

were amateurs and some were not very scientific, but others thought that they were doing science appropriate to the time and felt that science should trump any other considerations. Such attitudes continued into the 1930s, but ethical scholars like Donald Thomson were unwilling to compromise their principles. The social context of science has impact on the science practised. To ignore that, particularly in relation to studies of colonised people, constitutes precisely that 'neo-colonialism' of which the authors were accused.

3. Even post-modernists admit that their approach to scholarship can be done well or badly, just as archaeology and other science can. Post-modernist scholarship can produce writing that is incomprehensible and ideas that undermine our work. But it can be counterproductive to dismiss everything that has been done.

For example, the people of the Admiralty Islands were not passive in the 'trade' of items with Europeans, but active participants, supplying goods to suit the demand from the foreigners. This opens up the whole question of how museum collections were created and how they have been used, simply by accepting that there can be more than one view of the way the world works.

The lesson here is that we make more progress when we understand more of the agency of people rather than characterising them all in one category (post-modernists; scientists; colonised peoples).

4. Genetics and linguistics can offer more and better answers to questions that once were the domain of archaeologists alone. It is not all straightforward, as shown by the conflict between the 'express train' and 'entangled bank' different models of Austronesian colonisation of the Pacific. The way ahead is not to dismiss these studies or get on our high horses of archaeological invincibility but to incorporate them as part of an overall synthesis.

Archaeologists should not be cowed that some studies seem to suggest results that they would not have predicted, nor accept them simply because there are lots of data. Often the new techniques are being applied to questions that were originally argued from excavated data alone. Few of the non-archaeologists described in *The Bone Readers* would have the skills to excavate a site stratigraphically on their own or as part of a team, nor conceptualise the problem until archaeologists bring it to them. Understanding of the past is a collaborative process where each contributor respects the other, and it is an iterative process in that interpretations may need to be revised as new data are tested and found to be relevant. Neither side can do it alone. I know some people understand and agree with that. I am not so sure that the tone of the book makes it easy to think that its authors do.

**THE ELUSIVE MEENAMATTA
PETROGLYPHS, TASMANIA: COMMENT
ON JO FIELD'S AND PETER D.
MCINTOSH'S 'A RE-EVALUATION
OF 'PETROGLYPHS' ON BLUE TIER,
NORTHEAST TASMANIA'**

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We would like to thank Field and McIntosh (2009) for their interest in our work at Meenamatta (Blue Tier). Science thrives through the testing of propositions, but such testing needs to be done of the data presented or the claims made, not on the basis of misunderstandings. For instance, nowhere in our paper did we suggest that the numerous aligned large holes at the site BT1 (their Figure 2F) are cupules. On the contrary, we specifically stated that we had no opportunity of examining these holes properly to form an opinion about their nature (Bednarik *et al.* 2007:163). And yet Field and McIntosh (2009:16) assert that we 'determined a minimum age for the BT1 petroglyphs of about 1000 years (Bednarik *et al.* 2007:167)'. No rock art specialist would define these large holes, which resemble the traces of core drills, as petroglyphs – and if we did not examine them, how could we have determined their antiquity?

Similarly, Field and McIntosh (2009:16) assert that we concluded at our site BT1 'the linear arrangement of cupules appears to be a distinctive feature of Tasmanian rock art'. Again, we did not define these vertical-sided holes as cupules; cupules are of hemispherical shape and were made with hammerstones. Secondly, the quoted generic statement, at the end of our conclusions, made no reference whatsoever to BT1.

The authors tried unsuccessfully to locate site BT2, which is unfortunate as it would have assisted them in distinguishing between cupules, rock drill marks and natural features. That site features several alignments of typical cupules on a sloping panel (Figure 1) as well as a few 'convergent lines motifs'. In one of these, quartz grains occur bearing several 'clear conchoidal fractures, which indicate that the grooves were made by percussion at a time when the feldspar surface had been about 2mm higher than at present' (Bednarik *et al.* 2007:164) (a retreat implying an age of at least a millennium). Field and McIntosh (2009:12, 16) chide us twice for not explaining how to distinguish anthropogenic and natural grooves, yet here is the answer: percussion petroglyphs can be recognised by traces of impact, such as cracked, fractured or bruised mineral grains or crystals.

Field and McIntosh (2009:13, 16-18) appear to be in full agreement with our finding that nearly all the linear grooves at the four sites we mentioned were caused by tree roots, but even here, their views are in need of correction. They believe that 'acidic dissolution of granite associated with the presence of tree roots' (2009:19) caused these grooves, presumably referring to a mycorrhizal process. We disagree and repeat that the process is kinetic, caused by minute movements in the near-surface roots of trees hugging the rock for support and swaying in the wind, in which fine sediment acts as abrasive.

The only petroglyphs the authors have examined are the circular depressions at BT3 (Figure 2), for which they offer no credible explanation. They correctly recognise that water ponding is not a viable explanation, because these pits occur on sloping and even vertical surfaces as well as on horizontal. They offer only the vague guess that these hemispherical pits formed



Figure 1 Some of the rows of cupules at site BT2, Meenamatta (Blue Tier), which Field and McIntosh have not seen (Photograph: R.G. Bednarik).

by 'slow dissolution of the softer minerals of the granite' (Field and McIntosh 2009:19). This explanation is offered without any examination of the lithology or microscopic condition of the surface, and it fails to illuminate why this process should occur only in one locality, why some of the pits form alignments, and why some of them contain quartz grains with percussion traces.

Field and McIntosh's (2009:18-19) critical discussion of the microerosion age estimation attempt at BT2 (the site they failed to find) illustrates a profound lack of understanding of both theory and method. Micro-wane development is not by abrasion, as they think; the angle α is always obtained from $\sim 90^\circ$ edges; 'the broadness of the fractured quartz' (Field and McIntosh 2009:18) (meaning?) is not measured; age is not indicated by abrasion; angle β is not used in age estimation (it is crucial in establishing why dimensions x , h , y , z , r and consequently A must all be proportionally equivalent); equation 4 is not used in dating, it merely demonstrates linearity of the process; β cannot possibly be a constant proportion of α ; the comment that we 'dispensed with trying to predict β ' (Field and McIntosh 2009:18) shows that the authors do not understand the theory (wane width is always measured); Černohouz and Solč's (1966) theory would only apply if erosion were limited to the edge, which it is not; apical retreat h by itself (which cannot be effectively measured under the microscope) is irrelevant to age, retreat $h+x$ would need to be measured (which is impossible) and Grosio is not



Figure 2 Some of the cupules at site BT3, which occasionally occur in alignment and evenly spaced, and are found on variously orientated surfaces (Photograph: R.G. Bednarik).

a valley; the summary of the Grosio project provided by the Field and McIntosh is confused.

What could have inspired Field and McIntosh to attempt a falsification of the Meenamatta petroglyphs' identification? They note that, as a consequence of our 'study, there has been an initiative to set aside an area of Blue Tier as a 'sacred site', and to redirect a public walking track' (Field and McIntosh 2009:12). Field, who designed that walking track for her employer, Forestry Tasmania, objects to such a political act, and has in the past denied the local Indigenous Noiheener group's connection to this area. However, attempting to explain away the cupules as some kind of mysterious solution phenomena is also a political act. This denial may suit Field and McIntosh's employers, the logging industry's lobby, which is keenly eyeing the remaining forests of the region. Our work needs to be separated from the political action it apparently prompted.

References

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REPLY

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We were pleased to see that in their comment on our paper (Field and McIntosh 2009) Bednarik *et al.* (2010) have withdrawn their previous assertion (Bednarik *et al.* 2007) that the regularly spaced circular depressions at their site BT1 are Aboriginal petroglyphs. For the record, Bednarik *et al.* (2007:167) previously determined 'a minimum age of the BT1 petroglyphs of about 1000 years' and concluded (Bednarik *et al.* 2007:163) that their investigations 'render the definition of the larger series [of cupules] at BT1 as mining marks specious.' Their comment (Bednarik *et al.* 2010) 'if we did not examine them, how could we have determined their

antiquity?' is valid – we had the same thoughts. Their denial that they had identified cupules at BT1 is also countered by their statement (Bednarik *et al.* 2007:167) that at their three petroglyph sites (BT1, BT2 and BT3) they found only two motif types: cupules and grooves; our response is that the circular BT1 features are certainly not grooves.

That percussion produces conchoidal fractures in minerals is not disputed. What is lacking is photographic evidence to support the contention of Bednarik *et al.* (2007:164) that they have found conchoidal fractures in grooves, and evidence to show that such conchoidal fractures are absent from grooves attributed to root abrasion. Without this evidence it is impossible to attribute grooves to human intervention. By selectively quoting from the conclusion of our paper Bednarik *et al.* (2010) imply that we did not consider root abrasion to be occurring on Blue Tier. On the contrary, abrasion by roots was considered likely, both by us in our discussion concerning Short Linear Grooves (Field and McIntosh 2009:16–18) and also by members of the 1957 Blue Tier Expedition (Field and McIntosh 2009:11). The processes of physical and chemical weathering (i.e. root abrasion and mineral dissolution) clearly proceed simultaneously.

The discussion concerning microwane retreat dating by Bednarik *et al.* (2010) is as unsatisfactory as the earlier statement concerning this method by Bednarik *et al.* (1992:283) that 'A, r, z α and β are all related in a complex fashion which I shall not attempt to explain here'. Two examples of unsound reasoning and conflicting statements will suffice to show the confusion: (1) Bednarik (1992:282) states that the angle β expresses 'the rate of wane development relative to surface retreat' but Bednarik *et al.* (2010:87) state that 'angle β is not used in age estimation'; (2) Bednarik *et al.* (2010:87) state ' β cannot possibly be a constant property of α ' but Bednarik (1992:Table 1) writes ' β is a constant at 60% of α '. While enthusiastically criticising our understanding of their dating methods, Bednarik *et al.* (2010) overlook our main concerns, viz.: (1) that no satisfactory calibration curve for microwane dating is extant; (2) that the microwane dating method has not been validated by independent techniques; and (3) that the theory behind the dating method is unsound. These are grave deficiencies that cannot be ignored.

The alignment of the small depressions on the photographed rock (BT3) of Bednarik *et al.* (2010:Figure 2) was discussed in our paper (Field and McIntosh 2009:19). We examined this rock and found that the small depressions occurred all over the visible rock surface and that the impression of an isolated line of small depressions had been emphasised by the light conditions and angle when the photograph was taken. Such small depressions are always associated with lichens and are common on the fine-grained granite boulders of Blue Tier.

The proximity of the partly soil-covered flat rock with regularly spaced small depressions (site BT2 of Bednarik *et al.* 2007) to the Summit Mine and to the similar prolific Type 6 markings made by miners (Field and McIntosh 2009) indicates that the BT2 markings have a mining origin. In relation to the alternative hypothesis put forward by Bednarik *et al.* (2007) it must be pointed out that the whole area was previously covered by dense rainforest (Figure 1); before the fires, forest clearance and erosion that occurred in the mining period, flat rocks like the one at site BT2 would have been obscured by dense forest understorey and covered with soil, and more prominent boulders would have been covered with mosses and shrubs, as they are in moister gullies and shady locations today.

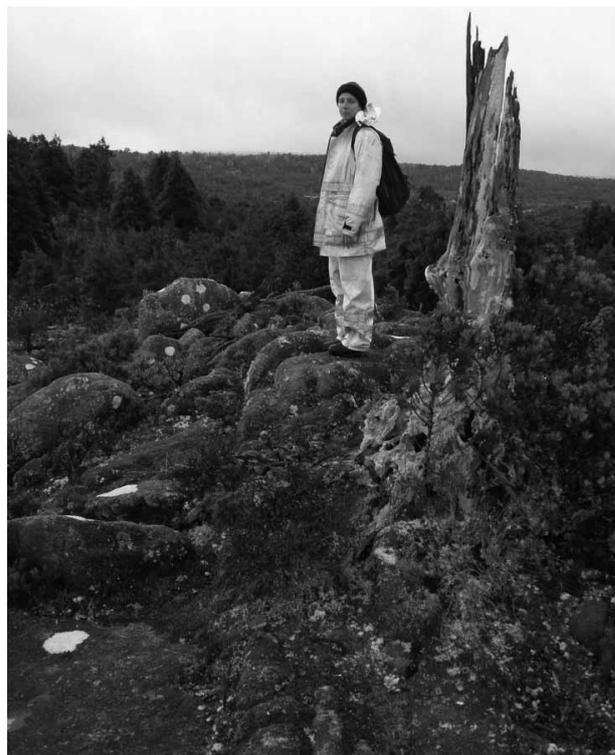


Figure 1 Evidence for previous rainforest forest cover on Blue Tier, in the form of stumps and fallen trees, is widespread (Photograph: Peter McIntosh).

As documented in our paper, solutional depressions in granite (gnammas) (Field and McIntosh 2009:Figure 2E) are widespread and their formation is well-researched. To apply the dismissive explanation 'mysterious solutional phenomena' (Bednarik *et al.* 2010) to these features shows a lack of understanding of geochemical processes as well as lack of respect for the careful work of the distinguished Spanish geochemists quoted.

For the information of readers, and contrary to the assertion of Bednarik *et al.* (2010), walking tracks on Blue Tier were not 'designed' by Field, but were constructed last century by miners and upgraded by a local community group in the 1980s. The gratuitous comments regarding 'falsification' of conclusions and political motives do not deserve further comment.

In summary, we find that Bednarik *et al.* (2010) have not produced new evidence that would justify changing our conclusions that 'there is no evidence that any marks on Blue Tier are ancient Aboriginal petroglyphs' (Field and McIntosh 2009:19) and that the area is important to preserve for its industrial archaeological heritage as well as for its natural features. The 'elusive Meenamatta petroglyphs' (Bednarik *et al.* 2010) on Blue Tier are aptly described.

References

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