (Figs 1 and 5). It is regarded as comprising the world's largest concentration of petroglyphs and Australia's largest collection of stone arrangements, and although there is no full inventory available, it is widely believed to possess in excess of one million petroglyphs. Its gradual destruction by industrial development has been the subject of numerous papers and one book (Bednarik 1973, 1977, 1979, 1994a, 2002a-i, 2003a-c, 2004a-c, 2005, 2006a-d, 2007a; Vinnicombe 2002).

Preliminary issues of antiquity

The question of the age of the Dampier rock art is of importance not only in the public sphere, it is also one of the key issues of scientific research. Without adequate estimations of age, rock art cannot be

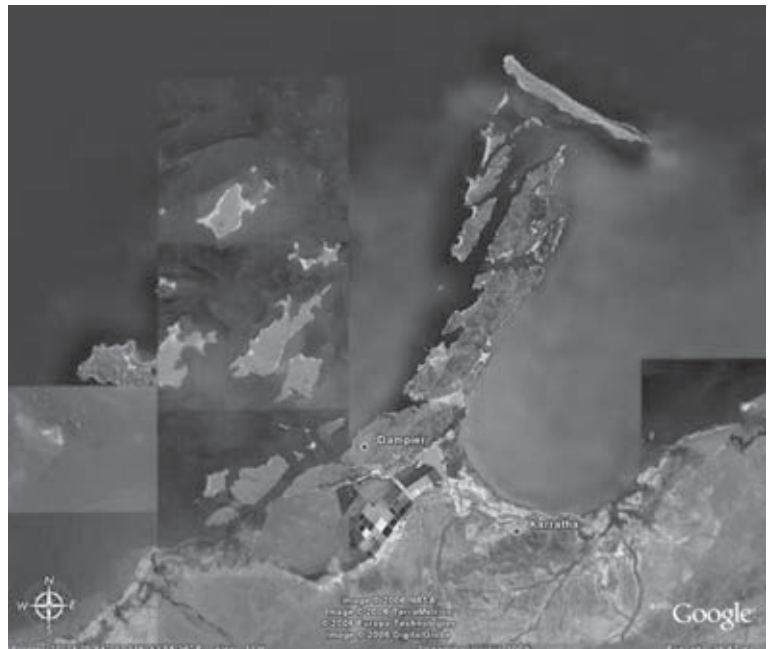


Figure 1a. Location of the Dampier Archipelago in Australia.



Figure 1b. Topography of the Dampier Archipelago and surrounds.

Figure 1c. Google satellite view of the Dampier Archipelago.



integrated into any archaeological model of a site or region. It exists then purely as an entity outside of archaeological constructs — in an archaeological vacuum. Archaeology has therefore always been keen to tackle the issue of rock art antiquity, but has almost universally failed in this endeavour (Bednarik 2007b: Ch. 7). This is partly because archaeology lacks a valid methodology to address this topic (as has been demonstrated at Dampier, among other sites), and partly because when the hard sciences do provide relevant information, it may be systematically misinterpreted by archaeology (Bednarik 1996, 2002j; Watchman 1999). The scientific disciplines, primarily physics, geochemistry, geomorphology and lichenometry, have furnished much relevant rock art data worldwide, but these provide generally not numerical ages of rock art. Rather, they constitute refutable information that in some way relates to rock art age, but needs to be interpreted in the context of the often numerous qualifications and reservations applying to this information (Bednarik 2002j). There is an array of methods available to estimate rock art age, but all of them need to be fully understood to appreciate the whole gamut of limitations applying to them. The agendas of the sciences and archaeology are very different. In the sciences, all claims of knowledge are qualified, ephemeral constructs based on current data, and they are subjected to falsification. Archaeology is the contrary, being based on confirmation of usually a priori propositions and a lack of falsifiability. The two systems of acquiring knowledge constructs are thus fundamentally incompatible.

The investigation of the antiquity of Pilbara pet-

roglyphs began in 1967, with my re-discovery of much of the massive concentrations of the region. It was one of the principal concerns in my early work there (Henderson 1969; Bednarik 1973, 1974, 1977, 1979). It took me thirty-five years to present the first data on petroglyph ages anywhere in the Pilbara that I considered to be of adequate substance to disseminate (Bednarik 2002k, 2002l), and I have not presented 'datings' from Dampier. Others have been less reluctant to express their views, sometimes after quite cursory examinations and always without conducting analytical work. The first comment concerning antiquity was by Clarke (1978), who ventured the guess that the older Dampier motifs might be in excess of 17 000 years old. He based this on the assumption that what he perceived to be desert varnish formed at the time of the Last Glacial Maximum. The name 'desert varnish' was subsequently replaced by the term 'rock varnish' (the frequent occurrence of the



Figure 2. Traditional Custodians Tim Douglas (left) and Wilfred Hicks at Gum Tree Valley site, western Murujuga.

phenomenon in arid regions is a taphonomic issue, related to high atmospheric pH rather than inherent to deserts; Bednarik 1980). Most of the ferruginous accretionary deposits found at Dampier are in fact not rock varnish. Moreover, no correlation can be demonstrated between cold climatic phases and varnish deposition, and we know that this accretion can develop within centuries, even decades (Engel and Sharp 1958). Scheffer et al. (1963) have shown that it is closely related to micro-organic activity, but in my view this was first suspected by the Australian Francis (1920). The nature of the coating was also misunderstood by Vinnicombe (2002: 22), who thought its characterisation as rock varnish would suffice, and who considered that this deposit at King Bay was derived from the substrate because it was of similar chemical composition. Nevertheless, Vinnicombe correctly suggested that at least the bulk if not all of the Dampier petroglyphs are less than 7000 years old.

An inadequate attempt to date Dampier rock art has been presented by Lorblanchet, rejected by Vinnicombe, McDonald and others since. As a specialist of limestone cave art in France, Lorblanchet had not worked with this form of rock art before and tried to construct an entire chronology of Dampier rock art, comprising various dated phases. In essence, he found part of a trumpet shell (*Syrinx aruanus*) seven metres from what he thought was an 'ancient soak' at Gum Tree Valley, not associated with any rock art (Fig. 2). He obtained a carbon isotope result (Ly-3609) from the shell that seemed to indicate it was 18510 ± 260 years old. From this he deduced that the rock art figures in the area 'were probably engraved 18500 years ago' (Lorblanchet 1992: 42). All his other seventeen carbon

'dates' (carbon isotope analyses do not provide 'dates', they offer only statistical formulations that are subject to numerous qualifications; Bednarik 2007b: 115–116, 139–141) from the two sites were significantly lower, and all except three were obtained from seashells. Obviously he has not demonstrated any relationship of this Pleistocene result with the petroglyphs, and it is highly questionable that this particular shell was carried approximately 130 km from the nearest coast during the Last Glacial Maximum. Large seashells, especially baler shells (*Melo amphora*), have on rare occasions been taken far inland (Bednarik 1977: 70), but at Gum Tree Valley there are substantial shell middens, generally of the late Holocene, and the probability of finding among these Dampier remains one solitary shell of the LGM seems extremely remote. In addition, the survival of one Pleistocene shell is unlikely, shells only survived well for such time spans at Dampier where they were buffered by high-pH water (see below, in reference to carbonate precipitates) or the presence of large deposits of shells. It is to be assumed that, at the time of the LGM, the Dampier regolith boulder formations were dry, barren rises in an extensive coastal plain whose peaks are unlikely to have attracted travellers or game. Since this Pleistocene 'date' from a surface shell remains unconfirmed by any other such date from the entire archipelago, it is perhaps best regarded as the result of contamination or error. Mollusc shells are far from ideal materials for providing reliable carbon isotope determinations. However, irrespective of these considerations, Lorblanchet's trumpet shell provides no more evidence for the antiquity of the rock art than the film spool I lost in a deep crevice between the site's boulders in 1968.

Lorblanchet presents numerous further unconvincing arguments to substantiate his long-range rock art chronology as well as various other unsupported archaeological interpretations. For instance, he proposes that 'the identity of the topographical distributions of the engravings and other archaeological remains on the sites can provide some important information' about the rock art. In particular, he perceives an 'obvious link' between shell middens and rock art, a relationship that 'cannot be explained by a common attraction to water holes'. On the contrary, rock art anywhere in the Pilbara is always most extensive in the vicinity of water sources, and where it is at all topographically possible, shell middens or other occupation evidence are also preferably clustered near freshwater. Therefore mere topographical coincidence provides no dating evidence, because the *common crucial denominator of the phenomenon category* (Bednarik 1990/91: 64, 1994b: 149) is water presence, not time. Moreover, and archaeologically more pertinently, Lorblanchet makes a cardinal error in his underlying assumption that his middens and petroglyphs are contemporary. He ignores the fundamental logic that the probability of two ar-

chaeological forms of evidence at a site being contemporaneous is inversely related to that site's 'popularity' or 'conspicuousness' (Bednarik 1989: 10–11). That logic decrees that the probability of two human activity traces relating to the same occupation is millions of times greater at some random location in a vast featureless landscape, than it is at a favoured site such as a rockshelter or waterhole. Or in other words, assuming that two activity traces, such as food refuse and rock art, are contemporaneous because they co-occur at a waterhole, is illogical. Similarly, Lorblanchet's (1992: 42) conclusion that the two site clusters he has studied at Dampier (Gum Tree and Skew Valleys; Fig. 3) 'are a kind of decorated dwelling' indicates an irrational transference of Eurocentric and etic interpretations onto a corpus for which in fact considerable ethnographic and emic information was at the time locally available. All of his topographical reasoning is subjective and easily refuted when tested against other localities at Dampier (conversely, it is equally futile when applied to a limestone cave in France). Most of the dense petroglyph concentrations are not accompanied by either shell middens or stone tool concentrations, except, obviously, by *mur-e* (Bednarik 1998). The latter are far more important to the study of the rock art than any archaeological evidence Lorblanchet presents, but he entirely failed to detect them, either in his excavations or on the ground.

Lorblanchet's detailed discussion of patinae is just as irrelevant. He took photographs of petroglyphs, 'in the same light condition, with the same colour transparency film', using a standard grey chart for calibration. He then projected these images onto a screen and measured on it the difference between the petroglyph and the adjacent rock with a photoelectric cell. In this way he sought to confirm his field observation that there were three categories of repatination, which he termed 1 (the earliest), 2 and 3 (very lightly repatinated). This is hardly a valid analytical method: during photography, film development and projection, many variables will affect accuracy and cannot be accounted for without proper colour calibration. Not surprisingly, Lorblanchet's results contradict his own categories (1992: Fig. 10, see especially the third diagram), but he apparently does not notice this, offering contrived explanations for what the peaks and troughs on his graphs might indicate: the heavily repatinated petroglyphs are the work of hypothetical gastropod gatherers, and the unpatinated images were made by 'bivalves gatherers'. Again, he links invented categories with other hypothetical phenomenon categories without recourse to sound evidence.

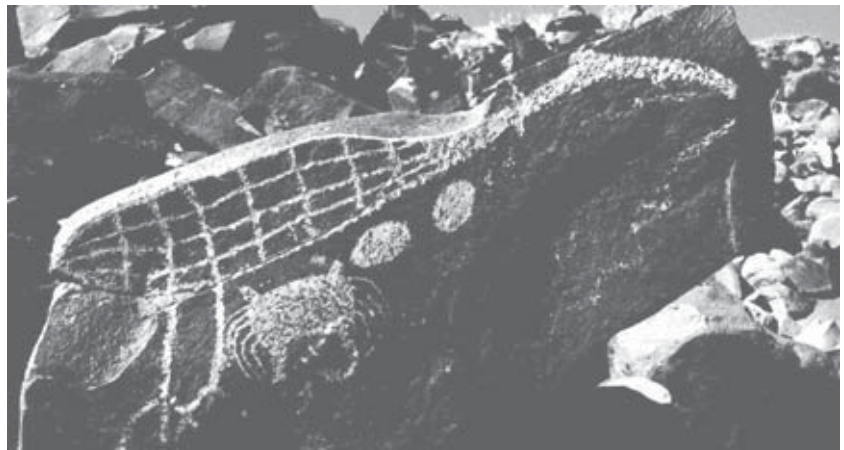


Figure 3. Skew Valley petroglyphs photographed in April 1968, at the time of the site's re-discovery.

The closer one looks at Lorblanchet's elaborate speculations, the clearer it becomes how he developed them from faulty *a priori* and unsound data. For instance, in pursuing his 'decorated dwelling' hypothesis further (1992: 44), he states that the number of temporary inhabitants at the Skew Valley site 'can be estimated between twenty and thirty', based supposedly on accounts of three early European visitors of the general region. One of them, W. Dampier, has not seen any of the local Aborigines or been to Murujuga, and the two others (F. T. Gregory and P. P. King) provide no clue to the number of residents at any of the many Dampier sites. In fact, the Skew Valley rock art site complex was not re-discovered until 1968 (Bednarik 2006a: 28) and never mentioned until then, therefore Lorblanchet's speculations are without factual basis. Next, he assumes that patches of grinding traces on rock indicate the former presence of women and children; they could just as easily, if not more convincingly, indicate the categorical *absence* at the time of women and children (e.g. if they indicated *thalu* sites). In reference to his Gum Tree Valley satellite site GTVK (which is in fact only a few metres from the main concentration), Lorblanchet believes that '[t]he total lack of grinding basins reveal[s] that a part of the population — the men only — frequented the places'. This doubly invalid argument (absence of evidence is not evidence of absence, and the proposition would be false even if that were not the case) leads him to a variety of conclusions concerning his 'identifications' of male and female areas, which like most of his other findings are without a basis in evidence or logic. His predilection of perceiving dwellings surfaces again when he mentions stone arrangements on a plateau near Gum Tree Valley (1992: 45) and incorrectly identifies them as 'stone huts' (their emic function is known ethnographically).

Lorblanchet's work at Dampier illustrates the difficulties one faces when untangling archaeological myths to separate fact from fiction. All archaeological

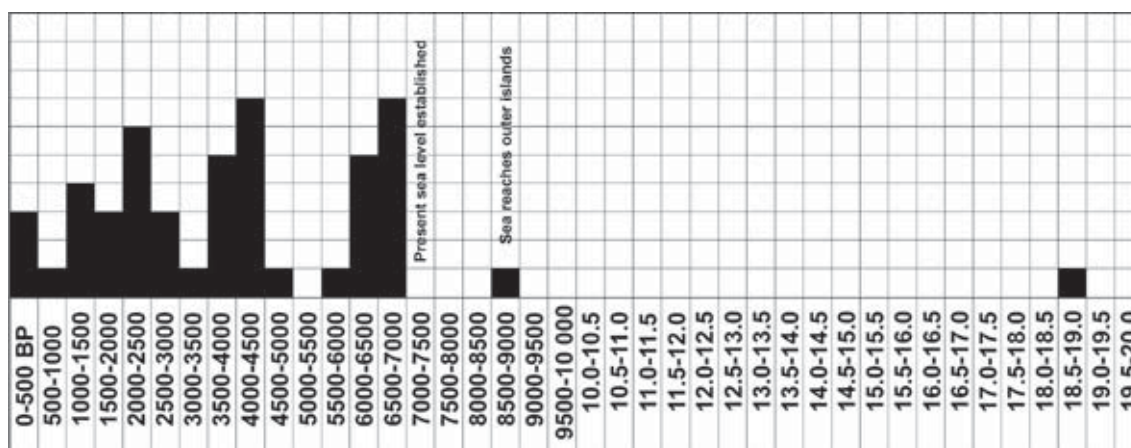


Figure 4. Histogram of the forty-nine carbon isotope results secured from Murujuga, showing the extreme isolation of the single Pleistocene 'date' reported from the entire coast of NW Australia.

works comprise elements of sound data, but in order to use these in a scientific sense they first need to be extracted from the general mass of speculations and knowledge claims many archaeological reports are dominated by, and this is often rendered very difficult by the tendency of authors to embed data in fictional explanations.

However, Lorblanchet's extensive excavation work did result in the acquisition of several apparent minimum dates for five rock slabs bearing small petroglyphs. He secured a few carbon 'dates' from three places at Gum Tree Valley site complex, and a sequence free of inversions from a midden at nearby Skew Valley. The five engraved rocks are related to the latter. These minimum dates range from about 2600 BP to 3700 BP, but unfortunately Lorblanchet provides no stratigraphical locations of the rock slabs. The occurrence and sizes of the petroglyph slabs within the shell midden could suggest that they were deposited as portable items, and their true ages may not be significantly greater than the carbon isotope results to which Lorblanchet relates them. He dates the occupation of the Skew Valley midden as beginning about 7000 BP, i.e. the time when the sea level approached its present elevation and the archipelago effectively emerged. It became a coastal zone about 9000 BP, when the sea reached the outermost islands, Legendre and Rosemary Islands. Therefore, if we accept the midden data Lorblanchet presents, although crucial information is missing from them (stratigraphical details are sketchy), they suggest that rock art was produced at Dampier by about 4000 years ago. Since it is apparent from the state of repatination of many Dampier petroglyphs that they are probably in excess of 4000 years old, this is not a controversial finding.

The distribution through time of the forty-nine carbon isotope results so far obtained from Murujuga indicates a first single date of 8520 ± 80 years BP, secured from the lowest *Terebralia* shells at Wadjuru Rockpool (Bradshaw 1995). This is followed

by a distinctive peak between 7000 and 6000 BP, and another between 4500 and 3500 BP, followed by some levelling off in the late Holocene (Fig. 4). This may indicate first archaeologically visible human presence at the time the shore line reached the archipelago, with an intensification 2000 years later, marked by exploitation of marine sources as the sea-level rise prompted the coastal population to occupy the islands permanently. Lorblanchet's $18\,510 \pm 260$ BP date (corresponding to a calibrated age of well over 20 000 years) is from an unstratified context, having been obtained from a surface shell fragment. It is entirely incongruous in this well-dated sequence, and should be attributed to contamination or to an exotic specimen (fossilised at the time it was deposited, perhaps as an item of body adornment or curiosity). Both explanations are much more plausible than the assumption that it was carried much further than 100 km, at a time that has yielded no other occupation evidence in the most intensively researched location of Australia.

Some writers have suggested that my dating of a few petroglyphs in the eastern Pilbara to the Pleistocene implies that the Dampier rock art, too, must be Pleistocene (e.g. McDonald and Veth 2005: 52). My age estimates of petroglyphs in the Abydos area, which do range to about 27 000 BP, are quite irrelevant to Dampier rock art. The petroglyphs in the Abydos-Woodstock and Spear Hill complexes occur on a granitic plain with a typical high-aquifer regime, i.e. a prime contender for early inland occupation. That area is 300 km from Dampier and has not become the occupation zone of a coastal population in response to the early Holocene sea level rise, as Dampier certainly has (Fig. 5). Well-watered inland regions may have been occupied in the Pleistocene, while dense occupation of present coastal zones is more likely the outcome of early Holocene demographic adjustments. Contrary to McDonald and Veth (2005: 152), the Dampier Archipelago has so far yielded no credible evidence of a Pleistocene occupation, nor

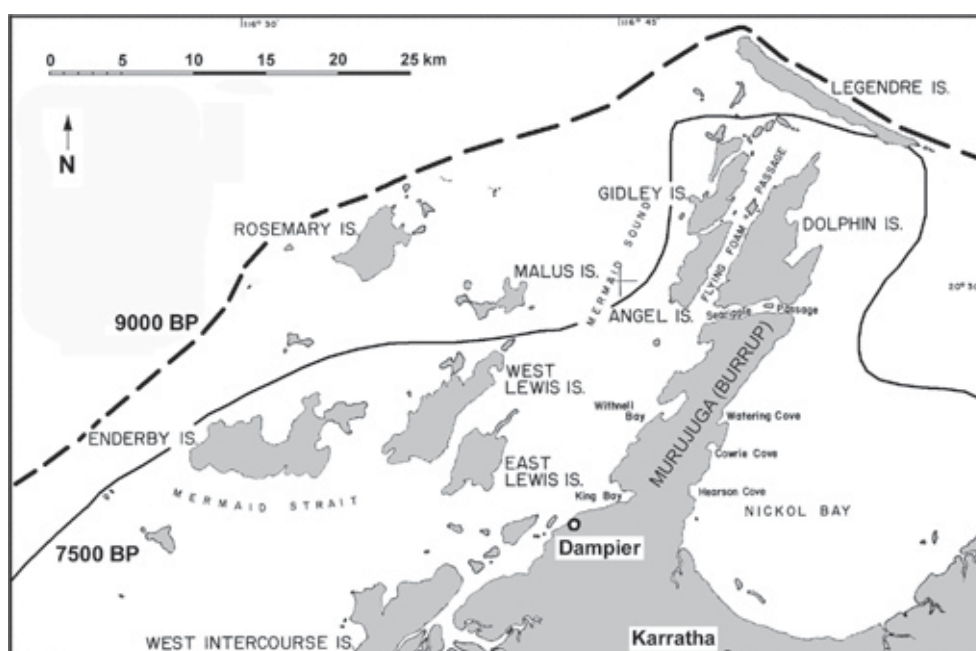


Figure 5. The Dampier hills relative to the shore about 9000 years ago (broken line) and 7500 years ago (full line), as the sea reached and isolated Legendre and Rosemary Islands.

has any other coastal site on the northern coasts of Western Australia. This is despite the expenditure of many millions of dollars on archaeological studies at Dampier alone — more than in any other part of Australia, or in any area of similar size in most parts of the world. It is always possible that, despite this immense effort, archaeologists have simply failed to find such early dating evidence, and the area was in fact occupied in the Pleistocene, but on the basis of the evidence as it stands the probability of this being established in the future appears low.

It is obvious from my previous publications and the present paper that, in my own endeavours to clarify the chronology of the Dampier rock art, I have been guided by scientific rather than archaeological imperatives. This work is dependent upon an understanding of the processes of rock weathering and re-patination, and will be presented in the second part of this paper. At this point I would just like to raise three other matters closely connected with the issue of dating. One of the archaeologically most neglected features at Dampier are the extensive travertine deposits commonly found, for instance in gully floors and as lenses. Vinnicombe (1987: 2) connects them with the limestone beds below the mudflats of Watering Cove. These are much more likely related to the formations found at Legendre Island and up and down along the coast, e.g. at Port Hedland. The high-elevation deposits at Dampier probably have another source. Vinnicombe attributes them to leaching from igneous rocks and soils and believes they are Pleistocene features. They can occur almost to the top of the regolith boulder piles, which renders this also unlikely. Soil development is very sparse, and always absent near summits, and the only component of the

dolerite or diorite often forming the upper facies that could yield the calcium carbonate would be Calcite and pyroxene, of which there appears not enough present to account for these easily degradable features. Thin sections suggest that the plagioclase occurs mostly as albite ($\text{Na}[\text{AlSi}_3\text{O}_8]$) at Dampier (though labradorite also seems to occur), seemingly excluding Vinnicombe's explanation. I favour the view that an overlying, pre-Quaternary deposit, now depleted, has provided this saprolithic material. The precise nature of these deposits remains to be established, but their importance is that they are readily 'datable', by several methods. However, such dating has not been attempted so far. The redeposited travertine contains occasional stone artefacts (Fig. 6), which must obviously predate the embedding travertine (or rather, its most recent re-precipitation). In the absence of credible evidence of a Final Pleistocene occupation by humans, it is quite likely that at least some of these exposed travertine formations are of the Holocene.

There is ample evidence at Dampier of the level of the Holocene transgression, which involved a sea level about 2 m above the present level (as also manifested elsewhere on Australian coasts; e.g. Bednarik 1980, 1994c). Much of this evidence occurred in the area close to Parker Point and may now have been widely erased by industrial work. Nevertheless, the conspicuous absence of mention of this chronologically important feature in archaeological reports needs to be mentioned. Finally, it needs to be considered that C. Dortch, who tried to locate petroglyphs below water at Dampier, has entirely failed in this, which suggests that their production was tied to the present sea level.



Figure 6. *Murujuga travertine deposit, with embedded flake artefact; bulbar surface, striking point and striking platform are clearly visible.*

Rock weathering at Dampier

Concerning the geology of the Dampier Archipelago, we have many versions in the archaeological and consultants' literature. If we begin with Virili (1977), he sees a Proterozoic coarse gabbro sill intruded between a granite and an overlying granophyre. Vinnicombe (2002) describes a jointed Archaean [Precambrian] substrate of gneissic granite with intrusive jointed outcrops of Proterozoic gabbro and granophyric rhyodacite. DEC (2006) presents a rather different geology: the 'igneous spine' of Murujuga is of granophyre and gabbro, i.e. plutonic rocks, on which basalts have been deposited, i.e. extrusive igneous rocks. Limestone is also present, Legendre Island consists of it and small travertine deposits occur throughout Murujuga, as noted. Only Woodside (2006) comments on the locally prominent dolerite dykes (probably metadolerite).

The frequent claim that most rock art occurs on granophyre (i.e. granite porphyry or plagioporphry) probably shows that rock types have been incorrectly

defined. Nevertheless, some archaeologists challenge correct previous rock identifications. For instance Bird and Hallam (2006) chide Stokes (1846) for correctly describing the principal rock at Depuch Island as 'columnar blocks of greenstone' ('greenstone' traditionally defined basic igneous rocks appearing green because of their content of chlorite, hornblende or epidote, and is thus a valid description: the colour of these rocks is determined primarily by pyroxene-derived chlorite and epidote). As noted previously in relation to Dampier (Bednarik 2002a), igneous rocks do not come in distinctive 'species', and they may be difficult to identify in the field. Often thin sections need to be prepared for this (Fig. 7), and even then, all separations between these rock types are quite arbitrary. These facies form continuums of three basic types (those that formed at great depths, and metamorphosed and non-metamorphosed rocks that formed nearer the surface), within which the relative proportions of their component minerals identify them. Most commentators have apparently gleaned their information from geological maps or copied it from previous commentary. This is not a pedantic point, because an understanding of the lithology is crucial for appreciating the weathering and repatination processes at work, which in the case of Dampier have been widely misunderstood. Yet they are crucial in two areas of knowledge: in the estimation of rock art ages and in conservation issues.

To characterise the geological background of the rock art at Dampier one might begin with the collision of the Mawson and West Australian Cratons about 1345 Ma ago. The supercontinent Rodinia began breaking up 750 Ma ago, and Gondwana formed 500 Ma ago. The edge of the present continental plate appeared when the rift with the Indian subcontinent commenced in the order of 135 Ma ago, resulting in the formation of the Indian Ocean. Extensive outcrops of basalts formed at that stage as magma was forced to the surface, and much of the extruded mafic rock along the Pilbara coast dates from that period, the early Cretaceous. In addition, contact metamorphism then formed a variety of minor rock types that found no mention. Neither has the batholithic structure and reticulate dissection displayed by parts of Murujuga been noted.

Most Dampier petroglyphs occur on relatively fine-grained Mesozoic rocks rather than the much older (Precambrian) porphyric facies, and these are usually free of quartz, or very nearly so. Therefore they are most likely dolerites, diorites, basalts or gabbros. A plagioporphry is dominated, obviously, by plagioclase, contains a good deal of quartz, some biotite and hornblende, and probably orthoclase as well. Comparatively little rock art is found on this type of rock at Dampier. In considering the respective weathering forms corresponding to the two main rock types present, great differences are immediately

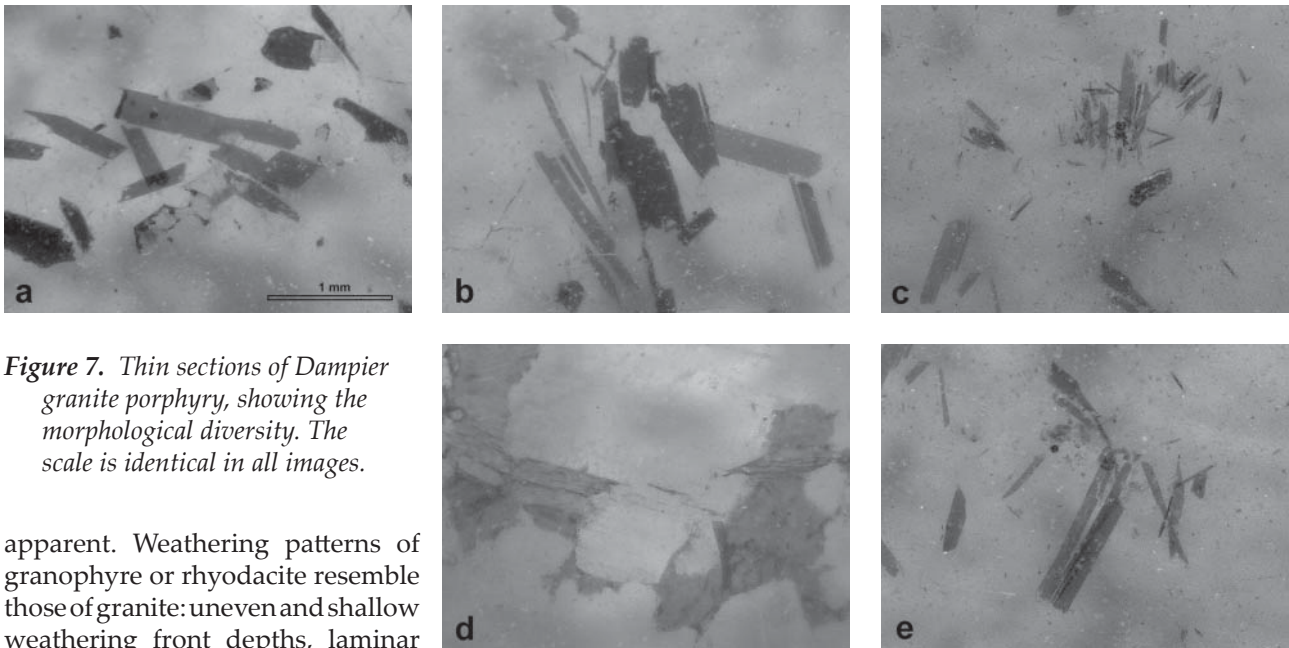


Figure 7. Thin sections of Dampier granite porphyry, showing the morphological diversity. The scale is identical in all images.

apparent. Weathering patterns of granophyre or rhyodacite resemble those of granite: uneven and shallow weathering front depths, laminar exfoliation, tafoni, insolation and fire spalling are the principal features, and the traces of lightning strikes are also much in evidence. The dominant felsic facies at Dampier is Gidley granophyre. The darker rocks show much more distinctive clast formation characteristics, which are highly relevant here and have been explained (Bednarik 1979: Fig. 1), and very pronounced cutaneous weathering. This is primarily attributable to their content of pyroxenes (augite) or olivine, whose iron content is shed or converted as a function of time, and the Dampier mafic rocks also contain occasional magnetite octahedra. Iron is absent in the felsic rocks at Dampier (Bednarik 1979). Consequently, when archaeological commentators refer vaguely to the 'oxidation' or 'weathering' of the granophyre and relate it to the characteristic dark-brown colour, they not only make the false assumption that this rock contains the necessary cations, they also introduce a fundamental fallacy into their concept of the repatination processes. Dolerite, diorite or gabbro, on the other hand, do contain the required iron, but even their ferruginous surface accretions are in the main not intrinsic phenomena, they are mostly migratory or derived from external sources (see below). The Dampier gabbros and dolerites can also contain calcite and prehnite, and hornblende crystals (longer and more slender than the similar dark-green augite hexagon crystals), although more prominent in the dolerite, are occasionally present in them.

Goodwin (1960: 304) reports that a weathering zone can yield sound and consistent information for relative dating. His study of petroglyphs at Vosburg, South Africa (Goodwin 1936), prompted him to list the following main agents governing patination on dolerite (apart from time, and the obvious edaphic and epidaphic factors):

a. Uncertain rainfall with extreme diurnal evapo-

ration.

b. Distribution of water on exposed surfaces.

c. Wind and solar radiation.

d. Presence of pitting, holding or funnelling moisture.

e. Action of organisms, notably birds.

The inhibiting effect of bird droppings was also noted by Wright (1968) and me, even though this substance is only weakly acidic (pH 5.9; Bednarik 1979: 30). This observation is important in appreciating the effects of prolonged exposure to lower pH regimes, such as those introduced by industrial emissions.

The study of rock patinae in the context of rock art studies was introduced in Australia by Trendall (1964) when he analysed six petroglyph samples from Depuch Island, 100 km east of the Dampier Archipelago and in many respects very similar to its conditions. The interpretations of his findings by Crawford (1964) have been challenged (Bednarik 1979), but that does not detract from the importance of Trendall's pioneer work, on which much of my own research in the region was modelled. Trendall perceives an extremely slow development of weathering rinds, suggesting that a skin of 0.2 inches (5 mm) may be well in excess of one million years old. My studies of fractured stone and stone tools at Dampier renders this figure quite reasonable, and it also agrees with the findings of Černohouz and Solč (1966), on whose scale a weathering rind (or zone) thickness of 5 mm on basalt corresponds to 1.1 Ma in central Europe. Moreover, Trendall's view that much of the weathering of individual clasts occurred beneath the surface, along joints, before the exposure of the boulder, has found extensive confirmation in my work of the 1970s.

Crawford (1964) interprets Trendall's data from six samples as correlating a fresh appearance of a



Figure 8. Digital and binocular microscopy in progress at Anchor Hill, Depuch Island.

petroglyph with a thin weathering crust, and a weathered, heavily patinated appearance with a thick weathering rind. I have demonstrated that whilst it is true that the three samples of heavy repatination coincide with those of deeply weathered rock, they also correspond with the most deeply engraved. Furthermore, heavy petroglyph patination coincides with greatest distance between weathering front and groove bottom. While the former dimension increases, the latter probably decreases as a function of time, unless there is a complete absence of surface retreat. Therefore of the oldest motifs, only the most deeply engraved may survive, a principle more elaborately developed and quantified by taphonomic logic (Bednarik 1994d). Correspondingly, very early

shallow petroglyphs are likely to have become obliterated (Bednarik 1979, 1994d).

Crawford divides the six Depuch Island (Fig. 8) samples into two groups of three: group A of around 2 mm weathering rind thickness, and group B in the order of 6 mm. He concludes that the colour variations in the petroglyphs reflect weathering rind thickness, i.e. that fresh appearance and shallow weathering are correlated. Hence he implies that repatination is a function of weathering depth. I have shown that this is a misconception (Bednarik 1979: 22). The maximum weathering depth of his group A samples is greater than the minimum of his group B, i.e. the two groups overlap. The sample is simply too small to make such generalisations. Moreover, if repatination were affected by the proximity of the weathering front, then the distance from groove bottom to weathering front would be the governing variable. Yet I have shown that

the two most deeply patinated of Trendall's samples (14921 and 14922) also possess the greatest distance X (between groove and weathering front; Bednarik 1979: Fig. 2). Therefore Crawford's interpretation is false and should not be used in underpinning archaeological deductions in the way it has.

The key issue, which has prompted archaeological discussions of these factors, is the question: can the distance between groove and weathering front influence the rate of repatination? If it does, the colour change of repatination cannot be a reliable measure of age. My analyses at Dampier are in part an attempt to resolve this issue. It needs to be clarified here that a 'weathering rind' is a cutaneous surface layer of chemically or physically altered rock whose thickness is related to time. The term *weathering front* can refer to the contour of the deepest visually discernible alteration of the general lithosphere (*sensu* Mabutt 1961), or of just a single clast (Bednarik 1979). In rock art science it is generally used in the second sense (Bednarik 2007b: 61–65).

In my studies of the Dampier regolithic clasts I deliberately focused on the mafic range, not only because it has attracted more petroglyphs, but also because these rocks show the most readily quantifiable weathering characteristics, which renders them more amenable to most of those analyses that are relevant to dating (Fig. 9). Their distinctive weathering rind is formed primarily by moisture, which affects the component minerals at various rates. The most susceptible in descending order are olivine, where present, then augite, followed by hornblende, then biotite (in the rare cases where it is present). The plagioclase, be it



Figure 9. Distinctive differences between deeply patinated and heavily engraved mafic rock (dolerite) and very lightly patinated felsic rock (granite porphyry), Waring Cove, Murujuga.

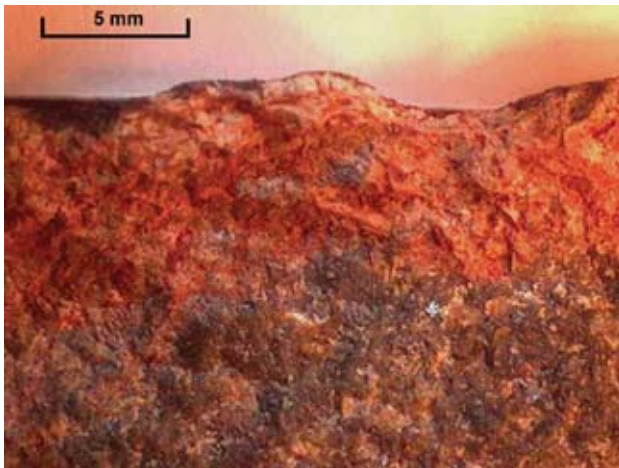


Figure 10. Cross-section through the outermost part of the weathering rind of Dampier gabbro, showing the dark layers at the surface and the converted iron inclusions in the weathered rock.

sodium or calcium based, is the most weathering-resistant mineral in these rocks which, as noted, contain little or no quartz. So the decay of the fabric of the rock from the surface inwards, caused by moisture, tends to first affect the iron-containing minerals, forming distinctive patterns that are visible in cross-section as rust-brown features under the microscope (Fig. 10). The rainwater carries atmospheric CO_2 (and now at Dampier significant other acidic anions provided by the hydrocarbon-based industries, see below) and this acidic solution is progressively neutralised as it percolates into the rock. The rock minerals take up hydrogen ions and water, releasing cations. Hydration and oxidation occur, and microscopic carious zones develop around individual crystals or laths. As the process continues towards its end products, the rock fabric finally breaks down. The existence of weathering rinds demonstrates, however, that the final mobilisation of rock mass proceeds at a significantly slower rate than the initial etching of crystals and conversion of iron compounds.

Of particular relevance to the science of the Dampier rock art is the formation of these weathering rinds, which on many rocks are quite pronounced zones of alteration whose thickness, importantly, is a function of exposure time (Fig. 11). If the process could be calibrated it would yield rough estimates of geomorphic exposure ages. Černohouz and Solč (1966) attempted this with basalts, claiming to obtain reliability to within 10% – 20%, but there has been no adequate attempt to pursue this possibility further (cf. Colman and Pierce 1981).

At Dampier I distinguish the following zones of mafic rock weathering rinds:

- I. Ferromanganous accretionary surface deposit, of a dark-brown, reddish-brown to orange or near-black colour, which may locally support a rock varnish veneer; the ferric phase dominates (see

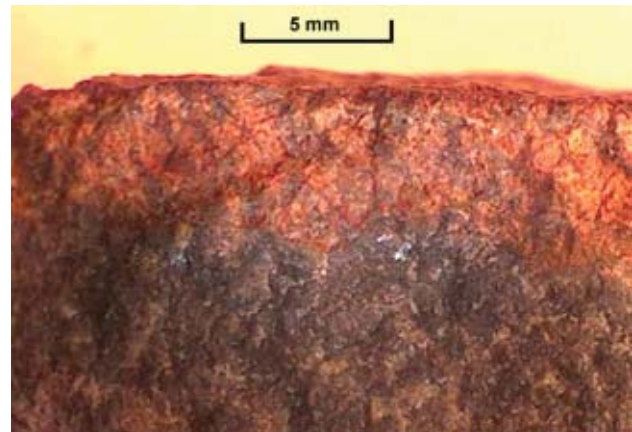


Figure 11. Cross-section through the weathering zone of Dampier gabbro.

- below).
- II. Thin zone of micro-porous, very iron-rich substrate comprising components of decomposed rock, such as laths of modified plagioclase and pyroxene, also of distinctly red-brown colour.
- III. Degraded and porous rock fabric that has changed its properties without changes to the bulk, of a macroscopically light-brown colour, patterned with microscopic 'veins' and lenses of iron oxyhydroxides, <1 mm on unengraved surfaces, but almost undeveloped under petroglyph grooves, and uniformly <40 μm thick.
- IV. Somewhat less weathered, mostly buff-coloured zone, but still comprising local oxidised iron concentrations, especially along discrete crystal laths, representing the deepest penetration of most oxidation and hydration. This is usually the thickest of the zones of weathering rinds. I regard its colour as the result largely of etching of crystals that has altered their optical properties (cf. Bednarik 1980, concerning cherts).
- V. The largely unaltered rock, of greenish-grey colour, microscopically composed of several shades of green, with individual crystals poorly visible. It may still sporadically contain microscopic patches of alteration, but they are insignificant and decrease with depth.

It needs to be emphasised that none of these zones is distinctly demarcated, each grading into the contiguous zones. Moreover, there are great variations in the characteristics of the weathering products, not only between different rock types, but also on rocks of the same type, even on different aspects of the same clast. This variability is particularly evident on rocks comprising large crystals, which on some of the coarser Dampier facies often exceed 10 mm length. In the 1970s I separated weathering zones by nano-stratigraphic excavation in order to wet-analyse the chemical composition of each layer, and in some cases to determine the abrasion pH of each of these zones (Bednarik 1979, 2007: 173–174). The purpose

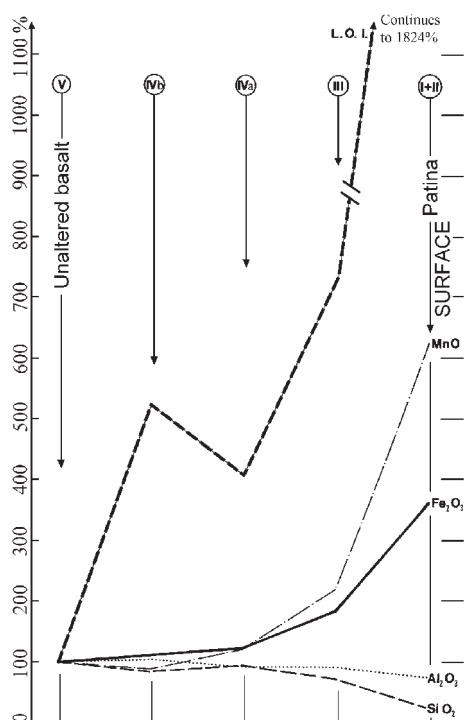


Figure 12. Nano-stratigraphic analysis of a thin weathering zone on basalt, showing the variation of the principal components from the surface (on the right) to the unaltered rock (on the left). Of particular significance is the high enrichment of organic matter towards the surface.

of these very laborious experiments was to help in understanding the weathering processes operating on these rocks. The most spectacular finding of this work was the dramatic surface spike in LOI values, which are accounted for mostly by organic matter (Fig. 12). I thus demonstrated that there is a gradual and distinctive increase from the unaltered rock to the surface, which indicates that the carbon system is open. This severely limits the utility of carbon isotope determination for the purpose of age estimation. My finding, that regolith clast and bedrock subsurfaces generally present open carbon systems, has since been confirmed by others (e.g. Nelson 1993; Watchman 1996), and remains the principal difficulty in dating rock art via carbon isotopes (see Bednarik 2007b: 127; Bednarik and Khan 2005).

Chemical weathering is almost invariably conditional upon the presence of water; even oxidation of minerals by gaseous oxygen appears to require water as an intermediary agent (Keller 1957). Such weathering is usually by means of very complex processes that are frequently rendered more intricate by the ability of many of their own products to accelerate them. For instance, a byproduct of the oxidation of pyrite is sulfuric acid, which readily reacts with numerous minerals; or hydrogen clays, themselves the result of weathering, will induce hydrolysis with their hy-

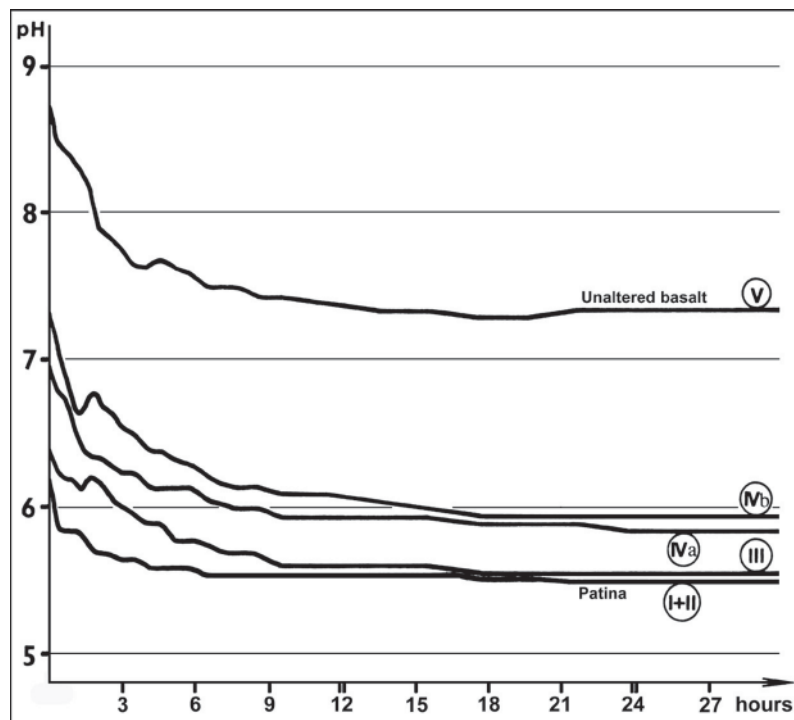


Figure 13. The effects of long-term exposure to distilled water of the layers of a basalt weathering zone separated by nano-stratigraphy, measuring the effect of hydrolysis on abrasion pH.

drogen ions. The atmospheric water solution travels within the weathered and thus porous zones of rock by means of gravity, capillarity or heat. Apart from gravitational and capillary water, two further types are commonly found in rock: hygroscopic water (as thin films on grains) and water locked in the mineral's chemical structure. Although atmospheric precipitation can range from pH 3 to pH 9, most weathering reactions occur in the acidic range. Most commonly, weathering commences with solution, and it can also include hydration, hydrolysis, oxidation, reduction, ion-exchange (including chelation) and carbonation. In the specific case of the Dampier mafic rocks, the ferrous iron among the iron oxides in the fresh rock is readily hydrated after it has been oxidised to ferric oxide. The fayalite component of the olivine (or derivative iddingsite), for example, may lose its Fe through oxidation, whilst its forsterite sheds its Mg by hydrolysis. The oxides of Ca, Na and K are readily removed by water.

The initial purpose of my arbitrary separation of weathering zones was to facilitate investigation of the effectiveness of the zones to neutralise the acidic solution percolating into the rock during prolonged wetting events. My determinations (to within 0.05 pH) of the abrasion pH of the weathering rind zones in continuous exposure to initially de-ionised water for 30 hours showed a distinctive pattern affecting all zones of the weathering rind (Fig. 13). The initial pH of the unaltered basalt is distinctively basic, while progressively lower values pertain towards the surface. The zones affected most by weathering are

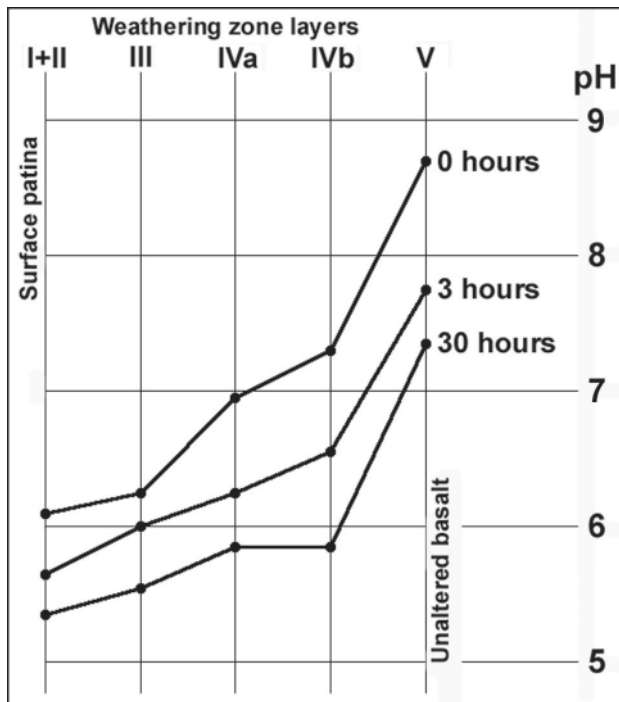


Figure 14. The reduction in abrasion pH of the layers of a basalt weathering zone as a function of time.

slightly acidic (between pH 6 and 7). Upon exposure to water, the pH of each of the five zones begins to fall sharply, especially in the innermost zone. After two or three hours its descent slows markedly, and the pH of all zones stabilises after about nine hours of continuous exposure.

These experiments showed the precise effects on separated layers of the weathering rind. In the natural system, we can assume that moisture takes some time to penetrate to the unaltered rock, therefore the weathering front will be activated only after the reduction of pH in the surface patina has commenced, i.e. the pH differential will be even greater. Of particular significance is the distinctive increase in pH through these layers, because it means there is no barrier to the outwards mobilisation of iron from the weathering front, or from any of the rind's layers. The solubility of iron increases about 100 000-fold through the lowering of the pH from 8.5 to 6.0 (Ollier 1969: 28; Bednarik 1979: 25). This range is fully covered by the range of pH encountered in the rind, which in effect suggests that the mobility of this element is vastly greater near the surface than it is at the weathering front, at any stage of a wetting episode. Unless there is a great pressure differential between weathering front and atmosphere (solubility also increases with pressure, temperature and turbulence), nothing could stand in the way of removal of iron to the surface, and this applies throughout an episode (Fig. 14). However, where the pressure differential is significant, the solution will precipitate iron oxides or hydroxides. This is in fact the pattern of microscopic evidence observed.

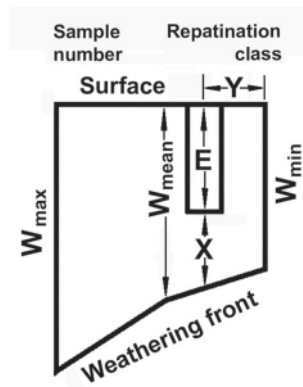


Figure 15. The principles of depicting graphically the relationships of weathering zone depth W (maximum, mean and minimum), maximal petroglyph groove depth E , weathering zone thickness under the groove X , and position of maximal groove depth relative to weathering zone depth, Y .

My analyses of the weathering of mafic rock types at Dampier suggested the need for simplifying the presentation of numerous metrical results in such a form that they could readily be appreciated. I introduced a graphic device in the 1970s that replaces confusing tables of results by rendering the various relativities immediately visible (Fig. 15). In this schematic format, the maximal, minimal and mean weathering depths (W_{max} , W_{mean} , W_{min} ; cf. Trendall 1964) are all shown as vertical dimensions. The upper line represents the rock surface, the lower the weathering front (see above definition). Therefore the shape of the line indicating the latter shows whether the weathering depth variation is greater above or below the mean: in the former case the line is deflected 'inwards', in the latter it deflects 'outwards'. The maximum depth of the petroglyph groove P is indicated, together with the position of the groove relative to the range of weathering depths measured (expressed by imagined dimension Y , the distance from W_{min} relative to the range of depths measured). Finally, the dimension X is the crucial distance between the base of the petroglyph groove and the weathering front at the location of the groove. This graphic device permits the instant appreciation of the critical variables in understanding the relationship between weathering zone and petroglyph depth, facilitating their easy comparisons from different samples, which is rather cumbersome to process cognitively in table format.

In addition, I used four arbitrary designations to define degree of repatination in the petroglyph groove:

- Very slight repatination, of petroglyphs thought to be only a few centuries old at the most. (There were no completely unpatinated specimens at Dampier.)
- Repatinated up to roughly half the intensity of the adjacent rock surface.

- C. Strongly repatinated, but with still readily detectable colour contrast.
- D. Fully repatinated petroglyph, no significant difference to adjacent surface is macroscopically apparent, although significant microscopic differences may be evident.

These definitions are, in retrospective, not what I would prefer to use as a classification system, but for the purpose detailed below they seem adequate. Any future work of this type should establish a more sophisticated taxonomy of repatination grades, based on microscopy and compositional data.

During the 1960s, physical damage to rock art at Dampier was most extensive in the area between Foul Point and Parker Point, i.e. in the area occupied by the initial industrial installations (pelletising plant, loading facilities and related infrastructures) and the town of Dampier. There was then no shortage of smashed and broken boulders bearing petroglyphs, and I found numerous specimens where petroglyph grooves had been sectioned at roughly right angle by fractures occasioned in the course of construction or clearance operations. I examined many such fractures and wherever they were suitable, I recorded the metrical details based on the described methodology. Several weathering depths were determined in each sample, whenever possible over 30 cm to 50 cm either side of the groove. Most of the data were collected from the coastal strip of what I designated Murujuga Area 2 (13 sets from 12 petroglyphs). One specimen was a naturally fractured zoomorph from just north of King Bay. Two more broken petroglyphs were analysed at the main petroglyph concentration of Wongama (Watering Cove), one of which provided two separate fractures truncating grooves of a maze-like pattern. A shattered thin spall was found near Wirlga-nurrayi (Deep Gorge, apparently fractured naturally, possibly by lightning); and one very deeply patinated petroglyph, broken during the construction of the initial railway line, was measured on the eastern boundary of Murujuga Area 1. The details of these nineteen sectioned petroglyph grooves are summarised in Figure 16.

The graphic device I use in this schematic depiction of a substantial matrix of data renders it easy to relate the variables weathering zone and groove depths to repatination state and the relative weathering depth at the groove location, and to detect whether there are any trends reflecting potential correlations between some of these factors. I perceive no obvious trends, although it could reasonably be argued that the sample is still too small. One would not expect any preference in groove location relative to weathering zone depth, and this is confirmed by the relative horizontal position of the grooves, ranging from that of Mu2-12 (in the deepest part of the weathering zone) to Mu5-1b (in the shallowest). This distribution seems perfectly random. It is obvious in the field that weathering depth can vary greatly on a single boulder, but there are also

ample cases where it remains relatively constant along a fracture. This is reflected in the overall angles of the base line of the diagrams, which range from very steep (Mu2-13) to almost horizontal (especially Mu1-1). Of particular importance is the pattern of groove depths versus weathering zone depths, and the resultant dimensions X (between base of the petroglyph groove and weathering front). There is no obvious correlation observable in these data, which suggests no connection between the level of repatination and groove depth *E* / depth of weathering zone under groove X. Or, in other words, repatination seems to proceed independent of *W*, *E* or *X*. It is clear that of the five deepest grooves recorded here, relative to weathering zone depth, two are of patina group C (Mu2-12, Mu2-13), two are of group B (Mu3-1, Mu5-2), and one is even of group A, the least patinated (Mu2-18). Moreover, the only two fully repatinated specimens (group D) both exhibit comparatively shallow grooves (Mu1-1, Mu7-1). This is despite the generally valid field observation that deeply carved motifs tend to be deeply patinated, which may well be an artefact attributable to human vision: shallow and fully repatinated motifs are notoriously hard to see in daylight (but are easier to see in raking light).

There are numerous other observations possible from the matrix in Figure 18; for instance, it shows that there is in most cases no attempt evident of penetrating all or most of the weathering zone. Only one specimen, Mu3-1, shows that percussion penetrated virtually to the unweathered and significantly harder zone. This may be coincidence, because it is also among the specimens with the least developed weathering zone. Nevertheless, the results also show unambiguously the lack of any endeavour to extend groove depth into the hard core of the rock.

The findings of this survey further underscore those I reported in the 1970s, such as 'the concurrence of the heavy engraving patination and greater distance between groove depth and weathering front' (Bednarik 1979). As I said then, this dimension can be assumed to slowly increase as a function of time (because the weathering front can only proceed deeper), 'while it is probable that engraved groove depth decreases (although necessarily at a far slower rate), unless we postulate a complete absence of abrasive (aeolian) or other erosion. This argument introduces the possibility that, of the oldest designs, only the deeply engraved remain discernible, while those of shallow depth have become unintelligible' (Bednarik 1979: 22). This was an early, nascent formulation of what fifteen years later became taphonomic logic (Bednarik 1994d), the most powerful analytical tool of scientific archaeology. It remains true that groove depth is likely to decrease with age, and that the distance between groove and weathering front can only increase. However, these changes require almost geological time spans to be effective, and are of little relevance to Holocene or very late Pleistocene petroglyphs.

One more point to consider is that if we were to apply the age estimates recorded by Černohouz and Solč (1966) to the values I report, we might conclude that most of the boulders at Dampier appear to be in the order of 1 or 2 Ma old. This seems not very likely to me, and I suspect, as does Trendall (1964), that much of the weathering of individual clasts is 'inherited', having occurred along joints before the exposure of individual boulders. Therefore weathering zone depth is not to be regarded as a measure of clast age, except where clear evidence of fracture can be observed. In my experience, fracture surfaces have uniformly thin weathering zones, certainly well below 5 mm thickness. If these assumptions were valid, my data would imply that most boulders were probably formed long before their exposure at the lithosphere-atmosphere interface.

Patination and petroglyphs

These preliminary considerations of the weathering zone, specifically on the mafic rocks at Dampier, clarify the well-known polemic concerning the repatination of petroglyphs: if a groove has been cut into the weathering rind beneath a ferromanganous accretion, will its repatination occur at an accelerated rate relative to unaltered rock? This question refers to the possibility that the closer proximity of the groove floor to the weathering front facilitates the rate of deposition of cations mobilised from the weathering front. It implies a significant misconception of the repatination process that is fundamentally important to assessing petroglyphs. We have seen that there is no evidence that repatination is directly related to factors of weathering, although that does not exclude the possibility that they may be minor influences. It also leads directly to the even more important nature of the 'patination', i.e. its outermost zone, the ferromanganous accretionary surface deposit, which I have termed Zone I of the stratified weathering zone. This is the ubiquitous dark-brown mineral coating found on all rock at Dampier, and in most arid or semi-arid regions. The overwhelming majority of the world's petroglyphs occur on such 'patinated' rocks.

In rock art science, the generalised and rather vague word 'patina' defines a visually obvious surface feature that differs from the unaltered lithology in colour or chemical composition (Bednarik 2007b). It is a collective, almost colloquial term for a great many

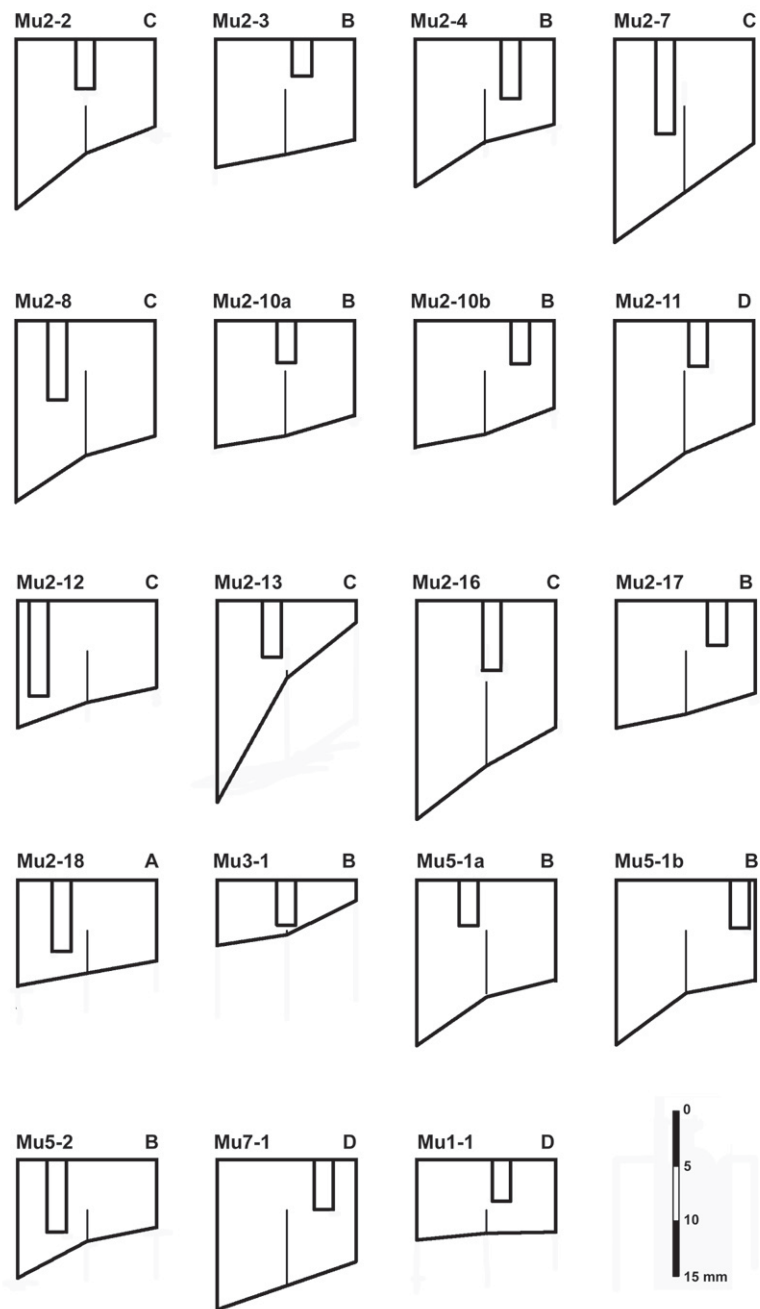


Figure 16. The schematic depiction of metrical data collected from nineteen petroglyph grooves fractured at various sites, see text for details.

phenomena, all of which are acquired gradually over time. Researchers have appreciated for centuries that they represent a measure of rock art antiquity (Belzoni 1820). However, their use in age estimation has so far remained difficult and controversial, not least because of misunderstandings. These include the mentioned issue of repatination, the extensive confusion about the nature of the patinae, and several similar issues.

The name *patina* can refer to the sheen or wear polish on antique surfaces, carbonate or sulphate skins on copper or its alloys, and to cutaneous alteration of rocks or stone tools that seem to indicate great age.

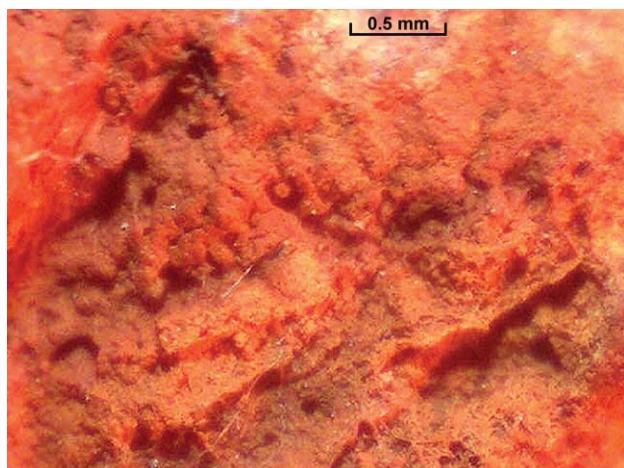


Figure 17. Close-up view of ferruginous patina south of Withnell Bay.

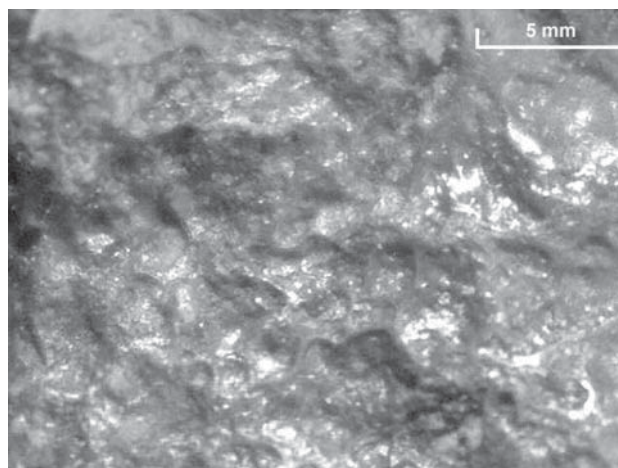


Figure 18. Close-up view of rock varnish at Dampier.

The latter surface rinds could be the result of bleaching or leaching (e.g. of sedimentary silicas; Bednarik 1980), 'limonite staining' (Goodwin 1960), mineral accretion (e.g. by rock varnish), chemical alteration of substrate components (most rock weathering processes are candidates), and abrasion or polish (e.g. by sediment grains or biological agents). Most rock patination is the outcome of more than one process, being merely the macroscopically visible outcome of several factors and their interplay. Patinae are either endogenous alteration products or the result of deposition of extraneous matter, although some specific forms, such as the oxalate patina on marble statues, might be attributable to a combination of both. The form of patination most frequently encountered in the study of petroglyphs consists primarily of iron compounds, presenting themselves as the ubiquitous dark-brown to orange-coloured or near-black coatings of rock surfaces, found particularly but not exclusively in arid and semi-arid regions (Figs 17 and 18). In practice, most ferruginous patinae seem to combine endogenous and exogenous forms on the same surface.

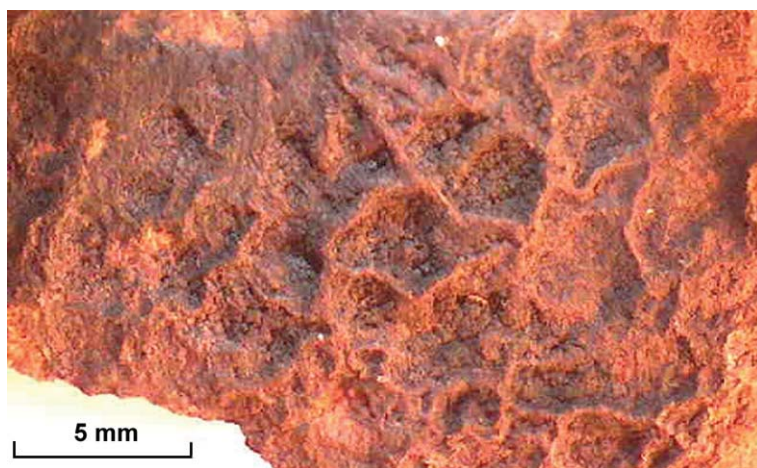


Figure 19. Tessellated ferruginous accretions on gabbro at Dampier, demonstrating significant re-deposition and precipitation by rainwater.

Archaeologists frequently assume that this is either an oxidation product or a rock varnish. The latter, often misused term should be limited to a very thin (<0.5 mm), frequently shiny ferromanganese skin that covers even rocks entirely free of its contributing cations (such as quartz), that is rich in Mn (Engel and Sharp 1958), and in whose laminar, stratified deposition micro-organisms are implicated (Scheffer et al. 1963). However, most deposits of broadly similar composition and appearance are not rock varnish and are perhaps best subsumed under the vague heading of *dunkle Rinden*, as which they were first described (e.g. Walther 1891).

There exists no agreed taxonomy of these surface deposits, probably because of their inherent polymorphism. Their thickness, colour, arrangement and composition are all highly variable, often at a millimetre scale. Microscopic examination of any boulder is likely to reveal great differences in all possible variables, relatable to factors such as surface inclination, orientation, morphology and substrate composition. Many such surface deposits are clearly subjected to continuous reworking and migration, especially mobilisation and re-deposition by water. This is particularly apparent from the tessellated microstructures often found, sometimes resembling a patchwork of rim pools staggered along an inclined surface (Fig. 19). Lace-like formations of amorphous silica often enhance such features (Fig. 20). Highly reflective zones are frequently observed, often of darker areas and of polished appearance. Immediately adjacent patches may be of very dull and lighter appearance, showing signs of microscopic erosion. To some extent these multifarious deposits can be related to the different degraded component minerals of the substrates, some of which have given rise to notable iron concentrations, but overall the deposits are accretionary and largely exogenous. This is suggested by the

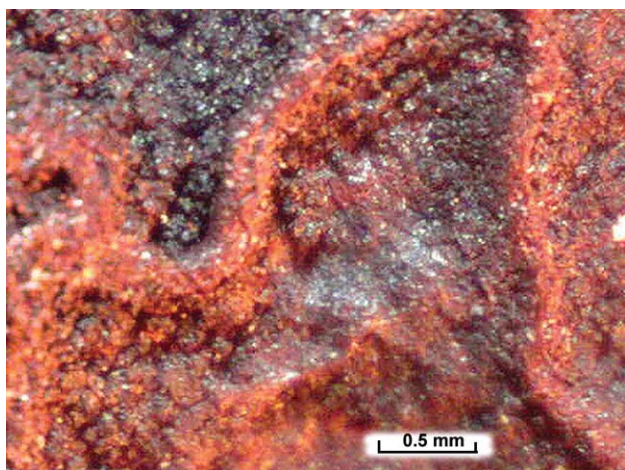


Figure 20. Microphotograph of one tessellation basin, showing the iron oxide/hydroxide accretion and the rim-pool-like silica lacing.

frequent evidence of multiple re-precipitation as well as by the many microscopic inclusions that may be observed in them, which are clearly of aeolian origin. At Dampier they range from polished quartz grains to charcoal fragments, from clays to various crystals such as tourmaline and possibly zircons, and I have even observed detrital matter from plants.

Where the host rock's substrate is practically free of iron, as on the felsic facies, the ferromanganous crust has to be from an extraneous source and in most situations must have been transported by wind. Indeed, this is often apparent from the striate appearance of the accretion on granite boulders in exposed locations. On substrates free of iron and manganese cations, the accretion rate of the ferruginous deposit has been roughly calibrated against time, through the study of dated inscriptions in the eastern Pilbara (Bednarik 2002k, 2002l). An incipient film of a few microns maximum thickness is established within a few years. It reaches a thickness of up to 30–50 μm after 100 years and up to 100–150 μm after 230 years. It does, however, remain very patchy even after many millennia, and the thickness should be regarded a measure of antiquity only at the coarsest level.

Macroscopically, these mineral accretions appear rather well consolidated and of often relatively even colour. However, a very different picture emerges under magnification of 60 \times to 100 \times . These deposits are in fact very fragile and clearly susceptible to mobilisation. Often they are only of low hardness on Mohs scale, readily friable and open to acidic degradation. Additionally, the iron oxides and hydroxides (Fe-oxyhydroxides) that determine the macroscopic colour are generally metastable, even under natural environmental conditions. The perhaps most common ferruginous component, goethite, can be reduced to haematite, lepidocrocite can be dehydrated to haematite, magnetite is metastable with respect to maghemite, which in turn ages to haematite. Thus,

the colour of these deposits is largely determined by the combination and state of the iron phases present, although other cations, specifically manganese, also influence colour variability. The phenomena we simplistically describe as 'ferruginous patina' are the present result of highly complex reactions and of continuing processes they are subjected to, especially repeated mobilisation and re-precipitation. It is impossible to understand them and their complex interplay and ongoing modification regimes without the use of field microscopy.

Of particular relevance here is their susceptibility to rapid degradation by specific changes to the environment. Petroglyphs at Dampier, and throughout the Pilbara region — or, indeed, in most petroglyph regions of the world — occur primarily as two basic types (see technological discussion below). In one of these, called *sgraffiti*, the stratification of the rock is utilised to create a colour contrast, which then forms the image. Typically these motifs are shallow, mostly in the order of 1 mm deep, because in creating them it suffices to breach the dark-brown patina and expose the light-coloured weathering zone beneath. The second form of petroglyph commonly found consists of deep grooves, from a few to dozens of millimetres deep. Although these exhibit of course the same colour contrast initially, their visibility is in part attributable to their groove relief. Upon full repatination they remain relatively visible, especially in oblique lighting, whereas the *sgraffiti* become obliterated by full repatination; they cannot then be discerned without the use of special techniques. Therefore they tend not to be counted in archaeological surveys. Moreover, in using relative repatination to distinguish chronological groups, archaeologists tend to create artificial rock art traditions by ignoring the taphonomy of rock art corpora (Bednarik 1994d). Because *sgraffiti* observed are almost universally of more recent ages (<5000 years), imaginary rock art sequences have been created of early deep petroglyphs followed by late shallow petroglyph traditions. Moreover, the current degradation of the patina through massive acidic industrial emissions tends to distort patination appearance. Lightly re-patinated petroglyphs are most rapidly denuded of patina and may then appear younger than they are, while heavily patinated surfaces will take considerably longer to shed their accretions. On the other hand, deposition of iron ore dry aerosols (fine particles from iron ore processing) would accelerate the repatination process and thus obliterate visibility of *sgraffiti*.

Between 70% and 80% of the surviving Dampier petroglyphs are *sgraffiti* (Fig. 21), and if they offer adequate colour contrast to be visible, they depend entirely on the survival of the patina contrast for their continued visibility. If the thin ferruginous patina is removed chemically, on the petroglyphs as well as the surrounding rock surfaces, the underlying light-coloured weathered rock becomes exposed, *sgraffiti*



Figure 21. Typical sgraffito petroglyph at Dampier.

and surrounding areas adopt the same colour, and most Dampier petroglyphs will then cease to be visible. Alternatively, if repatination is accelerated by iron ore dust accretion, the same applies. *These factors are of fundamental importance to the continued survival of most Dampier rock art* (Bednarik 2002a).

Deterioration of patinae at Dampier

The loss of rock art at Dampier (Bednarik 2002a; Vinnicombe 2002) occurs mainly in two forms: through the instantaneous physical destruction of sites being in the way of development (by bulldozing, grading, excavation or blasting, see Fig. 22; and since 1980 also by relocation of decorated boulders, see Fig. 23), and through the slow destruction of the rock patination by industrial emissions. Additional but minor factors are vandalism by site visitors, pilfering and natural

deterioration¹. The process of chemical deterioration by gaseous emissions remained beyond detection until the 1980s, when the first clear indications of patina loss were noticed. Since then there has been an increase of acidic emissions as the Northwest Shelf LNG installation at Withnell Bay enlarged its capacity, and a new polluting industry, the Burrup Fertilisers plant, was added. Continuous monitoring of selected sites indicates a corresponding pattern of increasing deterioration of the ferruginous surface accretion, on which most petroglyphs depend for their continued existence (the preliminary results of this study will be presented in part 2 of this paper, taking into account the measurable effects of the catastrophic chemical spills from the Burrup Fertilisers plant during 2006). At present, in early 2007, Woodside Energy intends to establish its \$12 billion Pluto plant at Holden Point, between Withnell and King Bays. This will involve a virtual doubling of the emissions of CO₂, NO_x and SO_x.

Five years ago I reviewed the levels of the emissions at that time and reported their effects on the ferruginous accretions (Bednarik 2002a). It became

1 Pilfering of rock art may be more frequent than is apparent, I know of several examples. Recently, an anonymous businessman from Sydney telephoned me with the following proposition: he has in his possession a boulder bearing an anthropomorph from the Dampier town site, which he saved from certain destruction in the early 1970s. He proposes that I offer it to a suitable party, perhaps a museum, for a large sum of money, and he and I would share the proceeds equally. He was astounded to hear that, firstly, no museum would pay any price for an illegally removed object, let alone the large sum he had in mind, and secondly, that the object was the rightful property of the Traditional Custodians. He could expect to escape prosecution by offering to return the artefact at no cost, having saved it from destruction. My potential business partner thought I was unreasonable and terminated the discussion.



Figure 22. Removal of petroglyph-bearing rock outcrop to make room for a tourist access road, May 2006.

evident within a year that my estimates had been very conservative indeed. The most damaging industrial discharge (but not the most dangerous to humans) is that of nitrogen oxides, which Woodside reported to be 5800 t/yr in 2001 (NPID 2002), i.e. at a level similar to all previous years for as long as this had been reported. However, in the next year this jumped to 11000 t/yr in 2002, and 12000 t/yr in both 2003 and 2004. This 'sudden increase' was prompted by my investigations: in March 2003, Woodside admitted publicly that the company had consistently lied about its emissions, and that these were in fact double the quantities the company had admitted for many years (Woodside 2003). This explained at last why the deterioration of the accretions seemed to proceed in excess of what I would have expected from the level previously admitted. My figures for the other major emissions, too, were too conservative, and it needs to be stated categorically that Woodside at Dampier is by far the greatest emitter of greenhouse gases in Australia, as well as the greatest air polluter in every other sense. For instance, the company admits emitting 33 000 t/yr organic compounds, including 1200 t/yr benzene (a carcinogen causing five forms of leukaemia, and a developmental and reproductive toxicant affecting blood production and destroying the immune system), 2000 t/yr n-hexane and 2200 t/yr toluene. The carbon dioxide totals about twelve million tonnes per year at Dampier now, and with the new Pluto plant this is set to rise to around twenty-five million tonnes.

On 25 July 2002, within a few weeks of the appearance of my paper in *RAR* (Bednarik 2002a), the state government of Western Australia announced that it would conduct a four-year study to investigate the issues I had raised. On 16 October 2002, the then Premier, Dr G. Gallop, announced a committee of nine members, the Rock Art Monitoring Reference Committee (RAMRC), to oversee this project. Exactly four years later, on 17 October 2006, the government released a report on the results of the *first* of these four years of study, conducted by a supposedly independent team from CSIRO.

The original objective of the CSIRO project was to 'investigate and report on impacts of proposed industrial developments on the rock art of the Burrup'. Specific research questions had been formulated by the RAMRC: are industrial emissions accelerating weathering; and if there is a significant and measurable problem, what management approaches can be recommended? The CSIRO project has not clarified these issues, or even attempted to do this. The objective to report on the 'impacts of proposed developments' (such as the Pluto plant and others) was completely ignored. No modelling of any kind was



Figure 23. Removal of one of thousands of petroglyph boulders from its site, about 1980 (after Vinnicombe 1987).

even attempted, and in that sense alone this report is significantly inferior to previous studies of the impact of Dampier industry, such as those by Sinclair Knight Merz (e.g. SKM 2003), commissioned and then mothballed by the state government.

The project has, however, provided excellent basic data on the quantification of some of the many relevant wet and dry aerosols, and in that sense offers substantial justification for the concerns first expressed by me. Most important of all, it provides independent confirmation that acidic precipitation now occurs for most of the year. 'Acid rain' occurred in eighteen out of twenty periods checked (CSIRO 2006: Table 13a). Acid rain is precipitation of a pH of <5.6 caused by anthropic agents, such as industrial emissions. At Site 8, the pH was 7.5 and 5.8 respectively in two periods, but in eighteen other periods it fell between pH 4.3 and 5.3, with a mean of pH 4.597. This represents a ten-fold increase in acidity (i.e. reduction of hydrogen ion concentration; the pH scale is a decadal logarithm) from the upper limit of acid rain. It means in practical terms that the rainwater at Dampier has the acidity of beer, but is slightly less acidic than lemon juice. To provide another comparison, two of the most highly polluted industrial centres of Australia, Hunter Valley and Latrobe Valley, have pH 4.9–5.2 and 5.4–6.4 respectively, i.e. are notably less acidic.

The rainwater pH of Dampier was mostly near pH 7.0 and 7.2 in the 1960s (Bednarik 2002a: 36), before the establishment of major pollution sources. It peaked occasionally at pH 7.6, but rarely fell below pH 6.5, the general background value for the Pilbara. Since then it has fallen gradually, almost certainly in response to the introduction of dry aerosols, especially after the commissioning of the Northwest Shelf facility in the early 1980s (Fig. 24). The granophyre and dolerite rocks of the Archipelago typically lack acid neutralising capacity (see above), and the ferruginous mineral crust covering all rocks is gradually degraded through the mobilisation of its cations, notably iron and manganese. Ford et al. (1994) have shown that

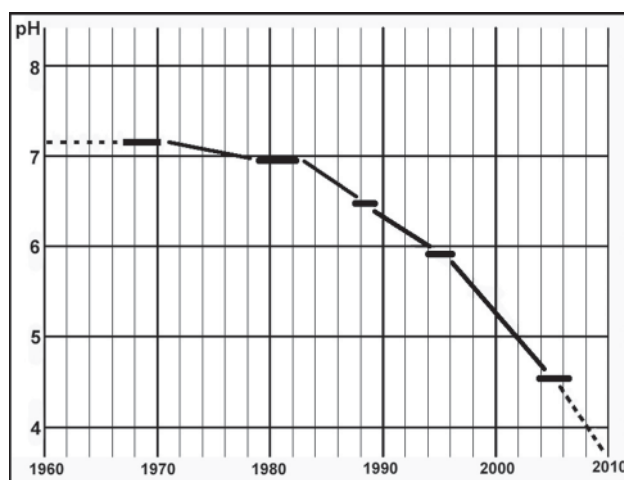


Figure 24. The mean acidity of precipitation at Murujuga from the 1960s to the present.

a reduction in pH of 2.2 units in the Napier Range, Kimberley, has increased rock solubility by 230%. While responses differ considerably between specific lithologies, it is to be expected that the significant fall of pH at Dampier between 1969 and 2005, of about 2.5 units, would affect rock surfaces profoundly.

The whole issue of the conservation of the Dampier petroglyphs revolves around the need to prevent the mobilisation of the surface crust's cations, especially iron and manganese, caused by a lowering of the ambient environmental pH. The solubility of iron increases very significantly in this range (about 100 000-fold through the lowering of the pH from 8.5 to 6.0, as noted above). Not only does the CSIRO report reveal a decrease of the precipitation pH from around 7.2 to an average of 4.6 for most of the year, its single measurement of pH 7.5 (16–23 March 2005, at sampling site 8) confirms that, under exceptional climatic conditions, the pristine values are still achieved — perhaps once or twice a year. For most of the year, precipitation (dew or rain) is now in the form of 'acid rain', containing carbonic, nitric, sulphuric and other acids.

In referring to the conditions in polluted cities in south-east Asia or Perth (cf. Rye et al. 1996), the CSIRO team implies that it has misunderstood the brief of its project: the ferruginous patina would never survive in such localities, or even form in the first place. Moreover, gaseous air emissions such as nitrogen dioxide travel enormous distances (Wenig et al. 2003), and the 'control site' CSIRO used in this project, at Mardie Station, is only 81 km from Dampier. Hence the CSIRO report's assumptions made about background levels are false. The quantified data provided by CSIRO shows that modelling predictions severely understated the level of air pollution (SKM 2003). For instance, the crucial Dampier nitrogen dioxide concentrations are now said to hover around 2000 to 3000 ppt (CSIRO 2006: Fig. 10), whereas predictions had been about 200 ppt three years earlier

(CSIRO 2006: Fig. 25). Compared to those of 4 ppt at Cape Grim in Tasmania (Rob Gillett, member of the CSIRO team, pers. comm. 20 October 2006), a site with relatively clean air, the Dampier levels are now close to a thousand times as high as at a 'clean' site. The cheap passive sampling method (Steinbacher et al. 2005) used at Dampier is so imprecise and unreliable its results are not even legally accepted in the European Union.

The CSIRO study has thus confirmed that the petrochemical industry at Dampier produces acid rain nearly all year round, but it has failed to investigate its effects on the rock art or the rock patina. It has therefore failed to address its terms of reference, which were specifically to study the effects of the emissions on the rock art; to assess the impact of future developments on the Burrup; and to advise on appropriate management measures. Instead these matters are considered here.

In May 2007, I prompted a series of questions concerning precipitation acidity in the state parliament of Western Australia, including: has the distribution of rain acidity (precipitation pH) across Western Australia ever been mapped? What is the current annual level of CO₂ emissions from the Dampier industries? Why is a nitrogen oxide levy not applied in Western Australia? The answers given were most illustrative (Hansard 2007). The government does not know of any rainwater acidity study, and '[t]he phenomenon of acid-forming rainfall is considered low risk in Western Australia, given the level of acid-forming gaseous emissions and rainfall patterns.' Bearing in mind the enormous level of emissions at Dampier (Bednarik 2006c), this illustrates the state of denial. More directly, the admission of the Minister for the Environment that the level of CO₂ emissions at Dampier, or anywhere else in the State, is unknown is a severe indictment of the authorities in this time of global awareness of climate changes. The State admits it lacks even the most basic quantification data on carbon emissions, so how can it consider such issues as carbon trading? Concerning the proposed NO_x levy, the Minister is happy with the present arrangement.

Acid rain, a term coined by Robert Angus Smith in 1852 when he noticed the connection between London's polluted atmosphere and the acidity of its rainfall, refers to wet precipitation that has a pH of less than 5.6. It is caused when rainwater, dew or snow react with anthropic gaseous emissions to form acids before reaching the ground. Airborne acidic materials can be deposited on the surface of the Earth in both wet and dry forms, as rain, snow, fog, dew, dry particles and gases. About a third of all deposition is thought to be dry. In studies of rock art deterioration through acid rain, the term 'through-fall' has been coined (Löfvendahl and Magnusson 2000). This refers to dry deposition in a plant canopy, where it remains 'dormant' until it is 'activated' by moisture, notably rainwater. As is to be expected, the solution resulting from this phenomenon is considerably more acidic

than the rainwater itself. Having acquired H^+ ions in the atmosphere, their concentration is further boosted as the acid rainwater percolates through the plant cover before it reaches the ground.

The effects of acid rain on a particular ecosystem depend much on its acid sensitivity (which is high in arid regions), its acid neutralisation capability (very low at Dampier), the concentration and composition of acid reaction products, and the amount of acid added to the system. Therefore it is relevant to monitor the soil pH, another crucial omission of the CSIRO project. Foliar effects on vegetation require prolonged exposure to severe acidity, but specific plant communities, such as those at Dampier, are very susceptible to soil acidification. Aquatic systems are particularly at risk, from precipitation, runoff and the effects on invertebrates living in aquatic sediments (at Dampier, coral reefs, mangrove zones and reproductive impairment of fish should be prime considerations).

Here we are only concerned with the effects of the Dampier acid rain on the petroglyphs, especially the dominant sgraffiti. The following crucial observation, made several years ago, was first reported in Bednarik (2006a: Fig. 126). I had noticed that in the immediate vicinity of trees, shrubs and spinifex (*Triodia* sp.) clumps, the ferruginous mineral accretion covering all rocks uniformly had begun to disappear. Beneath the canopy of trees, almost all the patina had been leached from the rock by 2004, especially close to the Woodside plant. A botanist consulted advised that there was no known process by which a plant could effect this (Vicki Long, pers. comm. April 2004). This process takes place wherever patinated rocks co-occur with vegetation, and its effectiveness extends at least 100 km from Dampier (Fig. 25). As noted above, much of the enormous acidic emissions of the Woodside plant, the largest source of such emissions in Australia, travel great distances, but a substantial portion is deposited close to the source. A realistic model of the relative distribution of the fall out can be obtained from the TAPM and CALPUFF predictions CSIRO (2006: Figs 24, 25) cites, but produced by commercial consultants (SKM 2003).

The 'bleaching' of the patina at Dampier by what Scandinavian rock art researchers have called 'through-fall' provides essentially an 'early warning system' of the long-term effects of the acidic emissions. What occurs in through-fall is that airborne acidic materials are deposited in dry form, within the three-dimensional space of the plant canopy. Rain occurs rarely in Dampier, but when it does these dormant substances are converted to acids and washed down to the rock surfaces below: they are in fact projected from a three-dimensional dormancy onto a two-dimensional surface, and therefore of much higher acidic concentration than the acid rain acting as their solvent or hydride source. Therefore the rocks subjected to through-fall have been affected far

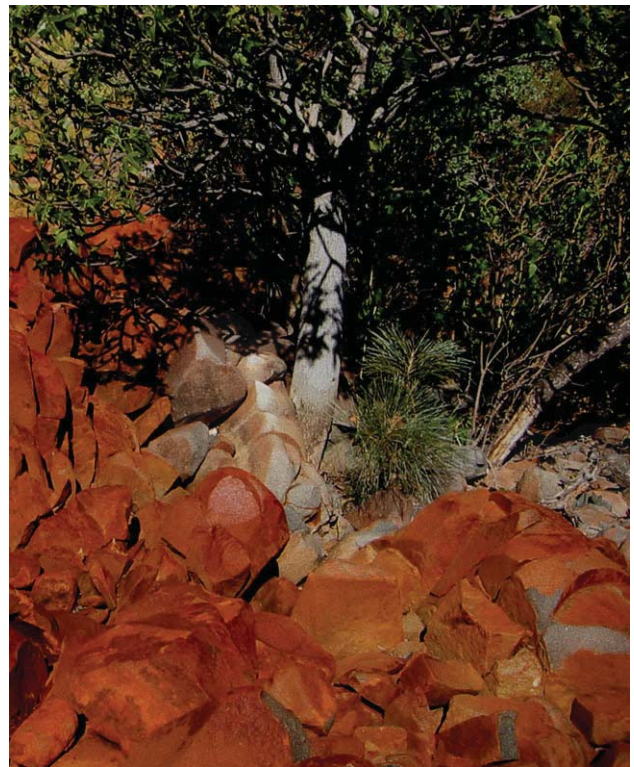


Figure 25. Typical 'bleaching' of patina beneath vegetation foliage.

more severely by mobilisation of cations than other surfaces, and the iron accretions on them have been almost completely removed already. The pH during such through-fall episodes varies greatly, but can be as low as pH 3.2. On the rock surfaces affected, the substrate is fully exposed and there is almost no iron or manganese left (Figs 26 and 27). It is perfectly realistic to extrapolate from this general observation and to predict that, in time, all of the brown patina in the region will fall victim to the atmospheric acidification, and most of the petroglyphs will fade beyond the threshold of being readily visible. In short, most of the Dampier petroglyphs are already doomed and I have predicted that they will begin to disappear in the late part of the present century (Bednarik 2002a).

Even the main culprit in the vandalising of the Dampier cultural precinct, the state government of Western Australia, fully accepts that the monument 'must be saved at all costs'. If there is to be any effort to preserve this treasure of humanity, I perceive a great urgency to determine the precise processes of patina deterioration at Dampier: what precisely happens at the microscopic level during a precipitation event? Does the aggregation of dry aerosols, particularly iron ore fine particles ($<10\ \mu\text{m}$), cause accretionary deposition? To what degree could the combination of the effects of wet and dry aerosols, contributed by industry, be arrested or at least retarded? The impact of future industrial developments at Dampier needs to be assessed in this light, the behaviour of present



Figure 26. 'Bleaching' of patina under tree near the Woodside Northwest Shelf plant, Dampier.

emissions needs to be better understood, and TAPM or CALPUFF predictions need to be made for new developments, such as the Pluto project or additional LNG trains at the Northwest Shelf plant. Acidity of the atmosphere as well as soil acidity needs to be monitored. Political rhetoric about the need to preserve the rock art needs to be matched by these initiatives. The deterioration of the Dampier surface accretions needs to be fully understood, not just for these immediate reasons: the survival of many of the world's petroglyphs is similarly incumbent upon finding ways to counter the effects of polluting industries in remote, arid or semi-arid regions. What is a crucial conservation problem at Dampier today may affect many other sites tomorrow — elsewhere in the Australian Pilbara and in many other parts of the world. Cheap, unfocused and misguided efforts such as those by CSIRO at Dampier will not solve the profound preservation problems we face.

Classification and technology of Dampier rock art

There are objective and subjective ways of classifying a corpus of petroglyphs, and in subse-

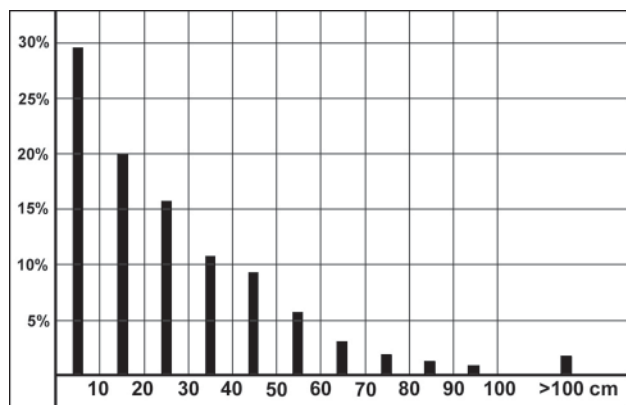


Figure 28. Maximum sizes of petroglyphs in Murujuga Areas 1 and 2 ($n = 7560$).



Figure 27. Comparison of 'bleached' and 'unbleached' rocks at the location shown in Figure 26.

quent chapters it will be attempted to flesh out the differences between them. Here I will focus on testable taxonomies, and on their nexus with aspects of the production of this rock art.

Objective classification of petroglyph motifs can be of many types. A typical example might be by maximum dimension, provided that we do not assume that the results could defy taphonomy or sampling errors. For instance, if we compare my quantification of petroglyphs by size in the parts of Murujuga I designated Areas 1 and 2 (Fig. 28), we note that it differs significantly from others' characterisation of the southern Murujuga corpus. Whereas Bird and Hallam (2006: Table 7) list 85% of this body as being between 0–30 cm, 12% between 31–60 cm and 2% >60 cm, my corresponding figures are 65%, 26% and 9%. Their percentages are derived from McDonald and Veth (2005: Table 23), which in turn refer to Vinnicombe's data. These are based on 1937 motifs measured at a series of sites. My figures, however, were secured from a greater sample of 7560 motifs, and they derive to a considerable extent from sites that no longer existed when Vinnicombe re-sampled these areas. That may explain why my percentages are much closer to those recorded in her small samples of central and northern Murujuga. Therefore the full reasons for these differences are not immediately obvious, but may be a combination of several factors, especially inadequate and selective sampling as well as recording of partially destroyed assemblages.

As noted above, the Dampier petroglyphs fall into two basic types, sgraffiti and relief petroglyphs. Generally, a sgraffito (from the Italian *sgraffio*, a scratch, from *sgraffiare*, to produce a sgraffito) is a form of marking created by cutting through one or several differently coloured layers to selectively expose one or more sub-layers beneath. It has been widely used on ceramics, where the designs were scratched into surface coatings or glazing to expose a different colour, and on building walls, externally or internally. In the modern version of the latter method, still widely used on building facades in parts

of Europe, two to several differently coloured layers of cement render are first applied, and bichrome or polychrome images can then be produced by cutting to the depth of specific layers and removing covering layers before the render has fully set. In the case of rock art, two or three differently coloured, naturally occurring layers are utilised (Bednarik et al. 2003). In most cases they are dark-brown surface patina, followed by a usually light-coloured weathering zone, and sometimes extending into a differently coloured unweathered zone. However, there are also examples on record of sgraffiti petroglyphs on other natural surface laminae, which have on occasion been mistaken for rock paintings (cf. Michelsen 1983). Despite the frequent occurrence of sgraffiti on archaeological ceramics of various periods, archaeologists commenting on rock art do generally not use the term sgraffito.

Because sgraffito petroglyphs tend to become almost undetectable upon full repatination, there is a strong taphonomic bias in favour of deeply pounded petroglyphs, which remain much more visible and are therefore inevitably over-represented in all sampling quantifications. Similarly, there is considerable confusion about other aspects of petroglyph technology, about the production techniques involved and about an appropriate taxonomy (Bednarik 1998). In Australia, an early effort to clarify these matters was published by Maynard (1977), but it has rarely been observed. Maynard distinguishes the following technological classes:

1. Scratching, a 'single stroke of friction'.
2. Abrading, 'repeated application of friction'.
3. Rubbing, covering a broad surface.
4. Pounding or direct percussion, with a hand-held hammerstone.
5. Pecking or indirect percussion, using an intermediate tool (chisel, gad or bit) in one hand, a hammerstone in the other.
6. Rotation or drilling.

Most writers since have used the term 'pecking' to describe any percussion petroglyph, yet I have argued that both pecking and drilling are methods that were either never used to produce petroglyphs, or used only on very rare occasions (Bednarik 1991). Having covered the topic of technological confusion in some detail elsewhere (Bednarik 1998), I merely note that I essentially follow Maynard's terminology, which is embodied in the *IFRAO Rock Art Glossary* (Bednarik et al. 2003). Most descriptions of Dampier petroglyph technology are idiosyncratic and qualitatively inconsistent. Moreover, they are often also misleading in the quantitative sense, because they are based on unrepresentative samples (of small areas), as well as individual preferences and perceptions. No field microscopy has been conducted in securing them, for the purpose of detailed determination of technique, and the significant terminological and

quantitative variations apparent between different authors suggest that their pronouncements may not be compatible. For instance, Vinnicombe (2002) defines four technological types of Dampier petroglyphs: pecked, scored, abraded and pounded (besides composite forms). However, it is clear that she uses the term 'pecked' in the sense of Maynard's 'pounded', 'scored' in lieu of Maynard's 'scratched', and what she describes as 'pounded' figures may well be 'rubbed' motifs in Maynard's system. Vinnicombe (2002: 16) wisely avoided quantification of her types, whereas McDonald and Veth (2005) present detailed quantifications. Using a small sample of 8386, subjectively chosen motifs, they distinguish between pecked (70.5%), pecked and abraded (19.8%), abraded (5.2%), scored (4.0%) and bruised/battered motifs (0.5%). However, the samples were not collected by these authors, but by several parties who used greatly differing definitional tools, and they even include many determinations from photographs (McDonald and Veth 2005: 86). The use of diverse and incompatible records is evident from the inclusion of the data from Veth et al. (1993), who arrived at the view that 'scored' motifs (presumably they mean 'scratched') are the most common, followed by 'abraded', with 'pounded' motifs last. The Veth et al. study considered the most northerly parts of Murujuga, which led Vinnicombe to suggest that 'a study of inter-site variability in relation to geographic, environmental and cultural considerations could highlight other significant differences'. Yet the findings of Veth et al. are unambiguously invalid, the pounded motifs are clearly the most common in the area they consider, and the scratched or scored motifs account for only a small percentage. Their idiosyncratic technological types are incompatible with those of anyone else; therefore the desktop study by McDonald and Veth (2005) effectively compares apples and oranges and calls them pears and lemons. The many inherent incompatibilities of their sources include significant differences in terminology, taxonomy and epistemology.

Even Vinnicombe's work, which is more reliable than that of most others who were involved with Dampier rock art, still contains numerous technological errors. For instance, she thought cupules were made by abrasion, 'grinding away an entire shape'. Not only would it not be possible to create cupules on hard rock in this way, all cupules I have seen made by traditional people were made by pounding (*sensu* Maynard; Bednarik 2007c), as were all those made in replication experiments (Bednarik 1998; Kumar 2007). Similarly, all work traces ever observed in cupules, including those in limestone caves, derive from impact. With the exception of G. Kumar, no archaeologist has observed cupule production ethnographically, or has created experimental cupules, or has studied a cupule under a binocular microscope. Abrasive action results in

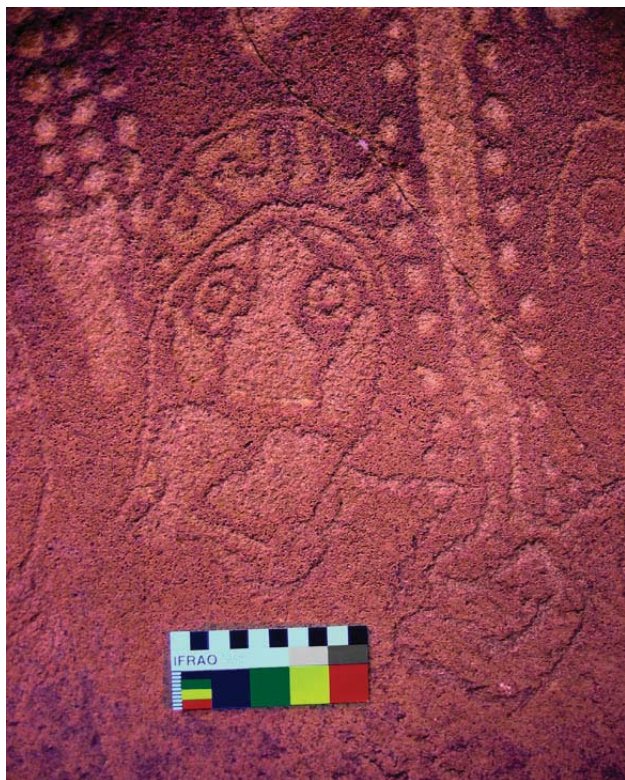


Figure 29. Typical 'negative petroglyph', near Woodside's Northwest Shelf LNG plant.

distinctive wear patterns easily recognised by the rock art scientist (striae, polish, sharp edges of truncated crystals or grains), as does impact (shattering of grains or crystals, conchoidal fractures etc.). All of the many hundreds of cupules I have examined microscopically, in every continent, were made by direct percussion (pounding). Technological determinations expressed by archaeologists are based on inadequate methods of analysis, in nearly all cases consisting of no more than superficial visual examination. These 'observations' are then compiled, assessed statistically and subjected to a variety of interpretations. At Dampier, these endeavours have yielded almost no *scientific* data.

It is also relevant that the most distinctive technological type of rock art found at Dampier, the 'negative petroglyph', has not been identified in any of these numerous surveys. A geoglyph was usually made by arranging rocks on the ground, but a 'negative' geoglyph (most common in South America) was created by removing dark-patinated clasts to expose unpatinated, light-coloured subsurface. Similarly, in a 'negative' petroglyph, the image is not formed by the light-coloured rock exposed through pounding away the dark patina, but by the areas of patina left behind as the areas around it were removed. Therefore most negative petroglyphs are enclosed in a surrounding line (Fig. 29). This is an extraordinarily inventive way of petroglyph production, which appears to be restricted to the Dampier Archipelago. It is rare even there; only a few hundred motifs of

this genre seem to exist. Some appear to represent stylised faces; others resemble the carvings found on wooden implements, such as shields and spears. The uniqueness of these petroglyphs seems to have gone largely unnoticed. Instead, commentators have focused on their diffusionist perception that these 'archaic faces', as they call them inappropriately, resemble those recorded at Cleland Hills, central Australia. No connection can be demonstrated between the two traditions, nor between the Dampier 'faces' and any other similar motifs elsewhere (which do occur in many parts of the world, even close to Australia). Moreover, the Dampier images are often quite distinctive, and the meaning of at least some of them is known.

In my own work I have observed, as Vinnicombe has subsequently also noted, that many motifs combine evidence of more than one production method. This is frequently hard to detect, because the most recent treatment tends to be the most prominent, the most readily visible. Even motifs apparently executed by one person, in one sitting, may combine more than one technique. Dampier rock art needs to be studied by an analytical methodology addressing production sequences (operational chains), the gestural detail involved, patterns of reuse, correlations with micro-topography and a host of other aspects of the art not concerned with the analyst's own reactions to the motif (what it reminds one of, or how it should fit into the worldview of the alien researcher). Motif identifications by archaeologists are epistemological transgressions and cultural appropriations, they lack scientific validity (see below). Motif counts and superficial speculations of technology, the only factors all archaeological commentary has been based on, derive from surveys by inadequately experienced personnel often lacking appropriate equipment and training. Having been universally guided by an agenda of determining the *meaning* of motifs, of establishing what they depict, this has been an epistemologically misguided approach. Most importantly, available indigenous interpretation has been completely ignored at Dampier. Finally, these endeavours have often omitted the fully repatinated sgraffiti, having failed to detect them, and they are uniformly devoid of an appreciation of the effects of taphonomic logic (Bednarik 1994d) on their data.

Even archaeologists have noted these significant problems with archaeological motif counts conducted at Dampier. Bird and Hallam (2006) state:

Classifying motifs can be complicated by a number of factors, including cultural bias, recorder expertise, and inconsistency between recorders. There is no standard recording scheme for the Dampier Archipelago and it is therefore difficult to compare the results of different surveys and even to be confident that recording has been internally consistent within individual surveys. It should be noted too that the records have been made from the perspective of a

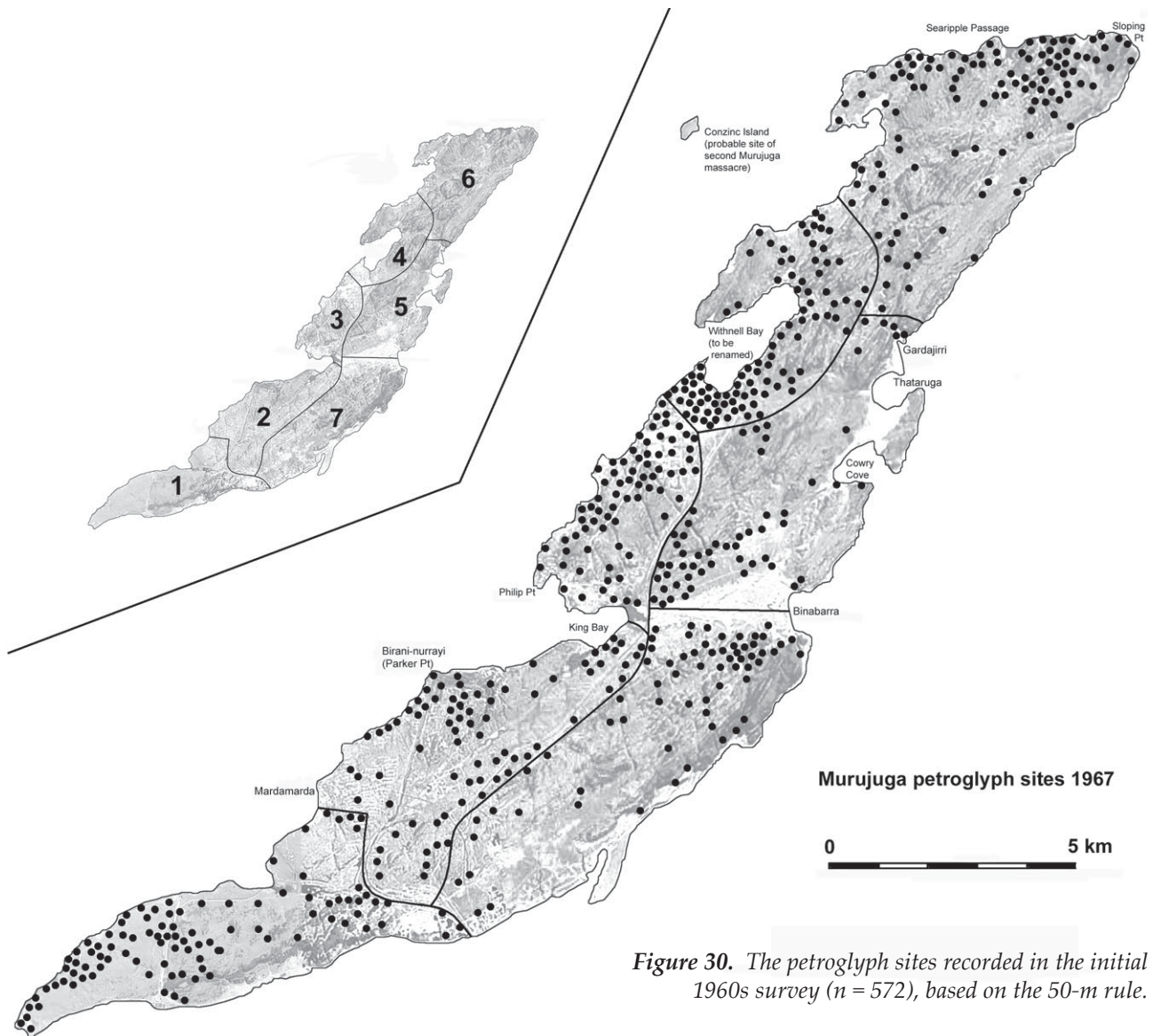


Figure 30. The petroglyph sites recorded in the initial 1960s survey ($n = 572$), based on the 50-m rule.

Western scientific paradigm.

Obviously, the last term used is an oxymoron: a Western paradigm is Western, not scientific. Nevertheless, Bird and Hallam's observations are intrinsically valid, as they continue:

It is difficult to compare different analyses because no consistent general typology for the motifs has been developed from a rigorous stylistic and spatial analysis. Different studies use different general groupings. Even the two most detailed classifications to date (McDonald and Veth 2005; Lorblanchet 1992) differ significantly and cannot be easily compared.

In fact the problem is much greater than that: to begin with, McDonald and Veth (2005) present only a 'desk-top' study of others' results and Lorblanchet only focused on two major site complexes. However, more importantly, archaeological surveyors have only recorded maximums of between 60% and 80% of the motifs present in any given area of Dampier, because of lack of experience, diligence and appropriate equipment. Yet even if they had recorded every motif still discernible, their sample would still have been severely truncated by ta-

phonomy, and all their statistical findings would be invalid in defining the corpus. If we add to this the fundamental incompatibility of all samples used in McDonald and Veth's 'desk-top study' of a small number of Dampier petroglyphs, collected under conditions of systematic biases, it becomes evident that interpretations derived from this exercise have to be statistically invalid. Finally, all these exercises, from Virili's and Lorblanchet's through to Bird and Hallam's, completely lack any ethnological input, and for that reason alone they are unscientific and ethically challenged.

It follows from these brief observations that the classifications and technological judgments found in reports about Dampier rock art are contradictory and largely without merits. Therefore, they are unsuitable for major syntheses and the work needs to be repeated, using very different guidelines. The classification and technology of Dampier rock art has not been addressed in a satisfactory fashion so far.

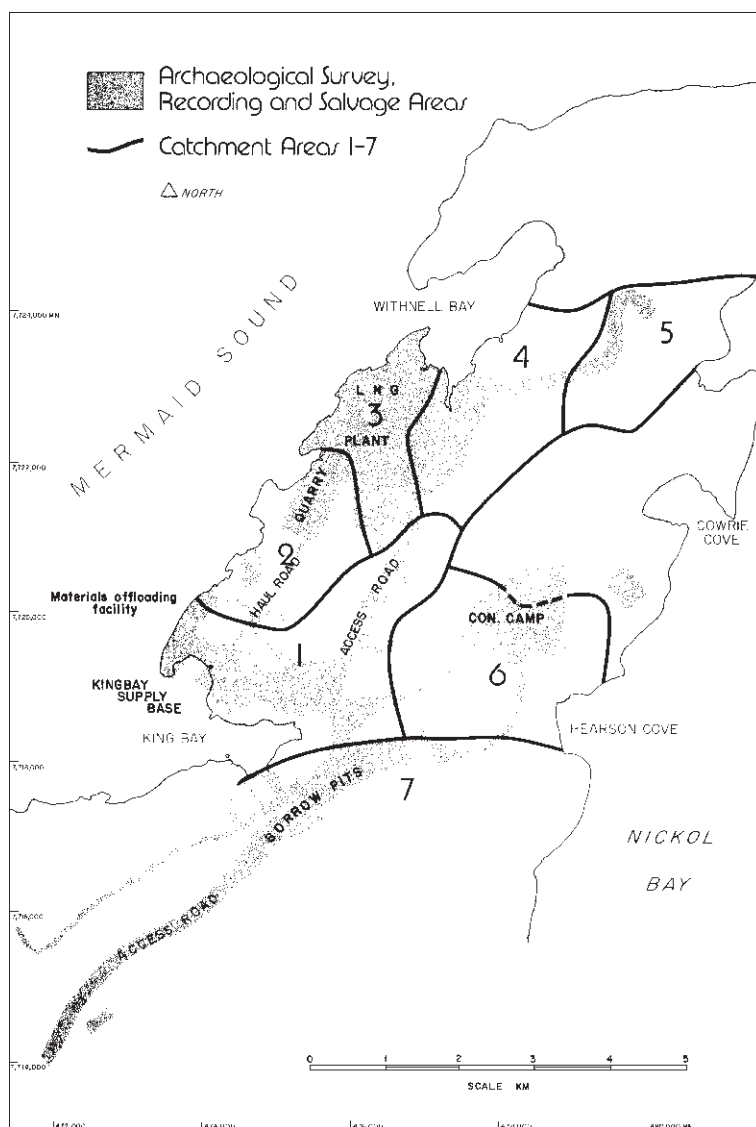


Figure 31. Principal areas considered by the Woodside salvage project c. 1980. The shaded areas indicate the areas surveyed and recorded (after Vinnicombe 1987).

The issue of quantification and distribution

Precisely the same applies to the issue of quantification: we have no credible estimates of two crucial numbers: how many petroglyphs were there in the Dampier Archipelago, or any part of it, in the early 1960s, and how many are there today? Consequently, the precise number of motifs destroyed since 1963 is also unknown, and there is every indication that the state government of Western Australia, the authority responsible for the safekeeping of this monument, prefers to keep it that way. It has consistently, for several decades, resisted all requests to create an inventory of the Dampier corpus, just as it has re-sisted all calls for a management plan for the archipelago. Initially, for the first ten to fifteen years of development at Dampier, the government allowed the indiscriminate and wholesale destruction of large swathes of rock art sites, making no attempt to collect any information on the rock art destroyed (despite requests from

me since 1969). After the introduction of the 1972 *Aboriginal Heritage Act* there was initially little change to practices, until the late 1970s when the practice of conducting archaeological surveys of areas being destroyed became established (Bednarik 2006a: 37). By that time, large sections of my survey Area 2 and smaller parts of Area 1 (Fig. 30) had been completely denuded of petroglyphs and stone arrangements, and the only records available on this destruction are those collected by me.

In my survey Area 2, which extends from Marda-marda in the west (just west of Dampier township; the name derives from *thurlamardamarda*, not from *marda*, i.e. 'blood', and apparently refers to the plentiful Sturt's desert pea; pers. comm. Barrarungu-B.C.) to the southern shore of King Bay, but excludes any of the Warura areas (incorrectly called 'Pistol Ranges' by archaeologists, who misunderstood a sign 'Pistol Range', pointing to the shooting range of a former pistol club at Dampier, and thought it pointed to the ranges between Hearnson Cove and the causeway), I counted 78 petroglyph sites.

This, however, needs to be qualified as follows. At the time, 1968 to 1969, I was aware of the work of the Centro Camuno di Studi Preistorici in northern Italy, which had begun operating under the direction of E. Anati a few years previously. It had established the practice of defining as a 'rock art site' a concentration of motifs that is separated from any other, in any direction, by a minimum of 50 m free of any rock art. It is important to note that subsequent researchers working at Dampier who attempted quantification of the rock art seem

to have used a different definition of 'site', based on a 25-m minimum clearance from other sites (some failed to specify their definition of 'site'). Bearing in mind that a very large part of Murujuga is dominated by fairly continuous occurrences of petroglyphs, it is to be expected that the number of sites one would count, based on the 25-m rule, is significantly greater than the number based on the 50-m rule. This is because, while it is reasonably common to find an art-free corridor of 25 m or more around discrete concentrations, finding such a corridor exceeding 50 m is far less common. In fact, the difference is in the order of 1:5, therefore the 78 sites I counted in my Area 2 would correspond to perhaps 400 sites had I applied the 25-m rule. My estimate of the number of motifs in this one area is 39 500, or an average of 500 motifs per site, whereas under the 25-m rule, there would have been only in the order of 100 motifs per site on average. However, the actual numbers of petroglyphs per site vary greatly,

and with one or two exceptions, the ten or twelve sites with the most numerous motifs in Area 2 were completely destroyed before 1971 (especially those on the coast to the west of Birani-nurrayi, where the initial jetty was established). Therefore, the proportion of motifs lost, among the highest on the island, is in the order of 26 000 or 27 000, even though almost half the sites have partially or fully survived.

Most of the rock art destruction or loss in my Area 1 occurred from 1970 to 1973, and although the land areas affected are not substantial (the Dampier Salt haulage road, the highest-lying part of Dampier town and along the railway and roads), again some major sites suffered severely. My estimate of a total of 63 400 petroglyphs in this zone includes a conservative estimate of 12 500 destroyed, even though only ten sites were fully or largely destroyed.

Beginning with the Northwest Shelf Project, i.e. from the late 1970s onwards, we have a semblance of 'monitored' records on the number of sites destroyed in specific areas (Fig. 31), even though numbers of motifs remain nebulous constructs and figures provided are plagued by inconsistencies. The Department of Indigenous Affairs (DIA) estimates that, based on transect surveys of selected areas, there should have been 3690 rock art sites on Murujuga, using the 25-m separation rule. This derives from a density of 56 archaeological sites per square kilometre, of which typically 41% are said to be rock art sites (Hansard 2005; cf. Bednarik 2006b). The DIA has also advised that, since it began keeping records (in the late 1970s), 350 sites have been destroyed and 119 have been relocated. The latter figure relates to the 1682 boulders bearing almost 2000 petroglyphs that Woodside relocated then (Fig. 23). On this basis, the DIA's figures suggest that, of an estimated 3690 initial petroglyph sites, 469 have been destroyed or their contents have been dumped in 'temporary' storage compounds. It has also been estimated (by the National Trust of Australia) that the number of sites destroyed before the DIA began keeping records would roughly double this total to, say, 900 sites. This percentage of 24.4% coincides with my independent estimate (published three years previous to the DIA disclosure in parliament) of 'between 20% and 25% of the original population of petroglyphs on Murujuga' having been destroyed (Bednarik 2002a: 30). The estimate also matches mine that about 160 sites were destroyed (50-m rule), 54 in my survey Areas 1 and 2, most of the rest in Areas 3,

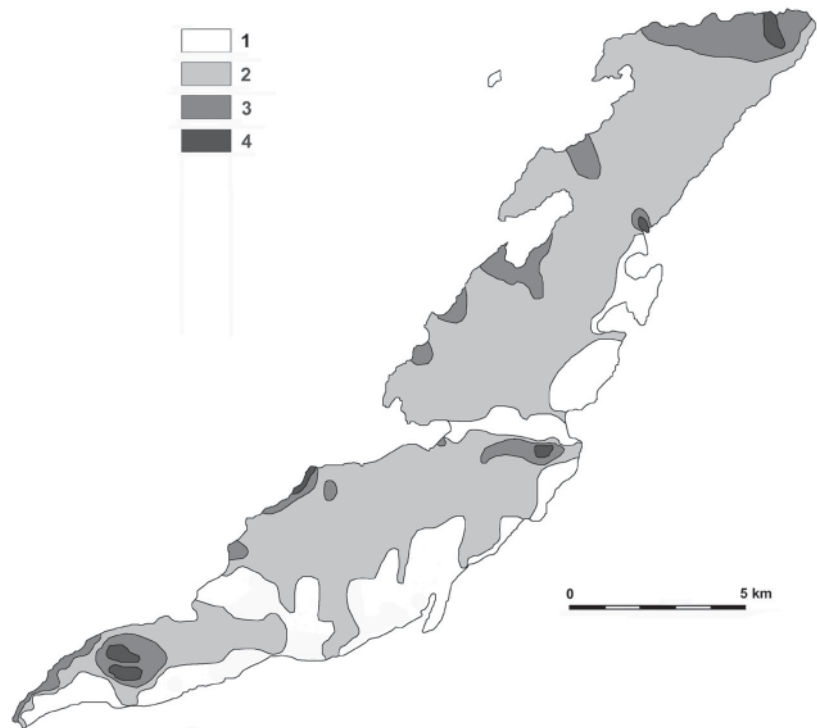


Figure 32. Relative density of petroglyphs on Murujuga recorded in the 1960s: 1 - lowest density, 4 - highest density.

4 and the southern part of 5 (Fig. 30).

These numbers are of considerable relevance and need to be examined more closely. I have not surveyed all rock art sites on Murujuga, only between 80% and 90% of the total. The total number of sites recorded by me, 572 (Bednarik 2006a: 32), therefore suggests that perhaps between 650 and 700 sites existed initially, based on the 50-m rule. This might translate into between 3250 and 3500 sites according to the 25-m rule, which is somewhat below the 3690 sites stipulated by DIA (based on Veth et al. 1993). However, there are a number of discrepancies in the DIA figures. If we assume that there were 23 rock art sites per square kilometre (41% of 56 sites, as the DIA estimates), this amounts to only about 2700 sites on the 117 km² land area of Murujuga. The difference is not substantial, but it suggests, as do my figures, that the estimate of 3690 sites might be too high. This can be readily explained by variations in site density, which are evident in Figure 32. On the other end of the scale, Vinnicombe has promoted very different site statistics. Having recorded 2009 sites in an area of almost 47 km², her average site density is just under 43 rock art sites per square kilometre, almost twice as high as the DIA predicts. Moreover, she claims that the average of general sites that contain rock art is not 41%, as contended by Veth et al. (1993), but is 75.5% (Vinnicombe 1987: 44). It seems almost impossible to account for such differences, particularly as the two survey areas overlap significantly. It appears that either the Veth et al. survey was too haphazard, or the

definition of 'site' was used much more loosely than in my work, or in the work of those who observed the 25-m rule. If we were to extrapolate from Vinnicombe's figures, we would have to expect well over 4000 rock art sites across Murujuga, which I believe could only be achieved by relaxing the 25-m rule.

Similarly, it is also true that other aspects of the figures provided by the DIA are even harder to explain. The DIA offers the figures 4776 (7.2%) motifs destroyed, 1682 (2.5%) relocated and 3327 (5.0%) preserved. This probably refers to the work done by Woodside around 1980, but it is a mystery to what base figure the cited percentages refer. Clearly they assume a total of about 67000 petroglyphs. It is generally assumed nowadays by archaeologists that a reasonable estimate of the number of petroglyphs on Murujuga is in the order of 500 000 (or one million in the entire archipelago, although Vinnicombe also mentions several millions), a figure first mooted by Lorblanchet. Clearly, this mysterious number of 67 000 is not related to any such estimate, because even the most conservative estimate ever offered, by me, is 315 000 counted or estimated number of petroglyphs. Since this accounts for between 80% and 90% of the total, we may deduce that my census, the only one attempting a semblance of comprehensiveness, and the only count taken before most destruction occurred, points to a former total of only 350 000 to 390 000 petroglyphs on Murujuga. Irrespective of the true total, it is obvious that the derivation of the DIA's mystery figure is unclear, unless it is the number of petroglyphs thought to have existed in the total areas impacted upon around 1980.

If we assumed that the DIA figure of 469 sites destroyed or relocated is correct, we could speculate that it would correspond to around 94 sites on the 50-m rule. This refers to locations in my Areas 3 and 4, although a few sites in Area 5 would also have been included. This figure appears to be very close to my estimate of just over 100 sites in that area. In very round figures, based purely on averages taken elsewhere, this would seem to translate into roughly 47 000 petroglyphs lost. Relatively little damage occurred in my Areas 6, 7 and in much of 4 and 5, perhaps less than 10 000 motifs having been affected there in total. On that basis, a total of around 95 000 petroglyphs appear to have been lost across Murujuga, provided the DIA figures are credible in respect of the areas they pertain to. This, of course, amounts to 24.4% of my maximal 390 000 petroglyphs, and thus perfectly confirms both my and the DIA's estimates of destroyed rock art at Murujuga.

There are, however, alternative means of estimating the probable extent of the destruction. We can begin by considering the admission of the Department of Industry and Resources (DOIR, pers. comm. January 2004) that 38% of the land surface of Murujuga has been developed in one form or another. Practically all rock art and stone arrangements that occurred on this

land have been destroyed, although a small part of the rock art has been relocated since 1980. Some attempts have been made to avoid the densest concentrations of rock art (although some of the largest sites have also been significantly impacted upon), which is the only reason why the percentage of rock art lost is not around 38%, but significantly less. More recently, the Premier of Western Australia, Alan Carpenter MLA, stated that the 'best current estimate is that some 42% of the Burrup is designated for industry' (Carpenter 2006; this seems to exclude residential and infrastructure land, because the area developed *and* designated is actually 47%). This offers yet another possibility of estimating the number of rock art sites destroyed. We know from the DIA figures that in the developed area where their most reliable data were collected, 66.3% of the rock art sites were destroyed. If we extrapolate from this knowledge and make the reasonable assumption that similar proportions of sites may have been or will be sacrificed elsewhere in the areas affected, we arrive at the result of 1028 sites, or 27.9% of the said 3690 sites. This is somewhat higher than the above 24.4%, but it firstly excludes all sites still to be destroyed, and secondly we have already noted that the figure of 3690 may be slightly too high. If we adjust for these factors, we are back to the 20% to 25% we have already arrived at by all other possible means, which thus emerges as the most realistic estimate.

By contrast, the claims made by the DOIR that only 2% of the rock art has been affected are absurd. The townsite of Dampier and the Parker Point facilities alone account for more than 2% of Murujuga's land area of 117 km², and not a single petroglyph has survived there to the best of my knowledge. The former Minister for State Development, John Bowler (recently sacked for corruption), insisted that only 5% of the petroglyphs had been destroyed (and he falsely stated on national television on 25 June 2006 that no more destruction would occur; O'Donnell 2006). Even the Friends of Australian Rock Art, which supports the IFRAO campaign to preserve the Dampier Rock Art Precinct, has quoted the figure of 13% destroyed (FARA 2007), again without any form of substantiation, or explaining its derivation.

The form of denial that should be of the greatest concern, however, is that uttered by senior academics in defence of the companies destroying Dampier rock art (Bednarik 2006d). According to one such public statement, only 'several hundred' petroglyphs had been 'damaged' at Dampier, which would amount to perhaps in the order of 0.1% of the total. This needs to be seen in the context of the dangers posed by the infiltration of Australian universities by hydrocarbon firms (O'Keefe 2007; Hiatt 2007).

It follows from all this that claims of the percentage of petroglyphs so far lost on Murujuga (and they obviously exclude the complete loss of rock art at Mistaken Island and East Intercourse Island, or Yir- dingara) range from 0.1% to about 25%. The highest

figures have been provided by the DIA, they agree broadly with my estimate, and they agree with an alternative analysis of the DIA figures, as well as with a realistic consideration of the potential impact demanded by the land area actually developed. Figure 32 shows the relative densities of petroglyphs on Murujuga before destruction, and if this map is read in conjunction with Figure 33, depicting the relative extent of destruction, it also becomes clear that roughly a quarter of the Murujuga rock art would be expected to have fallen victim to development.

This introduces the topic of petroglyph distribution. In my initial research I sought to develop a predictive model of distribution, but there are very few general rules. Most obviously, the densest concentrations of motifs inevitably occur in reasonably close vicinity of ephemeral rock pools, waterholes or soaks. Some of these number tens of thousands of figures. Rock expanses close to the shore also tend to harbour substantial accumulations. Smaller concentrations, however, can be found on plateaus, boulder piles and along some of the valleys, sometimes in rather unexpected places. The largest sites also tend to have a series of satellite sites around them (Bednarik 2006a). Numerous petroglyph sites co-occur with stone arrangements and many individual motifs are clearly associated with them, whereas in other cases, the two forms of cultural remains occur independently and appear to be un-connected. Other cultural sites are very much harder to detect, and have been omitted in the archaeological reports. These include several types of sites where ceremonial activities have been conducted, which are detectable only by the culturally in-formed eye. Grinding patches occur frequently, and again some cultural knowledge is useful to discriminate, most importantly, between utilitarian and non-utilitarian features. The occurrence of petroglyph sites close to stone quarrying evidence noted by Vinnicombe is probably fortuitous; quarrying evidence seems more consistently in proximity of major occupation sites.

Lorblanchet (1992) suggests that what he regards as the more recent petroglyphs in the few sites he examined are associated with middens. In reality, almost no rock art can be directly related to middens, because co-occurrence in the same general vicinity at a 'favoured' locality in a heavily used cultural landscape cannot be accepted as proof of association (Bednarik 1989). Moreover, the overwhelming majority of petroglyph sites are not close to middens.



Figure 33. Relative density of destroyed petroglyphs on Murujuga recorded in the 1960s: 1 - lowest density, 4 - highest density.

Shell middens on Murujuga are mostly quite shallow, except at some of the westernmost sites; examples that are significantly more substantial occur on West Intercourse Island.

The archaeological surveys

Although archaeologists have worked at Dampier since 1974, the amount of knowledge they have generated for the discipline in the decades since is unsatisfactory, despite enormous sums of money lavished on these efforts. Industry rather than the state has funded most of them. For instance, just one of the several major companies operating at Dampier, Woodside Energy, claims to have spent about \$5 million between 2002 and 2006 on archaeological surveys (Laurie 2006a), and this company alone has been underwriting many more millions of dollars of such work between 1978 and 2002. The effectiveness, relevance and objective value of the extremely lucrative archaeological consultancy industry at Dampier needs to be examined: what is its value to the companies concerned, to the owners of the rock art (the local indigenous communities), to the discipline, and to society at large?

Although the rock art is 'owned' (in the non-indigenous sense) by the Aboriginal people of the area, because of its outstanding cultural and scientific importance it effectively forms part of the common heritage of all humanity. Corporate interests pay for the work of the archaeological consultants, yet in effect it is supposedly conducted to protect

society (humanity, in the case of such a substantial monument) from having its heritage needlessly destroyed. The destruction of rock art is illegal in Australia, and it is the role of the archaeologist consultants to help prevent such damage. That role is enshrined in legislation, and the consultants owe their livelihoods to this very legislation. It prescribes the requirement of major developers to engage the services of archaeologists, ensuring that cultural sites are not destroyed needlessly. However, the legislation of Western Australia reserves for the State Minister for Indigenous Affairs the power, under Section 18 of the *Aboriginal Heritage Act 1972*, to allow the destruction of sites in *exceptional cases*.

In practice, all applications to permit destruction of Dampier rock art under Section 18 seem to have been 'exceptional', because almost all of them have been approved. In the first five years since the Labor government took office in February 2001, 238 Section 18 applications under the 1972 *Aboriginal Heritage Act* have been processed (Hansard 2006). Of these, only one application has been refused, and there has not been a single prosecution under the Act, despite rampant rock art vandalism in Western Australia. The same pertains to earlier periods, and all the way back to the late 1970s. For instance in 1991, the then WA Minister for Aboriginal Affairs, J. Watson, stated that out of the 43 Section 18 applications that had come before her in the previous financial year, she approved all but two (Hansard 1991). Of the 52 Section 18 applications made by Woodside in 1978, 51 were granted (Bednarik 2006a: 37). There has only been one successful prosecution under the *Aboriginal Heritage Act 1972* from 1972 to 1995 (Campbell-Smith 1995) and it is self-evident that this piece of legislation is open to corruption. In a political system that is as notoriously corrupt as that of Western Australia, where several government ministers have been sacked for corruption in late 2006 and early 2007 (some of whom had been instrumental in the destruction of the Dampier monument), chicanery permeates the government. It is against this political background that the destruction of Dampier needs to be viewed to understand it fully.

At Dampier, hundreds of sites in question were inevitably destroyed since the 1972 Act came into force, after the initial period (1962 to mid-1970s) of entirely unrestrained destruction of all cultural material that was considered to be in the way of 'development'. We have seen that in all, an estimated 900 sites of rock art (based on the 25-m clearance rule) have been destroyed of the estimated 3690 sites (DIA figures) on Murujuga (Burrup), the largest of the Dampier islands (Bednarik 2006b). More have been destroyed on other islands.

The number of archaeological consultancies at Murujuga (called Burrup Peninsula, a colonialist name introduced in 1979; Murujuga is in fact an island, not a peninsula) between 1978 and 2006, over a period

of almost three decades, is unknown to me, but is so substantial that a relatively small area of less than 30 km² is in an archaeological sense the most intensively researched place of Australia. Indeed, this represents quite probably one of the densest concentrations of archaeological activities in the world, in terms of land area covered relative to money spent. It would be comparable in intensity to such places as favoured localities in Egypt or Mesopotamia, and probably exceeds the archaeological efforts lavished on an equal land area in the French Dordogne. There is, however, a staggering deficit of tangible outcomes of this archaeological work, which has focused primarily on Murujuga's rock art. For instance, if it were not for my endeavours since 2002 to make this monument known (in order to save it from complete destruction), the public of Australia would know virtually nothing about it. Indeed, when I presented the plight of the monument in various programs on national television in 2006, the most strongly recorded public reaction was, 'why has nobody ever told us about this national treasure'.

As incredible as it may sound, there have been no publicly available books on the rock art of Dampier and the rest of the Pilbara region, and almost no academic papers have been published other than my work. With most of the research work paid for by the proponent companies, archaeologists were discouraged from publishing their findings, Vinnicombe (1987) being the notable exception. There are dozens of unpublished, internal and inaccessible reports (for a partial list see Vinnicombe 2002). These are often owned by the corporate masters who commissioned them, which raises yet another issue of ethics. The local Indigenous communities own this cultural heritage, in the first instance, and in the second it forms part of humanity's collective inheritance. Yet companies own most of the information collected by their archaeologists, and can restrict access to it as they please. This is reminiscent of other corporate appropriation of areas of public concern, e.g. in the fossil fuel industry, the pharmaceutical industry, tobacco industry and the emerging genetic engineering industry. In this case, archaeologists participate in such a pathological system, much in the same way as some anthropologists have been involved in pathological anthropology (e.g. Price 2005; Houtman 2006, 2007). To protect their vested interests, referees have maliciously rejected papers I submitted for publication in the late 1970s about my work at Dampier. In effect, the numerous archaeological projects resulted in an endless parade of internal reports, either owned by companies or hoarded by state agencies and not publicly accessible. It is self-evident that this is the primary reason why both the public and the international research community have only become aware of the Dampier rock art through my efforts, amplified in the last few years by those of several other parties prompted by me.

To the best of my knowledge, no archaeological consultant at Dampier has ever made a successful demand that a proper inventory of the rock art, the stone arrangements or the archaeological sites (occupationsites, middens, stone toolscatters, quarries etc.) of the Dampier Archipelago be made. Until my exposure of the Dampier scandal in 2002, no attempt was made to preserve threatened rock art sites, and no consultant has had the professional integrity and courage of requesting his or her corporate employer to cease the unnecessary destruction of the monument by relocating proposed development to another site. Yet one archaeologist has chided me, a lone activist, for not having developed conservation and management plans, i.e. for not having done the work of the paid CRM industry (Taçon 2007). In fact, after this incredible expenditure (of probably in the order of \$10–30 million) over several decades, the only comprehensive survey of Dampier rock art ever undertaken, in the sense that it covered the entire main island rather than a small part of it, is still my original survey of the 1960s. It was conducted by just one researcher, funded entirely by him. Finally, after decades of advisory work by countless archaeological and other consultants, Australia's largest cultural monument remains without both the management plan and the inventory I have called for since 1969. It is still not a National Park and is still not listed on the World Heritage List, as requested by me years ago (Bednarik 1994a).

These circumstances are extraordinary and need to be examined, when compared to the amount of published work from archaeological efforts of this magnitude in other parts of the world. In the French Dordogne, in Egypt or Mesopotamia, hundreds of tomes have been published about archaeological work conducted there, and thousands of research papers. One of the principal purposes of research work is to present its results to the discipline, preferably in published form. Scholars are generally keen to offer their work in this way, but there has been a notable reluctance in publishing the work conducted at Dampier. In fact, one slim booklet of 70 pages (Vinnicombe 1987) and one article (Lorblanchet 1992) are the only reasonably substantial published reports by others prior to 2002. Vinnicombe's volume was a paid-for promotional tool of Woodside Offshore Petroleum Pty Ltd and of limited circulation, and I published Lorblanchet's paper after its appearance had been delayed for over fifteen years. In scholarly work, it is generally expected that previous research must be properly acknowledged. None of the published or unpublished archaeological reports about Dampier rock art has ever made any mention of the three years of research by me in the 1960s, even though it was well known that I was the first researcher to study this corpus of rock art. Indeed, I re-discovered nearly all rock art on Murujuga, yet it was only in 2002 that the first archaeologist ended 35 years of neglect of my

work, shortly before her death (Vinnicombe 2002). As recently as 2005, an unpublished desktop review of Dampier rock art subtly questions that I even worked at Dampier in the 1960s (McDonald and Veth 2005: 22), but then contradicts itself and calls my distinctly systematic Dampier work 'ad hoc' (2005: 161).

One potential explanation for the lack of published work about Dampier rock art is the low scientific calibre of research. Having seen some of the unpublished reports by archaeological consultants it is amazing that large mining or petrochemical companies would willingly pay millions of dollars for such mediocre work. Most of it would not pass peer review for academic publishing. However, poor grammar and spelling, blatant errors of fact, plagiarism and lack of rigour can all be corrected, and with the vast resources available to these consultants, there appears to be no reason why their reports could not have been rendered publishable.

The explanation is likely to be found in the symbiotic relationship that developed between large resources companies and the archaeological consulting industry, because of the introduction of the 1972 legislation to protect cultural heritage in Western Australia. Archaeologists were able to facilitate the circumvention of protective laws where the presence of cultural sites impeded the development aspirations of the powerful companies, by preparing the data needed for Section 18 exemptions. It was not in their economic interest to convince their corporate masters not to destroy the heritage, and it would have adversely affected the lucrative consultancies to draw public attention to the ongoing destruction. Similarly, open peer review would have endangered the highly profitable consulting industry at Dampier, or indeed in the Pilbara generally.

The generic point is well illustrated by a case in Chile. An archaeological impact study for a mining company that cost \$20 000 found that the El Mauro rock art needed to be protected. A year later, in 2005, a second study facilitated the destruction of the sites, but its prize tag was \$2 million. There was no significant difference between the two studies in terms of scope or coverage, in fact the second study benefited substantially from the previous work done (P. Bustamente Díaz, pers. comm. June 2006) and should have been cheaper. This seems to provide an indication of the price of facilitating rock art destruction: contract prices can be inflated as much as one hundred times.

In the Dampier case, Laurie (2006b) has recently investigated the silence of archaeologists, noting that they declined to be interviewed on Dampier issues. She also found the Western Australian Museum to be less than communicative, which is not surprising. The Museum has been implicated in the destruction at Dampier from the very beginning; in fact it was a blunder by the Museum's experts that caused the dilemma initially. In 1962, an expedition by the



Figure 34. Destruction of petroglyph sites through the removal of boulders, February 2007, Holden Point area, 'Site A'.

Museum claimed to have surveyed Dampier Island and reported that there are only 200 petroglyphs in the Dampier Archipelago (Crawford 1964: 56). It was this bungled survey that led to the decision to place Dampier Port where it is, thereby ushering in the progressive industrialisation of the previously pristine island, which now covers 47% of its surface. Had the Museum's team done its work at Dampier properly, the huge concentration of rock art would have been re-discovered five years before I did so, and the Hamersley Iron harbour and town would have been established elsewhere, perhaps at Cape Preston, Cape Lambert — or, indeed, Depuch Island, the original choice. Since then, the Museum (a recipient of funding from Woodside) has failed to support calls for protection of the Dampier rock art, and has been involved in facilitating its destruction (e.g. by Dampier Salt in the 1970s).

The failure of public archaeology in Western Australia has its parallels in the manner of conducting public anthropology in that state, which has been the subject of several recent studies, among them Ritter (2003) and Chaloner (2004). Moore (1999) makes these pertinent observations:

Some anthropologists have gravitated towards operating within the cognitive and political bounds set by the interests of development and, in so doing, have not always seriously questioned or critiqued their own involvement in these programs ... those with the greatest financial interests in the developments exert power by controlling the terms for discussing the projects (Moore 1999: 232).

Typically the proponent is likely to prefer an anthropologist consultant who has done no previous work with the community in question (Moore 1999: 245).

This is also observed by Brunton (1991), although he argues in favour of the developer's 'right to choose'. Moore continues:

Developers argue that their consultants need to be 'independent', yet how can they be economically

independent when they are in their employ? ... By using independent consultants — those 'from industry' — consultants are maintained who are entirely dependent upon the development industry (Moore 1999: 248).

As Moore observes most perceptively, the concept of 'independence' is in this context only a code word for 'economic dependence'. The exercise of power is embedded in advancing the interests of the resource developers, which is what the consultants do. Not surprisingly, internal differences among consultants occur often in such a power-based system, as for example concerning the Marandoo and Yakabindie mining projects (Moore 1999: 241), during the Rindos affair, or the 'academic fascists' controversy in Western Australian archaeology (Bednarik 2006a: 37–38). The principle of 'anthropology in the service of power' (Escobar 1991: 659), so well illustrated by the many anthropologists in the employ of the CIA (Price 2000, 2005) and other covert agencies, applies equally in archaeology (Trigger 1985, 1989). There are numerous studies or commentaries on intellectually corrupting pathology in anthropology (cf. Houtman 2006, 2007; McNamara 2007). Much less attention has been given to the perversion of archaeology to serve the interests of those engaged in destroying cultural monuments (Fig. 34).

From the perspective of the discipline, the work produced by the myriad consultancies at Dampier and elsewhere in the Pilbara is of limited value. None of it was undertaken to meet broad scholarly, scientific or even archaeological objectives; their brief was always determined either by corporate interests or by the state. Terms of reference were set by the desire to place developments at specific locations, and by the need to destroy heritage sites in the process. Therefore, irrespective of the question of academic quality of the actual research work, the briefs determining these research projects excluded basic requirements of research, such as the need for random sampling of data, comprehensiveness of research design, or the consideration of research questions that were relevant to the discipline, but not to the employer of the researcher.

The overall result at Dampier is well illustrated in Figures 30 and 35, comparing the distribution of rock art sites I recorded between 1967 and 1970 (total 572 sites based on the 50-m clearance rule) and that of the sites recorded by all subsequent efforts (based on 25-m or lower clearance rules). The two maps compare the results of my earlier efforts to secure comprehensive recording with the results of consultancies focusing on specific issues posed by the corporate clients. Predictably, my distribution map, which I estimate accounts for 80% to 90% of the rock art existing in the 1960s, indicates a reasonably even distribution of sites where there is rock, with concentrations located mostly around semipermanent or seasonal water sources. The combined distribution map of the subsequent work (Fig. 35), acquired

Figure 35. The relative density of the petroglyph sites recorded by the post-1970 surveys, based apparently on the 25-m rule in most cases. (The number of sites recorded in the general King Bay and Withnell Bay area is much greater than the marks on this map.)



at the cost of many millions of dollars, shows the occurrence of sites mostly in areas impacted upon by the footprint of industry. Moreover, it necessarily excludes all of the hundreds of sites that had been destroyed by the time these surveys commenced. Clearly, it is an unscientific technique to record only those occurrences of a phenomenon that are going to be destroyed, or otherwise directly impacted upon by industrial development. In that sense alone, all post-1970 research work at Dampier was inferior to my earlier work. The deliberate exclusion of my work from consideration in the subsequent reports is therefore not only a breach of scholarly etiquette, it severely impairs the value and scientific relevance of the archaeological work conducted by the consultants. Studies that are conducted in isolation, based on terms of reference skewed by sectional economic interests, and admitting for consideration only similarly partial surveys can only yield unrepresentative, biased and scientifically distorted results. The discipline is not well served by them, or by any synthesis derived from them, or any form

of public pronouncements based on such narrowly based studies.

Separating archaeology from science

All of these qualifications apply before we consider the scientific merits of the actual methods and results of these consultancy projects at Dampier. In general, the reports they engendered consist of attempts to define the rock art statistically, which is the main thrust of the 'archaeological approach' to rock art study practised in Australia. These subjective constructs are then routinely used as a basis of considerations of style, and in attempts of defining traditions, leading to endeavours of establishing a sequence and relative, if not absolute, dating of the rock art.

This approach has no scientific basis and is of no more than anecdotal value, yet it is the only 'science' so far offered by archaeologists in reference to Dampier rock art. It is based on a psychological process in which the alien researcher, who has no comprehension of the cognitive or perceptive world of the creators of the

rock art, uses his or her own cognition and perception to guess the iconographic meaning of motifs, or their semiotic relationships with other motifs, with other cultural features or with other aspects of sites or of the cultural landscape. The professed purpose of this practice is to create a taxonomy of motif types, which is then used as the basis of various kinds of statistical exercises. Mathematical precision is thus substituted for scientific method, in the expectation that it will somehow conjure up a science of the rock art. The subjective nature of this naive and clearly neo-colonialist method should be self-evident, and yet it continues to be almost universally paraded as an 'archaeological method' of studying rock art.

No amount of statistical juggling can compensate for the complete absence of any reliable ethnographic data in the Dampier reports. Without sound ethnographic input, *taxonomic entities or systems of rock art are autogenous, etic constructs of untutored observers*. They are freestanding formulations generated by auto-suggestion, the psychological process by which a person induces self-acceptance of an opinion or belief. There is nothing in the training of an archaeologist that would suggest an improved proficiency in determining the meaning or taxonomic categories of rock art motifs, relative to that possessed by any other person not attuned to the culture that created the rock art. These constructs may be based on what one observer believes to perceive, or on several observers, even on complete consensus among all alien observers, but they are no substitute for testable (refutable) propositions. Such 'archaeofacts' or 'egofacts' (Consens 2006) are typically inaccessible to refutation; hence they are worthless to science. Moreover, they defy the requirement of replication: subjective processes of perception refer to what occurs in one brain, and cannot necessarily be replicated in another brain. Or, in practical terms, no two archaeologists will use precisely the same neural criteria to determine motif types, so no two determinations of the same corpus will be identical. Scientifically, the tautological interpretations and taxonomies of rock art by archaeologists, at Dampier or elsewhere, only provide a sound basis for the study of the psychology, perception, cognition or reality construct of the individual archaeologist in question; but they can tell us nothing reliable, or scientifically relevant, about the emic meaning or significance of rock art motifs. Their only scientific relevance is what they can tell the cognitive scientist about how one archaeologist processes visual data and forms a taxonomy of petroglyph motifs. 'In fact, the iconocentrist paid no attention to "details" and provided his readers with fictitious narratives that are at best "emically" unacceptable, and at worst "etically" paralysing' (Montelle 2007).

None of the numerous archaeological consultancy reports about Murujuga rock art rises above this unscientific format, and none comprises any

significant component of ethnography. I have conducted extensive research of this kind near Dampier (Karratha Station, Roebourne, Wickham) and at several other Pilbara locations, beginning in the 1960s. Having interviewed numerous male elders, but having been cautioned many times that this was restricted information, I have published only minimal aspects of this information, i.e. only unrestricted detail. However, the absence of indigenous information about the rock art or archaeology in the many consultants' reports is not due to the same factor, but is attributable to the almost complete lack of such research. Indeed, until the final part of the 20th century, local Aborigines were not involved in any capacity in research concerning the rock art or archaeology of the Pilbara (except by Bruce Wright and myself), nor were they ever asked for any permission or opinion (Vinnicombe 2002; Bednarik 2002a, 2006a).

Since most of the pronouncements archaeologists have made about Dampier rock art relate to their etic taxonomies, it is difficult to extract from their reports those data that may have scientific relevance. When data and fictional interpretation are intertwined, their separation is not a straightforward process, because it is not necessarily apparent how much such systematic issues have contaminated data. Since statistics of iconographic identifications are the inherent basis of the consultants' reports, their relevance depends largely on the veracity of these subjective identifications. Any statistics based on them are propped up by these simplistic constructs, and *any models upheld by the statistics of etic motif numbers are archaeological fantasies*.

The problem therefore originates from the discrimination of motif types, and I will illustrate the underlying issue with an example. There are more or less circular motifs in rock art. At Dampier as well as elsewhere in the Pilbara, these have a variety of meanings, many of which are in some degree sacred. Some of them are highly restricted. Although I know only a limited number (four) of correct interpretations for circular forms, I do know that the uninitiated researcher could not possibly understand these or other categories. S/he is likely to invent and count such categories as 'full outline circles, infilled circles, circles associated with other features, ovals, infilled ovals' and so forth. For instance in her survey of King Bay, Vinnicombe distinguished and counted twelve types of circles and fifteen types of ovals. To informed observers, her taxonomy is absurd. Even at the most basic, purely geometric level, this scientific and Western approach is doomed: there are hardly any perfect circles, so an observer creating a subjective taxonomy must somehow discriminate between a circle and an 'oval'. This is obviously an arbitrary process, especially when we remember that much of the fieldwork is conducted by students and inexperienced observers who merely tick off boxes on

a recording form. This process cannot discriminate between motifs to which other components were added at a much later time (e.g. the internal barring or the radial lines associated with a circle), which provides very significant information to the careful observer. It cannot discriminate between circle/oval motifs made by a man or a woman, and yet quite clearly some of these motifs are identifiable as such, and therefore have very significant differences in meaning. Nor can this process recognise intentional juxtaposition or any other context-related basis of meaning, which are of greater significance than most of the variables archaeologists typically register. At this point it becomes apparent that such an 'archaeological' taxonomy is based on the biological concept of species, but by transferring it to semiotic entities of unknown meanings that lack genetic variables, we impose an arbitrary order that has no validity in the real world. It would be akin to the taxonomy of an interstellar traveller who comes upon urban traffic signs and, assuming the lines and squiggles are not random decorative patterns, but represent religious symbols of some kind, proceeds to deconstruct them, finding lines, ovals, serifs and so forth. If he then subjected these information bits to statistical analysis, he would no doubt arrive at some kind of propositions about the nature of the religion of road users, but we can assume that at the simplistic level this structural analysis was conducted, it would yield nothing of relevance.

This principle (which I have long referred to as the 'CCD [crucial common denominator] of phenomenon categories' issue) applies right through the enormous range of motifs we find at Dampier, from the simplest to the most complex. (*The CCDs of all valid rock art motif taxonomies are emic.*) For instance, one of the most famous complex panels (vandalised by latex casting) is in a small canyon draining into Whitnell Bay (named after a murderer, John Whitnell; Bednarik 2006a: 17), called 'Climbing Men Panel' by archaeologists. It comprises many motifs executed at different times, but the name derives from several groups that are interpreted as humans climbing trees (or whatever), including one on an adjacent rock. One archaeologist even provides a shamanistic explanation for these petroglyphs, as people climbing Jacob's Ladder. This entirely unfounded notion (the author had not even been to the site, let alone made any attempt to interview anyone) has been published in an 'academic' journal by the University of Sydney. (It also helps illustrating the follies of the 'shamanistic interpretations' of rock art.) In early 1970 I obtained a precise explanation for some of the elements of this panel, including the 'climbing men'. They are indeed male, although that is not graphically evident, but that is the only valid part of the popular interpretation. They are certainly not climbing, not on Jacob's Ladder or anything else, they are on the ground — on flat ground in fact. The

ritual activity they are engaged in could not possibly take place on a tree. The compositions are of highly restricted nature, yet the panel has been seen by tens of thousands of women and children. It has been so much defiled that today the custodians have to be content with preserving just the secrecy of meaning. I concur with them: even the finest scholars possess only a partial construct of reality, and knowledge of sacred content will not improve that state. Most archaeologists are instruments of the state and serve a neo-colonialist agenda (see e.g. Trigger 1989). Some authors regard them as 'molesters of the past' (Campbell 2006), which may be rather harsh, but it is true that the archaeologist is regularly the destroyer of archaeological resources (Frankel 1993), ranging from site sediments to rock art.

Having benefited incredibly from the knowledge of my indigenous teachers during the early phase of my research at Dampier (see Bednarik 2006a), I formed research priorities that differ very significantly from those of all archaeologists who subsequently worked there. Most importantly of all, I realised that my ethnographic understanding of the iconography was inadequate to attempt an emic taxonomy. I could only provide fragments of such a model, and to do so I would be obliged to disclose information that was given to me in confidence, and with the warning that making it publicly available would have dire consequences (some of which were described to me). I realised then that the creation of subjective rock art taxonomies was not only scientifically futile, it amounts to a devaluation of intrinsic significances, a debasing of cultural beliefs, and quite probably even constitutes a step in the destruction of these through academic appropriation. Having explained this many times, I have used the same sceptical approach in all of my rock art studies since the late 1960s. In other words, I accepted that valid taxonomies are not attainable without very extensive emic knowledge and understanding, which is far beyond the faculties of archaeology as it is practised. Therefore, one could count motifs in crudely descriptive terms, but not explain them, except in some subjective fashion. It is easy to walk around large petroglyph sites in a systematic pattern (by 'transects'), counting the 'noughts and crosses' one perceives on the rocks, thus creating an inventory that reflects the perception of the 'analyst'. Nor is it difficult taking photographs of petroglyphs and projecting them onto paper to trace them, thereby filtering out practically all the scientifically important information about the motifs to obtain just one set of *doubtful* variables (Bednarik 2007b: 55–84). The scientific study of rock art is not well served by this approach, which offers only distortions and abstractions. I have undertaken a great deal of detailed grid recording in the Pilbara (in many cases the motifs I recorded no longer exist) but eventually realised that this, too, has severe limitations. Other scientists have wrestled with the same problems, and

my solution to them has generally been that there are alternative, scientific ways of studying rock art, and that while they may not be as glamorous or readily accessible to neophytes, they do offer falsifiability, and thus a sound basis of testable knowledge.

My endeavours at Dampier in the four decades from November 1967 to November 2007, and elsewhere, have been heavily influenced by this epistemological stance. I therefore present no motif taxonomy and limit the use of abstracted recordings to making specific rather than generic points. Most importantly, I present no interpretations of motifs, for the reasons set out in Bednarik (2007b: Ch. 8), and despite the fact that I do know many of the correct interpretations. Most are not available for public consumption.

Preliminary conclusions

Having thus declared my position on what constitutes a science of rock art (for more details, see Bednarik 2007b), I have here presented some of the key foundations of a science of Dampier rock art. They include credible estimates of how much rock art there is on Murujuga, how much has been destroyed so far, and they focus on the lithology, the weathering of the Dampier rocks, and the forms of rock patination found on them. This has led to a detailed consideration of the factors effecting the deterioration of this rock art, through the introduction of major acidic emissions since the 1980s. I have attempted to clarify the inconsistencies and contradictions in the literature on the classification and technology of Murujuga petroglyphs, and I have explained why the archaeological work to date provides almost no scientific data. I have also considered some preliminary aspects of the antiquity of Dampier petroglyphs, showing that we have currently no published evidence that could withstand scientific scrutiny. In the second part of this paper I will develop from these strands a strategy of gaining a scientific understanding of this rock art corpus. Other aspects of this cultural heritage precinct, especially the much-neglected stone arrangements, will also be considered in it.

I apologise for two things in this paper: for waffling at such length about matters of scientific background, and for my uninvited criticisms of archaeological incursions into rock art research. Concerning the second matter, I crave the indulgence of open-minded archaeologists, because I believe an archaeology that eschews fair criticism of its practices or performance has no academic legitimacy.

I invite responses from any archaeologists who feel that my criticisms of the work at Dampier are unwarranted, and as editor of this journal I will publish them if they are properly presented. What could be fairer than to replace the lack of transparency we have so far had at Dampier with proper public discussion?

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Robert G. Bednarik
P.O. Box 216
Caulfield South, VIC 3162
Australia
E-mail: auraweb@hotmail.com

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