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Government of **Western Australia**
Department of **Water and Environmental Regulation**

Murujuga Rock Art Monitoring Program: Independent peer reviews of Monitoring Studies Report 2024 and supporting documents

Department of Water and Environmental Regulation

May 2025

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Acknowledgements

The Department of Water and Environmental Regulation recognises the Traditional Owners and Custodians of Murujuga; the past, present and future generations of Ngarda-Ngarli, and their ongoing connection to this sacred country. All aspects of the program will be conducted with respect for, and be guided by, the cultural law, knowledge and practices of the Circle of Elders, Traditional Owners and Custodians of Murujuga.

The Murujuga Aboriginal Corporation and the Department of Water and Environmental Regulation jointly oversee the implementation of the Murujuga Rock Art Strategy.

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1 Background

The Murujuga Rock Art Monitoring Program (the program, MRAMP) will monitor, evaluate and report on changes and trends in the integrity or condition of the rock art and determine whether the rock art is being subject to accelerated change; specifically, whether anthropogenic emissions are accelerating the natural weathering, alteration or degradation of the rock art. The program will underpin the Environmental Quality Management Framework (EQMF) for managing environmental quality to protect the rock art on Murujuga. This will enable timely and appropriate management responses by the Western Australian Government and stakeholders to emerging issues and risks.

The program is a joint initiative of Murujuga Aboriginal Corporation (MAC), and the department, delivered by WSP¹ and Curtin University, with assistance from consultants at ArtCare, University of Wollongong, University of Melbourne, and L & K Engineering.

Reports and documents produced as part of the monitoring program are subject to independent peer review by experts of national and international reputation in relevant fields. Independent peer reviewers are engaged by the department, in consultation with MAC. Peer review is an iterative process, and it is not uncommon for reviews to take several months, especially where the subject matter is complex and requires input from multiple experts.

The department and MAC are jointly responsible for overseeing the program, with MAC providing cultural and heritage expertise.

This document collates all the peer reviews completed between February 2024 and March 2025 for the following documents:

- Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2024 (the 2024 Technical Report) (Section 2)
- Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Report 2025 (Section 3)
- Murujuga Rock Art Monitoring Program: Monitoring Studies Data Collection and Analysis Plan Amendments 2024 (Section 4)
- Methodology Statements for component studies, as referenced in Section 2.1 of the Monitoring Studies Report 2024 (Section 5).

All comments are those of the independent peer reviewer and may not reflect the views of the department or MAC. The department has only edited the content of reviews where required to correct minor errors or clarify issues arising and therefore references to page numbers and section numbers may have changed in the published documents. The methodology statements (Section 4) have not been published, however will form the standard operating procedures where the methods form part of the ongoing monitoring program.

¹ Calibre was acquired by WSP in June 2023

2 Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2024

2.1 Dr António Batarda Fernandes (2024)

Document title:	Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2024
Current document revision:	COPP21065-REP-G-105 (Rev 0)
Reviewer:	Dr António Batarda Fernandes
Date review commenced:	June 2024

Item no.	Section	Peer reviewer comment 23 June 2024	MRAMP team response 27 August 2024	Peer reviewer close-out comment 09 September 2024	MRAMP team close-out response 20 December 2024
1	General	As a general opening comment, I would like to commend the team on all work done, and, most of all, a flexible stance that allows, when justifiable, gathered data to further inform, and alter if needed, measurements and experimental methodology so adapting to previously unobserved issues, in what is a pioneering research program. Two questions need careful attention going forward as noted below: pH measurements and the rapport between past, present (and future) emission levels. Some typos, orthographic mistakes, and phrasing	Thank you. Noted. Specific comments addressed below.	-	-

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		issues – I have just noted more blatant ones – should be dealt it in careful revision of the text.			
2	Summary of key findings to date Porosity	The first paragraph is written in such a way that it implies that it first states that “Spatial gradients of porosity amongst the sample rocks from Murujuga have been determined. Whilst the variation in porosity is only on the order of 3-5%, it appears that increased rock porosity is significantly associated with the industrial area and NO2 gradients but not associated with other environmental variables at this stage” and only afterwards that “For technical and other reasons this effect has only been investigated in granophyre to date”. I suggest rewriting this passage so that it is made clearer that this (major) find is only valid, for now, regarding granophyre.	The text has been amended to clarify that only granophyre results are available to-date.	I couldn't locate the section “Summary of key findings to date” that in the previous version of the Report appeared just after the “Executive Summary”. Also, a search on some sections of the original text I cited in my comment turned 0 results. Can you please clarify?	Edited in response to comments from other reviewers.
3	Summary of key findings to date Air quality	I understand that this is the summary section, but I suggest considering including relevant bibliographical references when mentioning “A growing body of evidence internationally and at Murujuga”, or, at least, noting the section where those references are stated.	An example reference has been included here; more can be found in the main body of the document.	See previous comment.	See response item 2

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4	Summary of key findings to date Air quality	The last paragraph mentions “that pollutants may be channelled or concentrated in geographic features such as gorges” which is an interesting find (namely because engravings concentrate there), to be further confirmed. However, as you note “geographic features” (plural) one gets the impression that there are more features where this has been observed. Is that the case or is it just gorges?	Any abrupt changes in terrain elevation may influence air transport and/or air quality. For example, quarries, cliffs, saddles between hills, etc. Gorges were highlighted as these tend to provide shade and to contain water sources – which in turn may explain the high density of rock art within such features.	Ok, but do see previous comments.	See response item 2
Introduction					
5	Section 1.4.3 Glossary	“Culturally Important Place”; “Culturally Important Site” These two terms refer to basically the same things, as I understand them and I’m afraid the expression “Such sites may or may not lie within a Culturally Important Place” does not make the issue clearer. Moreover, are Places determined by “Traditional Owners” and Sites by others? Whom, in that case?	These two definitions have been combined and amended in consultation with MAC.	Ok	✓
6	Section 1.4.3 Glossary	Environmental Quality Criteria (EQC) “Both types of EQC may be either “Interim” (preliminary).” Is there something missing from this sentence? Can be either interim or...??	The sentence has been amended to read: “Both types of EQC may be “Interim” (preliminary), however it is expected that EQC will be finalised in 2025/2026 (subject to periodic review)”	Ok	✓

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7	Section 1.4.3 Glossary	<p>“There are many examples where a fully developed patina/varnish layer has regrown in engraved grooves, these examples are generally considered among the oldest petroglyphs for this reason.”</p> <p>I understand the logic of this conclusion; is it possible to measure patina thickness, also for rock art dating (or at least relative dating, i.e. establish if motifs are older than others)?</p> <p>Also, I believe this sentence, because it conveys an important suggestion (age of petroglyphs) should be rewritten; note the spelling mistake (were) and perhaps not using a comma but splitting in two different sentences.</p>	<p>This section has been revised for clarity.</p> <p>There is a separate (ARC Linkage) research project led by Prof Jo McDonald from UWA which aims to date petroglyphs.</p> <p>It may be possible to measure patina/varnish thickness in engraved regrowth through XRF or similar however this would require extensive development and validation.</p>	Ok	✓
8	Section 1.4.3 Glossary	<p>Weathering</p> <p>This definition should be revised as it is confusing and has some phrasing/orthographic issues.</p>	Definition revised	Ok	✓
Study methodologies, verification of competency, and quality assurance					
9	Section 2.3.3.2 Quality assurance of iime-of-flight secondary ion mass	“iime-of-flight”	Corrected	Ok	✓

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	spectrometry data				
Laboratory analysis of field samples					
10	Section 4.2.9.3 Impact of weathering on patina preservation	<p>“Therefore, patina formed on these highly weathered surfaces has a greater preservation potential than patina formed on relatively smooth surfaces that may be less strongly weathered, although grain morphologies and textural relationships also control the development of topography on a rock surface.”</p> <p>This is quite interesting; can thus be suggested that more weathered surfaces ‘benefit’ more from patina protection?</p>	<p>This is an interesting possibility, and we have commented on it with the new text “The converse relationship may also apply; if patina protects the weathered rind from further mineral dissolution, then weathered rind below thick patina may have a greater preservation potential than weathered rind below thin or absent patina. However, further work is necessary to explore this possibility.”</p>	Ok, let’s wait for further work on this.	Ok
11	Section 4.2.10.4 Approach	<p>“Other phases were identified by XRD, but many of these form part of the weathered rind (e.g. chlorite, clinozoisite, and stilpnomelane) and were neglected.”</p> <p>Why? Should such an option be further discussed?</p>	<p>This is now clarified in the text as follows: “Some of these minerals do not form under surface conditions and are observed within the fresher rock or weathered rind (e.g. chlorite, clinozoisite, and stilpnomelane). They do not form part of the patina, so they were not included in model patina.”</p>	Ok	✓
12	Section 4.2.10.4 Approach	<p>“These results are shown in red.”</p> <p>Why is this mention not included for simulation 1?</p>	<p>The colour of the lines is now mentioned in the simulation descriptions.</p>	Ok	✓
13	Section 4.2.10.6 Discussion	<p>Regarding what is correctly questioned in this point, to what</p>	<p>We have now clarified in the text that detailed comparisons between</p>	Ok	✓

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	Caveats Approach to equilibrium	degree you have confidence detailed observations will strengthen conclusions regarding this issue?	observation and calculations will establish the proximity of these systems to thermodynamic equilibrium. Modified text reads “Detailed observations of those systems are required to establish the extent to which thermodynamic equilibrium is approached or achieved for equilibria deemed significant. Such observations will strengthen the conclusions presented here. Significant equilibria include pH-buffering clay mineral equilibria with aqueous solutions, Fe- and Mn-oxide dissolution in the patina, and feldspar dissolution within the weathered rind and fresher rock.”		
14	Section 4.2.10.6 Discussion Caveats Approach to equilibrium: measured versus predicted pH	“(ref, App whatever).” ?	This has been deleted and the correct references have been added.	Ok	✓
15	Section 4.2.10.6 Discussion Caveats Approach to equilibrium: measured	So, as general comment on pH, after somewhat lengthy considerations on the reliability of pH as a measuring means throughout the different documents already produced by MRAMP, pH as a measuring variable will continue to be used in the Program?	This comment refers primarily to rock surface pH. However, it is worth noting that the project also measures deposition sampler pH and rainfall pH. Both of these measures do not suffer from the same issues as rock surface pH as the samples have	Ok, let’s see how rock surface pH, which previous studies noted as a relevant variable, can be reliably used for the purposes of	Ok

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	versus predicted pH		sufficient time to reach a true equilibrium. Regarding rock surface pH, despite the reservations mentioned, the method does appear useful to some extent. The clear signal from the bushfire/wildfire exposed rocks for example. However, a final decision on retention of rock surface pH (or otherwise) will be a focus of future work.	this research program.	
16	Section 4.2.10.6 Discussion Mineral assemblages	“It is possible that these kinetic limitations prevent precipitation of these, and other, minerals, at Murujuga.” Is this a suggestion or will it be possible to substantiate such a claim?	This is highly likely given that surface environments are almost all in disequilibrium states. However, it is not within the scope of work to substantiate this claim. Text clarified “It is highly likely that kinetic limitations on nucleation prevent precipitation of these, and other, minerals, at Murujuga.”	Ok	✓
17	Section 4.2.10.6 Discussion Key findings	Key finding 5 How will this affect reaching MRAMP targets?	These findings are not sufficient, alone, to meet the MRAMP targets. However, they can be combined with the results of other programs, such as the chamber studies, to meet targets. This has been clarified in the text. “Given these uncertainties, it is necessary to combine the findings of the inorganic geochemistry program with those of the chamber studies to meet the MRAMP targets.”	Ok	✓

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18	Section 4.4 Chamber exposure (accelerated degradation) testing Section 4.4.1 Objectives	“It remains possible that the current level of anthropogenic emissions at Murujuga may not have a measurable impact on the rock art – or an effect below the level of measurement accuracy and precision inherent in field work.” Since this is major uncertainty regarding the paramount goal of MRAMP, what do you propose to counter this, if indeed true?	Determining the minimum anthropogenic pollution/exposure levels required to impact the rock(art) is a major focus of ongoing work. It is expected that that this will largely be undertaken through chamber studies.	Ok	✓
19	Section 4.4.2 Methodology	“Methodology has been significantly revised in response to (1) the geochemistry findings (specifically porosity)”	Thank you.	-	-
20	Section 4.4.2 Methodology	“Therefore rainfall (of sufficient magnitude) may be considered conceptually to flush or neutralise reactive air pollution deposition during the dry periods” And of insufficient magnitude? (also considering thaw, if applicable) Will it aid in air pollution deposition?	Temperatures in the region do not reach freezing levels so freeze/thaw situations are not a consideration. Lower magnitude or intensity rainfall events may tend to concentrate air pollutants on the rocks however this remains under investigation via the 3-bucket deposition sampler.	Ok	✓
Air quality monitoring network establishment and operation					
21	Section 5 Solar/Passive Sites	“It is expected that a public website for display of these data will be made available in year 3”	Noted and agreed	Ok	✓

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		This is commendable but it must be well explained to the public in laymen terms.			
Statistical analysis of component studies					
22	Section 7.1.5.1 Changes in elemental concentration	“Sufficient HFO was only available to conduct a single test with one hour exposures on days one and 2 of the 6-day experiment.” Why?	We have experienced significant difficulties obtaining representative Heavy Fuel Oil samples as only a limited number of authorised inspectors have permission to board ships and collect samples. Also this fuel is not readily available as it is illegal to use in land-based diesel engines. We eventually secured a source from petrochemical refinery and will be obtaining a larger sample from the same source to conduct further experiments in year 3.	Ok. Perhaps it is worthy to mention this in the text.	Comment added to text
23	Section 7.1.5.2 Changes in pH	“It may be that the time to equilibrium is longer than the time available for measurement in the field. While the dominant process may be an acidic type reaction, the system may react to neutralise this acid, making pH a challenging metric.” Again, issues with pH reliability as a metric...	Agreed regarding rock surface pH in general. However the post wildfire rock surface pH curve and subsequent measurements on the same rocks show a convincing response. So the method appears to respond to sufficiently strong signals. pH measurement from rainfall and from the deposition samplers can be considered more reliable as it excludes any processes on the rock surface.	Ok	✓
24	Section 7.1.5.4 Confirmation of	“This work thereby confirms the dissolution hypothesis can occur, at	This is laboratory confirmation of the mechanism which likely created	Ok. However, I’m somewhat intrigued	Perhaps the comment was

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	physical weathering	<p>least at the exposure levels undertaken in the chamber, albeit at a relatively short duration of 6 days.” How confident are you that this can be confirmed in the field?</p>	<p>increased porosity. It would likely be challenging to measure the same dissolution by-products in the field. Mainly due to problems around representative sample collection. It is instead proposed that we measure permeability as a surrogate of porosity, to ensure that porosity is not continuing to be increased through anthropogenic activity.</p>	<p>by the last sentence: can permeability also increase “through anthropogenic activity”? Can you also please further elaborate on why permeability should be used to ‘avoid’(?) increase “through anthropogenic activity” vs. increase in porosity “through anthropogenic activity”? See also comment 21 by John Henstridge.</p>	<p>unclear. Porosity can only be measured (accurately) by invasive techniques such as cutting and imaging the rock. If we can demonstrate that the pores are sufficiently connected, then permeability can be used as a non-destructive replacement for porosity (e.g. imagine an open cell foam / sponge-like material vs closed-cell material like bubble-wrap). Given the findings, it is important to ensure that current emissions levels are not causing any further increase in porosity, which can most easily (and non-invasively) be measured in the field via permeability measurement.</p>

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25	Section 7.2.5 Laboratory colour measurements using JAZ instrument	“Therefore, these 3 experiments demonstrate that the JAZ instrument can operate in Murujuga, even in extreme temperatures.” So, both will be used concomitantly? Are gathered data totally comparable? (Note also what is stated in 7.2.7, notably at the end of the section)	Both instruments have been used concurrently for 3 field campaigns to date. It is intended that the JAZ will be retired and replaced with the HDX.	Ok	✓
26	Section 7.10.1 Background	“It has been found to date that apart from infrequent mist and rainfall events, the samplers spend ~99% of the time in the dry position” This should be rephrased, as it is somewhat contradictory.	This paragraph has been rewritten to aid clarity.	Ok	✓
27	Section 7.10.5 Rainfall pH	“It is possible that rainfall was more acidic in the past when NO ₂ and SO ₂ levels in the airshed were higher.” Can this proven/strongly suggested?	Bednarik (2007) reports acidic rainfall, however through personal communication has stated these were single point measurements at one location (so 5 measurements in total). Unfortunately, more extensive historic datasets do not seem to be available. However, given the Bednarik data and expected higher emission levels of these key pollutants in the past, combined with higher sulphur in (land) transport fuels, it is a reasonable proposition.	Ok. Perhaps you can add this discussion to the text.	Additional text has been added to address these points.
Associations between variables					
28	Section 8.2.1 Background	“It is currently hypothesised that patina is not currently formed at	The question of proof is now discussed briefly. New text reads. “It	Ok	✓

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		<p>Murujuga anymore. If the patina is not formed any more. this might affect weathering rates because the substrate, without the patina coverage, might become more exposed to dissolution.”</p> <p>Can these two suggestions be proven? Moreover, patina not forming anymore (temporary or permanently) means that the (inside) of the carved petroglyphs stop being covered by patina... which would be advantageous insofar they remain brighter. (Also, punctuation)</p> <p>See also comments #7 and #10.</p>	<p>is currently hypothesised, but not proven, that patina is not currently formed at Murujuga anymore. If the patina is not formed any more then this might affect weathering rates because the substrate, without the patina coverage, might become more exposed to physical or chemical weathering. Conversely, patination may increase dissolution within the weathered rind, if aqueous solutions that pass through the patina become more capable of mineral dissolution by the addition of organic acids or other components. These details are not yet known.”</p>		
Key outcomes overall from the second year of scientific studies					
29	Section 9.1 Summary	<p>“Given previous industrial emissions in the region of highest porosity were 2–3 times higher in the past, these past emissions appear to be the most likely contributor. However, this is still a subject of active investigation.”</p> <p>Does available data permit to safely say that past emissions were higher? Furthermore, have industrial gases contributed to patina formation? And does the fact that pollution is now lower (apparently) help in explaining why patina formation has decreased/stopped?</p>	<p>We have estimated emissions from the previous Dampier Power Station (DPS). At peak operating capacity while operating on heavy fuel oil, we estimate that emissions from this facility would at minimum be equal to current total emissions in the airshed. Discussed briefly “However, it is not yet known if industrial gases have contributed to patina formation or any changes in patina formation rates as industrial emissions have increased and decreased over time. This area is the subject of active investigation.”</p>	Ok	✓

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30	Section 9.1 Summary	"Features such as gorges (which also tend to contain a higher concentration of engravings." See comment #4	Responded to this comment in our response to comment 4 above.	Ok	✓
31	Section 9.2 Further work	Important and well delineated next steps.	Thank you	-	-

2.2 Professor Geoffrey Michael Gadd (2024)

Document title:	Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2024
Current document revision:	COPP21065-REP-G-105 (Rev 0)
Reviewer:	Professor Geoffrey Michael Gadd
Date review commenced:	June 2024

Item no.	Section	Peer reviewer comment 24 June 2024	MRAMP team response 27 August 2024	Peer reviewer close-out comment 11 November 2024	MRAMP team close-out response 20 December 2024
1	General	An extensive report has been presented encompassing a very large amount of data in several key areas and supported by detailed methodology and statistical analysis. Some conclusions and correlations have been drawn in some areas, although several aspects of the research and accompanying analyses are ongoing. Some tasks are yet to commence. Methodological modifications are justified and adequately specified. As previously mentioned, there is rather low integration of findings between the different areas although this should be a target for the next phase of research as more ongoing research is completed. The very large amount of data in the main research areas	<p>We thank the reviewer for these encouraging comments.</p> <p>We agree with the assessment that there is so far only limited integration of findings between the different component studies. This is indeed the target for the next phase of research.</p>	OK	✓

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		dominating this report, and the substantial appendices, are evidence of dedicated and sustained research endeavour, data acquisition and analysis.			
2	Executive summary	The summary is clearly written and emphasises referral to previously produced relevant reports and documents. Developments in sampling over the report period and analyses continuing from Year 1 are summarised. As expected from a longer-term project, and natural/anthropogenic phenomena that have a long-time scale, many of the component studies are in progress and in several areas definite conclusions are not yet possible. A summary of key findings in several component areas is provided, although the microbiome studies are not included. Comments on some of these areas will be made later in the appropriate sections. It is clear much more data has been collected in several areas with some clear trends evident in some but also wide variability in others. The lack of concrete conclusions may be resolved through ongoing and future work. As mentioned, more complete integration and analysis of datasets is a target for the next report.	Taken as a comment and agreed. The summary has been revised to include further commentary on studies including the microbiome work.	OK	✓

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Introduction					
3	Section 1.1 Background	<p>Largely repeats the summary and objectives of the project initially, following on with scope of the report and the relevant conclusions from years 1 and 2. Several key areas for laboratory analysis have been completed with others the subject of ongoing research. The stated exclusions are justified because relevant data has not been obtained due to various delays or the time periods necessary for adequate collection and analysis.</p> <p>Minor comments: p11 – bioweathering definition – bioweathering refers entirely to weathering mediated by biological entities or processes. Physico-chemical factors independent of a biological component are just under a weathering definition.</p> <p>I think it should read “...processes resulting in the release of metals (not minerals) from the rock, and/or formation of new minerals.”</p>	<p>Taken as a comment.</p> <p>Bioweathering definition corrected.</p> <p>Corrected as indicated.</p>	<p>OK</p> <p>OK thank you</p> <p>OK thank you</p>	<p>✓</p> <p>✓</p> <p>✓</p>
Study methodologies, verification of competency, and quality assurance					
4	Section 2.1	Seven methodologies that have been updated are listed. Further comments may be made in the relevant part of	Most updates are minor revisions which do not have data quality/analysis implications.	OK very good. Thank you.	✓

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		the report review template but does such updating have any implications for the previously obtained data using the original methodologies? Can all the data be reliably integrated using both the former and updated methodology protocols?	In some cases (e.g. rock surface pH) the updated methods increase the level of confidence in more recent data, but do not render earlier data invalid. Our analysis of data includes appropriate checks for discrepancies between the older and newer versions of a technique, including changepoint detection methods, analysis of variance, and cross-calibration.	OK OK	✓ ✓
5	Section 2.2	Some training in practical and theoretical skills for future monitoring is described as ongoing. Is this training for after completion of this project or during the remaining time?	This training is focussed on equipping the MAC rangers with the appropriate skills to take over the monitoring program in 2026.	I see, thank you.	✓
6	Section 2.3	Quality Assurance has been carefully considered in depth for all areas included including sampling, data analyses, experimental controls, and calibration of analytical equipment.	Thank you	OK	✓
Monitoring studies undertaken in the field					
7	Section 3	What was the rationale/reason for not microbiome sampling in year 2?	The published study design specified that microbiome sampling would be performed in years 1 and 3 of the study, in wet and dry seasons. Analysis of the microbiome samples is costly; the budget is finite; the proposed design was determined to be the most efficient use of resources,	I see. Thank you for the explanation.	✓

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			and to achieve the desired statistical power.		
8	Section 3.1	Table 3.1. Can the months corresponding to wet, dry and shoulder seasons be specified here? What is meant by “Organic microbiome”?	This has been added to the text to clarify. This has been changed to Organic / Microbiome	OK OK but what is being considered under “Organic”? Microbiome and vegetation are clear. Does it refer to analysis of organic substances?	✓ Organic refers to organic geochemical analysis
9	Section 3.2	A minor point – the tables of the numbers of samples/measurements taken are not Fieldwork Outcomes – the outcomes are the derived data and conclusions.	This heading has been changed to Fieldwork progress	OK	✓
10	Section 3.3	The variation from the planned methodology is justified.	Thank you	OK	✓
Laboratory analysis of field samples					
11	Section 4.1	Clearly written with interesting and well-presented results.	Thank you	OK	✓
12	Section 4.2	The majority of geological mapping has been completed. There is an impressive amount of obtained data in this section, most of which I am not competent enough to comment upon	Taken as a comment	OK	✓

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		except to complement the detail and quality of the analyses and imaging.			
		<p>A comment/query: through all these observations with the micrographs was there ever any significant evidence for microorganisms/microbial effects in the rock surfaces/patinas etc? On p140 there is brief reference to co-location of Mn with cyanobacteria but I cannot distinguish what is a cyanobacterial component in the referred Fig. 4.83b. Certain cyanobacteria can be involved in carbonate deposition and biomineralization in stromatolites but I assume this is not relevant in these terrestrial locations? What is the possible reason for a Mn/cyanobacterial co-location?</p>	<p>Our analysis of correlations between microbial populations and rock surface composition has been extended and moved to Chapter 8 (Section 8.2) where various conjectures about microbial involvement are discussed in the light of the available statistical evidence.</p>	<p>OK, I see that this section has been extended further. It has not been clarified what is the cyanobacterial component in the referred Fig. 4.83b, which I cannot discern with confidence.</p> <p>No comment on the carbonate/biomineralization query.</p>	<p>This has now been clarified in the text with references to the appropriate Section and Figures. Have added to the caption for 4-83 "It is possible to speculate that the sponge-like, C-rich parts of the image form part of a biotically-mediated structure."</p> <p>To explain the link between the Mn/cyanobacteria, which is also explained in section 8.2, we have added the text "The Cyanobacteria are thought to accumulate Mn as a way to combat oxidative stress (see section 8.2 for more details).</p>

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					Comment around carbonate deposition and biomineralization was taken as a comment. We do not see carbonates in the Murujuga patina although calcite forms veins in the fresher rock (see section 4.2).
		In view of the results of the analyses in this section, does it appear that any bioweathering component is insignificant or not readily detectable?	We have speculated on the role of bioweathering in section 4.2.9.1, with the text “It is not possible to distinguish between these hypotheses using inorganic chemical techniques. While it is highly unlikely that any process at Murujuga occurs without a biological/microbial contribution, the role and magnitude of the effect of these contributions remains to be determined. Integration of organic, inorganic, and microbiological investigations are required to better constrain the role of microorganisms.” Integration of the inorganic results with the organic geochemistry and microbiome programs is required to gain more certainty on this question, and this is work in progress.	It is possible to speculate from results of inorganic chemical analyses such as mineralogy if any mineralogical changes include minerals that are also known to be produced through biotic mechanisms (see also comment below). Yes, integration of findings from the three main categories of work is required though I	It is hard to distinguish between biotic/abiotic processes with these types of analysis. As stated in the text “While it is highly unlikely that any process at Murujuga occurs without a biological/microbial contribution, the role and magnitude of the effect of these contributions remains to be determined.”

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				feel that such attempts should already have taken place through better interaction between the organic, inorganic and biological areas.	This work has not yet been undertaken at the time of writing.
		Following on, where “weathered rind” is mentioned, are you concluding that the weathering is entirely natural physico-chemical/anthropogenic weathering with NO biological/microbial component? or has this not been able to be determined?	It is highly unlikely that any process at Murujuga occurs without a biological/microbial component, but the role and magnitude of the effect of those components remains to be determined. This has now been stated in the text – see addition above.	OK	✓
		I notice that several of the minerals detected can also result from biogenic transformations as well as geochemical/mineralogical processes.	It may not be helpful to attempt to distinguish between biogenic and mineralogical processes because the biome is probably always playing a role.	I do not understand this comment which is contradictory to previous comments and at odds with some of the objectives of the research. It would indeed be helpful if biotic and abiotic processes could be distinguished hence my several queries on this aspect.	We agree that it would be helpful if abiotic and biotic processes could be distinguished clearly but this is not possible at this stage of the research. Further, some, if not all, processes may have biotic and abiotic components, which is the context for the ‘not helpful’ comment, which I hope I have

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					expressed more clearly here.
		There appears to be no integration of the investigations and results found in this section with microbiome analysis. What are the reasons for this? Is it because microbiotic presence or activity could not be detected in the geoanalyses or was it not looked for, or are the results so far from microbiome analysis incomplete or inconclusive for example?	This integration is still in progress. An RDA analysis has now been added (section 8.2) which shows what appear to be correlations between microbial abundance, Mn and porosity.	OK	✓
Microbiome laboratory analysis					
13	Section 4.3	<p>Key objectives of this aspect are microbial community assessment and association with the weathered rind/bioweathering processes that might be occurring. As mentioned above, any integration/complementation with the geological/mineralogical investigations is not mentioned. Is the continued separation of the two themes going to continue or will there be some attempt to provide an integrated summary of the processes involved in total weathering?</p> <p>The variations from planned methodology are justified.</p>	<p>We will be doing more integration of results and analysis between the disciplines in the future. The work done so far on integrating results from different disciplines is sequestered in Chapter 8. Section 8.2 discusses connections between microbiome and mineralogy.</p> <p>-</p> <p>Corrected</p>	<p>OK. I enjoyed reading Section 8.2. I notice that a focus of this was patina development with rather little on bioweathering processes.</p>	<p>We agree with the reviewer's assessment. In the Year Three Report we expect to have gained a better understanding of possible bioweathering processes in which the microbiome may be involved, after studying the metagenome, metatranscriptome sequencing as well as the microbiome chamber studies.</p>

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		<p>Minor point – use “fungi” not “fungal microbes”</p> <p>Clearly, several molecular analyses are ongoing or yet to be started, but several areas have been completed. No summary of the data obtained so far or any conclusions are provided in this section.</p>	<p>Agreed.</p> <p>These are provided later (section 8.3)</p>	<p>-</p> <p>Good</p> <p>OK</p> <p>OK</p>	<p>-</p> <p>✓</p> <p>✓</p> <p>✓</p>
Chamber exposure (accelerated degradation) testing					
14	Section 4.4	<p>No comment on this except there is no summary of any results or conclusions achieved so far. Appreciate that this is an ongoing longer-term aspect of the project.</p>	<p>Conclusions are reached after appropriate statistical analysis. In the case of the chamber studies, the statistical analysis is presented in Section 7.1 and conclusions are summarised in Section 7.13.1.</p>	<p>I see, thank you.</p>	<p>✓</p>
Air quality monitoring network establishment and operation					
15	Section 5	<p>I have no specific comments arising from Section 5 apart to complement on the impressive instrumental setup.</p>	<p>Thank you</p>	<p>OK</p>	<p>✓</p>
Spatial mapping and surface modelling					
16	Section 6.	<p>I have no specific comments arising from Section 6.</p>	<p>Noted</p>	<p>OK</p>	<p>✓</p>
Statistical analysis of component studies					

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17	Section 7.	<p>There is an impressive amount of data in this section and clear and detailed attention given to statistical analysis. The data from the component studies is separately dealt with and a useful summary provided at the end.</p> <p>Some minor comments:</p>	Thank you	OK	✓
		<p>Section 7.1</p> <p>The geochemical and biological/biogeochemical chamber studies have been carried out separately and it is stated that these will be integrated later through combined experiments or data fusion. What was the reason that combined experiments were not carried out from the start? Was this too demanding in terms of analyses or space needed? It is stated that the geomicrobial studies have not yet commenced.</p>	<p>Preliminary studies on rocks with a “live” patina showed significant heterogeneity and autofluorescence. Sample size for combined studies would need to be exponentially higher to account for these features and the fact that many pre-post analysis are incompatible and would require additional replicates for microbiome and geochemical analyses.</p>	<p>OK, I can see that there might be difficulties with sample sizes etc, though it is stated that combined experiments will be carried out later, hence my query as to why they were not done from the start since they are scheduled to take place (as other comments below).</p> <p>I do not understand the sentence “many pre-post analyses are incompatible...” or what analyses this might refer to.</p>	<p>The inorganic chemical workflow utilised bathing the rocks in ultrapure water pre and post experiment as a means of simulating rainfall and eluting reaction by products. SEM/TEM imaging is also conducted periodically. The current preferred approach for microbial experiments is pre and post in-situ confocal microscopy on labelled cultures. The washing process used in the organic studies would likely interfere with confocal</p>

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					<p>microscopy. The SEM/TEM would kill the microbes in vacuum.</p> <p>Furthermore, biosafety protocols with labelled (GMO) cultures would not permit them to be imaged in geochemistry laboratories as they are not PC2/AA certified and are in a different facility.</p>
		<p>Section 7.4 Regarding the pH differences between campaigns and significance of rainfall, it might be relevant that organisms need moisture to grow and metabolize, and that most microbial rock dissolution mechanisms depend on inorganic or organic acid production which can result in pH lowering.</p>	<p>As such, the decision was made to switch to microbial cultures and commence separate geochemical studies immediately to confirm the mineral dissolution/porosity hypothesis. It is expected that combined experiments will be conducted once the microbial chamber studies have commenced. The microbial cultures have been obtained and prepared and the final approvals are almost complete before these can commence.</p>	<p>Related to previous comment. There are comments elsewhere about the microbial cultures. Can a time for commencement be specified at this stage?</p>	<p>The microbial cultures have been prepared and are being maintained. We expect to commence late 2024 or early 2025.</p>
		<p>Section 7.8</p>	<p>Corrected</p>	<p>OK</p>	<p>✓</p>

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		It is usual to refer to filaments not hyphae for Actinobacteria and Cyanobacteria. Only fungi have true hyphae.			
		Another point regarding pH is that this can vary considering in an organism's microenvironment from low values at points of acid production. However this is difficult to measure, but a total pH determination might miss such spatial variations at micro- and nanoscales.	Agreed on organic acids and pH contributions. We are attempting to detect these organic acids from the organic analyses and the microbiome chamber studies.	OK	✓
		Cyanobacteria do not "create a carbon source", their carbon source is atmospheric carbon dioxide which is fixed in photosynthesis to make carbohydrates for biosynthesis and energy.	Corrected	OK	✓
		Actinobacteria and cyanobacteria are not generally regarded as significant biodeteriorative agents affecting rock substrates. Only certain cyanobacterial genera can penetrate or bore into rock substrates, generally carbonates. Any comment on this?	That is correct. At this stage the 16S rRNA taxonomy is not sufficient to identify the genera of <i>Cyanobacteria</i> that are present on the rock. From our understanding of the literature, many <i>Actinobacteria</i> and <i>Cyanobacteria</i> that have been previously identified in similar environments are thought to penetrate deeper into the rock as water becomes more available and are then more protected from UV. How deep is another question.	I thought that some genera had been identified which is why the organisms obtained/to be obtained for culture studies have been chosen (other comments elsewhere). Seems to be a contradiction on this point. This response comment does not	Yes, a portion of the cyanobacteria reads were classified to genus; however, the majority are not. At this stage we do not have conclusive evidence from our data that the microbes are penetrating into the rock. In view of the porosity findings, we think it is possible

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				directly address my query. Are the actinobacteria and cyanobacteria present capable of direct penetration of the rock substrate?	that microbes may be able to penetrate into the rock, but it is yet to be determined whether the microbes contribute to the porosity.
		Presumably “myelinated” should be “melanized”? Several fungal genera identified are known rock-inhabiting fungi, usually slow growing and melanized. Because of the often harsh environments they prosper in, their role in biodeterioration may not be dramatic and very slow because of e.g. heat, dryness, and their very slow compact growth form.	Melanized, Corrected. Yes absolutely, we are hoping to get a better idea of how active these fungi are through the cDNA amplicon sequencing and metatranscriptomics.	OK I think this will provide interesting information but not give an idea as to how active they are.	✓ We will have an understanding of transcriptional activity, which is tied to cell activity.
		Section 7.13.8 It is interesting that fungal diversity was greater in dry periods when for, e.g. soil, the reverse would be expected. Could this be due to the melanized rock-inhabiting microcolonial fungi being more able to survive such conditions? I have no specific comments arising from Sections 7.2, 7.3, 7.5-7.7, 7.9-7.13.	Thank you for this observation. We agree that this might occur.	OK	✓
Associations between variables					

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18	Section 8	<p>Section 8.2</p> <p>There is a good discussion of Mn and putative microbial involvement in MnO formation. This is still quite an unresolved matter in the literature, with most attention focussed on bacteria, less on fungi, but with many bacterial and a few fungal genera capable of MnO formation in the environment. I am not aware of such an association with cyanobacteria but elevated Mn levels could eventually result in their transformation into oxides by further other microbial activity as the authors hypothesize. I have no other specific comments on this section.</p>	<p>There is literature showing some Cyanobacteria being capable of accumulating large amounts of manganese, which is hypothesised to be an antioxidant strategy. Given the oxidative stress in these environments, it would be possible the cyanobacteria on these rocks are accumulating a lot of intracellular manganese, and upon cell death, the manganese is becoming oxidized into the layers we see (Lingappa et al. 2021).</p>	<p>I see. Comments on the Mn source are elsewhere in this document.</p>	<p>✓</p>
Key outcomes overall from the second year of scientific studies					
19	Section 9	<p>General Summary: This section clearly and accurately summarises the main findings and associated conclusions from the work carried out over the second year and in conjunction with previous analyses where relevant.</p> <p>There is little reference to the microbiome studies in this section. Suggestions for Future Work and Conclusion are justified.</p>	<p>Thank you</p> <p>Discussion on the microbiome analyses to date have been added.</p>	<p>OK</p> <p>OK</p>	<p>✓</p> <p>✓</p>
References					

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20		There is an extensive bibliography although I see that microbe/rock references are rather dated and low in number.	Appropriate additional references have now been provided. Some references are dated however are citing original research describing the isolation of genera present in our data.	OK but microbe/rock references, especially relating to any rock surface processes, rock microbiomes etc. are still very few. Maybe this does not matter here as the experimental work is the most important part but might be important should any microbe/rock work be written up for publication in the future.	We agree. We are only citing research directly relevant to the results at this point in time.
Appendices					
21		There is a massive amount of well-presented data, images, figures, and methodological details in the Appendices.	Thank you	OK	✓
List of figures					
22		An extensive list is presented.	Thank you	OK	✓
List of tables					
23		An extensive list is presented.	Thank you	OK	✓
General					

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24		See Section 1: General	Noted	OK	✓

2.3 Dr John Henstridge, Data Analysis Australia (2024)

Document title:	Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2024
Current document revision:	COPP21065-REP-G-105 (Rev 0)
Reviewer:	John Henstridge
Date review commenced:	June 2024

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1	General	<p>Overall, the report demonstrates much good work done over the past year and it is clear that in many areas the scientific basis for ongoing monitoring is being laid.</p> <p>That said, there is still much to be done. While measurement processes are being established, there is still not a well-established baseline for many of the monitoring variables. Indeed, most of the statistical analysis presented is at the exploratory stage. Further development of the analysis and further data is required – in many cases the former requires more of the latter.</p>	<p>We agree with this assessment and thank the reviewer.</p> <p>We agree broadly with this assessment. The original study design envisaged that the full study period would be required in order to collect sufficient statistical information to perform many of the planned analyses. It is also clear, as the reviewer indicates, that some of the methodological choices required for the final analysis will be informed by data that is yet to be collected.</p>	<p>Acknowledge.</p> <p>Acknowledge. It is clear that some data items will only start to be properly collected towards the end of this study period.</p> <p>It is good to see optimism, but in some respects, this was one area where</p>	<p>✓</p> <p>Agreed</p>

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		<p>One area that I have concerns about is the photogrammetry. While it is evident that various methods are being trialled, it is not clear yet that these are shaping into being part of a monitoring program. The question “is this likely to be able to measure changes to the rock surface in the field?” is still not answered. The report also recognises issues in the colour measurement, although that appears to be better progressed.</p> <p>A very minor suggestion regarding the report format. Including the section name and number on the running header or footer would make is very much easier to find things. I spent much too long trying to get to the right page.</p>	<p>Photogrammetry appears to be a promising technique for showing visual changes at sufficiently fine resolution. It is expected that a detailed evaluation of the technique will be provided in the year 3 report and a decision made at or shortly after that point about the preferred ongoing monitoring of rock art for the visual and/or spectrographic appearance of the rock art.</p> <p>We thank the reviewer for this useful suggestion. It has been implemented.</p>	<p>MRAMP could learn from the experience of CSIRO who, over 14 years, continued to have troubles.</p> <p>Acknowledge.</p>	<p>Agreed</p> <p>✓</p>
2	Executive summary	The Executive Summary gives a good balance of the achievements, the findings and the limitations.	We are pleased with this assessment.	Acknowledge.	✓
Statistical analysis of component studies					
3	Section 7.1.5.1	The proposal to use the sum of NOx and SO2 concentrations as a simplified measure of dose or exposure does not seem appropriate unless it is a weighted sum as, comparing Figures 7.1 and 7.2 there is a factor of 10 difference in their effects. (Note the differences in the x-	We agree with this comment. The analysis has been repeated using NOx+10'SO2 as the index of dosage, and the results are reported in the updated Year 2 Report.	Acknowledge.	✓

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		scales.) I strongly suggest NO _x +10'SO ₂ . (Or something close to that.)			
4		The fitted curves in Figures 7-8 to 7-10 suggest interesting behaviour since superficially the shape is statistically significant, suggesting a threshold effect. I accept that the evidence is tentative and with the structure of how the data was collected this may be an artifact. I recommend that further tests should attempt to fill in the gaps and to extend the range to pin this down (and possibly show it is not significant).	We agree that further experiments should be conducted to eliminate alternative explanations and to establish the dose-response relationship. Several such experiments are now in progress.	Acknowledge.	✓
5	Section 7.2	<p>The colour measurement work appears to be still affected by instrument problems, with the JAZ instrument giving problematic results (such as negative reflectance measures, the newer HDX instrument having hardware reliability issues and what appear to be persistent problems of repeatability).</p> <p>The good news is that interfacing the instruments to a computer and getting close to real time analysis has enabled greater effectiveness, for example detecting and replacing poor measurements while still on site.</p> <p>I question the continued emphasis on the JAZ instrument since it is end-of-</p>	<p>We agree.</p> <p>We agree with this assessment. This is an important outcome of the studies phase.</p> <p>Agreed. The HDX is the preferred instrument. The JAZ instrument was</p>	Good to see agreement here. Ideally future investment in getting the JAZ instrument to function properly should be minimal.	We agree with this assessment. During every campaign, the Statistical team checks the data from the JAZ and HDX instruments. Campaign 9 has just finished; the HDX instrument worked well, even on the very hot days. There was only one site at which the Statistical team assessed the data as being unacceptable and for

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		<p>life and cannot provide the basis for a longitudinal fieldwork program. Replacing it with the HDX instrument would appear to be optimal.</p> <p>In this regard I note the substantial variation between measurements displayed in Figures 7-27 and 7-28. Not only do the JAZ measurements vary from Campaign to Campaign, for the Campaign 7 the JAZ and HDX measurements are very different. It is not clear that any of the measurements are close to giving an acceptable baseline.</p> <p>(I may have missed some comments since the paragraph on page 231 appears to be truncated.)</p> <p>Regarding negative reflectances, I can appreciate that the method of calculation where it is necessary to subtract a background reading can give negative estimates, possibly due to photon level noise. However, presenting such estimates is poor practice. I recommend investigating smoothing the raw readings before applying the $P = \frac{S-D}{R-D} \times 100$ formula. In general, an estimation process that can give nonsensical estimates is not</p>	<p>temporarily recalled from retirement when the HDX was temporarily unusable.</p> <p>Agreed. Further analysis is required.</p> <p>Paragraph on p231 has been fixed, now all text in Section 7.2.10 is on one page</p> <p>Agreed. Estimates that do not belong to the parameter space should not generally be reported, and they can be removed using post-processing techniques like those described by the reviewer. In this report, we display such estimates when it is useful to indicate remaining problems with the raw data, for example, to portray differing rates of anomalous data across different sites. The yearly reports do not always present a settled protocol. Ultimately,</p>	<p>Acknowledge, but stress the priority of this.</p> <p>Acknowledge</p> <p>Acknowledge, but I would prioritise the improvement on estimation as without it it is difficult to monitor data quality.</p> <p>Acknowledge</p>	<p>which a revisit was required.</p> <p>✓</p> <p>✓</p> <p>We recently began implementing Principal Component Analysis (PCA) of the spectral reflectance data, as an alternative to conversion to CIELAB coordinates. This led us to examine the negative reflectance measures obtained using the old JAZ instrument. We discovered that these negative values arise when</p>

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		<p>desirable except at a purely exploratory stage.</p> <p>I am puzzled by the comment on page 228 that “some of the lines are wiggly due to a big variation between the 10 targets”. An average of smooth curves should be smooth so presumably this is either due to some targets not being smooth or, of more concern, it is an artifact of the estimation process.</p> <p>My final comment for this section is I could see no reference to how the procedure for reliably getting to the same exact target points in each campaign is working.</p> <p>Overall, this work on colour measurement appears to need more</p>	<p>techniques such as those mentioned by the reviewer will be applied.</p> <p>This requires further investigation.</p> <p>Regarding target relocation, the approach used for the work was initially evaluated independently by the spatial scientists using RTK/total station laser-based methods and the relocation accuracy was found to be at the measurement limit of the method ($\ll 1$ mm in this instance, nominally 0.2 mm). The relocation accuracy has not been reevaluated; however, target photographs are taken each time and could be reevaluated using image analysis if there are questions over this.</p> <p>The current system for target relocation uses pattern matching to a reference image using the zoom capability of the smartphone imaging system. It will be better than the accuracy achieved in the initial evaluation.</p> <p>Agreed.</p>	<p>Acknowledge and I look forwards to seeing quantitative reporting of this at some stage.</p> <p>Systems to match a current image of the rock surface with a reference image sound promising and can be quite efficient when the scale and orientation are reasonably accurate. Again, some quantitative assessment is required on a range of rock surfaces.</p>	<p>the dark current values are higher than the sample measurement values (S sample sensed photon count). There are two possible causes for this fault: 1) stray light entered during the dark current measurement; 2) the light probe was faulty or failed to activate during the sample measurement. We have added this fault to Appendix G.3.1.</p> <p>✓</p> <p>✓</p> <p>We agree that this may require further investigation. Target relocation was measured on multiple sample rocks initially and determined to be < 0.1mm. Images of the relocated target positions are taken for each sequential measurement, and</p>

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		to be done before it can be said to be providing a reliable monitoring process.		Acknowledge.	these can be analysed to determine relocation accuracy if a suitable method can be determined. ✓
	Section 7.4	-	-	-	-
6	Section 7.5	A very minor note, comparing Figures 7-49 to 7-52 with Figure 7-53, the latter has an inverted y-axis. A problem with this sort of software that is often inconsistent but can lead to confusing diagrams.	<p>Agreed.</p> <p>The sign of the scores is relative and does not have a fixed interpretation on its own. The scores in RDA are projections of the original data onto the canonical axes defined by the explanatory variables. The sign and magnitude of these scores depend on the relationships between the response variables and the explanatory variables in the dataset.</p> <p>Over the campaigns, the same chemical species are positively (or negatively) correlated with the same rock types. For example, Ti and Fe are positively correlated with DOL for almost all campaigns, including campaign 6 (Figure with the inverted y-axis).</p>	Acknowledge. The problem of random signs on estimated basis vectors is one where software developers have been lazy. For the past 40 years I have used a simple system of multiplying by -1 if necessary to get a majority of coefficients positive – solves 90% of the problem.	This would require manual intervention in our analysis pipeline. We shall attempt to implement an automatic solution for the next annual report.
7	Section 7.6 and 7.7	The development of the SEM imaging and analysis to form a measure of porosity is an innovative step that	We agree. We judge this to be an important step.	I acknowledge that the late adoption of the TinyPerm meter	Paired estimates of porosity (from electron microscopy

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		<p>gives an added depth dimension to the measurement process.</p> <p>And the new TinyPerm porosity meter appears to be giving reasonable data. The natural question is how these two might relate to each other. Comparison of Figures 7.65 and 7.72 suggests that they might but since this is a geographical correspondence there might be other explanations. Is it planned that there is a more explicit comparison, sample unit by sample unit? I appreciate that this might get complex, as the two measurements are not the same but closely related.</p>	<p>The TinyPerm measurements were not part of the original study design but were introduced at a late stage of the project. For a rigorous comparison and cross-calibration of permeability and porosity measurements, the permeability of rock surfaces should have been measured in the field several times, before the same rocks are extracted, sectioned and imaged under the electron microscope. This cannot be performed retrospectively on the existing rock samples (and the remaining sample material is too small for permeability to be measured.) Although the experiment cannot be repeated from scratch within the remaining time and budget (porosity measurement is labour-intensive and time-consuming), a reduced version of this experiment will be carried out. Each of the air quality monitoring stations includes four prepared cubes of fresh rock which are exposed to the environment. In the remaining field campaigns, TinyPerm measurements of permeability will be taken from these cubes, and thin sections of the cubes will later be taken for measurement of porosity. This will enable an explicit and rigorous</p>	<p>means that earlier porosity measures cannot be matched to it. However, it seems worth doing if only to demonstrate that the two measures are closely related as intuition suggests. Presumably the TinyPerm meter is much easier and cheaper to apply in the field.</p>	<p>of thin sections) and permeability (from the TinyPerm device) will be obtained from several sources, including the prepared cubes of rock which have been exposed to the environment. These paired observations will be studied to identify any relationship between porosity and permeability measurements at Murujuga.</p> <p>The advantages of the TinyPerm technique include the fact that it is non-invasive, repeatable, and easy and cheap to apply in the field, whereas the porosity measurements are destructive and expensive. In the Scientific Studies phase, the TinyPerm device will allow us</p>

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			comparison of the two measures, as the reviewer envisages.		to greatly increase the spatial coverage and sample size of observations related to rock condition. In the Monitoring Program, the TinyPerm offers the possibility of conducting long term monitoring. If instead it were proposed to perform these activities using direct porosity measurement by destructive microscopy, this would be unlikely to be acceptable to the traditional owners and would be very expensive. An additional question concerns the utility of the permeameter method to detect change over time.
8	Section 7.7.3	It says that “no anomalous behaviour was observed” but the outlier in Figure 7-68 is extreme. Makes me worry about the two sample t-test.	The reference to “no anomalous behaviour” was imprecise and poorly worded.	It is good that the outlier had minimal effect. It strikes me that an apparently high permeability	If the seal between the permeameter and the rock is poor, then the values are high and

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			<p>The two-sample t-test was repeated with the outlier removed; the result did not indicate significant evidence of a difference between the two groups.</p> <p>This has been added to the text of the revised Year 2 report.</p>	<p>may be due to a poor seal of the meter against the rock surface so outliers of this type are probably an indicator of a technique that might be improved. It is a form of outlier that can introduce biases.</p>	<p>unrepresentative of the rock permeability. However, there are typically clear indications that measurements are erroneous. For example, the pressure curve can be observed graphically in real time and steep or stepped decreases typically indicate a poor seal. Further, when the permeameter is removed after a good measurement, it is possible to hear the vacuum release. Nevertheless, as with all field instruments, data quality is expected to improve in line with operator experience.</p>
9	Section 7.11	It is good to see that the work on the reliability of the IVL passive samplers is continuing, particularly in the use of paired tubes in the field so that precision can be evaluated. It is	Agreed. The use of replicates has proved to be important. The establishment of two monitoring stations close together has also been valuable.	Acknowledged.	✓

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		<p>obvious that the technology is not as precise as may have been hoped.</p> <p>In particular the measurement of SO₂ is not good (Table 7-5). It is also notable that the correlation of SO₂ with other gases, while significant, is not high enough to provide a surrogate. Given that SO₂ is a pollutant of concern, particularly in regard to the effects of HFO, is this going to create problems?</p> <p>I note that some of the VOCs are almost as imprecise.</p>	<p>One should also remember that the passive samplers serve as low-cost imprecise estimates placed at a large network of sites across Murujuga to provide information about spatial trend. Passive sampler estimates are calibrated (in terms of measurement bias) against the much more accurate, powered, air quality monitors. Agreed. Comments above apply.</p> <p>In theory, SO₂ measurements have an equivalent limit of detection and linearity to the other (non-VOC) passive samplers. The results may be influenced by the low levels of SO₂ at many sites. This could be investigated in year 3 once a further body of passive and real time data has been obtained.</p> <p>VOC measurements are challenging as non-methane VOCs are present only at very low levels within the airshed. It is proposed that IVL VOC measurements be discontinued once 12 months of data can be analysed. Measurements of methane and total non-methane VOCs at powered sites is planned to continue.</p>	<p>Acknowledge</p> <p>Noted</p>	<p>✓</p> <p>✓</p>
10	Section 7.8	The genomic analysis of the microbiome is at an early stage. The	Agreed.	Acknowledged	✓

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		report observes difficulties with questions such as the number of species detected as this can be affected by material sample sizes. Hence the work is essentially exploratory.			
	Section 7.9	-	-	-	-
11	Section 7.10	Much of this is exploratory with more plots than commentary, but one important part is the testing for similarity of co-located samplers. It is good that this demonstrates that the measurements may be repeatable.	Agreed.	Acknowledged	✓
12	Section 7.11	<p>Passive samplers have been a difficult issue from the beginning and the analysis here appears to indicate that this difficulty continues. In terms of the statistical issues:</p> <p>In 7.11.4, A key section that looks at duplicates and hence the repeatability of measurements, the text appears to use the term “variance” when referring to standard deviation. (It makes no sense to say the variance is an order of magnitude less than the mean since there will have different units.)</p> <p>The problem of SO₂ in 7.10.4 suggests the use of a log scale. Was this tried?</p> <p>In 7.11.5, Tables 7-6 and 7-7 giving the ANOVA results appear to be wrong and do not match up with the</p>	Section 7.11 has been completely rewritten; the statistical approach to duplicate observations has been changed; results have been re-computed and corrected.	<p>Acknowledged</p> <p>I assume the page 321, para 1, line 2 of the new report is meant to read “4 week average” rather than “4 very small averages”.</p> <p>The revised section clarifies a number of issues. I am particularly pleased to see that a censored analysis treatment of BDL is proposed.</p>	<p>This has been amended.</p> <p>Thanks</p>

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		text. In table 7-6 the p value for the station effect of HNO3 is very significant contradicting the text. In Table 7-7 that is a one way ANOVA for stations still refers to periods and several rows have identical numbers.			
13	Section 7.12	A key issue for the powered sites which is presented are the continuous recordings of SO2 and other gases. The time plots show a number of events where readings are particularly high, as well as some daily periodicity. It would be very useful to know whether these correspond to particular wind directions and events at the industries. I appreciate that the presentation is exploratory but this may be a key issue and it would be good to see that such analysis will be possible.	It is expected that this will be explored in more detail in the Year 3 report once a larger dataset has been obtained.	Acknowledged	✓
14	Section 7.13	This is a summary section. Relevant comments for this were covered above.	Noted.	-	-
Associations between variables					
15	Section 8.1	This analysis relied upon the natural variation between sites to examine correspondences between the deposition and passive samplers. What are reported as “strong relationships” may have statistical significance (not clear) but they do not	Agreed. Further analysis is now included, along with a more careful discussion of the weaknesses of the analysis.	Acknowledged. There may remain an issue of whether the apparent (but probably real) correlation is simply due to seasonal	Agreed. A longer duration of field observation is important in order to resolve these competing explanations.

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		look visually strong. This creates real concerns.		<p>effects. If so, it becomes harder to assign causality.</p> <p>I note that in the plots in this revised section the X and Y scales are quite different making it difficult to readily see what the exponent of a power relationship might be. However, it appears that the variation in the passive sampler data is less than that for the corresponding dry bucket deposition. I am not sure which of these two measures is more likely to be related to effects on the rock surfaces, but it does suggest that the dry bucket measures may have less noise and is hence better at picking up real variation.</p>	<p>The dry bucket data have greater variability than the passive sampler data. Typically, the passive sampler data vary by 1 to 2 orders of magnitude, while the dry bucket data vary by 2 to 3 orders of magnitude. We agree that this is not easily readable from the plots. We also investigating which measurement is more relevant to processes occurring on the rock surfaces.</p>
16	Section 8.2	This section uses appropriate exploratory statistical methods to	Agreed. The analysis is purely exploratory at this stage. Various	Acknowledged	✓

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		<p>understand the relationship between the microbiome and other variables. The relationships between certain species and MO and porosity are strong but it is not clear what the significance levels mean since the species were presumably selected for their relationships. Not a problem at this exploratory stage but multiple comparisons need to be kept in mind.</p>	<p>caveats must be kept in mind, including selection effects and multiple comparison.</p>		
17	Section 8.3	<p>The overall finding from the comparison of the Ramboll models with the actual data is that the correspondence is not particularly high. While the Ramboll model was used to design the study (such as the set of sites) this affects the optimality rather than the validity of the design.</p> <p>A question for the future is whether the data collected by MRAMP can be used to better calibrate or refine the Ramboll model.</p>	<p>We agree with this assessment.</p> <p>Discrepancies between Ramboll model predictions and actual data could be explained by various factors. It would therefore be difficult to use such discrepancies to identify detailed flaws in the space-time modelling algorithm. However, the overall average concentration of gases observed by MRAMP can certainly be used to calibrate the assumptions about sources of these gases used in the Ramboll modelling.</p>	<p>Acknowledged</p> <p>Acknowledged. If major shortcomings are identified in the Ramboll model, is there any option of rerunning improved versions? While there is a gap between the empirical observations and the model some questions will remain as to completeness of the work and which parts are right.</p>	<p>✓</p> <p>This is outside the scope of the project, as the Ramboll modelling was separately commissioned, however the Government would be likely to arrange for it to be rerun in this instance.</p>

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18	Section 8.4	I note that this investigation of porosity is “for the sake of demonstration”. It is a competent example of what can be done. It also showed how variably the sites on Murujuga are.	Agreed.	Acknowledged.	✓
19	Section 8.5	This section makes only top-level comparisons of passive sampler data from industry (Woodside) and MRAMP. I agree with the reluctance to go too deeply into this as the passive samplers are difficult and it seems unlikely that they would accurately match, as well as having the problems of different time periods and different sites.	Agreed.	Acknowledged.	✓
Appendices					
20	G.1	The smoothing methodology described here is well developed and appropriate provided that the underlying processes being estimated have similar smoothness. Applying it here makes implicit assumptions that the geology is relatively continuous, the same geology that led to the separation of islands, the different rock types and the forming of the peninsula. The robustifying steps will partially handle this but I suggest it needs some explicit cautions or explanation.	The limited analysis presented in this report focuses on one rock type, granophyre, and we acknowledge that the analysis does assume that the porosity response is smoothly varying across spatial locations within granophyre. This does require further checking and will be addressed in the subsequent analysis.	Acknowledged.	✓

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21	G.2	The development of the SEM imaging and analysis to form a measure of porosity is an innovative step that gives an added depth dimension to the measurement process, that is how does porosity increase or decrease with depth from the rock surface. But it does mean that there are now two closely related measures – porosity and permeability. It is acknowledged in Section 7.7.1 that permeability is a surrogate of porosity, one that can be more readily measured in the field. I appreciate that the two concepts are not directly related in that permeability includes the concept of connectivity of the voids and this can give difficult geometry. However, the development of a measure of porosity when in the field it is likely that permeability will be measured strongly suggests that need to understand the connections.	Agreed.	-	-
22	H	Covered in the comments in the main part of the report.	Noted.	-	-
23	I	It is clear that the micro-photogrammetry is still at a development stage, although progress is evidently being made. I had hoped that the report would include more detail on how well the 3D images are being recorded with respect to how they will be used in the monitoring. Giving sections as in Figures I-9 and I-	We plan to investigate these issues in more detail in the year 3 report.	Acknowledged. I look forward to the year 3 Report.	Thanks

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		<p>13 are illustrative but not very informative.</p> <p>In particular, how repeatable they are is critical is repeated images are to be used to detect surface changes. If they are not repeatable (tested by coming back and reimaging at least several days later having used the equipment elsewhere in between) then the method may be less useful than hoped. There is a need to record two such images, demonstrate that they can be registered with each other and then quantify how they differ.</p> <p>In reading this section I found it confusing to work out the precise quantitative imaging parameters. For example, even though a DSLR camera is used, it is not clear whether this is full frame or APS-C. Even though a 5× objective is being used, it is not clear what magnification was being achieved as it seems from the photographs that the back focal distance was less than the common 160mm. It would be useful to include a table that includes for each method</p>	<p>Statistical methodology for comparing repeated 3D images of the same surface is being developed for the project, based on the following references:</p> <p>Baddeley, A. (1992) Errors in binary images and an L^p version of the Hausdorff metric. <i>Nieuw Archief voor Wiskunde</i> 10, 157—183.</p> <p>Huang, H.-C., Cressie, N., Zammit-Mangion, A., and Huang, G. (2021). False discovery rates to detect signals from incomplete spatially aggregated data. <i>Journal of Computational and Graphical Statistics</i>, 30, 1081-1094</p> <p>Two methods have been deployed for the close-range photogrammetry.</p> <p>The first is that discussed by the reviewer. The following responses are offered. Andrew Thorn has a published paper on this technique that will be referred to in future reports and can be provided.</p> <ol style="list-style-type: none"> Two repeat models have not been fully taken at this stage, however the method employed uses the open-source software CloudCompare to determine morphological change over time. 	<p>Acknowledged</p> <p>I appreciate the additional detail provided here. I believe that this detail should be in the Appendix so that a reader who has some familiarity with optics/photography can follow what is going on. For example, I was trying to consider issues such as depth of field to work out the effect of and need for focus stacking for</p>	<p>✓</p> <p>We agree that more development and documentation of the (final) method/approach is needed. Further comment is provided in response to the separate (additional) letter/review which the same reviewer provided, and yet more development/validation will be forthcoming in subsequent reports.</p>

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		<p>the full details including actual working distances, the actual magnification, image sizes in pixels, effective pixel sizes in the object plane and the step sizes between images.</p>	<p>2. The magnification is not critical and in the development phase about 6 objectives were all tested for magnification and focal depth. A Zeiss 5x proved to be optimal in both regards. The surface imaged in each frame is c. 16 x 12 mm across a total area of 100 x 100 mm. In the development phase a 6x Leitz gave an image almost half the size of the 5x Zeiss with very short focal length. Given that the Zeiss already requires 5,500 images to collect a full set of images, closer imaging was considered unworkable for the aims of the study.</p> <p>While magnification can be determined this is not the primary aim. What is important so to be able to scan a surface at sufficient detail to capture the level of detail sought. The current photogrammetric model can resolve particles < 50 micron, which compares to a sand grain which will be typically 200-1000 microns or the more uniform grain size of, for example, Carrara marble at 600 micron median grain size.</p> <p>3. The camera being used is not full frame but rather a lighter 24Mp that is more functional I the field.</p>	<p>both methods but was unable to do so. (I am not sure I believe the statement “The Zeiss objective has a depth of focus of 4 mm” which seems far too great.) I also find some of the terminology confusing – for example the term “image size” seems to refer to the size the field being photographed. It is also not clear if the microscope objectives were being used in their design configuration and hence whether the claim of low distortion is correct.</p> <p>The more I revisit this section the more I have questions. At the very least, in the Year 3 Report the optical and photographic methodology should</p>	

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			<p>4. The 160 mm focal length referred to is one of the standard lengths for microscopes but not relevant to the use of objectives mounted to cameras using the manufacturer’s mount. What is necessary here is that the objective can focus the collected light onto the sensor with sufficient resolution. A planapochromat lens has been used to ensure a distortion free image. The working distance of the objective is c. 80 mm, which allows for the all-critical ring light illumination to cast an even light across the scene to remove shadows. At any location 20 images are taken at 1mm vertical increments. The objective has a depth of focus of c. 4 mm, meaning that any point in the scene is in focus in at least 4 frames. The set of 20 vertical images are then “focus stacked” to produce one sharp image from top to bottom of the profile. The preferred software for this is Helicon Focus, evaluated among about 6 contenders to give the best results. Hence approximately 275 fully focussed images are submitted to Agisoft Metashape for final photogrammetric construction. A three-dimensional</p>	<p>be made very much clearer.</p> <p>I am not being critical of the overall approach – it strikes me as innovative and perhaps the only method that directly addresses the physical degradation of the rock surfaces. However, the current detail does not give the scientific credibility it requires.</p>	

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			<p>scale is inserted at the edge of the scanned area for future mensuration, however morphological change is better determined by Cloud Compare, which specializes in the comparison between two aligned models. Once a vertical set of images is gathered the scanning frame automatically advances the camera 60% of its relevant dimension in the X-axis before advancing the same percentage in the Y-axis to commence a new row.</p> <p>While the method described above, which relies on an SLR mounted to a scanning frame, was proposed at no more than three sites, this was considered too few to lack spatial integrity within the study. A second less refined approach has been implemented at all 54 study surfaces, after discussion with the spatial scientist. This technique relies on imaging a larger area of 150 x 150 mm using a 200 Mp smartphone camera. The phone employs a magnifier app that give an images size of c. 30 x 20 mm. The target area is first imaged in portrait then landscape modes to provide c. 70 images of the target area. The magnifer app</p>		

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			<p>employed maintains constant illumination to eliminate shadow. One key difference between the techniques is that the magnifer app offers autofocus and hence the focus stacking procedure is not required. The other more significant benefit is that all 54 sites are being imaged in a process that only extends site time by 15 minutes.</p> <p>At 300 micron resolution, this approach is not providing the same level of detail as the frame scanner, however it is providing a well resolved view of the surface and subsequent models can be processed through Cloud Compare. The strength of this technique lies however in other areas.</p> <p>The spectroscopic alignment system involve visually aligning the probe and light spot to recognizable rock surface features, which can be a combination of pits, extrusions or patination features. Each time the probe is aligned to any of the 540 spectroscopic targets a pair of images is taken to confirm the accuracy of the realignment. It has been noticed that in 10-15 of these alignment images features have been removed. Often these are patination features. The registration images are not of</p>		

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			<p>sufficiently high quality and hence the low level macro-photogrammetry described here is producing highly resolved reference surfaces to observe this transience. This imaging system will provide both morphological and chromatic evidence of change. Equally it is a technique that can be implemented and maintained by MAC Rangers without requiring specialised or laborious systems.</p>		
24	J	<p>Whilst this Appendix covers a transition to the newer HDX spectrometer, it appears that there are still significant issues to be resolved, including the connections between the separate components. My concern is that much effort is still spent on the JAZZ instrument that is probably past its reasonable life while the project must be considering how to establish a methodology that will work for the next 10+ years. This would be less of an issue if the instruments seemed capable of working to a “true” standard, so each one could be calibrated to that. However in both this project and in the previous CSIRO work this does not seemed to have been achieved. Hence continuity of instruments is important and that probably means establishing a</p>	<p>We agree that the ideal approach would be to settle on the HDX for all ongoing measurement if it proves sufficiently reliable. However all such instruments only have a finite lifespan before new models are released and old models are no longer supported. Therefore a method is needed to permit instrumental change if this approach is to be used for long term monitoring.</p>	<p>It is good that planning is considering the future where the now new HDX instrument will be outdated and not supported. (As I had mentioned, changing instruments was one area where the CSIRO team failed to achieve consistency so the long term analysis was impossible.) The extent to which experience with the JAZZ instrument helps in this not so clear.</p>	<p>We agree with this assessment. Comparability between results from the (obsolete) JAZZ instrument and the new HDX instrument is yet to be fully evaluated. However, since they are from the same manufacturer and use many equivalent components then we hope this will be relatively straightforward. Nevertheless, if this technique is to be included in long term monitoring, a method for such equipment</p>

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		baseline with a modern instrument and keeping to that instrument.			change and comparison will be essential.
List of figures					
25		Comments on specific figures are made above.	Noted.	-	-
List of tables					
26		Comments on specific tables are made above.	Noted.	-	-
General					
27		<p>It is necessary to keep remembering that the aim of the project is to establish a comprehensive monitoring system based upon the best possible science. Hence issues such as repeatability, calibration and reliability are key statistical issues. Exploration of a more scientific nature such as exploring relationships between variables also has many statistical issues.</p> <p>Overall, I find that major progress has been made on statistical methodology and any suggestions I have made for improvement are generally minor.</p>	<p>We agree that this is the ultimate aim, and hence that repeatability is a key issue.</p> <p>MRAMP has dual aims of (1) determining whether anthropogenic emissions from nearby industry are changing the integrity or condition of the rock art and whether it is being subjected to accelerated change, and (2) developing a comprehensive monitoring system based on science.</p> <p>We are pleased with this overall assessment.</p>	Acknowledged.	✓

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		I am not so able to comment upon many of the specific disciplines involved in MRAMP, but I am concerned about the slow development of the colourimetry and photogrammetry areas. Clearly there have been instrument problems in both but we are yet to see what a monitoring method might actually look like.	We agree with this assessment. Rather than characterising the delays as instrument problems, we would point out that these devices are intentionally being subjected to an unprecedented level of scrutiny in order to establish the basis for a rigorous monitoring program, and this process is taking longer than envisaged.		

2.4 Professor Janet Hergt (2024)

Document title:	Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2024
Current document revision:	COPP21065-REP-G-105 (Rev 0)
Reviewer:	Professor Janet Hergt
Date review commenced:	June 2024

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1	General	<p>The sections I read are detailed and thorough. I have highlighted (in yellow) a range of typos/other corrections on the report and added corrections/questions in red text.</p> <p>The sections I was asked to review are:</p> <p>Section 4.2 Geology and inorganic chemistry laboratory analysis (all sub-sections)</p> <p>Section 7.6 Inorganic chemistry – porosity (all sub-sections)</p> <p>Section 7.7 Inorganic geochemistry – permeameter (all sub-sections)</p>	<p>New text has been added close to the first description of porosity, in the granophyre section. “Given the potential significance of porosity for</p>	Excellent	✓

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		<p>Sub-section 7.13.6 Key findings from statistical analysis: Inorganic chemistry – porosity</p> <p>Sub-section 7.13.7 Key findings from statistical analysis: Inorganic chemistry – permeameter.</p> <p>I would recommend the inclusion of another sub-section early in Section 4.2 to gather together the textures and characteristics of voids unambiguously produced by dissolution. This section should also include the kinds of disaggregation etc features associated with physical damage during sample preparation. The sections that follow could then remove the references to “physical damage during sample preparation cannot be ruled out” that I find distracting.</p>	<p>patina stability, it is useful to consider features that unequivocally or strongly support formation of porosity by mineral dissolution, and those that may be produced by sample preparation. Features likely to be produced by dissolution include:</p> <p>Preferential removal of soluble minerals, such as albite and microcline, where less soluble minerals, such as quartz, remain (e.g. Figure 4.2-8). Mechanical removal is unlikely to produce this distinction.</p> <p>Voids that replicate the shape of a complex mineral. The complex and convoluted shapes of, for example, quartz--feldspar intergrowths, are difficult to produce by mechanical means.</p> <p>Gradation in porosity that consists of voids that replicate the shape of individual mineral grains from the fresher rock towards the patina.</p> <p>Pore sizes that match the grain size of the rock. Mechanical damage is likely to produce larger voids that cross-cut multiple grains.</p> <p>Retention of internal structures, such as exsolution lamellae and cleavage or twin planes that are replaced by</p>		

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			<p>other phases. Mechanical damage is unlikely to produce this distinction. The formation of gaps between some mineral pairs, such as quartz--feldspar, but not others, such as quartz--quartz (e.g. Figures 4.2-19 and 4.2-27(b,c)).</p> <p>The presence of precursors to mineral dissolution, such as the formation of edge pits and irregular grain boundaries (e.g. Figure 4.2-27).</p> <p>The presence of soft minerals, such as sheet silicates (clays, white micas) adjacent to voids. Mechanical damage would remove these soft minerals as well as the adjacent phase.</p> <p>In contrast, features produced by mechanical damage typically form parallel to the patina--weathered rind interface, cross-cut multiple grains, and show planar or close-to-planar interfaces.”</p>		
2	Executive summary	Clearly articulated and well explained.	Noted		
Introduction					
3	Section 1.1 Background	Seems clear to me.	Noted		

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4	Section 1.4	Abbreviations missing include: APT, pe, p-value, p(d), Si.	APT, pe and p-value have been added to Table 1.4-1. Si has been added to Table 1.4-2 p(d) is our notation for the porosity of the rock at a depth of d microns below the rock surface. This is not a standard technical term. It is defined in Appendix G.2.5.	OK	✓
Laboratory analysis of field samples					
5	Section 4.2	It's unclear whether the table of contents in the final report will list all subsections, but the more detailed "Contents" I was sent prior to accepting this role was missing 4.2.10, 7.6.4, 7.6.5, 7.6.6, 7.7.3, 7.7.4 (noting that 7.7.2 needs to be renumbered 7.7.4).	The Table of Contents now includes subsections.	Good	✓
6		Initially, the figures appear close to the text that references them. Unfortunately, early in this section, the figures become distanced from the text requiring considerable scrolling between pages and pages of intervening material. Please place each figure as close as possible to the text that first cites it.	Figures are now positioned as closely as possible to the corresponding text.	Thanks	✓
7		Throughout this section (and critical to the report) is some tension between the production of voids via mineral dissolution and the possibility that at least some of the porosity may be an	Section 4.2 has been thoroughly rewritten and now includes detailed discussion of porosity. In particular, section 4.2.5.1 discusses the features of voids produced by dissolution, and	Excellent	✓

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		<p>artefact of the plucking of mineral grains during thin-section preparation. It may be useful to have a separate sub-section early on that addresses this directly. It should include the types of features expected of voids produced via dissolution (textures, the minerals most prone to dissolution etc) and the differences between these and what might be expected from voids produced by sample preparation. There are numerous observations made (and images provided) that clearly show dissolution features (e.g., the patina growing into void spaces). There may perhaps(?) be examples of weathering that produce soft/brittle material that could be preferentially removed during thin-section preparation, but this would (at its most extreme) be a subset of the voids observed.</p> <p>Might it be possible to compare the original rock billet to the photomicrographs in Figures 4-15, -25, -32, 38 and -44 to see if polishing has enhanced the pore spaces in these slides? Also, in Figure 4-43, are the delicate mineral 'linings' in the void spaces new minerals growing into holes, or the remains of minerals that have been variously plucked out of</p>	<p>those produced as sample preparation artefacts.</p>		

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		<p>these domains? What are those needle-like /skeletal minerals?</p> <p>There is a need to be more explicit about the causes of pore-space development in the different lithologies (particularly the granophyre given its significance to this report). Can the amount of 'plucking' be estimated or can it be essentially ruled out as 'insignificant'?</p>			
8	Section 4.2.5.1 p.49	Sentence commencing "The porphyritic character...": 's' is missing in phenocrysts.	Done	Good	✓
9	p.50	Caption to Figure 4-7. The sample name should be included. Indeed, the names of the samples illustrated throughout this report should be available in the caption (e.g., 4-8, 4-9, 4-11...etc).	Done	Good	✓
10	p.50	The first reference is made to changes in the porosity as a result of mineral dissolution. This is followed on p.51 by a comment acknowledging that the mechanical removal of minerals in the more highly weathered domains should be considered. It would be useful to add a separate section somewhere early on to 'consider' this more thoroughly. There is good petrographic evidence to support selective dissolution (provided at	New text added (see comment 1) to summarise the petrographic evidence for selective dissolution.	Acknowledged.	✓

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		various points in 4.2) but it would be helpful if it could be pulled together somewhere early in this section (even if it means pre-empting some of the observations described and illustrated in subsequent sub-sections). Also, in these highly weathered domains, are there soft alteration minerals that would be expected to be 'mechanically' plucked out if this were the explanation for the increased number of holes observed? If these remain, might this not provide further evidence in support of dissolution? The development of porosity is clearly a crucial aspect of this report, so it needs to be dealt with convincingly early in this section.			
11	p.52	Typos "flacking" (flaking), "stoichiometric" (stoichiometric)	Typos fixed	Good	✓
12	p.52	Ilmenite is described here but is not included in the histogram in Fig 4-16. It is grouped with hematite/magnetite?	Ilmenite was accidentally omitted from the histogram in Fig. 4-16. Its average abundance is 0.23 wt.%	OK	✓
13	4.2.5.2 p.53	Odd use of the term "primary" here. I think the text is referring to the "dominant" or "phenocryst" or... phases as the sentence reads "...primary and groundmass minerals...". Many of the minerals are not 'primary' in the petrogenetic sense (i.e., they are not the minerals that crystallised from the original magma).	Agreed. "Primary" has been replaced by other terms, such as 'minerals present within the fresher rock'	Good	✓

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		These rocks have experienced low-grade metamorphism with associated changes in mineralogy and/or mineral compositions. I'd suggest these occurrences be reworded.			
14	p. 55	Second paragraph: "weather" should be "weathered" or "weathering".	Done	OK	✓
15	4.2.5.3	Figure 4-15. The second last line of the caption refers to "In (c), ..." when it should be (d).	Done	OK	✓
16	4.2.5.4 p.59	In the sentence commencing "Dolerite is dark green, ...", there's a space missing in the text in brackets.	Space added	OK	✓
17	p. 60	The last paragraph in this section refers to rare halite. How would halite survive thin-section preparation at all (assuming water was used)?	Halite identification is part of the XRD analysis, which does not use the same preparation techniques as thin-section preparation. No water was used in the XRD sample preparation.	OK	✓
18	Section 4.2.5.5 p.61	Typos: Paragraph 2: "then" should be "than". Another mention of halite (really?) and reference to TiO ₂ which should be 'rutile'.	Typo fixed. Same response for halite as 17. The TiO ₂ phase could be rutile or anatase and this has been clarified in the text "and a phase identified as TiO ₂ that is most likely rutile or a weathered equivalent such as anatase"	Good	✓
19	Section 4.2.6.2 p. 63	Paragraph 3. "...except made for sample...". Is the word "made" supposed to be something else or simply remove it?	Removed	Good	✓

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20	p.73	Figure 4-25 (and similar figures of this kind...e.g, 4-15, 4-32, 4-38, 4-44): The caption refers to panel c, but it should be d. Also, where are kaolinite, pyrophyllite and patina captured in the key to panel c?	Done. Captions fixed. The legend to panel (c) shows only the most abundant phases and this is now captioned as follows: (a) plane-polarized photomicrograph (b) TIMA-BSE (c) TIMA phase map showing the most abundant phases, and (d) distribution of clays and precursor minerals, shown in the legend as rock forming minerals, for the same portion of sample RS09_BAS, displaying the transition from fresher rock to weathered rind domain. In (b), the red colour shows the region of the sample with epoxy resin, highlighting the enhanced porosity of the domain. In (d), clay minerals correspond to browner colours shown in (a). Clays include phases identified at the TIMA such as muscovite/illite, K-alt-chlorite, kaolinite and patina.”	OK	✓
21	p.78	In addition to the issues with the caption noted above, the wrong sample number is included here. These are images of one of the granites, not a gabbro. Replace with the correct sample code.	Done.	Good	✓
22	p.81	Figures 4-35 (caption is oddly worded) and 4-36 (I'm not seeing what the caption describes?)	Done. Fig. 4-35. Caption rewritten to read “Plane-polarized (a) and crossed-polarized (b) photomicrograph of the transition from the weathered rind to the fresher rock in dolerite. Despite the bright	Good	✓

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			illumination, the section is quite opaque due the presence of fine-grained Fe-oxides and clays.” Fig 4-36. Agree that the “Brighter rims of chlorite and Fe-oxides form rims surrounding quartz.” Are hard to see. Words deleted.		
23	p.82	Figure 4-37. What else is in the material that surrounds the dissolved augite? The caption mentions chlorite and finer-grained labradorite...but what's the bright white and pale grey material?	Done. Added to caption “The brighter material is Fe-rich clays or oxides.”	Good	✓
24	p.85	Figure 4-40. Is the rock dominated by plagioclase? Panel b makes it look like it's a similar amount of plag and cpx?	Done. Overall, the modes are about 50:50 but augite forms phenocrysts and plagioclase dominates the groundmass, or matrix. Clarified in the text “The matrix of the rock is dominated by fine-grained crystals of plagioclase, which surrounds anhedral augite grains.”	Good	✓
25	p.92	Figure 4-48. The change to the colour scheme (fresh rock now in orange compared with earlier histograms) is confusing. Make all colour schemes consistent. Also, in this figure, either label each of the 4 panels with the phase name or indicate in the caption which panel represents which mineral.	The colour schemes for all these types of plots are now consistent. The caption has been updated to include (a) to (d).	Excellent! Good.	✓ ✓

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26	p.93	Figure 4-49. The top panel is slightly offset from the element maps below it making comparisons more difficult. Realign the top panel.	Noted. This image was created by a researcher who has recently left the project for another job. This will be fixed before the final version of the report.	Good.	✓
27	p.94	Figure 4-50. The last sentence in the caption needs to be reworded. Also, although I can see 'hot spots' in Na here and there, they also occur within the feldspar, so I'm unclear what the significance is of a few small patches close to the cpx?	Caption rewritten to read "SEM-EDS map of gabbro acquired over altered augite (central grain) and labradorite (surrounding grains). The distribution of Mg, Fe, Na and Al are heterogeneous. Localised enrichments in Na may represent albite or other Na-rich alteration phases."	OK	✓
28	p.95	Figure 4-51. Is iron expressed as Fe ₂ O ₃ or FeO here?	Done. Its FeO. Caption rewritten to clarify. "A comparison of the average wt% FeO in augite rims in the fresher rock and weathered rind. The weathered rind is Fe-depleted relative to the fresher rock."	OK	✓
29	4.2.7.1 p.96	First paragraph...change one of the references to "report" to something else (e.g., include?). Second paragraph last line, "then" should be "than".	Done. New text reads "The observations included here only report details of" Typo fixed.	OK	✓
30	p.97	The last line in the caption needs to be reworded.	Done. Caption reads "Macro length-scale characterisation of patina. Images (a) and (c) are of unpolished cut surfaces of rock billets showing cross sections through AQ11_GPH and AQ15_GBR, respectively. Images	OK	✓

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			(b) and (d) are top views of the billets shown in cross-section in images (a) and (c)"		
31	p.98	Figure 4-53. It's unclear what the arrows are pointing to in panel d. Can this image be enlarged for clarity?	Not done. The arrows point to quartz and clays, which are shown in detail in subsequent images. If we changed the number of panels of this figure to show (d) at larger scale then the other panels would get smaller. Have added a remark that "Features are shown at higher resolution in subsequent images."	I guess this is OK as a work around. At least it's clearer that the reader shouldn't worry about what the arrows are pointing at in panel d.	Ok
32	4.2.7.2 p.99	Paragraph 1. There's an extra hyphen in the brackets in line 4 that should be removed.	Hyphens changed to "to"	OK	✓
33	p.99	Paragraph 4. "common" should be "commonly".	Fixed	OK	✓
34	p.99	Paragraph 5. There's odd spacing in some of the references cited. Either a space missing or a space where there shouldn't be one.	Spacing fixed	OK	✓
35	p.100	The caption to figure 4.54 needs a little additional text for clarity for panel c (...of patina wraps around the edge of a fractured...). Also, there's no need for the word "Please" in the last line. Simply: Note the ...	Done. Text added to read "a thin film of patina (bright rim, around 5 µm thick) wraps a fractured quartz grain that protrudes from the surface of granite sample AS02_GRT" "Please" removed	Good	✓

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36	4.2.8 p.102	"...a small number of samples WERE selected" not 'was'. Also 'based on' is used twice in this sentence which is clumsy.	Changed to "were" Second "based on" deleted	Good	✓
37	p.103	Figures 4-57 and 4-58 are both missing in the document I received. Only their captions exist, so I cannot comment on these images.	These images are present in our copy of the document, and in the annotated copy received from the reviewer.	In the revised version they are there and the captions are clear. All good.	Agreed.
38	p.110	I think the sample number in the caption is wrong. The label on the on the image is "granite" whereas the label in the caption refers to dolerite.	Done. Caption was incorrect, now fixed. This is sample AQ07_GRT.	Good.	✓
39	Section 4.2.8.1 p.111	Various typos...missing words. <ul style="list-style-type: none"> • Paragraph 1. "...that ARE...also observed. • Paragraph 2. "...Na and K ARE enriched...". • and "...K intensities AS they become" (not and). • Paragraph 3 "destruction" (not distruction). • And "The presence of a elongate" should be "...of AN elongate ..." 	<ul style="list-style-type: none"> • Added • Added • Changed • Changed • Done 	Good	✓
40	p.112	The caption to Figure 4-66 should refer to the sample number, despite reference to Figure 4-61.	Done, sample name added.	Good	✓

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41	p.114	Paragraph 2. Are there hyphens missing in the concentrations of SiO ₂ and Al ₂ O ₃ ?	Done. Yes. Hyphens added.	Good	✓
42	p.115	The caption to Figure 4-68 should refer to the sample number, despite reference to Figure 4-62.	Done. Sample name added.	Good	✓
43	p.116	Figure 4-69. The current format has 6 legends/keys which run together and are confusing to follow. Can we have a single legend/key for this figure?	Done.	Excellent!	✓
44	4.2.8.2 p.118	Paragraph 1 "...in the appendix" Which one?	Added appendix section number	Good	✓
45		Paragraph 2 needs rewording. Also, Figure 4.58 is missing in the document I have.	Done. Paragraph reworded. "The area of patina investigated for samples AQ08_GPH and AQ11_GPH corresponds to the area shown in Figure 4.2-50(a) and 4.2-54, respectively. Microstructurally, samples AQ08_GPH and AQ10_GPH show similar patina that is developed on localised topographic irregularities, extends downwards into the weathered rind, and fills features interpreted as pre-existing rock fractures and/or dissolution cavities."	Good	✓
46		Paragraphs 3 and 4 need rewording for clarity (as indicated on the marked-up version of the document).	Done. Changes made as requested.	Good	✓

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47		Pages 119 etc... I'm having a difficult time determining what constitutes visual "heterogeneity" in the figures over these pages. It's also unclear why some elements are included and others not...and some replaced by other components (e.g., in Figure 4.71 CaS+ is replaced by Ni compared with Figure 4.70). See comments on marked up version of the document.	Done. Captions simplified. The choice of ions was made to show the most interesting features for each sample, so the word 'selected' has been added.	OK...the wording is much better (and more objective).	Thanks
48	p.124	Rewording as indicated.	Done.	OK	✓
49	p.125	It's a little difficult to follow what we're looking at in this figure. Panel a) seems to be enclosing a patch that includes a triangular black area surrounding a brighter mineral 'island'. I initially thought this bright mineral 'island' was the albite (i.e., we were shown its location in the rock) which was confusing given the location of the red rectangle in panel b). Perhaps the larger albite grain in a) could be roughly outlined and labelled to make this clearer. The caption to panel b) is also a little confusing "the irregular rims of albite..." suggests the bright edges are rims of albite formed on some other mineral.	Added "Albite is the mid-grey mineral that occupies most of the field of view." To the caption of (a). Caption clarified. "region of TEM lift out, showing that the rim of the albite grain has a higher BSE brightness than the core." We have been asked to shorten the captions, so it is not possible to add this material. Label added for the curved lines. I can't work out what other features are not evident. Happy to add labels if specific features are requested.	Yes, that's helpful. Ah, yes, that makes more sense. Although, if the majority of panel a is albite, what's the black stuff? It's just that the 'core' of this single(?) grain is not the same as the 'interior' domains of the irregularly shaped albite (which I think is what panel b is trying to show?). Here, within a single	Thanks The black material has been labelled resin and the interior and rim of the albite grain have been labelled as requested. Core has been replaced by interior elsewhere in the document. Resin has been labelled at the top and the bottom of panel (a) as requested.

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		<p>The first part of the caption should briefly describe the general context (as noted in the accompanying text). i.e., “TEM nanoscale images of a relict albite grain in sample xxx. The grain preserves evidence for extensive alteration and dissolution. a) location within the grain...etc...”</p> <p>The various domains/features described in the 3 panels should be labelled.</p>		<p>grain, the edges adjacent to the black stuff are brighter than the interior parts of the grain. The use of the word ‘core’ is misleading and confusing (at least to me).</p> <p>If the caption can’t be lengthened to explain this, perhaps add labels to:</p> <p>Albite and the black stuff in panel a (labelling whatever the black stuff is...resin? ... filling existing porosity?).</p> <p>“Albite rim”, “Albite interior” (not core) and ‘the black stuff’ (resin?) in panel b.</p> <p>Label the areas of resin (top as well as bottom?) ...and whatever the tiny bright euhedral looking thing is (even if it’s some unknown inclusion). The yellow arrow is great.</p>	

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50	p.127	<p>The Mn-rich bands in Figure 4-79 do show elevated K, Ca etc except for the very top where the Mn content is low (black) whereas the brightness for these elements (and Al) is very high. This should be addressed in the caption.</p> <p>It would also be useful to have a sense of where this area is located within the sample.</p> <p>Table 4-10. How is “error” being defined here?</p>	<p>The Mn-rich bands are weakly enriched in these elements, and this has now been stated.</p> <p>However, we are unable to make the captions longer because of other constraints.</p> <p>We are unable to show the area of lift-out within sample EX03_GPH without adding another figure to the report so we have added the phrase “(not shown)” to the text.</p> <p>The error is one standard deviation.</p>	<p>OK</p> <p>OK</p> <p>OK</p> <p>Good</p>	<p>✓</p> <p>✓</p> <p>✓</p> <p>✓</p>
51	p.128	Two typos in the caption to Figure 4-78 need to be corrected as indicated.	Typos have been fixed.	OK	✓
52	p.129	<ul style="list-style-type: none"> • APT is missing from the listing of acronyms and their definitions. • Rewording as indicated. • Clarification regarding the nature of AQ15_GBR as indicated. 	<ul style="list-style-type: none"> • APT added to Table 1.4-1 • Done. Reworded as indicated. • Done. Relative levels of NO2 from the passive samplers added to the text. 	<p>OK</p> <p>Good</p>	<p>✓</p> <p>✓</p>
53	p.130	Additional labelling of Figure 4-80 to improve clarity.	Done. Specimen info added to the caption.	OK	✓
54	p.131	Did the atom probe really detect Tc? This would be most unusual given that it doesn't exist in nature. The	Done. Probably not. Tc has been deleted. Suggested changes to the text made, or text rewritten.	Good	✓

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		presence of Ru and Os are also very surprising. Please check that these aren't analytical artefacts. Some small changes to the text could improve clarity (as marked).			
55	p.132	Figure 4-81. A fascinating figure but rather challenging to follow (see comments). The caption is confusing as it refers to atoms, ions and molecular species. What does the atom probe detect...if it's ions, then references to 'atoms' should be changed to 'ions'. Also, how are molecular ions (interpreted to be water) detected?	Done. Clarified with "Each dot in the reconstruction represents one detected charged species, which may be a mono-atomic ion or a molecular species, and which is assigned to a particular element or molecule (e.g., Na, H ₂ O). For simplicity, not all inferred atoms are displayed ...". These words are also added to the text.	Perfect, thanks.	✓
56	p.133	Figure 4-82. See comments above regarding ions vs atoms.	See 55. Same text added to the caption.	Good	✓
57	p. 134-135	Table 4-11. How do the M-numbers relate to the target sites in Figure 4-80 (perhaps label the targets in that figure). The totals for M9 and M16 are low. There is mention in the text about missing peaks for complex molecular ions and the data being preliminary, but 5 of the 7 samples return totals close to 100% so some comment on the cause of the low totals might be useful here.	Only needle M4 is shown on Fig 4-80, and M4 has now been added to the caption of Fig. 4-80, and we have added "Specimen M4 is shown on Figure 4.2-76(d)." to the table caption. Comment on low totals provided. "Most of the totals are above 98%, but specimen M9 returned a total of ~70 wt.% and specimen M16 returned a total of 83.8 wt.%. These low values indicate that some peaks have not been assigned to elements or species, and further work is necessary to	Good Much clearer.	✓ Thanks

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		How is the H in water accounted for in these analyses? Might this go some way to explaining the low totals in M9 and M16?	<p>identify missing peaks that may represent a combination of complex molecular ions. H and H-bearing species are detected by ATP, so their presence does not explain the low totals. Because of the low totals, these results are considered preliminary.”</p> <p>H is detected by ATP directly, either as a mono-atomic ion or as molecular species such as H₂O. This has been mentioned in the text – see previous response.</p>	OK	✓
58	Section 4.2.9.1 p.136	There are various examples of references that include brackets where these are unnecessary. See highlighted text.	Citation style now conforms to the reviewer’s recommendation (i.e. citations within a parenthesised clause are not typeset with parentheses around the year). Individual citations have been corrected as indicated by the reviewer.	Good	✓
59	p.137	In my view this section would be stronger if a clearer discussion of the evidence for the development of pore space was provided much earlier. This section could then refer to the earlier summary. See comments.	Done. See response to item 1	Acknowledged.	✓
60	Section 4.2.9.2 p.138	“sensu lato” should be in italics. Last paragraph first line: suggest rewording to “rather than the underlying rock”.	Spelling fixed and text italicised	OK	✓

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61	p. 139	Last line of first paragraph. Remove additional bracket Also, at the end of the third bullet point, "in situ" should be in italics.	Citation has been fixed Done	OK	✓
62	p. 104	Second line of the last paragraph: "pattern" should be "patina"	Changed	OK	✓
63	p. 141	Paragraph commencing "Sub-surface voids...". This paragraph again refers to the possibility of void production via mineral plucking during sample preparation. Would these look the same as voids produced by dissolution? Please see earlier comments about having a separate sub-section early on to remove doubt about what is being shown/said here. This part of the text could then refer back to the earlier section.	Done. Text modified to read "However, based on the criteria listed in Section 4.2.5.1, it is likely that part-filled sub-surface voids are likely to reflect weathering processes rather than preparation artefacts."	Good	✓
64	Section 4.2.9.3 p. 141	Second last line "chemistry" should be "chemical" (see comment).	Changed	OK	✓
65	p.142	Typos as indicated by highlighted text. 'Pattern' should be 'patina' and 'exclamation' should be 'exhumation'.	Typos fixed	OK	✓
66	p. 143	Is the first line on this page a new bullet point (point 6) or is it the second line of point 5 on the previous page? It is possible that corrections suggested here will alter the formatting and this will become clear in the final document.	Yes, this sentence is part of point 5. Now both sentences for this point is on the same page.	Good	✓

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		I think the word “different” is missing in the first paragraph. See highlighted text.	“different” added	OK	
67	p. 144	Figure 4-83. The caption refers to a ‘red box in (a)’ but there is no red box on this image in my copy of the document. Add box.	Done. Red box added.	Good	✓
68	p. 145 p. 146	The caption to Figure 4-84 shows an explicit example of where thin section preparation can physically modify the sample. This could be used in a very early section to acknowledge this as a potential problem and describe what textures/features result. Are there any fine crystals of anything retained in the void spaces? Are the edges angular or cusped etc? Similarly for Figure 4-85. What is believed to be physical modification resulting from sample prep, and what is ‘natural’ in these images?	Figure 4-84 has been moved to the pore space discussion as suggested. Reference to the different features considered characteristic of mineral dissolution has been provided in the granophyre section. Caption has been modified to read “Note the grain-scale features and complex void--mineral interfaces that support mineral dissolution rather than void formation by mechanical preparation.”	Good Good	✓ ✓
69	Section 4.2.10	This subsection doesn’t appear in the more detailed Table of Contents that I was sent. It is extremely frustrating to have to scroll through so much text to find the figures being referred to and then find a way back to the text that cites it to	We have fixed the Table of Contents to include subsections, now displays 4.2.10. The position of the Figures now follows the text closely, where possible.	Good Thanks!	✓ ✓

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		continue reading. Please locate each figure close to the text that first cites it.			
70	Section 4.2.10.2	It might also be useful here to reiterate that not all of the minerals have been identified in all of the materials (particularly in the patina) and thus any modelling is necessarily a simplification as we cannot predict the stability of minerals we haven't identified.	Done. Suggested text added.	Good	✓
71	Section 4.2.10.3 p. 148	A couple of typos to be corrected. I'd also recommend breaking the very long sentence in the first paragraph into at least two (see highlighted text). The first sentence of the second paragraph could be clearer (see suggested replacement text).	Typos fixed. Section rewritten with shorter sentences. First sentence rewritten to state that "The modelled mineral assemblage of the patina, weathered rind, and fresher rock were based on the TIMA and XRD results."	OK OK OK	✓ ✓ ✓
72	p. 149	Two highlighted areas of text need the addition of the word "in".	Added	OK	✓
73	p. 149-150	The last paragraph on page 149 and first on page 150 need to be reworded as they are difficult to follow in their current form. See highlighted text.	Done. Reworded as suggested or rewritten.	OK	✓
74	p. 150	The description of Simulation 2C doesn't make sense. Is the second	Done. Correction as suggested.	OK	✓

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		reference to SO2 incorrect? Should it be NO2?			
75	p. 150	The description of Simulation 2D has a hanging reference to "...a fixed concentration of" without explaining 'of what'? Possibly a typo left from some cut-and-paste?	Done. Corrected as suggested.	OK	✓
76	Section 4.2.10.5	In the first line of this section, the term 'pe' appears. Although it is defined here, it should also appear in the abbreviations/definitions summary at the start of this report.	Done. The term 'pe' is now defined in the text and in the table. Has been added to Table 1.4-1.	OK OK	✓ ✓
77	p. 151	Last paragraph. The saturation index "SI" should also be included in the abbreviations/definitions list at the start of the report.	Has been added to Table 1.4-1	OK	✓
78	p. 153	I'm unclear why 'alkali feldspar' is used when it seems the mineral being referred to is K-feldspar? It's a little confusing given that Albite is an alkali feldspar (being the Na-endmember). Can references to alkali feldspar be replaced by K-feldspar or is there some rationale for retaining it in this section on modelling (or indeed elsewhere)?	In this section, alkali feldspar has been switched for K-feldspar, or potassium feldspar at the beginning of sentences. Alkali feldspar is used elsewhere to refer to feldspars that can be potassic or sodic.	Good, this is much clearer.	Thanks
79	p. 153	Some of the simulations are missing from Figure 4-92 so it's difficult to	Done. Corrected as suggested.	Good	✓

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		reconcile the text on this page with the figure. In the first line of “Mineral phase stability” I think the text should read “unpatinated weathered gabbro” not “unpatinated fresher gabbro”.			
80	Section 4.2.10.6 p. 154	The first sentence in the second paragraph is very long. Suggested rewording provided for highlighted section.	Done. Paragraph has been rewritten with shorter sentences.	Good	✓
81	p. 154	The first sentence of the paragraph commencing “Albite, anorthite, and diopside...” is convoluted and confusing. Labradorite is not used in the modelling, albite and anorthite are. Reword this sentence. Remove apostrophe in it's.	Done. Sentence reworded. “Albite and diopside are considered to be proxies for labradorite and augite for the purposes of the modelling.” Apostrophe removed	Good OK	✓ ✓
82	p. 154	Under the heading of ‘Adding a time dimension’. It is unclear what “the hydrodynamics and geometry of patina-precipitation interaction” means. Please clarify.	Done. Definitions added. “Here, hydrodynamics refers to the rate of fluid infiltration processes. The geometry of patina--precipitation interaction refers to the area and shape of pores within the patina that are wetted by infiltrated fluids.”	Excellent!	✓
83	p. 154	This same paragraph refers to 100 cubic cm of fluid and 2 kg of patina-covered weathered rind...but the simulations used 1 litre of fluid (i.e., 1,000 cubic cm). Please check the	Done. Simulation set 1 used 1000 cm ³ of fluid and simulation set 2 used 100 cm ³ of fluid.	Good	✓

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		calculations to ensure the text properly presents the results here. Reword the start of the sentence that follows regarding the scaling of this result to smaller volumes of fluid and patina.	The whole section has been rewritten and sentences shortened.	Good	✓
84	p. 155	First line, "observation" should be plural (add 's'). Paragraph 2: There's a one-line acknowledgement relating to the impact the microbiome may have on variables such as pe and pH "on lengthscales...". But the microbiome may have a much bigger impact on the composition of the fluid interacting with the patina and rock surfaces, as well as the availability of various elements. I wonder if a little more could be acknowledged here, perhaps referring to a section within the report describing the microbiome (which I have not read). This also relates to the last paragraph on this page (see comments).	Added Done. Additional text added relating to the microbiome. "Through these parameters, and the formation of organic acids, the microbiome may impact the composition of the fluid interacting with the patina and rock surfaces, and the availability of various elements. The results of research into the microbiome at Murujuga are presented in Section 4.3."	OK Perfect.	✓ Thanks
85	p. 155	Paragraph 3. The last line needs to be completed.	Done. Last line modified.	OK	✓
86	p. 156	Various references to 'alkali feldspar' when the phase is K-feldspar. Why not simply use the more specific term and remove any ambiguity/confusion?	Done. Switched to K-feldspar	Good	✓

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87	p. 156	Bullet point 2: The wording is confusing here (transitioning from fresh rock to fresher rock?!). A suggested form of words has been included to replace the section highlighted.	Done. Rewritten as suggested.	OK	✓
88	p. 156	Correction of “phases” to “elements” in the second last paragraph.	Done. As suggested.	OK	✓
89	p. 157	In ‘Suggestions for further work’ is there any plan to investigate the changes the microbiome make to their environment? e.g., do they expel molecular ions that may enter solution and enhance/reduce the dissolution/precipitation of various mineral species in the underlying weathered/fresh rock?	Done. We thank the reviewer for this excellent suggestion. An answer to this question would provide valuable insight. However, such investigation is challenging and is beyond the scope of the present project. We have shown this “Ideally, it would be possible to investigate the changes that the microbiome makes to its environment. For example, the microbiome may catalyse reactions, or locally alter element concentrations by expelling molecular ions that may enter solution. Changes in fluid composition may enhance or reduce dissolution and precipitation of minerals in the patina and in the underlying weathered/fresh rock. However, such experiments are challenging and beyond the scope of the project at the current time.”	No worries. This way of handling the issue is fine and makes it clear the problem is acknowledged, but out of scope.	Thanks
90	p. 157	In ‘Key findings’ These findings are immensely important, but I wonder what the impact of the microbiome	Done. Section rewritten and additional text added in response to point 89.	The first finding needs to be tweaked to remove the	Fixed. “Hold” deleted.

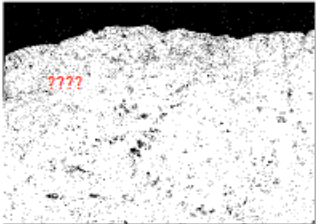
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		might be. For example, is there sufficient evidence from the existing work to suggest the microbiome might have only a second order impact and that the results from the current modelling etc are robust? The suggestion in the final paragraph on this page suggests not, but this could be made more explicit (perhaps in the re-writing of the very long sentence in bullet point 5.		clumsiness in "...is unlikely to hold be attained readily...". Is the suggestion that "equilibrium is unlikely to be attained readily, and if it is, it is unlikely to retained/hold(?)..." The third bullet point doesn't need the comma after 'equilibrium'	Thanks. The comma has been removed.
Statistical analysis of component studies					
91	Section 7	The heading for section 7 in the table of contents reads "Statistical analysis from each component study". Not sure which is correct but highlight it here in case the report needs to be changed.	Title now reads 'Statistical analysis of data from each component study'.	OK	✓
92	Section 7.6 Inorganic chemistry – porosity p. 259	It needs to be explicitly stated up front that this section deals only with granophyre samples...and why this is the case. Will the other lithologies be investigated this year?	Text has been added to explain that the analysis focuses on granophyre. We consider it very likely that the porosity analysis will be strongly dependent on rock type. This means that in the exploratory phase, the analysis should be done separately on each rock type. The SEM preparation and imaging is very time-consuming, and the images	Good. These comments are all entirely sensible and are now hopefully clearer to readers of this report.	Thanks

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			<p>are of such high resolution that the computational and statistical analysis is time-consuming. Hence there was limited data available at the time of writing.</p> <p>The first three SEM images produced were from Gabbro samples. However subsequently the other 28 images were from Granophyre. Thus, the images available at the time of preparation of the report were overwhelmingly from Granophyre.</p> <p>Curves of porosity versus depth (for individual images) were computed for all the SEM images available before the cutoff date. These curves are shown in the Appendices to the report.</p> <p>For the analysis of spatial variation in porosity, it made sense to analyse Granophyre first, because (a) the available data were mostly from Granophyre, and (b) Granophyre is widespread across Murujuga and is therefore less likely to be confounded with spatial location in our analysis. Other rock types were reportedly more friable/fragile when prepared for SEM.</p> <p>Another subtlety is that, in the spatial analysis of variation in porosity, it is</p>		

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			<p>necessary to 'compare like with like', which implies that a single set of algorithm parameters should be specified for the image processing steps (opening radius, smoothing parameters etc) and that the analysis should use only one of the options for the single-numerical-value index of porosity. These decisions were too difficult to make for Gabbro with the small sample size available.</p> <p>It is certainly the goal to include all rock types in the analysis. Ultimately that will be done using statistical modelling in order to rigorously incorporate information from those rock types which are rarer and more spatially concentrated. That will happen sometime in the coming year.</p>		
93	Section 7.6.1. p. 259	Perhaps label the image to point out the void spaces filled with resin.	<p>The figure (now 7.6-1) has been annotated to point out one of the void spaces and the external space.</p> <p>There are many small voids in this sample, and the black areas of the binary image show all areas that have been determined to be void for a given intensity threshold. Different thresholds were tried to determine their impact on calculating porosity and results are shown in the appendix.</p>	Good	✓

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94	p. 259	Caption to Figure 7.59. I know it's clarified in the text, but it's a little confusing to refer to this in the first instance as a "microscope" image as this may suggest it's an optical image taken in plane-polarised light. This is important because opaque phases would appear black in such photomicrographs which would negate the approach. Simply reword as 'Scanning electron microscope, backscattered image of sample granophyre AQ03 in thin section.	The term "microscope" has been replaced by "scanning electron microscope" throughout, where appropriate, and images are labelled as backscattered electron images.	Good, thanks.	✓
95	p. 259	The text below Figure 7-59 refers to "...determine how the rock porosity changes with depth way from the rock surface" and goes on to say "...and investigate the spatial variation in porosity." How are these different? Does the spatial variation relate to the individual sample...or is this referring to the spatial variation in porosity across Murujuga? Please clarify this sentence.	This has been clarified. Spatial variation refers to large scale trends across Murujuga.	Good, thanks.	✓
96	p. 259	Last sentence. I think the word "summarised" is more appropriate than "sketched" here.	Agreed and done.	OK	✓
97	Section 7.6.2 p.260	In other parts of the document (including the text immediately above this subsection) lengths are given in xxxmetRES. In this section	We thank the reviewer for drawing attention to these details. We have fixed the whole document: if there is an exact number then we use μm , e.g.	Great, thanks.	✓

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		xxxmetERS appears to have been adopted. Change the text in this section to xxxmetres. Similarly, while I think 'microns' is fine, elsewhere 'micrometres' has been used. Choose one and stick to it.	20 µm; If describing something then we use micrometres.		
98	p.260	Paragraphs 4 and 5. Unlike section 4.2, some of this is written in the first-person plural. Reword to remove 'our, we' etc and tighten up the wording elsewhere (see comments).	Agreed. The document has been edited throughout to ensure that the passive voice and third person are used.	Good	✓
99	p. 260	Paragraph 5 refers to Figure 7-60, but this figure is missing in the version of the report I have. This is also true for panel a) in figure 7-61.	<p>These 2 figures are present in our copy of the document and in the annotated copy received from the reviewer. They are reproduced below.</p> <div data-bbox="1070 900 1413 1150" data-label="Image"> </div> <p data-bbox="1025 1198 1458 1238">Figure 7-60: Binary image obtained by thresholding Figure 7-59. Binary value 1 (white) interpreted as rock; binary value 0 (black) interpreted as void space. Include sample code and rock type.</p>	On my copy there are the red question marks and red text below the caption...but neither of the figures appears?	Corrected.

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			<p>a) b)</p>  <p><i>Figure 7-61: Distance transform. (a) binary image; (b) distance transform of (a). AQ03, field 4 of 4.</i></p>		
100	Section 7.6.3 p. 262	Again, reword to remove references to the first-person plural. See suggested text.	Done.	OK	✓
	p. 263	<p>In Figure 7-62 a), how are linear features managed in 3D calculations? Does the approach being applied handle such features? Again, I'd suggest rewording the caption to the SEM backscattered image rather than calling it a "microscope image" as this might be misleading.</p> <p>In panel b) of this figure, the text and data symbols are difficult to read/distinguish between and the resolution seems poor.</p>	<p>Linear features do not require specialised treatment. Further detail about the methodology has been added in Appendix G.2.</p> <p>"Microscope" has been changed to "scanning electron microscope" throughout.</p> <p>All images have been revised to improve quality.</p>	<p>Good, thanks.</p> <p>OK</p> <p>Excellent.</p>	<p>✓</p> <p>✓</p> <p>✓</p>

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101	Section 7.6.4 p. 264	Again, the first-person plural referenced in the text need to be corrected. See suggested rewording. There's also an issue with the use of past vs present tense. (e.g., "were proposed in Appendix...", but of course they still are proposed in that Appendix, so the tense should be present).	Grammar has been corrected.	OK	✓
102	Section 7.6.4 p. 264	The last line in section 7.6.4 indicates that "Porosity index values for all samples are reported in Appendix H1". I looked for a table of values but can only find maps similar to Figure 7-63. Are the values tabulated anywhere? What are the uncertainties on these values (noting the 3 significant figures and 4 decimal places used in the key to Figure 7-63)?	<p>It was originally intended that all individual values of porosity index would be tabulated, but the tables are prohibitively large. Data files containing the porosity values are available on request.</p> <p>The last line in section 7.6.4 has been amended to state that these values are 'depicted graphically' rather than 'reported'.</p> <p>The 3 decimal places used in the figure legends were a software artefact. They are now much simpler and usually involve at most 2 significant figures.</p> <p>Uncertainties on individual porosity values are not yet available (but will be obtained in the near future). These are not required for the Monte Carlo approach used here.</p>	All good, thanks.	✓

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103	Section 7.6.5 p. 264	Remove text in the second sentence (as shown) and correct the typo in the caption as highlighted.	These errors have been corrected as suggested, thank you.	OK	✓
104	p. 265	Correct typo (upper case G) as indicated in line 1.	These errors have been corrected as suggested, thank you.	OK	✓
105	Section 7.6.6 p. 265-266	I struggled with the text in this section and feel there must be a better way to explain the approach being used for a general reader of the report. I'm also a bit confused as I thought the null hypothesis was that the mean porosity has the same constant value everywhere. So, it seems odd that where this fails (i.e., the orange shaded area in figure 7-65) is labelled "true" in the key rather than "false". I've obviously misunderstood the null hypothesis...so this needs to be made more clear (see red text that explains my confusion).	<p>We have attempted to clarify the explanation. The text has been expanded, and the conclusions are restated in terms more accessible to general readers.</p> <p>Replacement text suggested by the reviewer has not entirely been adopted. The language of hypothesis testing uses very specific terminology and phraseology, aimed at preserving the precise meaning of the test outcome. This terminology has been retained for accuracy, but it is now accompanied by a longer and more accessible explanation.</p> <p>The label "true" in the right-hand panel of Figure 7-65 (now Figure 7.6-7) indicates that the local test outcome was statistically significant, so that the null hypothesis is rejected, at the location in question. Heuristically the label "true" can be interpreted to mean that the porosity is elevated at the location. Text has been added to explain this interpretation.</p>	Thank you for taking the time to make this section so much clearer. All good!!	Thanks

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106	p. 265-266	Another point of confusion in this section is the introduction of another term using the letter p. The text refers to porosity index, average porosity, maximum porosity and then starts talking about 'p-values'. It may be prudent to clarify the latter is completely unrelated to porosity and is a statistical test of significance.	Text has been added to clarify this distinction.	Thanks.	✓
107	p. 266	Caption to Figure 7-65. Instead of referring to the 'left-hand plot' and 'right-hand plot' why not use their labels a) and b)?	Text has been updated as suggested, thank you.	OK	✓
108	Section 7.7.1 Inorganic geochemistry - permeameter p. 267	Typos in first and second paragraphs as indicated.	Thanks. Text has been updated	OK	✓
109	Section 7.7.2 p. 267	Correct typo and reword to improve clarity as indicated.	Thanks. Text has been updated.	OK	✓
110	p. 268	It's pedantic, but the first paragraph is rather clumsy and could be re-written to improve clarity. I would prefer to see reference to "a reduction in pressure" rather than "after a certain amount of pressure has dropped". I think this paragraph is trying to explain that for low permeability samples (that would otherwise take several minutes to analyse) the instrument has an "Extrapolation	Paragraph rewritten. "The TinyPerm 3 was used to obtain five permeability measures from each spot. If the permeability is of the order of a few mD then each measurement can take several minutes to complete. Lengthy measurements are less reproducible than shorter measurements because it is difficult to	Yes, I thought so. Thanks for rewording.	Thanks

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		<p>Data” feature. This feature can be triggered in one of two ways. Either using the reduction in pressure (or “Decay Amount” as a percentage) set to a specified threshold, or a period of time (Extrapolation Time), whichever occurs first.</p> <p>Is the last line in this paragraph correct? Should “Extrapolation Data” be “Decay Amount”?</p>	<p>maintain consistent pressure on the instrument plunger, particularly where the rock surface is difficult to access. To mitigate these issues, the TinyPerm 3 algorithm has an option, referred to as the Extrapolation Data function, which reduces the time required for measurement. If this function is enabled, the permeability is calculated by extrapolating data collected over a measurement of user-determined length, referred to as the Extrapolation Time, if pressure has not dropped by a user-determined amount, referred to as the Decay Amount within that time. For the Murujuga measurements, the Extrapolation Time and Decay Amount were set to 30 s and 50%, respectively, as recommended by the manufacturer.</p>		
111	Section 7.7.3 p. 268	<p>The text here refers to Figure 7-68 (the results of measurements made over 30 vs 60 second time intervals). There needs to be some conclusion to this section. What was decided on the basis of this result? Was a protocol employing 30 second measuring intervals adopted?</p>	<p>Done. Text modified to explain “In the current method, the Extrapolation Time is set to 30 s and the permeameter is applied to a dry surface.”</p>	Good	✓
112	Section 7.7.4 p. 268	<p>Correct the typos, tense etc and reword as indicated.</p>	<p>Typos and tenses have been corrected.</p>	OK	✓

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113	p. 268	Last paragraph. Figure 7-71 appears to illustrate some combined permeability for each lithology...presumably using data from the individual sites (shown in Figures 7-69 and 7-70). How were these determined? Given the variations in porosity (from the previous section) is it meaningful to combine the permeability data in this way?	The figure indicates the range and distribution of permeability values measured on rocks at different sites, grouped according to the rock type. This is meaningful to the extent that it identifies the range and distribution of values that can be expected and may provide initial evidence that the permeability is dependent on rock type. It does not constitute a complete statistical analysis.	OK	✓
114	p. 269	Figure 7-67. The caption could be clearer (see text). Also, do these 8 sites represent a range in lithologies or only one/a few? Either add the lithology to each of the site codes or label the pairs of datasets with the lithology code as shown for EX06.	The validation experiment was performed on rocks of various lithological types. This information is not included in the graphic for simplicity. The paired t-test takes the lithology information into account.	I note the lithological code has been added to the x-axis labels...so OK.	✓
115	p. 271	Figures 7-69 and 7-70. It would be easier to compare these figures if the same scale was used on the y-axis. Add lithology codes to the two figures so we know which sites relate to which rock types.	We decided to show only boxplots of winsorized data. They are just truncated versions of the un-winsorized boxplots. We deleted Figure 7-69. Boxplots updated with rock type.	OK	✓
116	p. 272	Figure 7-71. These two plots would be easier to compare if they had the same y-axis scale.	Panel (b) of this Figure seems unnecessary and deleted.	OK	✓
117	p. 273	This figure (7-72) doesn't seem to correspond to the text on p. 269.	Thank you. We have modified the text as follows:	OK.	✓

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		<p>Although the central Burrup Peninsula does appear to be characterised by high permeability (bright yellow) to my eye the area that has high porosity in Figure 4-64 also contains some of the lower permeability results. It would be worth expanding the caption to acknowledge this and modify the text on p. 269 accordingly.</p> <p>Also, the scale in Fig 7-72 is not logarithmic so the caption should be corrected.</p>	<p>'The pattern of spatial variation in permeability appears broadly similar to that observed for the porosity data. However, it is noted that some of the lower permeability results in the south-east of Dampier. Since permeability was measured for the first time in MRAMP, the method may still be in the learning curve. In future campaigns, permeability will be measured more carefully. Also, the presence of extreme values in the permeability data is an additional challenge that was not present in the porosity data'.</p> <p>A spatial plot of observed data has also been added.</p> <p>Thank you. Caption has been corrected.</p>	<p>OK</p>	<p>✓</p>
118	Section 7.13.6 p. 353	It should be made explicit in this small section that the region of elevated porosity was identified using only the granophyre at this stage of the study (i.e., use the word 'granophyre' rather than 'rock' in the first line).	Done.	OK	✓
119	Section 7.13.7 p. 253	Correct references to past tense as indicated.	Done	OK	✓

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Appendices					
120	B p. 400-435	See highlighted text and comments.	Thanks. Based on your comments in annotated pdf, Appendix B has been updated.	OK	✓
121	G 1.4.2. p. 592	'skew' should be 'skewed' shouldn't it?	The terms 'skew' and 'skewed' are used interchangeably in statistical literature. We are happy to use 'skew'. Changed as requested.	OK	✓
122	G2 p. 596	As with the porosity section in the report (noted elsewhere in this review) references to "microscope image" in this section may be confusing as these are not optical microscope images. Simply reword as 'Scanning electron microscope, backscattered image of...' and make it clear what the sample is.	Have replaced 'microscope' by 'scanning electron microscope' where appropriate in Appendix G.	OK	✓
123	G.2.1 p. 596-597	Change "millimeters" to "millimetres" and decide on 'microns' vs 'micrometres'	Meters corrected to metres. Also, updated microscope to scanning electron microscope, back scatter image.	OK	✓
124	G.2.2 p. 598	What do the 'integer values' coloured in panel b represent? What are these 'certain assumptions' and are they valid in this context?	The integer values are labels assigned to each fragment of void space. All void pixels which are connected in the image are assigned the same label. The labels have no meaning and are assigned by the software algorithm automatically to distinguish the groups. Similarly, colours have no meaning and are	Good OK.	✓ ✓

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			<p>used simply to distinguish different fragments. We have attempted to improve the explanatory text.</p> <p>This is a well established principle of quantitative microscopy. Essentially the assumptions are that the section is 'representative'. There are several alternative versions of the assumptions, which we do not have space to discuss in the report, but which are well documented in the cited references. We are satisfied that the assumptions are valid in this context. A full discussion of this methodology will be published in a scientific journal article.</p>		
125	G.2.3 p. 598	Correct tense, and missing word (step?) as indicated.	Tense corrected and missing word added as suggested.	OK	✓
126	G.2.5 p. 600	Figure G-14. It's still not apparent what the relevance/importance of the different integer values is and hence why panels b and d in this figure have coloured pixels. Should there be a key to explain which colours correspond to which integer values? Also, in panels c and d the pixels should be 20 not 10.	This has been clarified in the caption and the text.	Good	✓
127	G.2.6 p.602	Small corrections as indicated.	Small corrections indicated have been made.	OK	✓

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128	p. 603	Figure G-19. Change “microscope” to “Scanning electron microscope backscattered...etc” as in other parts of the report.	Microscope has been changed to scanning electron microscope throughout, images are specified as “back scattered electron images”.	OK	✓
129	H.1 p. 611	Change “microscope” to “Scanning electron microscope backscattered...etc” as in other parts of the report.	Microscope has been changed to scanning electron microscope throughout, images are specified as “back scattered electron images”	OK	✓
130		The porosity curves in this appendix seem to be of poor resolution. Even in Figure H-3 (that is larger than the others) it’s difficult to resolve the different data symbols. What do the different colours represent? Should there be a key and more information included in the caption?	All curves have been updated and graphics regenerated for better resolution. Extraneous symbols have not been included as they are not discussed in this text.	Good	✓
131	H.1.2.1 p. 614	Figure H-4. What kind of image is this and what is the scale? Add info to image and caption.	Backscattered electron image from scanning electron microscope – caption updated to indicate this.	Good	✓
132	p. 626	Table H-2. Are the values for Opening Radius listed as pixels or microns? Both are used in this report, so this should be specified.	The opening radius for this table is expressed in microns. The table caption has been updated to reflect this (also the later table from porosity versus elevation in appendix H.3)	Good	✓

2.5 Associate Professor Ron Watkins (2024)

Document title:	Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2024
Document revision:	COPP21065-REP-G-105 (Rev 0)
Reviewer:	Associate Professor Ron Watkins
Date review commenced:	June 2024

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1	General	The researchers of MRAMP have compiled an impressive amount of information from the two years of investigations. They have applied a very wide range of field and laboratory techniques. They have generally recognised where certain of these may have limitations and have been proactive in proposing modifications and/or alternative methods to trial in the future.	Thank you	✓	✓
2		In general, this Report is well written and the text free of typographic errors. It is very long. It purports to "present a summary of data collection, laboratory studies and analysis undertaken within the second year of scientific studies". The degree to which it	Parts of chapters 7 and 8 have been rewritten. Some Tables and Figures have been moved to the Appendix. Hopefully now these chapters are clearer and give a more concise summary of the statistical analyses conducted.	✓	✓

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		<p>summarises might be questioned. In parts it does appear to have the function of a repository of 'all that has been done" rather than a clear and concise 'summary'. Of course, the Project is to decide how data and information can best be stored and how it is presented and generally made available to all stakeholders.</p>			
3		<p>I have two overall criticisms of the reporting:</p> <p>There is a tendency in some sections for the use of overly elongated and convoluted sentence construction. I contend that this is not best applied to scientific writing. A maxim is that one can understand what is contained in three short sentences far more readily than that contained in a single sentence of three times the length. It becomes a significant problem when sentence length and on-rolling multiple directions leads to confusion or ambiguity of the scientific argument.</p> <p>There are frequent shortcomings in the presentation of the Figures. The captions are often minimalistic, even amounting to 'here it is make of it what you will'. In such cases, there is</p>	<p>We have revised the document and corrected the sentence structure where possible.</p> <p>We are required as far as possible to adhere to the DWER/government style guide, which stipulates "Keep the graph's caption short enough to fit on one line"</p>	<p>✓</p> <p>Note from DWER: DWER's Style Manual is the preferred approach where possible. However, DWER acknowledges that</p>	<p>✓</p> <p>We have revised and expanded figure captions where appropriate.</p>

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		<p>most often discussion of the figure in the main body of text.</p> <p>However, convention is that each figure must contain, by way of a caption, key, etc. the means by which it can be basically understood when referred to in isolation.</p>		<p>this is a scientific report, and aspects of DWER's Style Manual may not be compatible. DWER is comfortable with longer captions for graphs and figures, particularly if it will improve reader understanding. Please consider this statement when responding to similar formatting requests (e.g. items 6, 85, 86, 96 etc.)</p>	
4		<p>I have made specific comments in my review, but I suggest the above two points be considered as potential improvements throughout the report.</p>	<p>Thanks. See above comments</p>	<p>✓</p>	<p>✓</p>
5		<p>The following specific comments also relate to the report as a whole: The description of the different (5) rock types at Murujuga, in particular, lacks rigour in places. Observations and interpretations are made that have profound implications without a satisfactory amount of discussion. Examples are the application of element ratios that are inappropriate for the purpose, which itself is not made clear; and the nature and</p>	<p>These issues are dealt with in their individual comments</p>	<p>✓</p>	<p>✓</p>

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		<p>significance of quartz in the mafic rocks</p> <ul style="list-style-type: none"> - Despite the use of "fresher rock", it appears that it is applied inconsistently with "fresh rock" and even "fresh(ish)" rock substituted for it. - Whereas the 5 rock types are consistently referred to, it would be useful to know what proportions of the petroglyphs are present on each type (I realise this may be addressed in a part of the Report I have not reviewed). 	<ul style="list-style-type: none"> - "fresher rock" is now used throughout the document - Text modified in section 6.2.5 to state "The proportion of rock art occurring on each rock type is not known" (as only approximately 5% of all Murujuga rock art is recorded and archaeologists do not often record rock type to a geological classification). 	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>
6		<p>In one, or two, places, information central to an argument made in the main body of the report is present only in the Appendices.</p>	<p>A decision was made that new/additional work, undertaken in response to findings or in order to confirm findings, would be placed in the appendices. We are happy to revise this if the reviewers have a strong preference.</p>	<p>I do, but I also understand the constraints of the prescribed formatting.</p>	<p>Taken as a comment.</p>
7		<p>Notwithstanding the general criticisms above, and the specific comments I have made throughout the Report, below, I would like to congratulate the authors in providing a wealth of new information that elucidates important details of the geology and geochemistry of the rocks at Murujuga pertinent to the conservation of the rock art.</p>	<p>Thank you.</p>	<p>✓</p>	<p>✓</p>

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8	Executive summary p. 1	<ul style="list-style-type: none"> • Para 3 Line 5: Omit "now" [superfluous] • Para 4 Line 3/4: Omit either "the" or "some" • Para 5 Line 1: Close bracket or preferably rewrite as "Operation of 18 solar/passive air quality and weather monitoring stations and 4 mains powdered air quality monitors (AQMs) is on-going." • Line 3: "data have shown" • Para 6 Line 2 "are ongoing" or better "are continuing" • Para 7 Line 1: omit "yet" [superfluous] 	<ul style="list-style-type: none"> • Done • Omitted "some" • Agree. We used your suggested sentence. • Done • Changed to "are continuing" • Done 	<ul style="list-style-type: none"> ✓ ✓ ✓ ✓ ✓ ✓ 	<ul style="list-style-type: none"> ✓
9	p. 2	<ul style="list-style-type: none"> • Para 1 Line 1: change "power" to "ability" • Para 3 Line 3: Note here and following, "data" are plural. • Line 4: the term "airshed" could be added to the Glossary Table 1-3. • Para 4 Line 2: change "measurement data" to "measured values" 	<ul style="list-style-type: none"> • We prefer to preserve the term 'power' because of its technical meaning in statistical science. • Done • Done • Done 	[no response from reviewer]	-
10	p. 3	When reading I assume "sample rocks" are locations of rock outcrops in the field, and "rock samples" are, as convention, pieces of rock removed for study in the laboratory. If I am right, this is a useful distinction.	Yes, this is correct.	✓	✓

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11	p. 3	Para 1 Line 6: "and similar minerals"? similar with regard to what criteria? Felsic minerals? Why not state these similar minerals?	Main minerals stated "of feldspar, pyroxene, and other minerals"		
12	p. 3	Para 3 Line 1: Change "2" to "two". Conventions vary but I suggest that in normal descriptive text numbers 1 and 2 should be spelled out.	We were following the Australian Government style guide but we have now switched to following DWER style guide – the numbers zero to nine are spelled out in text, and numerals are given for all other numbers, except at beginning of a sentence.	✓	✓
13	p. 3	Para 4 Line 1: Beyond rock surface pH and rainfall pH, what does "pH data" refer to?	This sentence has been corrected to read: "An extensive body of rock surface pH, rainfall pH and deposition sampler pH data have been obtained."	✓	✓
14	p. 3	Para 6 Line 1: Change "These show" to "This shows" [body is singular]	Done	✓	✓
15	p. 4	<ul style="list-style-type: none"> Para 1 Line 1: "/ce NO2"? "2" should be subscript. Para 3 Line 4: [strictly] "to often expose" should be changed "often to expose" 	<ul style="list-style-type: none"> Done Done 	<ul style="list-style-type: none"> ✓ ✓ 	<ul style="list-style-type: none"> ✓
16	Introduction p. 5 - 7	Page 5 <ul style="list-style-type: none"> Para 3 Line 1: "requires a multi-year" [awkward] change to "comprises a 5-year" or "comprises an initial 5-year" 	<ul style="list-style-type: none"> Changed to "comprises a five year" Done 	<ul style="list-style-type: none"> ✓ ✓ 	<ul style="list-style-type: none"> ✓

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		<ul style="list-style-type: none"> • Para 5 Line 1: Change "accurately measure" to "Measure accurately". • Para 7 (in 1.3.1) Line 1: Change "following" to "as follows" or "in the following sections" <p>Page 6</p> <ul style="list-style-type: none"> • Para 5 (in Fieldwork) Line 3: Omit "in all" after duration [superfluous] • Line 4: "the five rock types" might better be written as "the five lithologies" <p>Page 7</p> <ul style="list-style-type: none"> • Para 1 Line 1: Change "are presented here" to "is presented here" or "the results are presented here". • Para 2 (in 1.3.2) Line 2: "where sufficient data are" • Line 6: Sentence beginning "Furthermore, ..." does not make sense. Possibly you meant "and significant delays"? 	<ul style="list-style-type: none"> • Changed to "in the following sections" • Done • We agree. But DWER's instruction was to write this report in plain language were possible. • Changed to "the results are presented here" • Done • Done 	<ul style="list-style-type: none"> ✓ ✓ ✓ ✓ ✓ ✓ 	
17	Section 1.4.1 Abbreviations	<p>Table 1-1: The definition of Eh is weird or incomplete. You seem to be stating how it is measured rather than what it is. Eh is most commonly defined as electrical potential or redox potential (from Hartree energy or energy of Hydrogen). I note that you give an explanatory definition of pH, so</p>	<p>Done. Definition of Eh modified to read "Oxidation reduction potential (ORP) relative to the potential of a Standard Hydrogen Electrode (SHE)".</p>	<ul style="list-style-type: none"> ✓ 	<ul style="list-style-type: none"> ✓

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		perhaps you should do the equivalent for Eh.			
18	Section 1.4.1 Abbreviations	The table caption states that it is a list of "General abbreviations" Given that the report is some 400 pages, and the listing is missing numerous general abbreviations, I suggest it is impractical to include all here. Possibly it would be best to designate the table as "Analytical abbreviations? See my notes for notable ones missing. Some chemical symbols are absent e.g. Zn, V.	Done	✓	✓
19	Section 1.4.1 Abbreviations	pXRF should be placed under P. XRF should be listed separately Vol. % should be defined as percentage by volume (cf. percentage by weight). Its use in thin-section microscopy is just one application where it is strictly equivalent to aerial percentage of minerals only if the rock displays no preferential orientation such as flow orientation or crystal lamination.	Done Done. Changed to "Percentage by volume of mineral, considered equal to area percentage measured on a thin section" It is not necessary to assume that the rock displays no preferential orientation. See e.g. Weibel (1978) cited in bibliography.	✓ ✓ # I will take this on report as I cannot access texts at present.	✓ ✓ Ok
20	p. 10	Table 1-2:	Done	✓	✓

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		In keeping with general format, "lead" should have a capital L.			
21	p. 11	<p>Table 1-2:</p> <ul style="list-style-type: none"> • Biofilm line 5: "can comprise a single species..." [omit "of"]. • Bioweathering line 1: "the combined action of multiple ..." [insert "of"]. • Chemolithoautotrophic line 1: " and utilize inorganic [omit "s"]. • Culturally Important Place (and elsewhere in Table) check word spacing. • Endolithic lichens line 2: Full stop rather than a comma after "cyanobacteria." <p>Environmental Quality Criteria line 5:</p> <ul style="list-style-type: none"> • Sentence beginning "Both types of EQC...." does not make sense. They may be either "Interim (preliminary)" or what? 	<ul style="list-style-type: none"> • Done • Bioweathering definition has been changed based on another reviewer's comment. • Done • Word spacing fixed • Done • Environmental Quality Criteria in Table 1.4-3 Glossary has been rewritten 	<ul style="list-style-type: none"> ✓ ✓ ✓ ✓ ✓ ✓ 	<ul style="list-style-type: none"> ✓
22		<ul style="list-style-type: none"> • Patina line 9: change "were" to "where". • Line 10: full stop rather than comma after "grooves". OR rewrite as "in engraved grooves, producing examples generally considered as being among the oldest petroglyphs at Murujuga." 	<ul style="list-style-type: none"> • Done • We have put a full stop after "grooves" 	<ul style="list-style-type: none"> ✓ ✓ 	<ul style="list-style-type: none"> ✓ ✓

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23		<p>Secondary Organic Aerosol (SOA): Q. Are they exclusively solid? Does the term not include also some liquid aerosols?</p> <p>Weathering rind line 3: According to your terminology, isn't the weathering rind underlain by 'fresher rock'?</p> <p>Weathering line 8: "anthropogenic activity, not considered as natural weathering". OR just omit "be".</p>	<p>Yes, some can be liquid. Corrected</p> <p>These definitions have been amended for clarity.</p>	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>
24	<p>Section 2.2</p> <p>Study Methodologies verification of competency, and quality assurance p. 17 -19</p>	<ul style="list-style-type: none"> • (2.2) Line 1: "and/or" • (2.3.1) Line 5: "Distilled water"? Later you say "distilled/deionised water". They are not the same and I doubt simple distilled water was used. • (2.3.3.1) Line 3 "data obtained are" Here and throughout report: datum = singular; data = plural. Don't fret, newsreaders on the ABC get it wrong! Modern usage may descend to incorrectness BUT through this report there is no consistency even within the same paragraph! • (2.3.3.2) Heading "time of flight" 	<ul style="list-style-type: none"> • Done • Done • Yes correct. Ultrapure water. This has been amended. • Done 	<p>✓</p> <p>✓</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p> <p>✓</p>
25	3. Monitoring studies	Table 3-1: "Inorganic geology"??	Changed to "Inorganic geochemistry"	✓	✓

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	undertaken in the field p. 21				
26	Section 3.2.2 p. 23	Line 13: Sentence is rather awkward. Suggest "A Bruker Tracer 5G was purchased for use in campaigns 2-7. For the earlier campaign 1, an identical hired instrument was used."	Done	✓	✓
27		Line 16: Re: "Therefore, 3 instruments" The widely accepted convention is that the low numbers 1,2 and 3, at least, are spelled out as one, two and three, except where part of a numerical sequence.	We were following the Australian Government style guide but we have decided to go with DWER style guide - zero to nine spelled out and numerals for all other numbers.	✓	✓
28	p. 24	Line 1: "Table 3-2 summarises" "28 weeks of field campaigns" [insert "of"].	Corrected	✓	✓
29	p. 26	<ul style="list-style-type: none"> Table 3-4: meaning of * after GPH in Campaign 4? 	<ul style="list-style-type: none"> Note: * indicates a repeat measurement on one rock art panel. Note is added to Table 3-4 (now Table 3.2-3) 	✓	✓
30	4. Laboratory p. 32	<ul style="list-style-type: none"> Table 4-1: For clarity, horizontal line required above the heading Rocks with fresh surfaces. Para 1 Line 1: "The TOC in patinated surface samples " [insert "in"]. 	<ul style="list-style-type: none"> Corrected Corrected 	<ul style="list-style-type: none"> ✓ ✓ 	<ul style="list-style-type: none"> ✓ ✓

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31		The TOC of the patinated surface was low compared to what? Clearly not that of the fresh rock which is a magnitude lower. Line 4: "rock samples averaged 0.04 wt%" [omit "at"].	The TOC of the patinated surface samples ranged between 0.05 and 1 wt% (on average 0.2 wt%). Corrected	✓ ✓	✓ ✓
32		Para 3 Line 4: Q. Should the Figure A-1 be relegated to an Appendix? Note there is no compulsion on the part of the reader to refer to the appendices. If the figure is relevant to any discussion/argument made in the text, it should be included in the main body of the report.	The figure is showing simple data in a complex figure. We think at this stage it is not relevant in the main text.	OK	✓
33		<ul style="list-style-type: none"> • Para 5 Line 3: MAC appears in the General Abbreviations (Table 1-1) but DBCA does not, and is not mentioned previously in the text. • Para 7 (4.1.4) Line 1: "have been completed." • Line 2: omit (wt%) [superfluous]. 	<ul style="list-style-type: none"> • Added • Corrected • Omitted 	✓ ✓ ✓	✓ ✓ ✓
34		Again, the "TOC of all rock surfaces is generally low" compared to what? Possibly rock surfaces elsewhere in the world? Para 8 Line 1: "can provide an insight into the ..."	Removed 'generally low' Corrected	✓ ✓	✓ ✓

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35	p. 33	<ul style="list-style-type: none"> Para 1 Line 3: "$\delta^{13}C$ f the fresh" change "f" to "of". Line 4: "analyses are in progress". (analysis vs analyses)? 	<ul style="list-style-type: none"> Corrected Corrected 	<ul style="list-style-type: none"> ✓ ✓ 	<ul style="list-style-type: none"> ✓ ✓
36		Para 4 Line 1: "hydrocarbons in all samples" "in general"? One or the other!	Removed 'in general'	✓	✓
37	p. 34	<ul style="list-style-type: none"> Para 2 Suggest change the sentence beginning "The following chromatograms are ..." to "The chromatograms shown in Figures 4-2 to 4-4 are ..." Also, the distributions of what? 	<ul style="list-style-type: none"> Agreed. Corrected 'distribution of hydrocarbons' 	✓	✓
38	p. 35	<ul style="list-style-type: none"> Figure 4-2: The chromatograms in my version of the pdf appear distorted. information/explanation is required for inexpert readers. e.g. naming of some peaks, such as the heptadecanes mentioned in the test, as reference. <p>I note that Figure 4-3 is much improved compared to Figure 4-2.</p>	<ul style="list-style-type: none"> The chromatograms are total ion chromatograms – the baseline is not constant 0, hence it appears somehow distorted. Agreed, basic labels have been added to the chromatograms as a reference. 	OK - a mention of this in the caption/test might be useful. ✓	Figure caption has been expanded.
39	Section 4.2.3 p. 41	There is no text whatsoever in this section? There should at least be a reference to Table 4-3.	A sentence has been added.	✓	✓
40	p. 46	Table 4-5: Rationale for showing Al hydroxide and lead sulphate and not	Done. Al hydroxide replaced by gibbsite. Lead sulphide replaced by galena.	✓	✓

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		the mineral form (gibbsite? Anglesite?) should be explained.			
41	p. 47	Table-8: Again, the reason for listing TiO? To my knowledge, it does not represent a natural mineral.	Done. Corrected to TiO ₂	✓	✓
42	p. 48	Para 4 (4.2.5.1): Practically impossible that calcite is an accessory mineral (i.e. a component of the primary igneous mineral suite) in the granophyre. It is presumably a secondary mineral.	Done. Corrected to "rare calcite, interpreted as secondary,"	✓	✓
43	p. 49	Para 1 Line 2: "phenocrysts of albite" [more than a single phenocryst].	Corrected	✓	✓
44		General: where correct and possible, it is generally preferable to describe igneous (and metamorphic) textures by means of "crystals" not "grains" as occasionally used by the author.	Done. This is an interesting point of discussion, and conventions vary across disciplines. However, my understanding is that crystal refers to a mineral grain with well-developed faces. In the Murujuga rocks, this is commonly not the case, particularly in the granophyre where vermicular morphologies are common, and we have used grain throughout.	I disagree that an anhedral crystal is not a crystal at all, given the consistent use of "grains" ✓	Taken as a comment
45	p. 52	Line 1: "flacking" - not a term I am familiar with.	Done. Corrected to flaking.	✓	✓
46	p. 53	Para 1 Line 9: change to "towards the surface, decrease in their modes being compensated"	Corrected	✓	✓

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47		Para 2 (4.2.5.2): Line 5: Re. "high volume percentage of quartz". Give the percentage.	Done. Percentage of quartz added with new text "quartz (2--20 vol.%, average value of 11.85 vol.%)"	✓	✓
48		<p>General comment: I am puzzled by the discussion regarding quartz content of the gabbro rock. The quartz content is said to be "high". If, as the descriptions imply, this is a primary constituent of the rock, it has significant implications for the characterisation and classification of the rock type.</p> <p>Strictly, gabbro contains no or very minor quartz (< 1-2%). A quartz gabbro may contain a greater (<5%) percentage of quartz.</p> <p>I am puzzled that despite recognising a "high" volume of quartz in the Murujuga gabbro samples, you do not mention it at all in the text, or in the captions to the numerous photomicrograph figures. e.g. Figs 4.18, 4.19 and 4.22. It is shown in the mineral keys of various figures, but with no comment. According to the histograms of Figure 4-26 the fresher rock contains an average of close to 12 vol% quartz. Accessory minerals are described in more detail that this major primary constituent!</p>	<p>Done. Text modified to read "Additional minerals include orthopyroxene (enstatite) and quartz (2--20 vol.%, average value of 11.85 vol.%). The high mode of quartz is in excess of that expected for a gabbro sensu stricto and may indicate that the field-based terminology requires revision."</p> <p>Ron – we are sending samples off for whole-rock analysis and should be able to update you in the Y3 report.</p>	✓	✓

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		<p>With such a high primary quartz content, the rock would be better classified as a quartz diorite or monzonite. The labradorite composition of the plagioclase, however, is typical of gabbro. I presume you have identified the quartz as primary, rather than a product of secondary (e.g. hydrothermal) or low-grade metamorphic alteration. Whatever the case, the absence of any detailed description/discussion of the texture of quartz, and hence its identification as a primary mineral should be addressed.</p>			
49	p. 54	<p>Figure 4-11 and elsewhere: The figure nowhere gives the sample number. Given that in the document, the Figures are frequently positioned on pages other than that of the relevant text, it is essential that the caption to each figure with photomicrographs contains the sample number. Furthermore, the type of sample (e.g. granophyre, gabbro) should in each case be stated, rather than requiring the reader to discern it from the sample number.</p>	<p>Sample number provided.</p> <p>The position of the Figures now follow the text.</p> <p>The sample ID includes abbreviated rock type. This is consistent with last year's report. The site, sample and rock type nomenclature are defined in Section 1.4.4</p>	<p>✓ great</p> <p>Yes was aware but naming rock type directly is preferable</p> <p>✓</p>	<p>✓</p> <p>Taken as a comment</p> <p>✓</p>

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50	p. 55	Figure 4-12: Caption line 2: As above, the caption should read "rind-patina from granophyre sample AQ08-GPH."	This is consistent with last year's report. The site, sample and rock type nomenclature are defined in Section 1.4.4	✓	✓
51		Para 1 Line 3: "is largely related to pyroxene alteration" [insert "to"]. Omit "Interestingly" Isn't everything?	Corrected Removed	✓ ✓	✓ ✓
52		Para 2 Line 5: Are you sure that the process of labradorite replacement by epidote involves dissolution of the feldspar rather than in-situ metasomatic alteration? If you are correct and empty void space occurs during the replacement, this is clearly a highly significant observation.	Done. The textural information is insufficient to distinguish between coupled dissolution and precipitation processes and metasomatic replacement, which may itself involve CDP, and the mechanism is immaterial for the purposes of this report. Reworded to clarify: "Aggregates of secondary epidote, interpreted as after plagioclase, are preserved."	✓	✓
53	p. 57	Para 2 Line 2: "Oxides comprise (remove "s").	Corrected	✓	✓
54	p. 58	Para 1 Line 6: The sentence beginning "Fractures in granite" should be changed to "Fractures in the granite" since granites are generally no more fractured than other igneous rocks. Your observation seems to be a convoluted way of saying that the quartz crystals are fractured. The fractures are mostly	"the" added Done. Reworded to clarify. "Quartz grains are characterized by sub-conchoidal fractures, and fractures on a larger scale, that do not show a preferred orientation, cross-cut the thin-section."	✓ ✓	✓ ✓

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		sub-conchoidal and are a characteristic/diagnostic feature of quartz seen in thin-section.			
55		<ul style="list-style-type: none"> Para 2 Line 1: Heading requires a space beneath. note punctuation: "...weathered rind and, where present, it is indicated by..." OR "... weathered rind, and where present, is " indicated by ..." Line 3: Perhaps change "around" to "surrounding" to make clear that it is not due to alteration of the quartz. 	<ul style="list-style-type: none"> Corrected Corrected 	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>
56	p. 59	Para 1 Line 3: Use of "phase boundary" is unusual in this context and has a distinct meaning in thermodynamics. Minerals are solid phases in thermodynamics but in mineralogical/textural studies it is better to use "crystal boundary" or "mineral grain boundary". Or in this case just "boundary".	Done. Reworded to clarify "Microcline dissolution is highlighted by voids on microcline--quartz contacts within granophyric intergrowths"	✓	✓
57		Be careful using "gap" as it infers the existence, past or present, of void space. "Gaps" seen in thin-section are commonly a result of differences in mineral resistance during the process of making the thin-section.	Done: Text modified to clarify "Microcline dissolution is highlighted by voids on microcline--quartz contacts within granophyric intergrowths"	✓	✓
58		Line 4: The text "From the fresher rock ..." to the end of the paragraph is equivocal, especially as it includes	Done. Text rewritten to clarify "The fresher rock to weathered rind transition is gradational and involves	✓	✓

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		"progressive". It would be advised that you clarify whether you are discussing the changes that take place at the fresher rock/weathered rind boundary OR generally in a progression from the fresher rock boundary upwards through the weathering rind.	an increase in clay minerals, in particular illite (~2 vol.%) and kaolinite from the fresher rock to the weathered rind"		
59		<ul style="list-style-type: none"> • Para 4: (4.2.5.4) Line 1: "mm up" (add space). • Para 5: Line 3: change to "altered to chlorite" • Para 6: Line 1: Change "better developed in" to "better developed upon" OR "better developed on the surface of" 	All corrected	✓	✓
60	p. 60	<ul style="list-style-type: none"> • Para 1 Line 1: Change sentence to " ... of the gabbro weathered rind ..." • Para 1 Line 2: Change "the 2 rock types" to "the two rock types" • Para 2 Lines 1/2: Use of "domain" in both cases is redundant [omit] 	All corrected	✓	✓
61		Para 3 Line 2: "an internal framework of crystallographic features" sounds grandiose but might you tell as what the features are? Cleavage? Twinning?	Done. Reworded as suggested. "The remaining skeletal crystals are characterised by an internal framework that shows crystallographic control by features such as cleavage, twin planes, and exsolution lamellae, and by rims that show local enrichments in iron oxides."	✓	✓

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62		Line 4: Again, it is prudent to remove "phase" and no meaning is lost. Also "dissolved" if justifiable, is highly significant.	Done. Rewritten as "Voids are common on the contacts between altered minerals."	✓	✓
63		Para 4 Line 4: "region of" is redundant [omit].	Omitted	✓	✓
64		General comment: When on the subject of the five rock types, it would be useful to mention the relative abundances of rock art on each. (I recognise this information may be presented elsewhere in the Report).	Done. This information is not known. We have stated this in the introduction to section 4.2.5. There are more than one million artworks on Murujuga, and not all of the rock types have been classified or have been classified incorrectly.	Yes, that's fine ✓ accepted	✓ ✓
65		Para 6: The description of basalt does everything to suggest to me that it is NOT basalt! For example, 1. the plagioclase feldspar is albite, not labradorite that characterises basalt. 2. Sanadine is rare in basalt and is expected to form significant amounts of phenocrysts only in trachybasalt (potassic basalt). 3. Quartz may be present in tholeiitic basalt and is rare in alkali basalt, and hardly ever forms phenocrysts of even small size. 4. Magnetite (hematite) is a major and defining mineral in basalt yet is apparently absent. Ilmenite can be abundant in high-titanium tholeiitic basalts but is accompanied by	Done. Text added. "The presence of albite rather than labradorite, alkali feldspar, titanite rather than ilmenite, quartz, and the absence of magnetite are interpreted as a record of hydrothermal alteration on the seafloor or during metamorphism."	Alternatively, it is a more potassic, evolved basalt. Texture and time will tell. ✓	Taken as a comment

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		magnetite. Titanite (sphene) is not commonly present in basalt.			
66		My reservations about the 'basalts' are not dissimilar to my comments on the unusual abundance of primary quartz you have described in the samples of Murujuga gabbros. On the mineralogical observations a monzo-gabbro and trachybasalt pairing of plutonic (coarse grained) and (hypabyssal) fine-grained equivalents is indicated.	Done. This query has been answered elsewhere (see comments 39 and 56). We have used the field names provided by the Geological Survey of Western Australia, but petrographic observations and whole-rock geochemistry indicate that some revisions may be required.	True. ✓	✓
67		<ul style="list-style-type: none"> Line 7: "radial vesicles"? A vesicle is a former gas bubble? Do you mean vesicles that have been infilled by a secondary mineral (= amygdale)? The infilling, often a result of late-volcanic hydrothermal activity is normally necessarily concentric and inwards, in which case radial growth of secondary crystals indicates a late recrystallization of the infill. On the other hand, in a more felsic trachyte, sphericular devitrification of a glassy groundmass commonly forms a spheroids of radiating quartz-feldspar intergrowths. Line 8: the presence of euhedral andradite garnet, and possibly calcite, would suggest these are 	<ul style="list-style-type: none"> Done. Reworded as follows "Two samples (RS12_BAS and RS09_BAS) host amygdales filled by radially distributed crystals". Done. Reworded as follows "calcite and euhedral andradite garnet are also present and may represent alteration phases or 	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>

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		xenocrysts after digested granite xenoliths?	xenocrysts related to assimilation or partial assimilation of granitic material."		
68		The heading "Weathering rind" is hanging.	Fixed	✓	✓
69	p. 61	<ul style="list-style-type: none"> Para 1 Line 4: "region" is redundant [omit]. Para 2 Line 2: "at <100 μm, then pores" change to "<100 μm, than pores" Para 4 Line 6: "devitrified interstitial glass" - possibly better. 	All corrected	✓	✓
70		<ul style="list-style-type: none"> Line 11: Sentence beginning "The percentage of ..." does not make sense. Also, "avg" change to "on average" 	<ul style="list-style-type: none"> Done. New text reads "On average, the percentage of holes is ~2 vol.% higher in the weathered rind than in the fresher rock." Corrected 	✓✓	✓
71		<ul style="list-style-type: none"> Para 5 (4.2.6) Line 2: change to "that form the weathered rind in the different rock types", [since you were previously discussing the weathering rind of the basalt specifically] Line 6: As advised previously, change "2 groups" to "two groups" 	All corrected	✓	✓
72		Para 6 (4.2.6.1) Line 4: "In the maps"? Where? What maps?	Done. "In the maps" has been deleted and a figure reference added.	✓	✓

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73		<ul style="list-style-type: none"> Line 8: "across the two domains: needs clarification: across the boundary of the two domains or across the domains themselves? Is a case where use of "domains" (fresher rock, weathered rind) complicates things unnecessarily. "change "into" to "within" 	<ul style="list-style-type: none"> Done. Reworded to clarify "resolution of changes in chemical composition and mineralogy on the transition between, and within, the two domains" Corrected 	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>
74	p. 62	Para 4 Line 3: "granite is characterized by enrichment of Fe and P" compared to what? [reader cannot be expected to refer to references in Appendices to find out].	Done. Text modified to read "At the millimetre scale, the weathered rind and patina of granite are enriched in Fe and P relative to the fresher rock, although the average concentrations are within uncertainty for the two lithological types"	✓	✓
75		Para 5 Line 3: As above, "across the dataset chlorite records a depletion in Mg..." in what sense is the trend? From fresher rock to weathered rind?	Done. Sentence deleted because the trends are not consistent.	OK	✓
76	p. 63	Para 3 Line 4 and elsewhere: "shows 2 distinct" [spell out 2]	Corrected	✓	✓
77		Para 4 Line 5: change "grainscale" to "grain scale"	Corrected	✓	✓
78	p. 67	Figure 4-18 Caption: Line 3: "replaces" add the "s"	Corrected	✓	✓
79	p. 70	Figure 4-21: Caption Line 2: "The colouration in (a) reflects alteration....." [specify plate (a)].	Corrected	✓	✓

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80	p. 73	<ul style="list-style-type: none"> Figure 4-25 Caption" Line 4: Again "phases" is an obtuse way of saying minerals. Line 5: "patina" is neither a phase or a mineral. Do you mean the specific Mn-patina? The caption appears not to mention plate (d) in any way? Note the same is the case for Figure 4-32 on page 78, Figure 4-38 on page 83 and Figure 4-44 on page 88. 	<ul style="list-style-type: none"> Done. The word phases has been deleted. The word patina was out of place and has been deleted. Captions for part (d) of the listed figures have been added with the words "distribution of clays and precursor minerals, shown in the legend as rock forming minerals," 	<p>✓</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p>
81	p. 75	Figure 4-27 Caption Line 2: Groundmass is usually written as one word.	Corrected	✓	✓
82	p. 90	Figure 4-46: A typical case - there are many - in which the caption gives the sample number but not the rock type.	<p>Done. Rock type provided as part of the sample ID – AQ08_GPH refers to granophyre sample from locality AQ08.</p> <p>The site, sample and rock type nomenclature are defined in Section 1.4.4.</p>	Understood as previously advised.	OK
83	p. 91	Figure 4-47: Note that as reproduced in my review copy, plate (d) is so dark as to obscure nearly all detail. Could it, and (c) possibly be made lighter?	A better-quality image has been used.	✓	✓
84	p. 92	<ul style="list-style-type: none"> Figure 4-48 Caption Line 2: To make clear, change caption to "between fresher rock (FR) and weathered rind (WR)" 	Corrected	✓	✓

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		<ul style="list-style-type: none"> "aside from those in RS04_GPH, feldspars" [insert "those in"]. It is unfortunate that the colour scheme (blue and orange) is reversed on this histogram is reversed compared to previous similar Figures! 	The colour scheme for all these types of plots is now consistent.	✓	
85	p. 93	Figure 4-49: For the benefit of a non-expert reader, this figure, which is highly instructive, requires the caption to provide some basic description of what is revealed by each of the five panels. e.g. the more intense (or brighter) the colour the greater the concentration of the element.	<p>Done. Words added as requested "Colour intensity increases with element concentration."</p> <p>However, we were unable to add words to describe the five panels in additional detail because we have been asked to reduce the caption length.</p>	<p>✓</p> <p>Understood and acknowledged. ✓</p>	<p>✓</p> <p>Taken as a comment</p>
86		General comment: The captions to many of the figures are poor in giving little of no explanation of what is shown. I appreciate that information may be presented in the main text, but that does not negate the fact that sufficient information must be provided in the caption to enable the reader to understand the salient features.	<p>Done. The style guide for the report states that figure captions should be short, and we have been asked by DWER to shorten them further.</p> <p>We hope that the reorganised figure placement so that they are close to the relevant text alleviates the issue.</p>	<p>Then so be it.</p> <p>✓</p> <p>Yes, that may help.</p> <p>✓</p>	Taken as a comment
87	p. 95	Figure 4-51: Once again the colours are reversed.	Corrected	✓	✓
88	Section 4.2.7 p. 96	<ul style="list-style-type: none"> Para 1 Line 5: "thin sections (30 μm in thickness) are...." OR "thin sections of 30 μm thickness" 	<ul style="list-style-type: none"> Corrected 	✓	✓

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		<ul style="list-style-type: none"> Line 6: change to "and the small mineral grain size" 	<ul style="list-style-type: none"> Corrected 	✓	✓
89		Para 2 (4.2.7.1) Line 4: Change after "representative" to end of the paragraph to "across the whole of Murujuga where there is extensive variation in surface topography within samples of each lithology"	Done	✓	✓
90		<ul style="list-style-type: none"> Para 4: Line 3: Change "for both lithologies" to" of both lithologies" "Notably"? Why? Spell out 2: "two units" 	<p>Corrected</p> <p>Notably has been deleted and words added "The contact between the weathered rind and the patina is sharp (Figure 4.2-49(a,b)), in contrast to the gradational fresher rock--weathered rind transition.</p> <p>2 has been spelled out.</p>	<p>✓</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p>
91		Line 4 "I suggest"... in regions where the patina has not formed or has flaked off"	Done. New text reads "in regions where the patina is not present, either because it never formed or because it has been removed by flaking or another process such as dissolution,"	✓	✓
92	p. 97	Figure 4-52 caption: Caption is confusing and incorrect? I suggest it might be re-written as "Images (a) and (c) are of unpolished cut surfaces of rock billets showing cross sections through granophyre AQ11_GPH and gabbro AQ15_GBR, respectively. Images (b) and (d) represent top	Done. Caption modified as requested.	✓	✓

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		views of the of the billets shown in cross-section in (a) and (c)."			
93		Para 2 Line 4: "Figure 4-52a" should be in brackets. Line 6: "Figure 4-52c" in brackets. Line 10: Change "is smoother then" to "is smoother than".	All corrected	✓	✓
94	p. 98	<p>Figure 4-53 caption: This is a pronounced example of the inadequacy of very numerous figure captions in this section of the report. It smacks of 'here are some SEM images of patina: 'sort it out for yourself'. The various structures and textures are discussed in the main text but this is not followed up by annotation of the photo plates. Where quartz and clays (d) are indicated by arrows, there is no explanation of what detail the arrows are showing. The arrow denoting weathering rind (a) appears to indicate a boundary between the rind and overlying smooth coating rather than an area of rind.</p> <p>Photos (a) and (b) have clear scale bars: in the remaining photos one is left to see the scale in minute text as displayed on the originals.</p>	<p>Partially done. We have been asked to reduce, rather than increase caption length. The figures have been reorganised and are now close to the figures and we hope that this alleviates the issue.</p> <p>Scale bars have been added to all panels.</p>	<p>I am cognisant of the restraints put on you regarding the annotation of captions. As stated, the photos are highly informative. They will be of high value to serious researchers.</p> <p>✓</p>	<p>Taken as a comment.</p>

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		<p>The orientation of the different photos should be given to tell where the view is a sectional one of the patina (e.g. (a)?), or a view of the actual rock surface.</p> <p>The photos are truly excellent and are potentially highly informative: they are let down by an unsatisfactory caption.</p>			
95	Section 4.2.7.2 p. 99	Para 1 Line 2: should be "found on" not in.	Corrected	✓	✓
96		Line 3: "remarkable" is rather subjective and surely not the main criterion for selecting each finding. Suggest you replace by "important", "salient", "critical", 'incisive', or 'significant'.	Done. Remarkable has been replaced by salient, as requested.	✓	✓
97		Line 6: some punctuation required "laterally discontinuous, complex, micro-deposits".	Done	✓	✓
98		Line 7: Omit "host rock" and replace by "weathered zone" or "fresh rock", whichever is applicable. "substrate" as used later in the sentence is a good term.	Done. "Host rock" has been replaced by "substrate".	✓	✓
99		Para 2 Line 1: As remarked previously, "Interestingly" is a 'nothing' word and can be omitted.	Omitted	✓	✓

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100		Line 2: "hanging patina" is referenced to Figures 4-55b and 4-59 but is not mentioned in the captions of either figure.	Done. Hanging has been added to the caption of Fig. 4-55b. The reference to Fig. 4-59 has been deleted.	✓	✓
101		Para 4 Line 3: "common" should be "commonly".	Corrected	✓	✓
102		Para 5 Line 8: Re. growth direction of the patina is upward from the formerly exposed surface of the rock" [insert "formerly", or "previously"]	Done. "previously" added, as suggested.	✓	✓
103		Line 11: "the direction of accumulation is inwards from the cavity walls" should be rewritten as "the patina accumulated progressively from the cavity walls"	Done	✓	✓
104		Line 12: reference is made to Figure 4-55b where the particular structure is not strikingly evident. Some annotation of the photograph (b) would be useful. Note that a void must be infilled inwards from the outer margin whatever the process of accumulation. I presume you mean that it is progressively infilled by concentric layers of patina accumulating from the outside surface of the void.	Words added to clarify "for example, the bright ovoid shown in Figure". Words modified as suggested "Moreover, based on their orientation, it is possible to infer that the patina was infilled progressively by concentric layers of patina that accumulated on the inner surface of the void.	✓ ✓	✓ ✓
105	p. 100	Para 1 Line 2: Change "At the base of the patina, the patina is characterised by..."	Changed	✓	✓

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		to "At its base, the patina is characterised by....			
106	p. 101	<p>Figure 4-55</p> <ul style="list-style-type: none"> • Caption Line 1: "Exotic" use in this context is puzzling? c.f its appropriate use on page 102 (4.2.8) with regard to trace elements. • line 2: insert comma before "respectively". 	<ul style="list-style-type: none"> • Done. Exotic has been removed. • Comma added 	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>
107		Para 2 line 3: Add (iv) after "gravelly patina" as this term is not mentioned previously.	Added	✓	✓
108		<ul style="list-style-type: none"> • Para 3 Line 8: the observation that "The darker patina is also characterised by a minor amount of consolidation" demands further discussion since it suggests a deformable state of the patina at the time of formation? • The previous use (line 6) of the "quartz is captured by the patina after it is released from the weathered rind by dissolution of microcline" may similarly suggest dynamic processes of patina formation. This is a very significant concept that requires further discussion in light of the hypothesis that the source of the 	<ul style="list-style-type: none"> • Done. We added clarification that "Consolidation refers to the degree of cementation and does not relate to the degree of deformation." • We have noted the significance of substrate-derived components with the text "If quartz grains are indeed locally derived then this observation is a significant finding because it shows that patina components are derived from the rock as well as from external sources, which are considered to dominate the patina (e.g. Dorn 1994)." 	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>

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		chemical components of the patina is external dust.	<ul style="list-style-type: none"> We have noted the significance of consolidation with the words “The BSE observations show a sharp compositional difference between the less-well consolidated darker upper patina and better consolidated brighter lower patina, indicating that patina formation is a dynamic process, with modification of patina material post-deposition.” 	✓	✓
109	p. 103	Figure 4-57: The figure is distorted in my copy of the pdf.	Fixed	✓	✓
110		<p>Caption line 4: "The top portion of the patina is characterized by an increase of microscale bright particles and a BSE-bright rim." So what? What causes the brightness? You must explain the significance of this in terms of the mineralogy/texture of the patina.</p> <p>Figure 4-58: Is similarly distorted and I cannot comment on the photo itself. Caption: somewhere amongst the captions of this series of figures showing BSE-SEM images you should explain what gives rise to the 'brightness'. I appreciate that this will have been mentioned in the main text but Figures need to be self-explanatory (cannot expect reader to</p>	<p>The following text was added to the captions and text, at the beginning of the 'cross-sectional microstructures' section:</p> <p>“The BSE brightness increases with average atomic number of the material analysed.”</p>	✓	✓

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		go searching for the relevant piece of text)			
111	p.105	<ul style="list-style-type: none"> • Figure 4-60: Add arrow(s) to (a) to direct reader to an example of a P-rich concretion. • Is the "film" in (b) of similar or different composition to the concretions in (a)? • In the caption "other" rock coatings is unsatisfactory (if figure is to stand alone). Suggest "varieties of rock coatings" 	<ul style="list-style-type: none"> • Arrow added and caption modified. • Text modified to clarify that the film is different: "highlighted by a thin film of a different BSE-brighter material" • Caption modified to state "BSE-SEM images highlighting variability within rock coatings" 	<p>✓</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p>
112	p. 107	Figure 4-62: Wonderfully illustrative SEM photograph. It would be improved by some annotation (arrows) of the features mentioned in the caption. It pays to consider the many readers who will have little experience of viewing rocks under the microscope.	Done. Labels added to figure.	✓	✓
113	Section 4.2.8.1 p. 111	<ul style="list-style-type: none"> • Para 1 Line 2: "that were also" [insert "were"] • Para 2 Line 8: "and K are enriched" [insert are] • Para 3 Line 4: better to use "spinfex-like texture" as "spinfex texture" is defined as having a magmatic origin and specifically relates to platy olivine crystals. 	<ul style="list-style-type: none"> • We inserted "are" to keep the tense consistent throughout this section. • Inserted • Done 	<p>OK</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p>

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114		Line 6: change "host rock" to "fresher rock" [it is with the fresher rock that you are presumably comparing elemental composition].	Done. Added "compared to the fresher rock and weathered rind"	✓	✓
115		Line 8: "these two elements" [spell out 2]	Done	✓	✓
116		Line 15: If the "substrate" is the weathered rind, why not be specific and call it so?	Done. Modified as suggested "Phosphorus is enriched in the patina and the weathered rind, where P is commonly associated with K and Na."	✓	✓
117	p. 113	Figure 4-67 Caption: The caption refers to the patina shown in Fig 4-64. However, the field of view is different. With the different colouration and degree of contrast between the two figures, it took me a little time to recognise the actual extent of the patina shown. It might be useful to in some way inform the reader that the area of interest (elemental compositions) is restricted to the relatively small areas at top left and top right.	Done. Text modified to read "Large area EDS-SEM map of part of the patina showed in Figure."	✓	✓
118		Para 1 Line 3: Suggest change to: "investigation of the composition as a function of vertical position within the patina, and"	Changed	✓	✓
119	p. 114	Para 1 Line 1: "the patina is strongly heterogeneous." In what way? I	Done. Modified to read "Element concentrations are highly variable"	✓	✓

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		assume you mean physically heterogeneous?	because the patina is physically and chemically heterogeneous”		
120		<ul style="list-style-type: none"> Para 2 Line 1: Decimal points missing from oxide values. Note: when referring to concentration ~, e.g. ~1.0 wt %, is most commonly used (rather than circa, ca.) 	<ul style="list-style-type: none"> Done. These are compositional ranges and an en-dash has been added. All “ca.” has been changed to “~” 	<p>✓</p> <p>✓ Note that in the modified para 2 p.115 there is a double space after a ~ and "1" is hanging and should be moved to 1 wt % on next line.</p>	<p>✓</p> <p>✓ Fixed</p>
121		It is difficult to reconcile your categorisation of elements (major, trace etc.) used in this paragraph When discussing the patina, you exclude Fe and Mn from "major elements" so I assume you are thinking of major elements in terms of those that are generally abundant in crustal rocks - SiO ₂ , Al ₂ O ₃ , MgO, etc. However, you exclude Fe which is the third most abundant metal in the crust, grouping it instead with the minor elements - P, Mn and S.	Done. The separation was between the less variable and more-variable elements, not between major and minor. Text has been modified to clarify, and now reads “The concentrations of these elements...”	✓	✓
122		Line 8: "with increasing with Ba" omit second "with"	Omitted	✓	✓

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123		Line 10: "Median element concentrations do not vary systematically with sample location." By 'sample location; do you mean within the patina or within the Murujuga area?	Done. Text clarified. "Preliminary investigations indicate that median element concentrations do not vary systematically with sample location on Murujuga but further work is necessary to confirm this finding."	✓	✓
124		Para 3 Line 1: "Mg, Al and Si spacing"? meaning?	Done. New text reads "The EMP results show a trend of decreasing Mg, Al, and Si concentration from the weathered rind–substrate contact towards the top of the patina, consistent with element distribution shown by the large area EDS maps of gabbro"	✓	✓
125	p. 114	<ul style="list-style-type: none"> Figure 4-68 Caption line 2: change to "Fe and Mn concentrations are strongly heterogenous". Re. "Al, Mg, Fe and P show systematic changes in abundance from the top to the bottom of the patina". This is not evident to me from the relevant photos. If this can be seen, some annotation is required or possibly reproducing at a larger scale. 	<ul style="list-style-type: none"> Done This may be a problem with the low-res images in the review copy, it looks ok now, so we've added "show systematic changes in abundance, shown by the colour intensity, from the top to the bottom of the patina" 	<p>✓</p> <p>Fine. ✓</p>	<p>✓</p> <p>✓</p>
126		Question: In the EDS-SEM maps, is the brightness due to element concentration affected by the grain size? I seem to recall that there is such an effect (absorption along crystal boundaries), but I may be wrong.	There is such a thing as 'edge effect' where the edges of grains appear brighter because the number of secondary electrons that escape the sample and reach the detector is higher at edges than from surfaces. So, if the grain is all edge then it might	<p>Thanks for the information.</p> <p>✓</p>	✓

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			appear brighter but this is not the case here.		
127	p. 117	<ul style="list-style-type: none"> Table 4-9: I note that when elemental data is recalculated to oxides, Fe is calculated as Fe₂O₃ which seems appropriate given the presumed oxidising environment of patina formation. Mn, however, is calculated as MnO. I guess you are following general practice for inorganic chemistry of crustal rocks. Given the low-temperature mineral content of the patina, with predominance of clays, I wonder how you have dealt with the presumed considerable H₂O+ content? If not directly measured, it would be useful to state the analytical totals before normalisation to 100%? Can you confirm that EMP detection limits for the reported elements is in the order of 50 ppm, as implied by stated values down to 0.01 wt % oxide? 	<ul style="list-style-type: none"> Done. Reviewer's comments on oxidation state are taken as comments. Table replaced by table that has not been normalised, so the content of water and other unanalysed material can be determined from the difference between the total and 100%. The detection limit is of the order of 0.05 wt.%. Values less than this have been replaced by BDL (below detection limit). 	<p>✓</p> <p>✓ very good</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p>
128	Section 4.2.8.2 p. 118	Para 3 Line 1:"the area shown" [omit "is"].	Omitted	✓	✓

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129		Para 4 Line 2: I don't see any metalloids in the list.	Done. Metalloids omitted.	✓	✓
130		Line 3: Change to "all of the patina samples analysed display"	Done.	✓	✓
131	p. 119	Figure 4-70: As I have mentioned numerous times, the figure captions in this section of the report have significant shortcomings. Written text on the margins of each map is too small. Scales are given on the y-axes but the units are not. Various interesting structures are shown on the maps but there is not mention of what they are. Each map is not numbered, and the ion is shown only in minute text (as on original photo). The maps show ions, e.g. Na+, whereas the caption shows elements, Na. CaS+ is not explained. Only seven of the twelve ions/elements are mentioned in the caption. The sole interpretation of the 12 maps is that the distribution of seven of them is heterogeneous. Could it possibly be otherwise? The maps could no doubt be more useful if reproduced at a larger size.	Done. The map will be at a larger size in the next version. We have been asked to shorten captions, not lengthen them. However, we hope that the reorganisation of the figures, so that they are placed close to the relevant text alleviates this issue. Caption reworded to state "ToF-SIMS map showing the distribution of positive ions for sample AQ08_GPH, and highlighting the compositional complexity"	✓ ✓ fully understand ✓	✓ OK ✓
132	p. 120	Figure 4-71: No units are shown for the vertical scales/colour variations. As, above, it is not satisfactory to rely of the labelling of each map by that shown in very small print on the	Done. The map will be at a larger size in the next version. Added "Colour scales relate to the number of counts and are not	✓ ✓	✓ ✓

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		margin of the photos. There appears to be a clear correlation between Ba+ and Mn+, as mentioned in the text, and also between K+ and NH4+ - worth a mention?	quantitative.” to the captions of Figs. 4-70 and 4-71.		
133	p. 121	Figure 4-72 Caption: Heterogeneity in the patina is also evident for Na+.	Done.	✓ assume done but I could not verify this.	OK
134		Suggestion for Figures 4-70 to 4-73: The maps, 12 in each case, are of too small size. They illustrate the patina which occupies around half of each map. Some maps give little or no information, as evidenced by lack of mention in the captions. It would be better to redo the figures each with fewer maps, choosing those which show significant compositional features. Along with larger maps, the labelling can be done at a readable size. Captions need to be more explanatory including the distribution of patina in maps of the different samples.	These figures have been made larger and the page changed into landscape. Captions have been updated to some extent but it is not possible to include full details because we have been asked to shorten captions, not lengthen them, so the captions have been simplified so that at least the level of information is the same for all panels. We hope that the reorganisation of the figures, so that they are placed close to the relevant text alleviates this issue.	✓ ✓ as previously, your predicament is understood and your responses are satisfactory.	✓ Taken as a comment
135	p. 124	Figure 4-75 Caption: line 2: "largely associated with" [replace "to" by "with"]	Done	✓	✓
136		Para 2 Line 7: "are interpreted as representing" Are the "bending/linear defects natural of a result of preparation of the foil?	Done. Clarified with "These features are natural and not an artefact of sample preparation."	✓	✓

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137		Line 8: Reference is made to Figure 4-46c. I can recognise only the faintest of lightening of the grey that might represent the linear defect? Mention should be made in the caption and/or annotation of the photo to facilitate recognition of the feature, if indeed, it can be seen.	Done. The curved linear features are clear in the copy that I have. Caption has been modified so that it states, "The curved lines are interpreted as local bending or linear defects (e.g. dislocations) of the crystal lattice."	✓	✓
138	p. 125	Figure 4-76 Caption: As above, no mention is made in the caption 76c of local bending/linear defect in the crystal lattice. Areas of microclustering of Cl and Na, as mentioned in the text, could be annotated.	<p>Caption has been modified so that it states, "The curved lines are interpreted as local bending or linear defects (e.g. dislocations) of the crystal lattice."</p> <p>Cl clusters are mentioned in the caption and are clearly visible. Caption modified to read "Na and Cl form collocated nanoclusters in the outermost part of the grain."</p>	<p>✓</p> <p>✓ co-located</p>	<p>✓</p> <p>✓</p>
139		Para 1 Line 1-4: I really cannot make much of Figure 4-76. I comprehend even less having read these lines of text. I am told Figure 4-76c is of a single albite grain. I am then told that the top part of the grain has a different composition to an "underlying grain"? Am I looking for two grains or did you mean to say the top part of the albite grain has a different composition to its lower part?	Done. Text has been clarified to read "Therefore, the top region of the albite grain is compositionally different to the underlying parts of the same grain."	✓	✓

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140	p. 128	Figure 4-78 Caption line 3: Is the fine material really fibrous? If so, what does it comprise?	Done. The image is 2D so features that are apparently fibrous may be planar. These features are referred as bands in the text and the figure caption has been modified accordingly to read "the fine layering is formed by bands of lighter material"	✓	✓
141	p. 129	Figure 4-79: Some annotation (arrow) is required in the Ti map to confirm which is the TiO nanoparticle – the small dot (upper half central of the view) or the larger object (mid-way right). The larger object, rich in titanium, does not appear to be present in the DF or other element maps?	Done. Caption modified to state "Ti-bearing nanoparticles are also shown." because both Ti-rich blobs are nanoparticles. It is true that the larger particle does not appear in the other maps. Taken as a comment.	✓ ✓ OK	✓ ✓
142	p. 130	Figure 4-80 Caption: line 1: Lack of punctuation: Add full-stop after "procedure." And comma before ", respectively."	Done	✓	✓
143	p. 131	Para 1 Line 3: According to Table 4-11, these are the trace elements determined.	Unclear which text this comment refers to. Text looks OK to me.	✓	✓
144		Line 9: What is the significance of specimen M9 - it is not mentioned on Figure 4-81b?	M9 refers to the atom probe ID. Text modified to clarify "Some parts of the needle extracted from sample AQ15_GBR, referred to as specimen M9," and M9 has been added to the caption of Fig4-81b.	✓	✓

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145		Line 10: Some greater explanation of the source and significance of the "molecular water" is required.	Done. Explanatory text added. "Molecular water refers to the presence of H ₂ O rather than OH ⁻ or other O- and H-bearing species, and these observations confirm that molecular water is hosted by the patina. The association of alkali elements and molecular water, combined with a rapid increase in evaporation rates, is produced by evaporation of fluid inclusions during APT analysis (e.g. Reddy 2020)."	✓	✓
146		How does it relate to the original patina? What form – presumably mineralogically combined in minerals? Is it affected by sample preparation?	Done. Text added to clarify. "This molecular water is likely to represent nanometer-scale pockets of fluid within the patina. It is not possible to determine its age or longevity from the ATP analysis. However, the presence of K and Na with the molecular water argues against an origin related to sample preparation."	✓	✓
147	p. 132	Figure 4-81: The caption is inadequate. Part (a) gets no mention at all. I assume the reconstruction on the left is a combination of all four ions but this must be told. In (b) in the bottom left reconstruction the colour differentiation is extremely hard to see, most notably for K and H ₂ O. The reconstruction on the right requires more explanation: what is H _x O? What	Done. Caption has been rewritten and now reads "3D reconstructions of atom probe specimens for AQ15_GBR. Each dot in the atom maps represents one detected ionic species. For simplicity, not all the detected atoms are displayed. (a) Specimen M10. Left panel shows superimposed Mn, Fe, K and Na. Right hand panel shows the distributions of these elements individually. The same colour scheme	✓	✓

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		in this context is an isosurface? There doesn't appear to be any matching of the distribution of H ₂ O and H _x O on the two reconstructions. What is the origin of the molecular water - crystallographic combined?	<p>is used for both panels. (b) Specimen M9. Upper left hand panel shows Mn and Fe. Lower left hand panel shows K, Na, Ti, H₂O. Right hand panel shows Si and a surface of constant H₂O concentration, in yellow, that highlights the regions of high H₂O content.”</p> <p>H₂O is shown on the left and right hand panels to clarify its distribution and help to distinguish it from K.</p> <p>H_xO different to H₂O and this is now clarified in the caption.</p> <p>The origin of molecular water is discussed in the text (see previous comment).</p>	<p>✓</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p>
148	p. 133	Figure 4-82: I assume the K isolated cluster is the small darker coloured region toward the top left of the cone. A simple annotation by arrow would be useful.	Done. The location of the K cluster has been described in the caption. “K forms a cluster on the upper left-hand side of the needle, which is co-located with an Fe- and Al-depleted surface.”	✓	✓
149	p. 134-135	<ul style="list-style-type: none"> Table 4-11 Caption: "at%" should be added to the list of abbreviations (Table 1-1). If it has not appeared in full previously in the text, it might usefully be spelled spelled out. Define s.. 	<ul style="list-style-type: none"> Now have “atom percent” at.% defined on first use. 	<p>✓</p> <p>✓</p> <p>✓ "of" not "on"</p>	<p>✓</p> <p>✓</p> <p>Thanks</p>

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			<ul style="list-style-type: none"> • s. defined in the Table with text “s. is one standard deviation on the measured value.” 		
150		The atomic totals in the Table range between 99.45 at% (respectable in, say, ICP-MS) and 70.7 at%. As usually done in conventional geochemical rock analysis, you should comment on the reasons for the lower totals.	Done. New text reads “Most of the totals are above 98%, but specimen M9 returned a total of ~70 wt. % and specimen M16 returned a total of 83.8 wt.%. These low values indicate that some peaks have not been assigned to elements or species, and further work is necessary to identify missing peaks that may represent a combination of complex molecular ion. For this reason, these results are considered preliminary.”	✓	✓
151	p. 136	Para 1: This paragraph appears not to be included under any section heading. Should it be placed as the first paragraph in Section 4.2.9?	Done. This paragraph was misplaced and has been merged with text at the beginning of section 4.2, which now reads “Investigations conducted during the second year of the research were designed to achieve the objectives of the MRAMP monitoring studies plan (point 2, Section 1.3 of the MSDCA Plan, 2022), to answer the specific questions outlined in Section 4.2.9 of the Monitoring Studies Report 2023, based on the preliminary results presented within Section 4.3 of the Monitoring Studies Report 2023, and are described in Section 3.2 of this	✓	✓

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			report as part of the fieldwork summary.”		
152		Para 2: (4.2.9) The heading Background is clear, and no problem to reading/understanding. I note, however, that it is unnumbered, and the format style is different to similar subdivisions earlier in the report. Perhaps a restructuring of the hierarchy would be an option?	Done. Headings reorganised so that all are numbered.	✓	✓
153		Line 2: "solutes in solution" - omit "in solution" [by definition all solutes are in solution] OR simply replace by "solutions".	Done	✓	✓
154		Line 8: "(e.g. hematite and gibbsite) would better be written as (e.g. gibbsite and hematite [relating more directly to the previous Al- and Fe-bearing oxides].	Done	✓	✓
155	p. 137	Para 3 Line 15: There is no need for the two sets of brackets). Line 19: reposition brackets "presented by Ramanaidou and Fonteneau (2019).	Brackets fixed	✓	✓
156	p. 138	Para 1 Line 13: Re. "the close proximity of the investigated sample (EX03_GPH) to the coastal environment, indicate that saline	Done. Have added text to state "Such processes may affect other samples from Murujuga, albeit to lesser extents, because no part of Murujuga	✓ excellent	Thanks

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		solutions may have been involved in hydrolysis and leaching." I agree. As I have noted, this may have been stated earlier in the descriptive text. Also note that all Murujuga samples, not solely EX03-GPH, are proximal to the ocean and will be under the influence of marine aerosols.	is more than four kilometres of the coast."		
157		Line 16: Is it possible that CO ₂ in fluid inclusions in the albite crystal may have contributed to the C?	Done. Have included the possibility with new text "Alternatively, it may represent C present in fluid inclusions that formed during conversion of the protolith granophyre to the weathered rock during hydrothermal alteration. It is not possible to distinguish between these hypotheses using inorganic chemical techniques; organic geochemical investigations are required to better constrain the role of microorganisms."	✓	✓
158		Para 2 (4.2.9.2) Line 1: I am not a latin scholar but isn't it "sensu lato"?	Correct "sensu lato". Spelling fixed	✓	✓
159		Line 2: Point of information: "Desert varnish" is very commonly described as being "purplish black" with, as you state, the dark purple-black colouration attributed to enhanced MnO ₂ content.	Thank you.	✓	✓

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160		Line 13: Strictly, patina cannot be a proxy for climate. Perhaps better to say "indicator of paleoclimate" OR "proxy for paleoclimatic variation".	Done. Modified as suggested.	✓	✓
161	p. 139	Para 1 Line 6: Use of "elements" here is potentially confusing. Suggest a change to "processes" or "activity".	Done. Modified as suggested.	✓	✓
162		Para 2 Line 5: The sentence beginning "However, some key" does not make sense. I think your need to omit "that".	Done. Modified as suggested.	✓	✓
163		Point 5: Question: Is alternation of Mn- and Fe-rich layers always present, as suggested? [if it is, this bears significantly on the nature of the formation process].	Done. No, layering is not always present. Text modified to state this. "High-resolution EDS maps and EMP analyses indicate that patina is compositionally heterogeneous and features, in some cases, alternating Mn- and Fe-rich layers".	✓	✓
164	p. 140	Point 10 Line 2: Note that use of "nitric" and "carbonic" has specific connotations regarding the chemical speciation of compounds. If you mean this, that's fine. If you don't it is preferable to use "nitrogen, sulphur, silica and carbon compounds.	Done. Text now reads "nitrogen-, sulphur-, silica- and carbon-bearing compounds"	✓	✓
165	p. 144	<ul style="list-style-type: none"> Figure 4-83 Caption: I cannot see the red box in (a) that is referred to. Insert "are" after "P" to make sense of the sentence. 	<ul style="list-style-type: none"> Done. Red box added to Figure 4-83. 	✓ ✓	✓ ✓

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		<ul style="list-style-type: none"> Remove comma after C (line 4). Could the increased brightness of carbon throughout the sponge-like area be a result of higher porosity and presence of resin in pores? 	<ul style="list-style-type: none"> “are” inserted and comma removed Resin-related query answered with the text “It is also possible that the C-bearing resin contributes to the C-signal. However, the sponge-like structure is brighter on the C image than the resin-filled hole towards the bottom part of the image. Under these circumstances, the C-signal within the sponge-like structure cannot be attributed entirely to the resin. This observation is consistent with those from TOF-SIMS, which also record the presence of C-bearing species within the patina.” 	✓ excellent	Thanks
166	p. 146	Figure 4-85: This is an example of a figure comprising four excellent SEM images that is devoid of any explanation of the individual photos. The caption is insufficient for experienced mineralogists and, I suggest, completely inadequate for the inexperienced readership. What is the effect on the stability of the patina caused by deterioration of the substrate and how is it exhibited in the photos?	Done. Caption rewritten to read “SEM-BSE images showing areas where patina overlies porous weathered rind substrate. As porosity increases, the area of patina: weathered rind contact decreases and the probability of flaking increases.”	✓	✓
167	Section 4.2.10.1 p. 147	Para 1 Point 1 line 1: While taken from the Monitoring Studies Data Collection and Analysis plan, the	Done. The wording is taken from a previous document, so it isn't appropriate to modify it.	✓ OK	✓

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		systems should be clarified as "these weathering systems".			
168		Para 2 (4.2.10.1) Line 1: The use of "thermodynamic properties to prove the properties of the system" is rather meaningless. Should be rewritten.	Done. Rewritten to read "use thermodynamic data for minerals, solutes, liquid, and gas phases to predict the characteristics of user-defined systems"	✓	✓
169		Line 3: "strength of tendency" is hard to quantify. Better to omit "strength of"	Done. Modified as suggested.	✓	✓
170		Para 3 General: In the context of Murujuga and the fields of study reported in this Report, it would be prudent to point out, along with the disequilibrium limitations stated, that the thermodynamic systems do not include any organic components, and thus any biotic reactions or catalysis. It is further vital to point out that the results of thermodynamic modelling reflect "closed chemical systems" whereas weathering/patina formation at Murujuga is manifestly an open system (chemical components enter and exit).	I agree that the biotic catalysis is not included in the modelling, and that such processes are likely to be important controls on the rates of reactions and the reaction pathways. However, thermodynamic equilibrium is independent of the route to that equilibrium, so whether the reactions are biotically catalysed or not is irrelevant for the purposes of the modelling. New text added "Therefore, the results provide a framework that aids understanding of the patina--weathered rind--fresher rock systems, but they cannot be applied directly to Murujuga because thermodynamic equilibrium is not achieved in these natural systems, and where, instead, biotic and abiotic processes, including catalysis control the rate and reaction pathways that would, on some timescale, lead to equilibrium. In	I take note of your point. ✓	Taken as a comment. ✓

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			addition, the models simulate systems that are closed to most components, so gains and losses of elements are not simulated quantitatively."		
171	p. 148	<ul style="list-style-type: none"> • Para 1 Line 7: Insert comma after "ratios," • Para 2 Line 4: Change "phased" to "phases". • Line 6: Change "neglected" to "excluded" [shows intent not chance] 	Done	✓	✓
172		Line 7: The sentence that began "these elements is overly long and very awkward. I suggest the latter part be rewritten "since the trace elements are hosted in a relatively few rare minerals and, consequently, impose insignificant control on the pertinent geochemical system."	Done. Rewritten with new text "These elements and their hosting phases are excluded from the calculations presented here. The concentrations of the trace elements that they host is low in Murujuga rocks and, consequently, the minerals that host these phases are present in such small amounts that they are unable to impose significant controls on the local geochemical environment.	✓	✓
173		Para 3 (4.2.10.4) Line 1: Change to "In the first, atmospheric gases CO ₂ , NO ₂ , and SO ₂ were equilibrated ..."	Done. Modified as suggested.	✓	✓
174		Line 8: "provide a maximum" of what? [I think use of "provide" may be wrong here]	Done. Wording changed to "For these reasons, the decreases in pH calculated here are likely to be larger than those observed in the field."	✓	✓

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175		<p>Para 4 Line 1: "In simulation 1"? You have not yet defined simulation 1. Change to "In a first simulation".</p> <p>You haven't really explained what a simulation is?</p> <p>Also, in so doing, you should emphasise that you are performing hypothetical calculations by means of the PHREEC software. [it is easy, especially for inexperienced readers to be lulled into believing that actual experiments are being described].</p>	<p>Changed to "In a first simulation"</p> <p>Done. Words added to state "Each set of calculations involved multiple simulations, where the term "simulation" describes a calculation of the consequences of chemical reaction between a fluid and a solid. The thermodynamic data used for the simulations are derived, to a large extent, from laboratory experiments, but the simulations are theoretical and do not represent physical experiments on the material at Murujuga."</p>	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>
176		<ul style="list-style-type: none"> • Para 5: Line 1: Change to "In a second set ..." • Question: Was "simulation one" also a set of simulations rather than a single one? It appears so from what is described in the later sections. These sections of the Report require checking to eliminate any confusion. The use of both "simulations" and "calculations" may cause confusion if not done carefully. 	<ul style="list-style-type: none"> • Changed • Each of the two sets of simulations, 1 and 2, include multiple simulations. This has been clarified in the text and is shown in Fig. 4-86. Text has been checked. 	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>
177	p. 149	<p>Para 3 Lines 2-3: The sentence beginning "These values" is most convoluted and hard to understand. "in" should be inserted before</p>	<p>Rewritten and sentences shortened. "These values are considerably lower than the measured partial pressures of atmospheric NO₂ at Murujuga</p>	<p>✓</p>	<p>✓</p>

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		"solution" and the complete sentence is better rewritten to simplify what you are saying regarding calculated and actual measured atmosphere: water relations.	(log10 NO ₂ = -8.27). However, unrealistically low pH values of 1 to 2 were calculated for higher NO ₂ partial pressures. These low values are attributed to the well-documented incomplete equilibration between atmospheric gases."		
178		Para 4 Line 2: "As for NO ₂ ," change to "As was the case for NO ₂ in simulation B,". The problem of understanding is repeated here and once again "in" should be inserted before "solution". The paragraph should be rewritten to give clarity.	Rewritten and sentences split. "These values are considerably lower than the measured partial pressure of atmospheric SO ₂ at Murujuga. However, low pH values of 1 to 2 were calculated for higher SO ₂ partial pressures. As for NO ₂ , the low values are attributed to the well-documented incomplete equilibration between atmospheric gases (Tottan, 2018).	✓	✓
179		General: On pages 149-150 describing thermodynamic calculations, the tense oscillates between past - "were compared with" and present - "so granophyre is used", etc. [use one tense consistently]	Rewritten to use past tense, except where present is required.	✓	✓
180	p. 150	Para 1 Line 2: "rock are equilibrated with, or buffered by, atmospheric"	Rewritten	✓	✓
181		Para 3 Line 3: Re. "These results are shown in red" Where? On which diagram? Note Figure 4-86 includes red, but shows scenarios not results.	Done. Figure numbers added.	✓	✓

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182		Para 4-6: Where are each of these set of coloured results shown?	Done. Figure numbers added.	✓	✓
183		Para 7: Line 2: "red lines on graphs" No graphs or figures are referred to.	Done. Figure numbers added.	✓	✓
184		Line 4: Where are the results of the "patina-free" simulations to be found?	Done. Text modified to clarify "and in these cases the patina-free simulations 2B and 2D may provide a more realistic guide"	✓	✓
185		<ul style="list-style-type: none"> Para 8 (4.2.10.5) Line 1: Would be useful to provide web address or person to which the request can be made. "pe" should be added to the abbreviations in Table 1-1 and its relationship to Eh explained. 	<ul style="list-style-type: none"> Done. Text now reads "Full results and input files are available on request from the Department of Water and Environmental Regulation". "pe" add to Table 1.4-1, and definition added to the text "pe is the negative log of the electron activity, and is a linear function of Eh at constant temperature" 	 ✓ ✓	 ✓ ✓
186		<ul style="list-style-type: none"> Line 4: Change "all species" to "all dissolved species" Line 5: use super script (N+5) 	<ul style="list-style-type: none"> Changed Fixed 	 ✓ ✓	 ✓ ✓
187	p. 151	Para 2 Line 6: comma before ", respectively.	Comma added.	✓	✓
188		Para 3 Line 2: use of hyphens in this case may be confused with minus sign. Suggest change to " for solutions where NO ₂ and SO ₂ are absent"	Done. Modified as suggested.	✓	✓

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189		Line 3: "pH to values of 8 and higher" Since pH values are decreasing you should state how much higher than 8. i.e. give the range of values "8 to?"	Done. New text reads "Increasing NO ₂ and SO ₂ concentrations are associated with a decrease in pH to values of 8-9".	✓	✓
190		Para 6: Line 1: "Mode" should be added and explained/reference given in the Glossary Table 1-2. "Modal amount" and "modal volume" are more friendly terms for non-mineralogist readers.	"Mode" added to Table 1-2. Mode defined in the text with "Mineral abundances are commonly referred to mineral modes. Here, the mode is measured in moles per kg of rock.	✓ ✓	✓ ✓
191	p. 152	Para 1 Lines 3-5: This paragraph is convoluted and difficult to understand. May I suggest it be rephrased as "Birnessite is the most stable Mn-bearing mineral in solutions where NO ₂ and SO ₂ are absent. With increasing SO ₂ , lower oxidation state Mn minerals hausmannite and bixbyite are stabilised relative to birnessite, whereas with increasing NO ₂ , MnO ₂ (gamma) is most stable. Birnessite has a Mn-oxidation state of +3.5, hausmannite +2.67, bixbyite +3".	Done. Modified as suggested.	✓	✓
192		<ul style="list-style-type: none"> Relevance of "(SI>0) in original sentence? "SI" should be added to Table 1-1 OR "Saturation Index" added to the Glossary Table 1-2. You should explain the relevance of g-MnO₂ in your discussion since it is exceedingly rare in nature and restricted to hydrothermal systems 	<ul style="list-style-type: none"> "SI" has been added to Table 1.4-1 abbreviation. SI has been added to the description of saturation index at the start of the results section. Words added to clarify "However, MNO₂(gamma) is exceedingly rare in nature and restricted to 	✓ ✓ ✓	✓ ✓ ✓

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		with temperatures far exceeding those encountered during weathering at Murujuga.	hydrothermal systems with temperatures far exceeding those encountered during weathering at Murujuga, so kinetic effects are expected to prevent its formation in natural systems under surface conditions."		
193		Para 2 Line 2: "Many minerals are over-saturated" [omit "also"].	Omitted	✓	✓
194		Para 3 (pH) Line 5: pH is normally qualified in terms of lower or higher, not more or less than. Change sentence to "The pH of granophyre substrates is generally lower than that within the gabbro.	Modified as suggested.	✓	✓
195		Paras 4-6 (solute...): From the heading, one can assume that all comments on chemical compositions in this section relate to Na, K, Si in the liquid phase. THIS must be stressed in the text since reference is invariably made to "granophyre patinated weathered rind", "patinated fresher gabbro", etc. This would, I'm sure, confuse at least a readership unfamiliar with PHREEQC-type modelling. The occasional, but not uniform, use of "simulations" and "scenarios", may further hinder understanding. I suggest these paragraphs be modified to ensure they are potentially less confusing.	Done. Modified by adding "concentration of the aqueous solution that coexists with the" where appropriate. Scenario has been replaced by simulation throughout this section, where appropriate.	✓ ✓	✓ ✓

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196		Para 7 "(Mineral modes)"	Modes defined – please see response to comment 182	✓	✓
197	p. 153	Para 2 Line 2: You state that "Similar trends are observed for the gabbro, although..." but the details of the systems are not the same. In the first sentence (granophyre) you describe trends for kaolinite in an unpatinated scenario and an unpatinated weathered rind. In the second sentence (gabbro) you compare these trends for kaolinite with those in unpatinated and patinated simulations. This is NOT comparing apples with apples! It is impossible to comprehend what you are meaning.	Rewritten to clarify. "In the granophyre, kaolinite modes increase with increasing NO ₂ for the unpatinated simulation (S2B) and decrease with increasing NO ₂ for the patinated weathered rind (Figure 4.2-86(d)). The change in kaolinite mode is positive for patinated and unpatinated gabbro weathered rind simulations (Figure 4.2-86 (h)), but the magnitude of the change increases with increasing NO ₂ for the unpatinated weathered rind and decreases with increasing NO ₂ for the patinated gabbro weathered rind."	✓	✓
198		Para 3 (Mineral stability) is similarly confusing. See my general notes on the Report)	Done. The word phase has been deleted. The words 'more stable' have been replaced by words relating to changes in the saturation index.	✓ ✓	✓ ✓
199		Para 4 (4.2.10.6) Line 1: Suggest "The results of the thermodynamic modelling have a number of implications"	Done. The results of the thermodynamic modelling provide some insights into patina--water--weathered rind--fresher rock systems at Murujuga.	✓	✓
200		Line 3: Are you sure you don't have things in reverse: Conversion of illite (KH ₃ O)(Al,Mg,Fe) ₂ (Si,Al) ₄ O ₁₀ to kaolinite Al ₂ Si ₂ O ₅ (OH) ₄ releases K+	Done. Reworded to clarify "conversion of illite to kaolinite involves exchange of protons from solution for K derived from illite, so that the number of	✓	✓

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		and other cations into solution decreasing the solution pH.	protons in the solution decreases and the number of K ⁺ ions increases.		
201		Line 5: Clay minerals are silicate minerals. Change to "clay minerals, or other low-temperature silicate minerals,"	Done. Modified to read "For example, alteration of diopside to form Mg-montmorillonite and dissolved Ca and Mg, increases pH."	✓	✓
202		Line 8: Can you expect the measurement of pH generated by placing water drops on a Murujuga rock surface and measurement within minutes to be constrained by clay transformations? The reactions are far too slow and water ingress into the surface too little. I would expect surface pH, so measured, to be primarily controlled neither by the pH of precipitation nor by clay chemistry. Readily soluble surface deposits, both alkaline (marine salt) and acidic (bush fire smoke, organics, atmospheric CO ₂) are likely predominant controls.	<p>Done. Here we are stating what other authors have stated, not what we think. I have reworded to clarify "his scenario differs strongly from those proposed by a range of authors who propose that conditions on the rock surface and substrate are controlled by equilibration of Fe- and Mn-oxides with water (Black 2017)."</p> <p>We agree that clay minerals are highly unlikely to approach equilibration with water placed on the rock surface on a timescale of minutes and this is stated later in the relevant part of the discussion.</p> <p>We agree that readily soluble surface deposits are likely to be affect pH of water placed on the rock surface on a timescale of minutes and indeed this is demonstrated by the pH of rock surfaces after bushfire. These considerations are included in the relevant section.</p>	<p>✓</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p>

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203	p. 154	In general: Communicating thermodynamic concepts in geochemistry is not an easy task. Unfortunately, the author/authors of the discussion on this, and adjacent, pages make things more difficult to comprehend by the use of extraordinarily long sentences with those numbering 56, 58, 66 or more words! Furthermore, the sentences each typically introduces multiple, often entwined, concepts. Good scientific writing characteristically is formed of concise, clearly written sentences each portraying a single main idea.	Done. Sentences shortened.	✓	✓
204	p. 154	Para 1 Line 1 and throughout: valency written as N(+5) is better written N5+.	All N(+5) have been fixed.	Is true of N(+4) and S species also!	This has now been fixed for all species.
205	p. 154	Para 2 Line 1: An example: the sentence beginning "The wide range of ..." contains 54 words and introduces a number of different relationships.	Sentence split.	✓	✓
206	p. 154	Para 3 Line 1: No need for hyphens "have higher K and Si concentrations"	Hyphens removed.	✓	✓
207	p. 154	Line 4: Change "would have a" to "may have a"	Changed	✓	✓
208	p. 154	Line 6: "health" is rather subjective and ill- defined term when applied to patina. Suggest change of "patina	Done. New text reads "a proxy for the formation or attrition of patina"	✓	✓

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		health to "the development and attrition of patina"			
209	p. 154	Para 4 Line 1: Useful to remind reader of the condition pertaining here: "With addition of NO ₂ and SO ₂ to the infiltrating solutions, albite, anorthite and diopside....."	Done as suggested.	✓	✓
210	p. 154	Line 2: "the dissolution of augite and labradorite, which are considered equivalent to diopside and labradorite, respectively" Did you mean to this - labradorite is equivalent to labradorite? Did you perhaps mean to say labradorite is equivalent to (an intermediary of) albite and anorthite?	Done. Equivalence to labradorite removed.	✓	✓
211	p. 154	<ul style="list-style-type: none"> • Line 3: Re. "Quartz is predicted to be relatively stable" How is the degree of stability measured with regard to the real situation? • What would the reaction between NO₂ + SO₂ and mineral quartz be such that a change in modal quartz is recorded? 	<ul style="list-style-type: none"> • Done. The word relatively has been removed. • Changes in modal quartz would occur as changes in the solubility of quartz as a function of pH. This is explained within the mineral abundance results section with the text "For albite, as for other minerals, changes in solubility do not derive directly from reaction between NO₂ or SO₂ and the mineral. Instead, NO₂ or SO₂ affect the pH and solubility is a function of pH." 	<p>✓</p> <p>✓ point accepted.</p>	<p>✓</p> <p>OK</p>

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212	p. 154	Line 5: Change "it's" to "its" Change semi-colon after granophyre to a full stop, and begin a new sentence "Quartz is a minor" Note. In the petrographic study of the gabbros at Murujuga an 'unusually high' quantity of (primary) quartz was reported.	Typos fixed Done. Reference to quartz in the gabbros removed.	✓ ✓	✓ ✓
213	p. 154	Line 6: add comma after "dissolution,"	Added	✓	✓
214	p. 154	Para 5 Line 5: What is the composition of the fluid?	Fluid has been replaced by "liquid water"	✓	✓
215	p. 154-155	<ul style="list-style-type: none"> Line 6: "1E-8 moles" is more usually written "1e-8 moles". Line 13: Remove hyphen after patina. Page 155 <ul style="list-style-type: none"> Para 1 Line 1: Change "limitations on" to "limitations of" 	<ul style="list-style-type: none"> All "1E-x moles" have been fixed Removed Changed 	✓ ✓ ✓	✓
216	p. 155	Line 4: The sentence beginning "For example," is 66 words in length. Such run-on sentencing, where one introduces a succession of clauses in succession, in my experience, takes many times longer to digest and understand than do two or three short sentences. I, therefore, have suggested that shorter, simple sentences, each with a single theme, is more appropriate for scientific	Done. Sentences have been shortened. We have emphasised that the calculations are hypothetical and used only to illustrate the point. Net rates of patina formation and removal are not known so it was not possible to use more realistic values.	✓ ✓ Even so, a comment that an annual growth of 0.2 mm is very many times greater than possible in nature would be usefully included.	✓ The rate of 0.2 mm per year has been reduced by a factor of 10

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		communication. Your use of hypothetical values to explain the net effect of patina formation and removal is fine. However, choice of more realistic values would be better so as not to give the reader the impression that 0.2 mm annual growth is feasible!			
217	p. 155	Line 10: "reduced, but still positive, patina growth rates" Make this singular "a reduced, but still positive, patina growth rate" Para 2 Line 1: You can omit "the assumption of".	Changed Done. Omitted as suggested.	✓ ✓	✓
218	p. 155	Line 2: The sentence beginning "However," immediately focuses on evidence to show why stable (full) equilibrium does not occur at Murujuga. You also comment on the well-documented disequilibrium in the Earth's atmosphere. These are all true but before going into specifics, if you choose to do so, you should state the obvious - that the conditions at the Earth's surface are not conducive to stable equilibrium. This is the "environment" and chemical systems are demonstrably non-equilibrium. Life, itself, is not in equilibrium state, the existence of mountains, rocks and minerals, other than clays and other low-T minerals, are all examples of disequilibrium, etc. etc.	Done. Changes made as suggested.	✓	✓

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		I suggest you say "The low pressure and temperature at the Earth's surface ensure that chemical reactions proceed generally only slowly". The 'environment ' is thus characterised by disequilibrium". Then you can give you specific Murujuga 'evidence'.			
219		<ul style="list-style-type: none"> Line 6: Best not to use "micrometres" and "microns" inconsistently. 	<ul style="list-style-type: none"> Done. Microns has been replaced by micrometres throughout this section. 	✓	✓
220		<ul style="list-style-type: none"> Para 3 Line 1: Space required under heading. You attribute the difference between the lower pH generally measured on rock exposures at Murujuga and the much higher ones calculated by PHREEQC modelling to incomplete equilibration of the water: patina during the field measurements. Rather something is surely amiss with the chosen system for modelling. Any amount of time to equilibrate the test water in the field pH measurement is not going to produce a pH of 11. Only very rarely does terrestrial spring water have a pH >9 (generally through dissolution of alkaline organic compounds. Normally equilibrium with limestone creates the highest pH waters of ~pH8.6. 	<ul style="list-style-type: none"> Space added High pH values of 9 to 11 are common in serpentinizing environments, although that is not relevant here. I agree that the discrepancy between observed and calculated pH cannot be attributed entirely to the short time scale of equilibrium. As Ron suggests, the high pH values probably indicate that that some process that occurs in real systems has not been captured by the model, or vice versa, and we have now stated this in the text. The modified paragraph reads "The calculated pH values of 8--11 are much higher than the pH observed on the rock surface of all rock types by the electrochemical field monitoring. This is partly 	<p>✓</p> <p>✓ very clear rewrite</p>	<p>✓</p> <p>Thanks</p>

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			<p>attributed to incomplete equilibration amongst the patina minerals and the water added to the rock surface over the relatively short time of 2 minutes during the pH measurement. Indeed, other workers note that equilibration amongst rocks and solutions occurs over multiple hours to days, rather than minutes (Aho and Weaver, 2006). However, pH values above 8 are seldom observed in natural or experimental systems for these rock types. Therefore, it is likely that one or more processes that modify pH, such as precipitation of secondary minerals or atmospheric interactions, have not been included in the model.”</p>		
221		<p>Line 5: Better to replace "occurs over multiple hours to days," with is only approached over multiple hours to days," [I know we use "equilibrated" loosely in environmental work but given you are making comparisons between the stable equilibrium of the phase diagrams with the actual rock surfaces, it is best not to suggest the state arrived at after hours to days is true equilibrium.] Complete reference(s).</p>	<p>Change made and reference complete.</p>	<p>✓</p>	<p>✓</p>

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222		<p>Para 4: I have serious misgivings about this paragraph. If I understand correctly you imply:</p> <ol style="list-style-type: none"> 1. The field measurements are close to those expected from the equilibration of pure water with atmospheric CO₂. This is true but you imply that there isn't much evidence of equilibration of water with NO₂ or SO₂. But why would you ever expect (rain) water at Murujuga to be pure water? Given the coastal location and inevitable influence of marine aerosols the precipitation might be at a relatively high pH (up to approx. the pH of seawater 8.2). You could equally make the case that the lack of elevated pH measured on rock surfaces is unexpected, and values ~5.6 may indicate the effects of acidic atmospheric gases? 2. De-ionised water used in the pH measurements is effectively unbuffered to pH change, i.e. the dissolution of miniscule quantities of sea salt or other precipitates on the rock surface can cause a relatively large change in the pH. With a nominal initial pH of 7, and variation is amplified by the Logarithmic scale. 	<p>This comment might be based on a conflation between rainwater and the pure water used for the field measurements. To fix this, we have clearly referred only to the pure water used for the field measurements in this section. New text reads "Many of the field measurements of pH are close to 5.6. This value is expected for pure water that has equilibrated with atmospheric CO₂, so the primary signal recorded by the field measurements may be that of equilibration between atmospheric CO₂ and the distilled water used for the field measurements. Some field measurements of pH are higher than 5.6. There are a number of possible explanations for these higher values. These include partial equilibration with clay minerals within the patina and dissolution of readily soluble surface deposits that produce high pH solutions, such as marine salt, soot, or ammonia.</p> <p>(Response to point 2 and 3) In later campaigns, we have added an ionic strength adjuster to the water used for the pH measurements, which should</p>	<p>✓ once again, a very good rewrite.</p> <p>✓</p>	<p>Thanks</p> <p>✓</p>

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		<p>3. With a timescale of pH measurement of 2 mins, it is doubtful that the distilled water reacts with anything other than materials on the immediate patina surface and any significant influence or "buffering" by clays in the patina or weathered surface is unlikely.</p> <p>4. After marine salts, I might expect organic acids or guano to be predominant. Line 3: What waters are you referring to in the statement "However, the higher predicted values are comparable to those measured for rainwater"? You state in Para 2 that the calculated values are from pH 8 - 11. Has rainwater of such alkalinity been measured at Murujuga? My recollection is that local rainwater is mostly circum-neutral and would not require equilibration with entrained dust/clay particles.</p>	<p>reduce its sensitivity. This has been added to the text. "If pure deionised water is used for the pH measurements, then the solutions are effectively unbuffered to pH change. Under these circumstances, the dissolution of tiny quantities of sea salt or other precipitates on the rock surface can cause a relatively large change in the pH. For this reason, and to stabilise pH measurements, an ionic strength adjuster was used in field measurements from campaign 5 onwards."</p> <p>Rainwater is fairly high pH at Murujuga. We are measuring rainfall pH in the wet deposition samplers at 21 AQM sites as well as rainwater samplers at 5 locations (Palmex Rain Sampler RS1). The former are analysed by the Chemcentre, the latter were installed on behalf of the UWA Geochemistry Centre, and they additionally measure pH for us as well as isotopic analysis. Therefore, we have 2 separate labs performing pH analysis. Both rainfall and wet deposition pH samples are up to ~8.5 as reported in the figures. We are also measuring dry deposition pH, which approaches pH 9.</p> <p>New text added to refer to the data: "As stated above, water placed on the</p>	<p>Thanks for the information.</p> <p>✓ OK</p>	<p>OK</p> <p>✓</p>

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			<p>rock surface for the field measurements is not expected to equilibrate with clay minerals on the time scale of the measurement. However, precipitation may interact with clay minerals that form dust within the atmosphere, where longer timescales are available for water--dust reactions. Indeed, the pH of rainwater at Murujuga is relatively high (7--8.5) (Figure 7.10-25). These high values may indicate partial equilibration with clay minerals, although other explanations, such as interaction with ocean aerosols, cannot be excluded."</p>		
223	p. 155-156	<ul style="list-style-type: none"> • Para 5 Line 5: In the sentence beginning "A number of silicate ..." remove either "that" or "but" to make sense. • Line 6: Remove "these". Page 156 • Para 1 Line 4: "There are 4 main classes of explanation"? Could you not write "There are four explanations". • Para 4 Line 7: "Simulations" is misspelt. 	<ul style="list-style-type: none"> • Removed "that" • Removed • Done • Fixed 	<p>✓</p> <p>✓</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p> <p>✓</p>
224	p. 156	Line 10: It would be useful to state the analytical technique.	Done. Analytical technique stated. "These values are below the detection limit of the Inductively Coupled Plasma--Atomic Emission	✓	✓

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			Spectroscopy (ICP--AES) technique used for analysis of the chamber studies product solutions (around 1e-6 moles per litre)		
225	p. 157	<ul style="list-style-type: none"> Para 1 Line 3: Close bracket after 4-55 Para 2 Point 5 Line 6" "system" [omit e]. 	Done	✓	✓
226		The section on Thermodynamic modelling concludes with a sentence containing a record number of words - 76.	Done. Sentence rewritten.	✓	✓
227	p. 157	Figure 4-86 Caption: It would be useful to mention the significance of the different colours of the cells (as was mentioned in the main text). Comment: As supplied to me as a review copy, this and subsequent Figures, and Table 4-12 are at distance from the pertinent text. As much of the explanation of the Figures is provided in the text, it would be beneficial to locate the Figures closer.	Done. Significance of colours mentioned, and figures moved closer to the relevant text in the revised version.	✓	✓
228	p. 159-164	Figures 4-87 to 4-92: Where features common to more than one Figure (diagram) such as "natural logs", these should be explained in the caption of the Figure in which they first appear, with "and following Figures".	Natural logs are only used in one figure. The text has been modified as requested and now reads "Concentrations on the y-axis are presented as natural logs for panels b--h. N(+5) refers to the total	✓	✓

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		When it is stated that concentrations are presented as natural logs, such as in Figure 4-90, you should say this applies to the elements Na, K, Si, but not N. How N(+5) on diagram axes and NO ₂ concentration in the caption are related should be explained.	concentration of species that include N with a valence of +5."		
229		<ul style="list-style-type: none"> In Figure 4-90: Why Y-axes are labelled "Ln [moles per kg] Na, but KLn[moles per kg]?" Some brief comments in an expanded caption on explaining what is shown in the diagrams would be very useful, given that a majority of readers of the Report are likely not familiar with thermodynamic modelling. 	<ul style="list-style-type: none"> Done. Y-axes are now labelled consistently. We have been requested to minimise caption length. However, the text and figures are now closer together, so I hope that this alleviates the issue. 	<p>✓</p> <p>✓ OK as before.</p>	<p>✓</p> <p>✓</p>
230	p. 163	Figure 4-91 Caption Line 4: "h" is missing. In caption of Figure 4-91 (page 163) Line 4: "h" is missing.	Done. (h) has been added.	✓	✓
231	p. 165	Table 4-12: Caption: "Granophyre mineral modes are based on the minerals featured in Figure 4-16." Same for "Gabbro mineral"	Done as requested.	✓	✓
232	p. 167	<ul style="list-style-type: none"> Para 1 (4.3.1) Pt 1b line 1: Sentence does not read sensibly in the context of point 1. i.e "to characterise diversity and relative abundance..." ["are present" is out of place?] 	Done. "Are present" deleted	✓	✓

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233	p. 167-168	<ul style="list-style-type: none"> Pt 1c line 1: Omit "Characterise" [repetition when in the context of P1.] Pt 2c line 1: Omit "to" Para 4 Line 2: Omit "due to" Para 5 Line 3: Better to replace "material" with "documents". Page 168 Para 1 Line 5: Omit "was" [the complete sentence reads better]. 	All corrected	✓	✓
234	p. 168	Line 6: AGRF is not mentioned in the glossary (Table 1-3) or to my recollection in the text.	AGRF add to Table 1.4-1.	✓	✓
235	p. 168	Para 2: Line 10 (and elsewhere in Report): insert a comma before "respectively".	Done	✓	✓
236	p. 168	Line 15: NMDS is not mentioned in the glossary (Table 1-3).	Added to Table 1.4-1.	✓	✓
237	p. 169	<p>Table 4-13:</p> <ul style="list-style-type: none"> Panel 4: line 2: "sequencing" [was misspelt]. Panel 5: line 2: "on sight" should be "on site"? Panel 6 line 2: "micribes"? Page 171 (4.4.2) Para 3 Line 1: "The design of the experiments is" [change "are" to "is"]. 	<ul style="list-style-type: none"> Fixed Fixed "microbes" fixed Fixed 	<p>✓</p> <p>✓</p> <p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p> <p>✓</p> <p>✓</p>

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238	p. 169	Para 4 Line 1: Suggest sentence be modified for clarity: "In brief, air pollutants in the chamber experiments are generated from hydrocarbon"	Modified	✓	✓
239	p. 169	Line 2: "fresh(ish)"? How does this relate to fresh and fresher rock? If appropriate, I suggest a change to "relatively fresh" or "reasonably fresh"?	"Fresher" has now been used throughout the report.	✓ And I am happy with that	Thanks
240		Para 5 Line 1: Change "exposures" to "exposed surfaces"; change "for 6 days incubation period" to " for a 6-day incubation period"...	Done	✓	✓
241		Para 6 Line 2: Omit "This is necessary as" and rewrite the sentence "In order to reduce the scope of the geomicrobial"	Done	✓	✓
242	5. Air quality monitoring network establishment and operation p. 173-176	<p>Page 173</p> <ul style="list-style-type: none"> • Para 1 Line 2: "a range of" [add "of"] • Para 2 Line 3: insert "whereas" after the comma. • Para 5 Line 1: "were installed at locations" [add "s"] • Para 6 Line 2: Beginning a sentence with "3" is not best practice. <p>Page 175</p> <ul style="list-style-type: none"> • Para 1 Line 1: "This contains a suite ..." 	<ul style="list-style-type: none"> • Added • Added • Added • Fixed • Fixed 	<ul style="list-style-type: none"> ✓ ✓ ✓ ✓ 	✓

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		<ul style="list-style-type: none"> Para 3 Line 4: "AQM sites" [make plural]. Page 176 Para 1 Line 1: "'rock cube' samplers have been" 	<ul style="list-style-type: none"> Fixed Added 	<p>✓</p> <p>✓</p> <p>✓</p>	
243	p. 176	Line 4: "etc. is not very informative. State what will be sampled for.	Amended to ... microbiome colonisation, pH and permeability	✓	✓
244	p. 176	Para 2 Line 2: "on island particularly suggestible to corrosion" - do you mean "on islands particularly susceptible to"?	Yes. Corrected.	✓	✓
245	p. 176	Line 5: "raspberry pis"?	As it is a computer not a food item, according to the github Raspberry Pi developer style guide, the The plural of "Raspberry Pi" is "Raspberry Pis", (no apostrophe). Capitalisation is now as per the style guide.	✓ Thank you.	OK
246	6. Spatial mapping and surface modelling				
247	7. Statistical analysis from each component study	Para 6 Line 2: To me "Fresh(ish) rock", as interior samples without weathered rind, means nothing. It is completely subjective and unscientific. How does it compare to "Fresher rock" which is a similar creation of	We have now used "fresher" throughout the document. It has been defined in the abbreviations and	✓	✓

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	p. 195	MRAMP which was vaulted as useful in previous reports. If you are to use "Fresh(ish)" you have to define it at least in terms of degree of alteration from fresh (unaltered) rock i.e what is the order of freshness: Fresh rock > Fresher rock > Freshish rock > weathered rind? An explanation in terms of mineralogy would be good.	glossary tables. It is the rock that is unweathered. For mineralogy, please refer to section 4.2.	OK	✓
248	p. 196	Para 4 Line 1: The initial two sentences are inconstant with previous one in this section: i. are written in the present tense, and ii. "pre and post exposure" has become "pre- and post-exposure" [the latter is the norm, but written language evolves. Whichever you consider best, you should be consistent.	This section has been edited for tense and consistency. It is somewhat challenging as we are essentially giving a progress report on ongoing work.	✓	Thanks
249		Line 3: "Pre/post exposure" is a further case of inconsistency	Fixed	✓	✓
250		Line 4: As the ICP-AES analysis determines quantity, change to "the concentrations of leached elements:	Changed	✓	✓
251		Para 5: Tense switches again to the present, before reverting to past in Para 6.	Edited	✓	✓
252		Para 6: Line 2: The sentence beginning "The exposure may be	Corrected	✓	✓

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		repeated ..." is a repetition of the previous paragraph and can be omitted.			
253		Para 8: Line 1: "Analysis are analysed"! [suggest change "are analysed" to "is performed"	Fixed	✓	✓
254		Line 3: tense changes at whim. Now we have "were measured: Why spell out Inductively Coupled Plasma-Atomic Emission Spectroscopy (ICP-AES) when you have previously used the abbreviation ICP-AES [e.g. in Para 4]?	Corrected	✓	✓
255		Line 6: "BDL" is given in the table of abbreviations, but has it been spelled out previously in the text?	Changed to "below detection limit (BDL)	✓	✓
256	p. 197	Figure 7-1: The caption is laconic in the extreme. What is shown by the dashed traces, as opposed to the solid line? The traces do not show the dose. Rather they show the effective concentration, don't they? They tell that the concentration of injected gas reduces to zero prior to the subsequent dose at t = 24hr. Is this actually the case in the chamber experiments?	The figure has been amended. The dose may be an average or overall value or may be a peak. So, we argue that the dose may be the peak or the integral of the curve(s). Both of which are inherently included in the figure. The figure is obviously a stylised/idealised example and varies due to reactivity of the gas. For NH3 the curve is a close approximation of reality.	✓ OK Fine	✓ ✓ ✓

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257		Para 3 Line 2: "no particulate contamination" [insert "particulate"].	Added	✓	✓
258	p. 198	<ul style="list-style-type: none"> Figure 7-2 Caption: Again, the caption is inadequate. What is shown by the grey shaded region? Was the hydrocarbon source in this case from exhaust 1 or 2? 	<ul style="list-style-type: none"> Description of the solid black line and grey shading is now in the caption. As stated in the text the hydrocarbon combustion experiments use cooled/diluted exhaust. Only NH3 experiments have used bottled gas, however bottled NO2 and SO2 experiments will occur in year 3. 	<p>✓</p> <p>✓</p>	<p>✓</p> <p>✓</p>
259		Para 3 (7.1.3.2) Line 1: "vol. ppm" or "ppm by volume"	Added	✓	✓
260	p. 199	<ul style="list-style-type: none"> Figure 7-3: Is the trace really the dose? - or is it a measured recording of the NH3 Concentration in the chamber? If the latter, did the NH3 concentration diminish to zero? If so, you should state the reasoning for running the experiment beyond 24 hrs. Time for surface reaction products to stabilise/crystallize? 	<ul style="list-style-type: none"> The dose may be an average or overall value or may be a peak. So, we argue that the dose may be the peak or the integral of the curve(s). Both of which are inherently included in the figure. Yes, the concentration decreased to 0 – or BDL (<0.1 ppm). <p>It is stated earlier in the text that all experiments are incubated for 6 days and the reasons given.</p>	<p>OK</p> <p>✓</p> <p>OK</p>	<p>✓</p> <p>✓</p> <p>✓</p>
261	p. 199	Para 1: Line 1/2: You do now address the meaning of "dose". What	At this stage we are determining the average exposure over the 6-day	✓	✓

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		is meant by "averaged over the 6-day period incubation period"?	experiment – or the integral of the exposure curve divided by the experiment duration. More sophisticated methods will be used later which will likely consider both peak and average values.		
262		Comment: A strange division of text into the briefest of paragraphs has appeared in this section of the report.	Corrected	✓	✓
263		Para 4 Line 4: Is "(after minus before exposure)" a specific term in statistics? I find its precise meaning a puzzle.	This has been rephrased to be more digestible for non-statistical readers.	✓ great	✓
264		Para 5 Line 3: "(after exposure minus before exposure)" Is it the same as "change in concentration (or effect)" after exposure"?	As above, has been rephrased.	✓	✓
265		Line 4: For the benefit of the reader who is not involved in the experiment, can you clarify what is meant by "peak-normalised".	Peak normalisation is a common signal processing method. E.g. see Wikipedia reference below. As such we do not believe it needs to be defined in detail. https://en.wikipedia.org/wiki/Audio_normalization	OK	✓
266	p. 200	<ul style="list-style-type: none"> Para 4 Line 2: "for this purpose: the best choice of method ..." [replace semi-colon with colon]. 	<ul style="list-style-type: none"> Fixed 	✓	✓

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267	p. 201	<ul style="list-style-type: none"> Para 2 Line 4: Change "smooth" to "smoothed" Line 5: As above. 	<ul style="list-style-type: none"> Fixed 	✓	✓
268		Figure 7-4: Meaning of the abbreviated rock names - e.g. "GPHF" - should be given in the caption of at least the first of this group/type of figure.	GPHF and GPHC is defined in the first type of these figures	✓	✓
269	p. 202	Para 4 Line 5: Change "hover" to "however".	Fixed	✓	✓
270	p. 206	Para 1 Line 1: How does your "after-minus-before change" cater for pH being a logarithmic scale? e.g. a 1-unit reduction in pH beginning at pH 7 involves only a tenth of the reactants/acidity as a 1-unit reduction from pH 6 and 1/100th that from pH 5. (see also Figure 7-13). Was change in acidity and alkalinity measured? I can appreciate that any change would be difficult to record by conventional titration.	<p>At this stage pH is not being used as a parameter for dose/response relationships for EQC determination. As you note the nature of the logarithmic pH scale would need to be considered if that changes.</p> <p>The pH of the quartz filter substrate "blank" is used as an indicator of the overall acidity/alkalinity of the exposure at this stage. Given the reaction which occurs on the rock, it is unclear at this stage the relationship between exposed acidity/alkalinity however the chamber studies data and thermodynamic modelling will assist in understanding these relationships, which we expect to explore more fully in year 3.</p>	<p>✓</p> <p>✓</p> <p>✓ That's fine.</p>	<p>✓</p> <p>✓</p> <p>✓</p>

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271	p. 211	Figure 7-13: Is comparison of pH change in different experiments and for different lithologies legitimate? Possibly it is if the starting pH is the same, but is that the case for different contaminant gases (e.g. SO ₂ , NH ₃)?	See above. We expect to be able to answer this in year 3 when we have more data.	✓ OK	✓
272	p. 212	<ul style="list-style-type: none"> Para 8 Line 4: "specied: change to "species" Line 5: "reducing" [omit space]. 	<ul style="list-style-type: none"> Fixed 	✓	✓
273	p. 213	Figure 7-14: Here is an example of where a more explanatory caption allows better understanding/observation of the figure. Could the physical change seen in (c) 'pre' and 'after' be a result of the Vibromet attrition during the sample preparation?	We focused on the holes to avoid any impacts of the polishing process, as stated in the text. This has been clarified further with new text "For this reason, observations focused on cavities within minerals, which would not have been modified by the Vibromet polish."	✓	✓
274	Section 7.4.1 p. 237	Para 1 Line 2: "of in previous research" [omit "in"]	Omitted	✓	✓
275	p. 239	Line 2: Spell out ISA [first time mentioned in this section?]	Done	✓	✓
276		Question: Where pH differences are presented on box plot figures, is the use of a linear +/- scale appropriate given pH is a logarithmically derived value? The dimension of the box will be greater/smaller depending on the median pH?	Boxplots do not require statistical assumptions; rather, they are used to evaluate statistical assumptions such as the choice of scale. The upper and lower limits of the box in a boxplot are determined by the first and third quartiles. A change of scale would simply transform the limits of the box to match the transformed quartiles.	[no response from reviewer]	-

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			The symmetry of the box about the median (indicated by the midline) can be used to assess the appropriateness of the choice of scale.		
277	Section 7.4.3 p. 240	Para 8 Line 1" The one question" is a most fundamental one. I would expect ALL rock exposures at Murujuga, an archipelago on an otherwise exposed coastline, to be affected my marine aerosols.	This is likely true, however the degree of impact is likely varied based on distance from coast and wind patterns. Also of note is the inland salt water cooling towers which have existed post industry, which create a fine salt source where previously there was likely to have been lower salt aerosol exposure. These are items for further study.	[no response from reviewer]	-
278	p. 246	Para 4: use of "2" and "3" in the text of this paragraph is unconventional, in particular the commencing of a sentence "2 of these"	Fixed	✓	✓
279	Section 7.6.1 p. 259	Para 1 Line 1: "visualising" really? Hopefully you were actually observing.	Visualise, dictionary sense 2: to make visible to the eye. Common terminology in microscopy.	Appropriately admonished!	Thanks
280		Figure 7-59 caption: Better written as: "Scanning electron microscope, backscattered electron image of thin section of granophyre sample from site AQ03. Surface at top of image. Physical size of image is about 10 mm wide by 7.5 mm high. It is far	Done	✓	✓

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		preferable to add a scale bar to the image.			
281		Para 2: Line 1: For absolute clarity "In this section of the Report". Possibly rewrite as "In this section of the Report we analyse such images to determine the rock porosity and to investigate its spatial variation, both laterally and with increasing depth from the surface".	Thanks. Changed	✓	✓
282	p. 251	Para 6 Line 2: add comma before ", respectively".	Done	✓	✓
283	p. 242-255	Comment: Although the plotting of Figures 7-48 to 7-55 is discussed in the main text, addition of a minimum information to the figure captions would greatly benefit the non-expert (Statistics) reader. Even the simplest of things such as the positioning of the rock names.	In the text an explanation on how to interpret these Figures has been added. In the results, additional text has been added	✓	✓
284	p. 256	<ul style="list-style-type: none"> Para 5 Line 4: My comments here have been stated previously. What do you mean by skewed? The choice of SiO₂, a ubiquitous major oxide present in all silicate minerals, and Ti a minor element most commonly present in ilmenite, and in titaniferous magnetite in lesser amounts, is a strange pairing with which to 	<ul style="list-style-type: none"> A statistical distribution is skewed when one tail extends much farther than the other tail. While rock types can be characterised by the ratios of various elements (for example a high proportion of Ti to SiO₂ is 	I am aware of that. Which way (trend) were the distribution skewed? Accepted. My comment referred to the common irregular distribution of ilmenite, and thus Ti, in granophyre.	In most cases the distribution is right-skewed (heavy right tail) Noted

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		<p>investigate a geochemical signature of Murujuga rock types.</p> <ul style="list-style-type: none"> Using a pXRF in the field, you are effectively determining the spatial distribution of ilmenite which is typically uneven. For what purpose? 	<p>suggestive of ilmenite), this analysis sought to investigate whether, for a given rock type, the precise proportions of elements were approximately constant across Murujuga. If this were not true, it would have caused complications for the analysis of other spatial trends.</p> <ul style="list-style-type: none"> Subsequently it was noted that pXRF results can serve as a surrogate for information about patina thickness. This forms another reason to investigate spatial trend in the pXRF results. 	<p>✓</p>	<p>✓</p>
285	p. 257	<p>Para 1: The reason for studying element ratios in the Murujuga rocks is not made clear and your interpretation of the results quite bizarre. You state that geoscientists often employ element ratios, but it is in well define contexts with distinct goals.</p> <p>You state that the element ratios do not vary greatly within each rock type. What were you expecting in large intrusive rock bodies? You do not directly comment on the very different abundances in the rocks of SiO₂ and Ti, but ratios of up to 4000 show that this element pairing is unsuitable for much in the way of rock</p>	<p>First paragraph reworded to clarify the reasons for pXRF analysis. "Patinated rocks of different types can look very similar to researchers who are not familiar with the different rock types at Murujuga, so the pXRF was used to provide supporting information to confirm rock identification. The pXRF provides information from the top few millimetres of the sampled rock, so it samples the patina, weathered rind, and fresher rock. Further investigations were undertaken to assess the utility of the pXRF as a tool to assess patina composition and patina thickness."</p>	<p>✓</p>	<p>✓</p>

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		classification/characterisation. I would like to see the actual Ti concentrations measured by pXRF and the calculated precision of the measurements.			
286		Figure 7-56 essentially indicates the relative abundance of Si in the rock types. The ratioing of SiO ₂ with S, Cl and V is even more dumbfounding. Why were these ratios chosen? They could well show some environmental implications. Without any clear reason, geoscientist readers will likely conclude "because they were there". The use of Cl ⁻ is particularly questionable given the proximity of the rock exposures to the ocean and the likely presence of marine salts on the rock surfaces.	We have rewritten the text to clarify the purpose of the element ratio mapping. The new text reads "The ratios of pairs of elements were used to assess the utility of pXRF as a way to monitor patina composition and thickness. The concentration of elements thought to be enriched in the patina (e.g. Ti, Cl, Mn, S, V) were compared with the concentrations of elements thought to be enriched in the substrate (e.g. Si). In this analysis, it was hypothesised that highly patinated samples would have a stronger signal from patina-hosted elements such as Ti, and a weaker signal from weathered rind and fresher rock-hosted elements such as Si. If this signal is a first-order control on element ratios then the pXRF could be used to map patina thickness across Murujuga. The distribution of single elements was also considered. However, element ratios measured by the pXRF are much more consistent than absolute values, so ratios were preferred for this assessment.	✓ Yes much better.	Thanks

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			The composition of the substrate varies with rock type; granite and granophyre have higher silica contents than basalt, dolerite, and gabbro. This variation provides a possible confounding factor to the proposed analysis. To explore the extent of this variation and the effect that it might have on Ti/Si measurements, boxplots of the ratio of silica to titanium were constructed (Figure 7.5-9), classified by rock type and by campaign. Note the logarithmic scale on the vertical axis."		
287	p. 258	Figure 7-58. I would like to see the actual Ti concentrations measured by pXRF and the calculated precision of the measurements.	A more detailed analysis including this information is planned for the next report.	✓ OK	✓
288	p. 260	Para 2 Line 2: "side lengths" change to "dimensions" Having read this, and the following paragraph, I am convinced that a scale bar should be superimposed on each image of this type.	The term 'dimension' can be misinterpreted so we prefer the unambiguous 'side length'. It is the intention to provide scale bars for microscope images but this has not yet been done.	? If it will be done, that's great.	The term 'side length' has been retained. This has been done.
289		Para 5 Line 1: Observation is clearly incorrect given the broad band of resin atop the rock surface. Better to write "In Figure 7-60, the void space appears to reduce progressively from the surface to depth in the fresher rock.	The statement was meant to exclude the resin that is exterior to the rock itself. The wording has been clarified.	✓	✓

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290		Para 7 Figure 7-61 and subsequent figures: You should consider putting much of this discussion of the panels into the figure caption (see my general notes).	See previous comments regarding style the guides we are required to use.	✓ as before, your response is understood.	Noted
291	p. 261	Figure 7-60: Show scale or dimensions of the image, and aerial relationship to that of Figure 7-59?	Figures 7-59, 7-60 and 7-61 are all exactly the same microscope field of view. Unfortunately Figure 7-59 was reversed in the document. These have been repaired and are now numbered Figures 7.6-1, 7.6-2 and 7.6-3 in section 7.6.	✓	✓
292	p. 262	Figure 7-61 Caption line 2: "field 4 of 4"? Spatial relationship of area shown in (b) to that of (a) should be clarified.	Panels (a) and (b) represent exactly the same field of view. Panel (b) is a transformation of panel (a).	OK	✓
293	p. 263	Figure 7-62: In (b) the symbols x and + for porosity features are practically indistinguishable on the diagram and in the key. Make larger and clear. Units of porosity not given.	Graphical symbols have been improved. Porosity is a dimensionless fraction.	✓ ?? really? φ is a fraction 0 - 1 or pu is 0 -100%	✓For clarity, we have added further explanation in the caption and in the accompanying text.
294	p. 264	Section 7.6.4; This section titled Porosity Index states the need for a single index of porosity for each sample. It proposes two possible options. These are discussed but nowhere is it stated how a single porosity index is derived from these options. As no index values are given, and mere reference made to an	It is unclear whether the reviewer is questioning the choice made between the two options, or the procedure for calculating each index. The text in this section has been expanded in an attempt to clarify it.	Apologies ✓	✓ ✓

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		Appendix, the reader is left with no idea of the nature of the single Index and how it was arrived at?			
295		<ul style="list-style-type: none"> Figure 7-63: The overly brief caption is generally meaningless to the anyone viewing the figure. The congested and overlapping appearance of the circular symbols makes the figure impossible to interpret, e.g. on the smaller island in the west (no North arrow shown) it appears that three porosity values are superimposed on a single location - there is no explanation. Similarly, larger circles in the northernmost part of the peninsula overlap on a single location (= multiple porosity measurements at a single granophyre exposure OR at multiple granophyre exposures"). Locations with low porosity values may be rare on the peninsula or simply obscured on the figure? There are no units given or explanation of the list of numerical values. These things will be obvious to the author but not to most readers and not understandable when the figure is referred to in isolation of 	<ul style="list-style-type: none"> Captions are required to be brief according to the style guide. More explanatory text has been added. The difficulty in presenting the porosity data graphically is that multiple rock samples were taken at the same location, and because of differences between individual rocks, several different values of porosity were observed at the exact same location. The graphical technique used here (which is well established in spatial statistics) is designed to portray the entire dataset faithfully including both 'signal' and 'noise'. Concentric circles occur if there are discrepancies between the porosity values at the same location. A single (blurred) circle 	<p>Understood</p> <p>✓</p> <p>This explanation could usefully be included in the caption or text.</p> <p>✓</p> <p>? as above</p> <p>✓</p>	<p>Ok</p> <p>✓The figure has been replaced by a different style of figure which avoids this problem by using 3D perspective</p> <p>✓</p>

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		the main text (see my general notes). Overall Figure 7-63, in its current form, is of little value. More clarity of the presentation, or use of a different kind of diagram is required.	<p>indicates that the porosity values are (almost) equal at the site in question.</p> <ul style="list-style-type: none"> Porosity is a dimensionless quantity. In future communications, alternative graphics will be considered. 		The caption and accompanying text have been clarified.
296	Section 7.6.5 p. 265	Para 2: The observations discussed in this section of the Report are potentially of considerable importance to the goal of the Murujuga project.	Noted	✓	✓
297		<p>Figure 7-63 is useless and, I suspect, Figure 7-64, while clearer, also has significant flaws. Use of smoothing must be justified. Although, the interpretation - e.g. porosity is lower in the north and west - may eventually be vindicated, it is evident that the areas of paler yellow and orange shadings are predicated by a general absence of measured locations. Similarly, any colour differentiation shown on the main Murujuga peninsula is a product of a sparsity of measurements in the extreme north and south. The critical observation "Larger values are observed close to the industrial area and towns" cannot be evaluated in detail since the towns and industrial areas are not shown on the figure!</p>	<p>A discussion of Nadaraya-Watson smoothing is provided in Appendix G including discussion of its assumptions, caveats and diagnostics. This allows the merits of the approach to be laid out in a general and neutral context without prejudicing the application to the porosity data.</p> <p>A fuller discussion of the methodology and its appropriateness in this context will be provided in a separate technical journal article.</p> <p>There are indeed fewer observations in the north and west of the study region. It is important to remedy this imbalance given the importance of the findings. The project has collected</p>	<p>OK</p> <p>✓ excellent</p>	<p>✓</p> <p>Thanks</p>

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			<p>additional rock samples from these locations to produce a more comprehensive dataset (yet to be analysed).</p> <p>The lower values (pale yellow and orange colour) in the smoothed image do not arise because of a paucity of data. This is not how the smoother works. A paucity of data in an area causes the few available observations in that area to be more influential in determining the smoothed values (higher 'leverage') for that area, but paucity of data has no consistent effect on the smoothed value, as discussed in Appendix G.</p>	OK	✓
298	p. 266	Para 1 Line 1: If the Monte Carlo test is deemed appropriate and the results significant, the description of the test should not be demoted to Appendix G-3.	An exposition of the Monte Carlo test in a general context was felt to be necessary so as to canvas the merits and weaknesses of the approach in general, without prejudicing the application to porosity data.	✓ OK	✓
299		Figure 7-65: The distributions of p-values and significance in this figure appear to have important implication for the Murujuga study but cannot be evaluated by this reader without more information.	These are advanced statistical techniques which may be unfamiliar to many readers. Their interpretation is, however, clearly stated at the end of this subsection.	✓	✓
300	p. 266	The text on page 266 retrospectively gives useful instruction on the efficacy of the spacial porosity distribution	See responses above.	OK	✓

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		shown of the various figures, BUT it is essential that information on the Monte Carlo and Global tests, currently only in the Appendices, is presented instead in the main text.			
301	p. 273	Figure 7-72: mD on the y-axis is not explained (Permeability; or milli-Darcy). The scale appears NOT to be logarithmic as stated in the caption. Am I correct in thinking the colour scheme for permeability is the reverse of that used for previous porosity figures? Unfortunate if true.	mD - (milli-Darcy) is mentioned in section 7.7.1 Para 2. Line 7 Thank you. It is not logarithmic scale. Caption has been updated. Permeability was represented by a new colour scheme. The selected colour scheme makes it easier to distinguish the various quantities of permeability when a spatial plot of observed permeability is added.	✓ ✓ ✓ very good	✓ ✓ ✓
302	8. Associations between variables	[No comment provided]	-	-	-
303	9. General summary p. 391	Para 2 Line 4: I suggest "...these past emissions may be the major contributor"	Changed	✓	✓
304		Para 3 Line 3: After ".... hypothesis from the literature (???)" provide the main references.	Added	✓	✓

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305		Para 3 Line 5: With present state of the thermodynamic modelling and the major question of its efficacy for a clearly non-equilibrium chemical/biochemical system such as the weathered rind/patina, I consider the comment "and this is supported by thermodynamic modelling" cannot be justified. It should be omitted OR significantly qualified to reflect the limitations of the current thermodynamic data.	Done. Caveat added. "Data from our bucket samplers of rainfall and dust deposition show that the system tends towards slightly alkaline/basic and this is consistent with the results of thermodynamic modelling, although care must be taken with this interpretation because the modelling assumes thermodynamic equilibrium, which is unlikely to be reached in the natural environment."	✓ good	✓
306		Para 4: Line 7/8: "ground level CH ₂ O or O ₃ can be expected to impact the microbiome at a sufficiently high concentration" - precise meaning is equivocal i.e. ground level CH ₂ O or O ₃ may impact the microbiome because they are at times in sufficiently high concentrations to do so OR they would impact the microbiome if they were to grow to a sufficiently high concentration but are not necessarily at that concentration currently.	Sentence revised for clarity	✓	✓
307		Para 5 Line 2: "which frequently contain particularly high concentrations of engravings).	Changed	✓	✓
308		Para 5 Line 2: Remove spurious bracket "("preceding "which".	Removed	✓	✓

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309		Line 3: Sentence beginning "Peak levels from point sources ..." should be rewritten as meaning is unclear. Possibly "often to expose very narrow bands of terrain to higher concentrations of airborne contaminants due to frequent high wind speeds"?	Revised for clarity.	✓	✓
310		Para 6 Line 1: The sentence beginning "The chamber studies " does not state specifically what the hypothesis from the porosity studies is? Presumably it is that porosity is highest in regions affected by past and/or present industry and population).	Revised to aid clarity.	✓	✓
311	p. 392	Para 2 Dot Pt 2: line 2: "more field data are needed" [data = plural].	Changed	✓	✓
312		Para 3 Point 2: The text beginning "As well as investigation " is not a sentence [has no verb]. You should add a verb OR replace full stop after "system" with a comma [so combining into a single sentence].	Sentences merged.	✓	✓

3 Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Report 2024

3.1 Dr António Batarda Fernandes (2024)

Document title:	MRAMP Interim Environmental Quality Criteria (October 2024)
Document ID	COPP21065-REP-G-107
Reviewer:	Dr António Batarda Fernandes
Date review commenced:	October 2024 (document re-issued for peer review at Rev D)

Item no.	Section	Peer reviewer comment Date: 17-11-2024 Revision: Rev D	MRAMP team response Date: 20-01-2025 Revision: Rev E	Peer reviewer close-out comment Date: 07/02/2025 Revision: Rev E	MRAMP close-out comment Date: 12-03-2025 Revision: Rev 0
1		As a general comment, I must note that considerable effort has been done to improve this report. However, I'm afraid I don't believe that intelligibility of the text has considerably improved. There are still some typos, but mostly sentences and paragraphs continue quite convoluted, and, at passages, they do not connect effectively. I will signal some of the issues	The document has been revised for readability	Overall readability has improved, despite some typos still ("anthropo- genic", p. 1)	This typo and other typos have been fixed.

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		<p>below, but overall, the text reads somewhat as “patchwork”.</p> <p>On the other hand, I found the conclusion more to the point and somewhat addressing the issue of future emissions that I noted previously in my review dated 27 June 2024 below. But I will discuss it in more detail in the ensuing comments.</p> <p>So, the report still needs further revision.</p>			
2	1.1	<p>“Environmental Quality Criteria (EQC) established thorough the studies”</p> <p>Example of a typo.</p>	<p>“thorough” changed to “through”</p>	<p>Ok</p>	<p>✓</p>
3	1.3	<p>“Due to the complex nature of the rock surface weathering processes taking place at Murujuga and the prolonged timescale over which (non-anthropogenic) weathering occurs, it may not be possible to develop EQC values for all gas species present in the Murujuga airshed that would typically be considered criteria pollutants.”</p>	<p>Criteria air pollutants are:</p> <ul style="list-style-type: none"> Ozone Particulate matter Carbon monoxide Lead Sulphur dioxide Nitrogen dioxide <p>Criteria air pollutants have been selected largely based on human health effects. Some of these pollutants are either (a)</p>	<p>I understand that this is a summary and as such should not include lengthier discussions already carried out in previous more extended previous documents.</p> <p>Nevertheless, can a passage be added to the text summing up your reply?</p>	<p>The text has been expanded/clarified and Criteria Air Pollutants have been added to the glossary</p>

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		<p>So, is it worth it to state here what can be done to counter this?</p>	<p>unlikely to impact the rock microbiome or (b) not present above trace levels at Murujuga or (c) naturally occurring in the rock/mineral structure in the case of lead (Pb).</p> <p>Geogenic particulate matter is an important source of manganese and iron for patina formation.</p> <p>Ozone is a secondary pollutant created by photochemical processes, primarily the reaction between NO₂ and organic compounds. Chamber studies using ozone are planned, however are challenging due to the stability of ozone. EQC for NO₂ and/or hydrocarbons to account for any ozone effect would likely be preferable and easier to implement from a regulatory perspective.</p> <p>Carbon monoxide is only present at very low levels compared to other gas species. It is also utilised in many microbial and cell biology studies to improve</p>		

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			<p>growth/viability, so it is unlikely to impact the rock art or microbiome at the levels present.</p> <p>Much of the above is discussed in the Monitoring Studies Report 2024 and will be expanded in future reports.</p> <p>Any criteria air pollutants for which EQC cannot be or are not developed will either be at trace concentrations in the airshed or have insignificant effect on the system.</p>		
4	1.4	<p>“Patina (...) There are many examples where a fully developed patina/varnish layer has regrown in engraved grooves.” Were -> where</p> <p>The fact that this typo remains somewhat worries me as I had noted it before (comment #7) in my review dated 27 June 2024 of the Murujuga Rock Art Monitoring Program: Draft 2024 technical report - COPP21065-REP-G-105 (Rev B) and the MRAMP team response of 27/08/2024 to my comment</p>	<p>Done</p> <p>We apologise for this oversight. We certainly take all reviewer comments very seriously. This issue was perhaps due to version control from having multiple document editors.</p>	Ok, thank you.	✓

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		noted that “This section has been revised for clarity.” So, this begs the question: are reviews being completely taken into account?			
5	1.4	Patina Moreover, on this definition, and I apologize for only noting this now, perhaps the rapport between “Biofilm” and “Patina”, for the purposes and scope of the research program, also as the team understands it (as you refer that “In the Murujuga context the texture and color of the rock surface is referred to as a patina”), could be further detailed in ?	Due to differences in definitions of specific features such as “desert varnish”, patina was selected as a more general and all-encompassing term for the rock surface, including all types of varnish and biofilms which may be present.	Ok, but could it be further detailed?	The definition has been further revised to specifically address biofilms and other biological material.
6	1.4	Weathering As noted in comment 4, this is another instance where I believe my suggestions were not considered. In comment #8 of my review dated 27 June 2024 I suggested that “This definition should be revised as it is confusing and has some phrasing/orthographic issues” to which the team replied, “Definition revised”.	This definition was written in conjunction with the Murujuga Aboriginal Corporation and specifically includes Degradation as their preferential term for anthropogenically accelerated weathering. It has been used in several documents. While we can see the reviewer’s point and attempt to take all reviews into account we often need to account for conflicting	Ok	✓

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		Nevertheless, the definition in the present Report is the same as the one included before.	advice/opinions between reviewers and traditional custodians.		
7	2	<p>“NOEC or equivalent (Piegorsch, 2014; Piegorsch et al., 2014; Pires et al., 2002; Hothorn, 2014; Van Der Hoeven, 1997; Van Straalen, 1997; Das, 2018; Jensen et al., 2019; Heringa et al., 2020; Fisher and Fox, 2023; Ritz et al., 2015; Fox and Landis, 2016; De Bruijn and Hof, 1997; Negri et al., 2021; Mebane et al., 2008; Green et al., 2013; Iwasaki et al., 2015).”</p> <p>Do all these need to be cited here? The issue is that for the rest of the report you do not include citations in such high quantity as here... I believe this impairs the fluidity, and therefore, readability of the text...</p>	These references were intended to indicate that the determination of no effect levels or concentrations are a rapidly emerging field. Perhaps these references may be better in a footnote, or conversion of the references to a bibliography (i.e. not requiring all articles to be cited in the text)?	Ok, I see your point. Perhaps a footnote in this particular passage?	We agree on the readability concerns however this would be a departure from the referencing style used throughout the documents to date. We will discuss this suggestion with the government as a possible editorial change prior to public release of the document.
8	2	<p>“frequentist techniques as well as Bayesian statistical”</p> <p>So which one do you prefer? Or will they be used complementary? This whole sentence feels somewhat disconnected from the rest of this section. Also, the use of</p>	Frequentist techniques have been used throughout the project to date, however several members of the statistical team are highly experienced in Bayesian methods as well.	<p>Ok, thank you, I appreciate the explanation.</p> <p>Does it make sense to add it, summed up, in the Glossary, in an entry called “Statistical methodology” or “Statistics”?</p>	We think it would be difficult to do justice to these fundamental concepts in a few lines for inclusion in the Glossary.

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		<p>new concepts (I believe “frequentist techniques” are being noted for the first time; at least I could not find it in the COPP21065-REP-G-105 (Rev B)), perhaps it is worth it to include it in the Glossary, together with “Bayesian statistics”?</p>	<p>The researchers seek to apply the most appropriate method or approach for the circumstance.</p> <p>This text was simply intending to highlight that Bayesian methods are common in no effect (and hence EQC) determination and as such there may be valid reasons for incorporating Bayesian methods in the future.</p> <p>All statistical methodology is based on the same mathematical theory of probability, but there are different possible interpretations of the meaning of probability, which lead to different methods for statistical inference (i.e. for drawing conclusions from data). Frequentist statistical methodology is based on the interpretation that the probability of an event (for example the probability that a coin toss results in Heads) is the average frequency of occurrence of the event in a long run of random trials (for example the average fraction of Heads in a large number of coin tosses). This</p>		

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			<p>was the standard approach to statistical inference until the late 20th century. It is still common practice to use frequentist methods unless another approach is explicitly stated. Formal statistical conclusions, such as hypothesis tests of significance, are frequentist techniques. Frequentist methods are used throughout this report unless otherwise stated.</p> <p>Bayesian statistical methodology is based on the interpretation of probability as a measure of an expert's degree of belief, and provides a way to update the degree of belief in the light of new evidence. It is well suited to applications where information from different sources must be combined -- for example combining experimental data, background scientific knowledge, information from similar but unrelated experiments, and data observed at different spatial scales. Bayesian methods are useful for calculating the best estimate of a number (for</p>		

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			<p>example, a toxicity threshold) given a variety of background information.</p> <p>A popular analogy is that frequentist methods correspond to the rules of evidence in a criminal court (where the standard of proof is `beyond reasonable doubt') while Bayesian methods correspond to the rules of a civil court (where the standard of proof is `the balance of probabilities'). Most applied statisticians use both frequentist and Bayesian methods in different contexts as appropriate to the application.</p>		
9	5.2	<p>“This estimate of historical emissions may explain the spatial pattern of rock surface porosity found from the Murujuga field samples, which is most elevated in the samples from the sites encompassing AQ07–09, AQ11, AQA1, EX02 and EX09. This would therefore suggest that anthropogenic acceleration of weathering (at least as expressed by the environmental indicator of rock surface porosity) may have</p>	Taken as a comment	Ok	✓

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		<p>occurred historically. Limited historic rainfall pH measurements from Bednarik (2007) suggest that rainfall pH may have been significantly lower (more acidic) than the neutral levels measured in recent years (Curtin University, 2024b). These observations may support the hypothesis of historic impact which may no longer be occurring under the current lower emissions scenario."Let's see if further work confirms this. See also next comment.</p>			
10	5.3	<p>"unlikely that the EQC proposed here will be exceeded unless current industrial activity changes substantially." At this point, I have to come back to my only comment (below) to the previous version of this report: "Is it foreseeable that emissions will increase (or further decrease) in the near future?"</p>	<p>Please refer to the comment below in the closeout box.</p>	<p>Ok. I note that you mention that "no new industry on the Burrup post current approvals". So, this begs another question: are the effects of new industry that has been approved but not yet (fully) active been considered in MRAMP studies so far?</p>	<p>Yes. Our understanding is that all known future emissions are accounted for in the Ramboll 2030 scenario. Furthermore the MRAMP monitoring design has accounted for these current/recent approvals since they were a known possibility at the commencement of the MRAMP project.</p>

Document title:	MRAMP Interim Environmental Quality Criteria (June 2024)
Document ID	COPP21065-REP-G-107
Reviewer:	Dr António Batarda Fernandes
Date review commenced:	June 2024

Item no.	Section	Peer reviewer comment Date: 27-06-2024 Revision: Rev B	MRAMP team response Date: 27-08-2024 Revision: Rev C	Peer reviewer close-out comment Date: 09-09-2024 Revision: D	MRAMP close-out comment Date: 20-01-2025 Revision: E
1	5.3 Precautionary actions	Is it foreseeable that emissions will increase in the near future?	The Ramboll (CAMx) air quality modelling includes a future (or “2030”) scenario which has a slight increase in NH ₃ emissions, however other emissions remain the same or lower under future scenarios.	Ok. However, one of the points of this comment was to inquire on the possibility that future industrial development on the region may lead to increase in emissions. That said, how will “other emissions remain the same or lower under future scenarios”?	Our understanding is that the Government have committed to no new industry on the Burrup post current approvals. Hence the assumptions in the Ramboll 2030 scenario are likely correct.

3.2 Professor Geoffrey Michael Gadd (2024)

Document title:	MRAMP Interim Environmental Quality Criteria (October 2024)
Document ID	COPP21065-REP-G-107
Reviewer:	Professor Geoffrey Michael Gadd
Date review commenced:	October 2024 (document re-issued for peer review at Rev D)

Item no.	Section	Peer reviewer comment Date: 25-11-2024 Revision: Rev D	MRAMP team response Date: 20-01-2025 Revision: Rev E	Peer reviewer close-out comment Date: 03-02-2025 Revision: Rev E	MRAMP close-out comment Date: 12-03-2025 Revision: Rev 0
1	Sections 1.1-1.3	Predominantly as in previous report with some rearrangements in presentation	Comment	OK	✓
2	Section 1.4	The glossary has been added. Some minor points: <ul style="list-style-type: none"> Under “photoautotrophs” heading, heterotrophs are referred to but have not been defined Under “photolysis” heading – better phrasing is “... broken down into smaller molecules...” rather than fragments 	<ul style="list-style-type: none"> Heterotrophs have been added back into the Glossary table. Photolysis has been updated using suggested phrasing. This definition was written in conjunction with the Murujuga Aboriginal 	OK OK I would have thought expert scientific advice would receive	✓ ✓ Taken as a comment.

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		<ul style="list-style-type: none"> Under “weathering” heading – as mentioned before “degradation” is an inappropriate term to use in the context of weathering: there is also no need to use this as you have defined what you mean by natural weathering and accelerated weathering 	<p>Corporation and specifically includes Degradation as their preferential term for anthropogenically accelerated weathering. It has been used in several documents.</p> <p>While we can see the reviewer’s point and attempt to take all reviews into account we often need to account for conflicting advice/opinions between reviewers and traditional custodians</p>	<p>precedence in assessing scientific content.</p> <p>I have made comments about use of degradation in the context of weathering before and now understand why they were never fully addressed. No further comment.</p>	
3	Section 2.1 Chamber studies	Essentially the same as in previous report – no comment	Comment	OK	✓
4	Section 2.2 Field studies	No comments on the additions (largely outside my expertise)	Comment	OK	✓
5	Section 3 EQC	No comments on the added sections (largely outside my expertise)	Comment	OK	✓
6	Section 4 Ongoing EQC	Essentially the same as in previous report – no comment	Comment	OK	✓

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7	Section 5 Monitoring	<p>Not my area so not so easy to interpret. Minor question:</p> <p>The figures have been modified from the previous report. Regarding Figure 7 and the text conclusion that "...no exceedances are observed based on the current interim EQC Guidelines..." What are the implications of this for future monitoring in the project?</p> <p>Re Section 5.2, no real comments except that the Figure 8 is difficult to assess as someone who is red-green deficient with poor differentiation of colours affected by this.</p> <p>Re Point 6 and the WHO annual NO2 guidelines, it is stated that "...these levels may not be relevant for geochemical weathering processes, however may be applicable to microbiome health". Is there any specific information that would support these two conclusions? Further, not all</p>	<p>Regardless of any exceedances during MRAMP monitoring to-date, it is vital that monitoring continue to ensure that levels of key pollutants do not increase. EQC will also be revised based on further data.</p> <p>The colours of the dashed lines in Figure 7 and 8 have been changed slightly so that the red/orange/green colours can be distinguished by and therefore more accessible to people with Colour Vision Deficiency (CVD).</p> <p>Noted and agreed. The intent of the comment was to suggest that bacteria and fungi may have a response to these stressors that more closely resembles a human physiological response, compared to the abiotic</p>	<p>Yes, agree</p> <p>Thank you! I appreciate this. It is a widespread problem with data presentation in current times.</p> <p>OK thank you. Re second sentence – I do not think that consideration of human physiological responses is particularly helpful, especially regarding bacteria. Fungal responses may be relevant in some cases as such eukaryotes are often used as "mammalian" models but many differences can occur, not least being the</p>	<p>✓</p> <p>Taken as a comment (agreed).</p> <p>Noted and agreed. However these are the only available levels to benchmark against (excluding higher occupational exposure limits, which are even less relevant).</p>

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		<p>components of a microbiome will be affected in the face of external stresses and a variety of differential responses may occur.</p> <p>Section 3 – this is a lucid summary justifying conclusions based on findings to date. I see that future work is proposed to consider the microbiome, an aspect of the project that has not featured in this particular report.</p>	<p>responses of a geochemical system.</p> <p>Noted. We are trying to accelerate the microbiome chamber studies as much as possible.</p>	<p>mechanisms of pollutant accumulation, cellular entry etc.</p> <p>Good luck. I hope they are successful.</p>	<p>Thank you</p>

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Reviewer:	Professor Geoffrey Michael Gadd
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1	General	This is a detailed report that reports the progress in establishing Environmental Quality Criteria based on the data obtained through years 1 and 2. It seems clear that further refinement of such EQC determinations will occur with future work.	Noted.	OK	-
2	Section 1 – Overview	Specific scientific comment on the scientific and theoretical aspects of EQC generation are outside my specialism but I have some general comments:	Noted.	OK	-
3		As EQC criteria are developed, how will these be enforced in the future regarding emissions from industrial activities?	The state government (DWER) are the regulatory body who will be responsible for implementing and enforcing.	Thank you.	-

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4		Mn was not detected in patina-free rocks, but where does the Mn come from in patina encrusted rocks? Does the Mn come from the substrate and/or aerial particulate deposition for example?	There is Mn in patina-free rocks, but in much smaller quantities than in the patina. The Mn is thought to come from aerial particulate deposition, and not from the substrate.	I see. Is there adequate data/information on the Mn levels that may be deposited aerially? Further, are the Mn-containing particulates Mn oxides for example or is the Mn present generally in other kinds of aerial particulates? In the particulates, the Mn is presumably in solid or bound form so by what mechanism are the organisms liberating this and accumulating it in their cells? Cyanobacteria for example are not generally associated with any solid/mineral dissolution mechanisms. (this point might be relevant to the causality comments on porosity: it seems more likely that the cyanobacteria are "taking advantage" of easier colonization of high porosity locations rather than increasing the porosity themselves. Any comment on this?)	We are undertaking chemical characterisation of bulk dust samples currently so will have this information in upcoming study report(s). For this reason, the form of Mn in dust is not yet fully known. The bulk of the mass of Mn is likely to be mineral oxides however some other forms may be present. We agree with the reviewer and our thoughts are along similar lines. We hope to be able to answer these questions in the following report(s).
5		Tables 1 and 2 are the same.	Table 1 is in the overview/executive summary and has been repeated in the main body. The document has been extensively revised in response to review.	OK thank you.	-

3.3 Dr John Henstridge, Data Analysis Australia (2024)

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Document ID	COPP21065-REP-G-107
Reviewer:	John Henstridge, Data Analysis Australia
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1	Overview	Overall this is a well presented document that tries to present a well-argued case for the specific EQC levels. I have relatively few comments to make.	Taken as a comment	Accepted.	✓
2	1.3	This section provides a good overview of the approach and provides a caution that the EQC levels are to be regarded as interim. It also presents the proposed interim EQCs. This section does not make it clear whether for the combined NO ₂ and SO ₂ guidelines	Taken as a comment The current guideline EQC requires and exceedance of NO ₂ *and* SO ₂ to trigger an EQC exceedance. This was	Accepted I have concerns about the *and* approach as in theory it could mean that for a single pollutant there is no limit. I welcome that	✓ Individual EQC is indeed a priority. WHO guidelines may indeed be irrelevant however are the only current exposure

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		whether the combination uses “and” or “or”. These are both acidic species so I expect a trigger is raised if either is above the guideline.	selected as the preferred approach at this stage as the EQC were developed by field and chamber studies using both gases and the values are lower than WHO human health guidelines. Future revisions will develop individual EQC for each of these gas species.	there is the intention to set up individual EQCs and hope that this can be prioritised. I note but am unsure of the relevance of the WHO guidelines.	guidelines, apart from (much higher) occupational exposure criteria.
3	2.1	The accelerated weathering to establish the LOEC appears to be well thought out except that it makes an implicit assumption that the effect of the pollutants is linear with respect to both time and concentration – that it, the only relevant quantity is the integral of the pollutant concentration over time. This may well be true, or it might not be. As I understand reaction rate mechanisms, if the pollutant concentration is the limiting factor in the reaction step that determines the overall effect, then this might be the case.	We confirm that in the calculation of the effect of historic emissions for this interim EQC, we have assumed that the effect is linear with respect to time and concentration. This working assumption is justified by the paucity of historical data on atmospheric pollution concentrations. The chamber studies show evidence of a nonlinear dose-response relationship, but this occurs at much higher concentrations than those occurring in the atmosphere. A linear approximation to the dose-response relationship seems appropriate for low concentrations (appealing to Taylor approximation). If this is accepted, then the key issue is whether the slope coefficient of	I note the change in the wording and the statement of the assumption that concerned me. I appreciate that there is an openness to reviewing this in the future as more information is available and the mechanisms are better understood. The cumulative buildup of material on the surface over time between rainfall events creates the distinct possibility of the impact not being linear with time. It also means that the time between significant rainfall events becomes important. I am not trying to make this over complex but it does point out that extrapolation from chamber studies is not straightforward. The uncertainty involved here is	Noted. Our current position is that linearity with respect to time and concentration is a working assumption which must continue to be scrutinised as more data are collected. We agree with this assessment. Dependence on rainfall history is one potential cause of non-linearity in this very complex system.

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		<p>This assumption is key to scaling from what is measured in the six days of the lab test to what might happen over a year in the field, giving the conversion factor of 60. Explicitly acknowledging the assumption and commenting on the reasonableness is required.</p>	<p>the linear approximation is accurately estimated by extrapolation from the chamber experiments. These provisional calculations will be subjected to further scrutiny in the coming year.</p> <p>The text has been amended as follows: “This conversion inherently incorporates a factor of ~61 reduction (60.875 for a year length of 365.25 days) as it averages a six-day dose over 12 months. It assumes a cumulative reaction process between rock and atmosphere in dry periods, the products of which are flushed during significant rainfall events. Such events are irregular, however, it can be expected that rainfall greater than 20 mm in 24 hours occurs at least every year based on long term climate data (Curtin2023). This assumption may change as further data is collected, however at this stage it is considered reasonable given a sufficient variation from the (approximately neutral) pH provided by rainfall is required</p>	<p>substantial which raises the issue of what a conservative approach might be.</p> <p>In principle it is possible to carry out a chamber experiment to explore this but this would have limited ability to extend to the lower concentrations that we are really concerned about.</p>	<p>Agreed.</p>

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			for any thermodynamic effect on the mineral assemblage (Curtin 2024)."		
4	3.1	I am concerned about the apparent inconsistency regarding the guideline levels for NO ₂ and SO ₂ (Table 3), and the tenfold difference in their effects on rock (Figures 3 and 4). I realise that the scales in the figures are dimensionless so a direct comparison is difficult (not clear why they do not use a more comparable y-scale) but the fact that the experiments appear to have used very different levels of pollutant implies that SO ₂ has a much greater effect. Hence it is reasonable to expect that the EQC guideline is lower by a similar amount. Something needs explaining.	Combustion processes generate SO ₂ from sulphur in the fuel and NO ₂ /NO _x from nitrogen in the air (unless there is also nitrogen in the fuel). Therefore most combustion processes using modern fuels have a significant mismatch between NO ₂ and SO ₂ levels. Future experiments will overcome this by utilising separate (single) species exposures in the chamber studies.	I can understand that observed levels a NO _x and SO ₂ will be different since they have different source mechanisms. I can also understand that the effects could be very different. This emphasises the importance of future single species chamber studies.	Agreed. Taken as a comment.
	5	The discussion of how multiple sites will be assessed according to the EQC does not make it clear how information/results across sites will be combined. Issues of multiple comparisons and correlations between sites will eventually come up and should at least be	The intent is that EQCs be applied/assessed across the entire archipelago such that all rock art is protected. The selection of monitoring sites was undertaken using appropriate models to ensure accurate reconstruction or interpolation of	Noted	✓

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		acknowledged. I would accept that it is probably too early for detail here.	air quality profiles across the entire region. Details on how the monitoring data from multiple sites will be assessed against the EQC are still under development. Issues of multiple comparison will certainly arise.		
	5.2	The “back forecasting” to see how these EQCs would have worked in the past provides a good indication as to how these EQCs might work in the future. The tentative conclusion that anthropogenic weathering may now be slower than before needs follow up work before it should be accepted. In particular consideration of a cohort effect (we are observing rocks at different periods after exposure) may need to be considered.	Noted and agreed. This work will be undertaken.	Noted.	✓

Document title:	MRAMP Interim Environmental Quality Criteria (June 2024)
Document ID	COPP21065-REP-G-107
Reviewer:	John Henstridge
Date review Commenced:	June 2024

Item no.	Section	Peer reviewer comment Date: 23-07-2024 Revision: Rev B	MRAMP team response Date: 27-08-2024 Revision: Rev C	Peer reviewer close-out comment Date: 29-11-2024 Revision: Rev D	MRAMP close-out comment Date: 12-03-2025 Revision: Rev 0
1	General	-	-	-	-
2	Section 1 – Overview	The overall purpose of this document is to propose the approach of scaling up exposure from specific experiments of observations to give annual measures of exposure that can be used to set warnings and triggers.	Noted	-	-
3	2.1	The approach taken here is very conservative as is appropriate at this stage in the study. This will be refined as more data provides a better dose response relationship and a better understanding of the effect of rain in removing pollution.	Agreed.	Acknowledged.	✓

Item no.	Section	Peer reviewer comment Date: 23-07-2024 Revision: Rev B	MRAMP team response Date: 27-08-2024 Revision: Rev C	Peer reviewer close-out comment Date: 29-11-2024 Revision: Rev D	MRAMP close-out comment Date: 12-03-2025 Revision: Rev 0
4	3	<p>I again note that I have trouble with the simple SO₂+NO_x when these two species have such different effects, as is illustrated by Figure 2 and Figure 3. It seems better to give greater weight to SO₂ that has greater effect.</p> <p>I am not sure whether the x-axes in Figure 2 and Figure 4 are correct. Should the first label be 5000 or 500? Either the labelling or the scale is not correct.</p> <p>The apparent non-linearity in the dose response supports the conservative approach taken in section 2.1. However it also raises the issue of how the rock surface reacts to varying concentrations over time and questions whether simple averages (as recorded by passive samplers) are enough.</p>	<p>Agreed. This has been done.</p> <p>Figure 2 has been updated</p> <p>We agree with this assessment. It is possible that transient peaks of emissions may be more important than overall average levels. This will be investigated once a larger body of data is available.</p>	Acknowledged.	✓
5	4	I agree with the caution expressed here.	Noted	Acknowledged.	✓

Item no.	Section	Peer reviewer comment Date: 23-07-2024 Revision: Rev B	MRAMP team response Date: 27-08-2024 Revision: Rev C	Peer reviewer close-out comment Date: 29-11-2024 Revision: Rev D	MRAMP close-out comment Date: 12-03-2025 Revision: Rev 0
6	5	Within the limitations of the current stage of the project, I think that the simple analysis of the proposed EQC based on SO ₂ and NO _x is appropriate.	Noted and agreed.	Acknowledged.	✓

3.4 Professor Janet Hergt (2024)

Document title:	MRAMP Interim Environmental Quality Criteria (October 2024)
Document ID	COPP21065-REP-G-107
Reviewer:	Professor Janet Hergt
Date review commenced:	October 2024 (document re-issued for peer review at Rev D)

Item no.	Section	Peer reviewer comment Date: 09-12-2024 Revision: Rev D	MRAMP team response Date: 20-01-2025 Revision: Rev E
1	1.1	Last sentence of paragraph 2. I think “thorough” is supposed to be “through”, or “throughout”?	Changed to “through”
2	1.4	The text in Table 2 relating to Biofilm is a little ambiguous (after the yellow highlighting). I believe “it” refers to the single species, but could be misread to mean that biofilms are not common in environmental contexts. Perhaps change to something like “...however biofilms in environmental contexts typically involve multiple microbial species”	Amended as suggested
3	1.4	The last two sentences in the entry in Table 2 for “Culturally Important Place/Culturally Important Site seem to include unnecessary duplication. Is this deliberate? If not, is it possible to combine these sentences?”	This definition was written by MAC. We have attempted to revise it as suggested however this will require MAC review.
4	1.4	In the entry defining photoautotrophs, there is reference to heterotrophs “defined above”, but I can’t see it as a separate entry	This has now been added to the table

Item no.	Section	Peer reviewer comment Date: 09-12-2024 Revision: Rev D	MRAMP team response Date: 20-01-2025 Revision: Rev E
		under "h"...is it within the description for another term? Perhaps define it as a separate term and add to the table.	
5	2.2	Third paragraph...the sentence I've highlighted is a little confusing. It may just be the locations of commas, but I think rewording would help clarify what is being said here.	The text has been amended for clarity
6	3.3	The word "determine" in the first sentence should have a d on the end.	Done
7	5	Remove n as highlighted.	Done

Document title:	MRAMP Interim Environmental Quality Criteria (June 2024)
Document ID	COPP21065-REP-G-107
Reviewer:	Professor Janet Hergt
Date review commenced:	June 2024

Item no.	Section	Peer reviewer comment Date: 25-06-2024 Revision: Rev B	MRAMP team response Date: 27-08-2024 Revision: Rev C
1	General		
2	Section 1 Overview	Seems fine	Noted.
3	5.2	p. 15 Final paragraph in this section. The possible historical contribution to rock porosity seems problematic. Are there ways to determine rates of change given that emissions have now been reduced? How many years of monitoring might this require?	We agree that it is vital to determine if the elevated porosity is due to historic or current emissions. Significant effort will be expended on this task, both through analysis of additional field samples and chamber studies. It may be possible to use the permeameter to non-invasively monitor a surrogate for porosity. If this is possible then these field measurements, combined with measurements on rock cubes in the AQMs will permit monitoring to ensure the porosity is no longer being anthropogenically increased.
4	5.3	p. 15. There's a space missing between "NH ₃ " and "as"	Corrected.

3.5 Associate Professor Dr Ron Watkins (2024)

Document title:	MRAMP Interim Environmental Quality Criteria (October 2024)
Document ID	COPP21065-REP-G-107
Reviewer:	Associate Professor Dr Ron Watkins
Date review commenced:	October 2024 (document re-issued for peer review at Rev D)

Item no.	Section	Peer reviewer comment Date: 05-12-2024 Revision: Rev D	MRAMP team response Date: 20-01-2025 Revision: Rev E	Peer reviewer close-out comment Date: 09-02-2025 Revision: Rev E	MRAMP close-out comment Date: 12-03-2025 Revision: Rev 0
1	General	The Report is well presented throughout and contains very few typographic errors. I empathise with the authors having the task of explaining often complex concepts relating to the development of EQC while, I assume, having a brief to include only minimal results that are presented comprehensively in the main progress report. As stated (Section 1.2), it is meant to be read in conjunction with no less than 5 significant reports. However, for this EQC Guideline report to be	Agreed. Taken as a comment	✓ The authors have attended to each of the points in my initial review. I have verified that all amendments have been made and are sound. Where they have argued the veracity of my suggested modifications, I have accepted the MRAMP position in all bar two significant cases - Items 22 and 31 . I would encourage the authors to pay further attention to these.	Noted and addressed below.

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		<p>understandable and its main deliberations interrogated, it has to contain a sufficient body of background information in the form of results and measured data. This is particularly important given the fact that the current Interim EQC are founded upon just two factors: the concentrations of NO₂ and SO₂ in the Murujuga airshed, and relatively small (lateral) spatial variations of surface porosity in the granophyre rocks. The formation of the latter is supported by initial laboratory chamber experimentation.</p> <p>The main criticism I have of this report is what I consider a lack of supporting evidence for a number of highly significant conclusions, albethey 'interim', that are presented. This is most prominent in the case of the porosity, where details of the spatial distribution and the means of assessment/results are almost completely absent. Indeed, any discussion of the porosity variation does not appear until a single paragraph on the last page of text (22). In</p>	<p>Point taken. We have presented a detailed treatise on the science underpinning this document in the year 2 report and didn't think it was sensible to repeat it here.</p> <p>We have added the text: "The design of these experiments was informed by observations of spatial gradients in porosity, which are</p>	<p>✓ As stated elsewhere, I am sympathetic to constraints imposed on the authors with regard to reporting limits and formats.</p> <p>✓</p>	<p>Thank you</p> <p>-</p> <p>✓</p>

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		<p>viewed, by a cynical readership, as a desire not to overstate possible environmental impacts, despite a stated allegiance in the report to a "precautionary approach". Any such speculation can easily be nullified by a sound discussion/justification in each case where presently an unsupported supposition occurs.</p> <p>I have recorded once again my questioning of the validity of the EQC for NO₂ + SO₂ requiring that the trigger has both components exceeding the individual threshold values.</p>	<p>inevitable synergistic interactions between multiple air pollutant species in the data utilised to date, it is prudent to select the chamber studies based values and combined (NO₂+SO₂) exceedance for the purposes of Interim Guideline EQC determination."</p> <p>In the case of the interim standard for NO₂, a decision was made to add this later when we realised we could develop EQC from the field porosity data and cumulative historic emissions (hence the restart of the review process for this document). Given the deadline it was only possible to utilise the historic peak (annual average) estimate. However additional text has been added to explain how this will be revised (down) in future.</p> <p>"More sophisticated approaches will be utilised in the future based on findings of the MRAMP studies work currently in progress and will likely include interpolated historic emissions levels from</p>	<p>✓</p> <p>✓</p>	<p>-</p> <p>-</p>

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			the entire region where significantly elevated porosity have been found, combined with standards derived from chamber studies.”		
3	General	<p>In this review, I have made numerous suggestions as to how the text might be improved to aid understanding. I accept that many may be seen as pedantic, and they are made solely in an advisory way, to be accepted or rejected as the authors choose.</p> <p>I have, as clearly as possible, explained the less numerous criticisms of the scientific content. I feel that these points should be addressed, as far as practicable, in order to enhance the value of the report.</p>	Noted. Suggestions for improvement are appreciated.	Thank you.	✓
4	1. Overview	<p>Page 1 Para 2 line 2: Omit comma after "studies," line 5: Change "thorough" to "through"</p>	<p>Done Done</p>	<p>✓ ✓</p>	-

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5		<p>Para 3</p> <p>Line 1: re. "The study". Which study? In the previous paragraph you state that there were multiple studies.</p> <p>Placement of "(art)" is incorrect e.g. implies there is a subject of art geology. Suggest: "..... all potential impacts of atmospheric exposure on the rock art (petroglyphs) with respect to geology/geochemistry, microbiome and visual appearance".</p> <p>Line 4: Omit "As such".</p> <p>Line 7: "Minerals" might better be replaced by "Components" as there is more than inorganic crystalline material that likely contributes to patina formation.</p>	<p>Clarified</p> <p>Edited</p> <p>Omitted</p> <p>Done</p>	<p>✓</p> <p>✓</p> <p>✓</p> <p>✓</p>	-
6		<p>Para 4 line 1: "to-date" includes a hyphen. Previously and subsequently in the document it is written without, "to date".</p>	Done	✓	-
7		<p>Page 2</p> <p>Para 1 Line 3: This sentence is convoluted. You could improve clarity of understanding "Standard EQC levels, denoted by the threshold of the red HIGH region on the Risk scale in</p>	Done	✓	-

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		Figure, represent limits above which sufficient evidence exists that impacts are likely to occur, within appropriate levels of confidence. Guideline EQC levels are set lower using appropriate uncertainty factors to determine the minimum level where any possible effects could theoretically occur."			
8		Line 6: Similar to above: "These levels are denoted as the threshold from green LOW to the amber UNCERTAIN regions of Risk in Figure 1."	Done	✓	-
9		Figure 1: The heading at top of the figure beginning "Description: Weathering ..." can, on first reading, appear ambiguous. Possibly better modified to "Description: Degradation of the rock art is not accelerated above the natural pre-industry rate as a result of current anthropogenic emissions"	This is taken from the Murujuga Rock Art Strategy, published by the government prior to our MRAMP project. As such we cannot edit it.	Of course! I should have recognised that. Apologies. ✓	✓
10		Para 2 line 4: may be improved as: " measurable and auditable, and standardised approaches given for measuring indicators and for comparison of	Done	✓	-

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		monitoring data against the criteria".			
11		<p>Page 3</p> <p>Para 2, line 1: Omit "studies" - awkward and unnecessary.</p> <p>Line 4; Omit "some environmental pollutants" and replace brackets with commas. ie. "... EQC values to be established for NO₂, SO₂, and NH₃, as these are the key gaseous pollutants."</p> <p>Line beneath Table 1 requires greater spacing from the table border and addition of a comma before ", respectively".</p>	<p>Done</p> <p>Done</p> <p>Greater space has been added between Table 1 and the notes about subscripts. Comma has been added.</p>	<p>✓</p> <p>✓</p> <p>✓✓</p>	-
12		<p>Para 5 dot point, line 1: Replace "Due" with "Owing".</p> <p>Line 3: suggest "gaseous species" instead of "gas species".</p>	<p>Done</p> <p>Done</p>	<p>✓</p> <p>✓</p>	-
13		<p>Page 4</p> <p>Para 2 Line 1: "The following report" appears out of place at this point in what I have been reading as the Report! Change to "This report" or possibly "The following sections of the report"</p>	<p>Changed to "This report"</p>	<p>✓</p>	-

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14		Page 5 Line 1 of the glossary needs to be respaced (at least it appears so on my copy).	Corrected	✓	-
15	2. Methodology for ECQ development	Page 7 Para 1 Line 2: Pedantic yes, but strictly LOEC, as defined in the Glossary is singular, therefore requiring "LOECs have been" or "LOEC has been".	Changed to "LOECs have been"	✓	-
16		Page 8 Para 1 line 4: "NOEC/LOEC" might be confused as meaning a ratio. Change to "NOEC and LOEC" Line 6: Here I think "Year 3" is better or else "third year of studies"	Done Changed to "third year of studies"	✓ ✓	-
17		Para 2 line 1: Having commenced a new paragraph, the use of "Nevertheless" is unclear and is unnecessary -. Omit it. Line 5, 6: The sentiment in the sentence beginning "Given the uncertainty" namely there is at the present time too few data to set guideline values precisely is prudent. However,	"Nevertheless" has been omitted. Addressed in comment number 2	✓ OK	-

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		<p>I question the justification in the use of "appropriate" in your decision to select the higher two guideline values. I feel you are on shaky ground to make such a statement <u>without</u> any mention of on what you base the "appropriateness". In my experience, it is commonplace for regulatory bodies to default to the lower values when guideline values have some uncertainty, so as to ensure environmental protection until such time as more data are available. See my earlier General Comments. My sense of this is that your decision is a counter one which could be interpreted by a cynical readership as indicating a preference of the Report's authors not to unnecessarily worry the emitters. Which does have merit but, if this is the case, it should simply be stated.</p>			
18		<p>Para 3 Line 2, 3: re: "These experiments have been designed to conduct"? You do not conduct accelerated weathering - you conduct accelerated weathering experiments! The</p>	<p>Changed</p>	<p>✓</p>	<p>-</p>

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		sentence is best written as "These experiments have been designed to induce/produce/promote* accelerated weathering of rock samples" * choose one of these words, or course.			
19		Para 4 line 2: Use of however is not well suited here. I suggest adding a full stop after "...2024b)." and starting the following sentence "Key points are summarised here." Note that I suggest this configuration at other places in the report.	Done	✓	-
20		Para 5 line 1: Rewrite as " Reagents used in chamber experiments leading to current Interim EQC were:" (omit "doses of")	We disagree with the term "reagents" as we are not predicting nor designing a specific reaction. E.g. in human and animal toxicology, exposed chemicals are generally termed "insults". Furthermore, many of the studies utilised "whole exhaust" exposure and not all chemicals would be considered reagents as some would be inert under the conditions but nevertheless may contribute.	✓ Point accepted.	✓

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			We understand the selection of the term "reagents" from a chemistry perspective, however the term would be inappropriate for the microbiome experiments and we wish to keep a general term for all studies.		
21		Para 6 line 1: Add "gaseous reagent" or "gas" before "dose" Line 3: Use of "agents" is questionable and potentially confusing. You are actually measuring leachable cumulative concentrations of selected elements. I suggest a change of text to "The response leachate concentrations were measured"	See comment above	✓ accepted, as above.	✓
22		Para 7: Be aware of the difference of using "lower limit of detection" and "lower limit of determination (syn. lowest reporting limit) - both unfortunately commonly written as "lld". Your reference to detection limit in Para 6 is, I believe, correct. It is likely so also regarding the values given for Mn, Fe and Al. However, it appears incorrect for the values given of Ca, Mg, Na and K where normally detection limits	We have used BDL/DL terminology in other documents so at this stage it is probably best to maintain consistency with terminology. These values are termed the Limit of Reporting (LOR) by the ChemCentre who undertook the analysis of the leachate samples. We have had extensive discussions on the use of ICP-MS/AES/OES and have run samples on all 3 instruments however LOR is	Your response does not alleviate my concern that the terminology in the text of the report is incorrect and potentially a source of uncertainty. Particular, mention in the MRAMP response that results provided by the ChemCentre are termed LOR (Limit of reporting) indicates that you may, on occasion, inadvertently be misrepresenting data.	The term limit of reporting (LOR) has now been included in the Glossary and added at the appropriate places in Section 2 (where previously referenced as BDL). A broader discussion of LOR and detection limits is likely warranted to allow consistency between documents going forward, and to ensure the

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		<p>by ICP-AES are a magnitude or more lower. I think the concentrations given for these 4 elements are the reporting values, i.e. lowest limit of determination, since resolvable peaks at various wavelengths should be detectable from the spectrum, although likely beyond the range of precise measurement.</p> <p>Throughout your experiments it would be the lowest level of determination that was important in contributing quantitatively to your summation of leached elemental values.</p>	<p>the same. We have also obtained the raw data from the ChemCentre for more detailed analysis in year 3.</p>	<p>Simply, you state that you have consistently within, and between reports, used BDL and DL. According to the Glossary, BDL is Below Detection Limit (and by assumption DL is Detection Limit. The results you received from the ChemCentre, as you state, are termed Limit of Reporting, synonymous with Limit of Determination. These two, BDL and LOR, are NOT equivalent, and are determined by the analyst in different ways. I do not have the space here to elaborate and am confident that the MRAMP team will be knowledgeable of the difference. Rather, I provide an example: In the rejoinder to my suggestion of using blank concentrations of Al to elucidate whether there was a contaminant source of this element in the structure of the experimental chambers (Report Item 23), you state that quartz-fibre filters did not return "detectable Al", i.e. the results were below the Detection Limit.</p>	<p>usage of the appropriate terminology.</p> <p>Noel Cressie (one of the lead statisticians) has investigated the concept of detection (/reporting) limits extensively. E.g. Cressie, N., Buxton, B. E., Calder, C. A., Craigmile, P. F., Dong, C., McMillan, N. J., Morara, M., Santner, T. J., Wang, K., Young, G., and Zhang, J. (2007). From sources to biomarkers: A Bayesian approach to human exposure modeling. <i>Journal of Statistical Planning and Inference</i>, 137, 3361-3379.</p> <p>We have agreed with the ChemCentre and other laboratories to obtain the raw data with a view to trialling the approaches suggested by Noel, with a view to reporting consistently across program documentation.</p>

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				<p>If my assumption that you derived this from ChemCentre results that were below LOR, then your statement is invalid, as the DL will almost always be lower than LOR. Be assured that I am not questioning your reasoning, in this example, which is sound, only your use of terminology. In a report dealing with a topic (EQC) in which accurate and precise geochemical data are pivotal, I do suggest that you appraise your present, and past, use of the terminology.</p> <p>With regard to your comment on the LOR being the same for two different techniques, ICP-AES and ICP-MS, that generally have sensitivities 1 or 2 orders of magnitude different, this is possible. However, when you state the diverse values of concentration listed on Page 9 are "detection limits", I find this extremely unlikely.</p>	
23		Page 9			

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		Para 1 Line 5: It may have been discussed elsewhere, but the experimental blank would provide evidence of whether the AI is from the chamber construction?	The text has been amended for clarity: "Blanks consisting of 25 mm quartz-fibre filters were utilised in the chamber and did not return detectable AI, however this element did not present a consistent trend so was omitted. This decision may be revisited in future."	✓ but see above.	✓
24		Para 2 line 10: Shorten "Alkali feldspar, which can form orthoclase, microcline, or sanidine, depending on the structure of the mineral," to Alkali feldspar, as orthoclase, microcline or sanidine, is the most likely source"	Done	✓	-
25		Para 3 line 1: change "exposed dose" to "dose of reagent gas to which rocks samples were exposed to produce a measurable response" Line 1, 2: "Rock types" is equivocal. Subsequent text states that it is "all experimental rock samples", so better to write as such.	See above re the term reagent. Changed. The text has been clarified to: "... therefore the combined response/effect for all replicate rock samples across all rock	OK ✓ ✓	-

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		Line 5: What is meant by "were combined in this manner". I appreciate that reference is made to the Report of Curtin University 2024b, but it should be simple enough to explain here in this document. This is particularly pertinent with regard to the statement of the importance of developing EQC based upon the most vulnerable rock type.	types were grouped for analysis..."		
26		Para 4 line 2: "this LOEC value" is more clearly written as "the LOEC value as derived above"	Done	✓	-
27		Page 10 1st line in equation note: add a comma after "test", 2nd line of equation note: the definition of T_a is unclear. The T presumably refers to a time interval, which in this case is stated as 1 year, but what is involved in "the recalculation to"? Is it not superfluous and can be omitted?	Done LOEC is recalculated based on averaging the cumulative dose over the 6 day experiment over a year. Such that in this case $T_c/T_a = 6/365.25$. Additional text has been added to clarify below the equation.	✓ ✓ this clarification is useful.	- Thank you
28		Para 1 line 2: "Mechanism of action"? Suggest:	Changed	✓	-

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		"It assumes a cumulative reaction process between rock and atmosphere in dry periods, the products of which are flushed during significant rainfall events" Line 3: Change to "Such events are irregular, however, it can be expected that rainfall greater than"	Changed	✓	-
29	2.2	Para 2 line 1: Change "scenarios and exposures" to "conditions".	Done	✓	-
30		Para 3 Information introduced in this paragraph and expanded in subsequent paragraphs is absolutely critical to the development of the Interim EQC, namely rock surface porosity that is currently the single recognised expression of atmosphere: rock reaction. It is important that the text is clear and comprehensive. I believe this brief paragraph could be improved by minor revision that clarifies what is meant by 'spatial gradients'. The spatial gradient, stated as 3%, in porosity, I assume to be a lateral or aerial variation. I also assume, but would like clarified,	Edited as suggested.	✓ I consider the edit is appropriate and adds clarity.	Thank you

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		<p>that any porosity change with depth below the rock surface, is either insignificant or not considered.</p> <p>Suggested revision: Lateral spatial gradients have been observed in the near-surface porosity of granophyre rock on Murujuga from samples collected from the field. This has prompted a hypothesis by which surface weathering is predominantly caused by abiotic processes. This is supported by the results of chamber experiments, both chemical and physical, as determined through microscopy. The variation in porosity found in the field represents a change in the order of 3% across the spatial gradient of the study region (Curtin University, 2023).</p>			
31		<p>Para 4 line 1: May be rewritten: "If the variation in porosity is the result of anthropogenic activity, it likely represents a cumulative effect over 57 years since the bulk of industrial activity in the vicinity of Murujuga commenced around 1966 (Hamersley News, 1969).</p>	<p>Changed</p> <p>Expanded explanation added. "Detailed statistical analysis (Curtin University, 2024) cannot explain this elevated porosity</p>	<p>The change made in accordance with my suggestion makes the following text "<i>approximately 57 years prior to MRAMP sample rock collection.</i>" superfluous. It should be omitted from the revised report.</p>	<p>Amended</p> <p>(ii). Text has been clarified.</p>

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		<p>Line 3: You need to state the evidence for the unqualified statement "The variation in porosity is most closely related to NO₂ rather than other compounds or resultant acids such as HNO₃."</p> <p>Line 4: omit "Given" and change "observed" to "recorded".</p> <p>Line 5: omit "this",</p>	<p>through natural environmental conditions or location variables. Also based on detailed statistical analysis of spatial gradients in porosity compared to current and historic spatial gradients in air pollutants, the variation in porosity is most closely related to NO₂ rather than other compounds or resultant acids such as HNO₃. SO₂ is slightly more complicated, as historic models show SO₂ gradients largely coincident with NO₂, however MRAMP measurements show SO₂ peaks on the outer islands, likely due to reductions in fuel sulphur, especially in non-marine fuels"</p> <p>Done</p> <p>Done</p>	<p>The expanded explanation is most useful but I draw your attention to ambiguity in the use of "peaks": "peaks on the outer islands" may be construed as either (i) there are a number of distinct peaks in SO₂ on the outer islands or (ii) the overall concentrations of SO₂ becomes greatest in the region of the outer islands. Further minor clarification would be useful.</p> <p>I don't think so.</p> <p>,as above, and furthermore "a a" has appeared. The sentence should read "These observations and the neutral rainfall pH recorded during MRAMP support a formation associated with drier periods....."</p>	<p>The second "a" has been removed.</p> <p>Sentence modified as suggested.</p> <p>-</p>

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		<p>"mechanism of action"? write alternatively "a formation associated with drier periods, rather"</p> <p>line 6: Replace "This resulted in a decision" with "In light of this, a decision was made to undertake"</p>	<p>Done</p>	<p>✓</p> <p>✓</p>	<p>-</p>
32		<p>Para 5 line 1: Omit "also"</p> <p>Line 4: You might consider adding "considered to be" before "the best" OR otherwise justify using best e.g. by it being the most complete, most applicable, etc.</p> <p>Line 4: re. "The Ramboll model averages across 1.33 km" averages what? Atmospheric gas concentrations?</p> <p>Line 5: Re. the sentence "Peak values and historic values are likely higher than estimated, for this region and due to reductions in sulphur in transport fuels over time, in turn reducing SO₂ emissions". I fail to make sense of this sentence, e.g. which peak values and historic values are higher than which estimates? Syntax and</p>	<p>Done</p> <p>Done</p> <p>Clarified</p> <p>Clarified</p> <p>Done</p>	<p>✓</p> <p>✓</p> <p>✓</p> <p>OK - still a little clumsy.</p> <p>✓</p>	<p>-</p> <p>-</p> <p>-</p> <p>This paragraph has been further revised.</p> <p>-</p>

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		<p>punctuation need attention if meaning is to be clear.</p> <p>Line 6: Depending upon how the previous sentence is resurrected, "However" should likely be omitted.</p> <p>Line 8: Add "those from" or "emissions from" after "it does not include".</p> <p>Line 10: Replace "NO₂/NO_x" with NO₂ and NO_x to avoid any confusion (with a ratio).</p>	<p>Done</p> <p>Done</p>	<p>✓</p> <p>✓</p>	<p>-</p> <p>-</p>
33		<p>Page 11</p> <p>Para 1 Line 1: I suggest sentence be rephrased: "A provisional calculation of the Interim Guideline EQC threshold was performed from the field observations of elevated porosity in granophyre, as follows:"</p>	<p>Done</p>	<p>✓</p>	<p>-</p>
34	3. Interim EQC	<p>Page 12</p> <p>Para 3 lines 3 - 7: I repeat my objection to the arguments put forward in this part of the paragraph as they appeared in the previous MRAMP Report. Namely, 1. I am not at all sure of your claim that the effects of NO₂ and SO₂ interaction with the rock surfaces will</p>	<p>1. We agree with this comment, however at present have no basis to differentiate the relative effects of the two species due to the combined experiments. This will be undertaken in the following year as we are now</p>	<p>✓ This is a perfectly fair response, and I understand you cannot do everything at once in a 5-year program.</p>	<p>Thank you</p>

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		<p>necessarily be similar. This on a number of grounds including differing nature of secondary nitrates and sulphates; the different interaction with bacteria and micro-fauna; and flushing characteristics of the anion species in solutions.</p> <p>2. The reliance on near-constant NO₂ and SO₂ ratios over time, historically and in future, when the types of industry that may be introduced onto the Burrup are unknown.</p> <p>3. The insistence that both NO₂ and SO₂ concentrations must exceed guideline values to trigger response, thereby seemingly giving free rein for atmospheric NO₂ theoretically to rise to any level and be "acceptable" so long as SO₂ (more easily scrubbed from emissions) remains low.</p> <p>I note that the MRAMP team response to my objections previously was very reasonable, most charitably stating that I was making a good point. I also am aware that the Interim Guidelines will unlikely be exceeded or invoked before</p>	<p>primarily undertaking single gas species exposures for this reason. We expect to present individual EQCs in year 3.</p> <p>2. The ratios of NO₂ and SO₂ and indeed the location of the SO₂ peak have not been constant over time as now clarified in the document. However the evidence is that NO₂ levels have always been much higher than SO₂. For this reason we felt able to impose a Standard EQC for NO₂, even though these data inherently include effects from SO₂.</p> <p>3. We have developed a Standard EQC for NO₂ so the emission cannot rise to "any level".</p> <p>Part of the precaution is that removing NO₂ from the atmosphere would likely utilise urea/ammonia, thereby increasing emission of that species. So we do not wish to set emissions levels too low without sufficient evidence, lest</p>	<p>✓ This is a valid point and I may have missed it, if it was stated, during my review. Synergistic effects are possible, even probable, but I would expect increase in NO₂ emissions to be of prime concern in the foreseeable future.</p> <p>✓ Excellent</p> <p>An interesting point.</p>	<p>Agreed.</p> <p>✓</p> <p>✓</p> <p>✓</p>

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		revision and before a much larger dataset is available from the chamber experiments.	it prompt action that is not advantageous. As stated above, we expect these issues will be resolved in the following year.	✓ Understood	
35	4. Ongoing ECQ development	Page 16 Para 1 Line 1 Might change "For" to "From"	Done	✓	-
36		Para 2: line 4: Change "is incorporated" to "are incorporated" - data are plural!	Done	✓	-
37		Para 4 line 1: Change "determine" to "determined" Line 2: Put a full stop after "standards." Begin a new sentence "Owing to inevitable synergistic interactions between multiple air pollutant species" Line 3: Omit hyphen in "to-date". Line 4: Sentence beginning "This is further reinforced" appears to repeat the argument for synergism made in the previous sentence. Is the sentence necessary?	Done Done Done Removed	✓ ✓ ✓ ✓	-

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38		Para 6 Line 2: The sentence beginning "Therefore it is important to develop" could usefully be simplified and dissected into smaller more comprehensible sentences.	Edited	Good try. You might consider a further variation: "Therefore, it is important to develop Interim EQC as soon as possible when appropriate levels of statistical confidence indicate a particular degradation mechanism affecting a specific pollutant or mechanistic pathway."	Incorporated in revisions (noting consideration of comments by other reviewers on this sentence).
39		Para 7 line 2: re. the sentence beginning "It may be possible to develop" I believe your point is that the natural source of PM is overwhelmingly dominant within the total measured PM and has significant stabilizing and creational effects on the patina? It could be written more clearly. You should also stress that industry and other human activities produce only a small fraction of the total PM and the significance of this portion will be difficult to isolate from the overall effects of PM exposure.	Edited for clarity.	✓ Edited very well. Clarity is improved and the role of iron ore dust is usefully added.	Thank you
40	5. Monitoring against the	Page 17 Line 6: Text might be improved " or preferable to reduce	Done	✓	-

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	Interim EQC	precursor species thus inhibiting secondary pollutant formation." Para 2 line 1: Change "set higher or lower" to "adjusted". "..... more dose-response data become available"	Done	✓ ✓ This was also done correctly	
41		Para 2 line 2: "due to" is incorrect and should be replaced by "as a result of" or "through" Line 3: The statement in the sentence beginning "Synergistic and" is almost certainly true and introduces terminology that could usefully be employed earlier in this report.	Changed to "as a result of" Noted however it seems most appropriate to mention here as these effects can best be examined once further data is obtained.	✓ ✓ OK. note, however, data are obtained. Don't worry the ABC and the BBC get it wrong, and apparently 'language evolves' so you are ahead of the times!	We agree that 'data are obtained' would have been better than 'data is obtained ' in our response at left. However, the current convention followed by statisticians is that data is singular when it is a source of information and plural when it is a collection of individual observations. Precedents for this usage include "the committee is/are"
42		Para 3 line 2: "monitored" could be changed to "measured" to avoid circling text.	Done	✓	-
43		Para 4 line 1: Perhaps add "atmospheric" i.e "It is recommended that the 4-weekly averaged concentrations of atmospheric NO ₂"	Done Done Done	✓ ✓ ✓	-

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		Line 2: " <u>a</u> quarterly assessment" Line 4: "and the design <u>be</u> such" Line 4: From the last sentence of the paragraph it is not clear whether you refer to new monitoring sites to be established by MRAS within industrial locations or the data from existing industry monitoring sites will be incorporated in the study?	Existing has been added to clarify. Sites EX01, and EX03-09 have existing industry monitoring stations, we are evaluating the utility of incorporating the (Radiello) sampler data in the EQC assessment.	✓	
44		Para 5 This paragraph would better be placed after paragraph 1 on this page.	Done	✓ excellent	-
45		Para 6 line 1: "annual averaged emissions"	Done	✓	-
46		Page 19 Para 3 line 6: Change "for these three" to "for the three"	Done	✓	-
47		Para 4 line 4: Omit "for"	Done	✓	-
48		Para 5 line 2: A short line possibly to account for the exceedances of the guideline EQC for NO ₂ at monitoring sites EX02 and EX09 would be useful - and would save a whole lot of wondering!	The text has been amended to clarify the comments and highlight the individual EQC that will be forthcoming. Caution should also be used in interpreting data from EX02/EX09/AQ01 due to <12	✓ Noted and OK.	- ✓

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		<p>In general: it has correctly been made clear in this report that present thresholds for pollutant risk are merely Interim Guideline values that will be refined as further data become available in the next two years of the research program. However, I cannot read this paragraph without feeling that it goes out of its way to highlight the absence of any possibility of atmospheric pollution risk. This, I believe, is unnecessary, even inappropriate, at the present early stage of the research studies and the numerous assumptions involved in EQC development. I have made my objection to the adoption of the mixed NO₂ + SO₂ Guideline known in a previous review and in the opening General Comments of the current report.</p>	<p>months of data. The revised graphs should aid interpretation of this point.</p>		
49		<p>Page 20 Figure 7 legend: Referring to the grey dots, do you mean "passive powered sites"? I think they are referred to elsewhere as "powered sites".</p>	<p>We refer to EX02/EX09/AQ01 as powered sites, however as per the study design report they also include the same equipment as the solar/passive sites and all MRAMP monitoring data in this report are from the IVL (passive) samplers.</p>	OK Clarified.	✓

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50		<p>Page 21</p> <p>Pt 6 line 28 "It is not suggested <u>that</u> they are relevant" Add "that".</p> <p>Line 29 Change last part of this sentence to read: "..., although they may have implications for microbiome health".</p>	<p>Done</p> <p>Done</p>	<p>✓</p> <p>✓</p>	-
51		<p>Page 22</p> <p>Para 1 I note that despite the variations in rock surface porosity being the sole recognised product of atmospheric: rock interaction by MRAMP and the basis upon which Interim EQC have been developed, significant mention of the lateral spatial distribution of porosity appears here for the first time in this report. I believe some detailed discussion of this subject should be included earlier in the report (see my General Comments).</p>	I believe this has now been addressed by the above revisions.	✓ It has.	✓
52		Para 3 line 3: Suggest replace "(anthropogenic)" to "industrial emissions-induced"	Done	✓	-
53		Para 4 line 3: Regarding the "increase in the Interim Guideline levels" I believe there	This is also a possibility. Agree with the rest of the comment.	OK	✓

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		is an equal argument that Guideline levels will be decreased on account of synergistic effects. The roles of synergistic, and antagonistic, effects will no doubt be a prominent topic in on-going MRAMP research.			
54		Para 5 line 1: "to date" - omit the hyphen. Line 3: "ultimately to be used"	First sentence has been omitted based on other reviewer comments. Done	OK. Probably justified ✓	✓

Document title:	MRAMP Interim Environmental Quality Criteria (June 2024)
Document ID	COPP21065-REP-G-107
Reviewer:	Associate Professor Dr Ron Watkins
Date review commenced:	June 2024

Item no.	Section	Peer reviewer comment Date: 17-07-2024 Revision: Rev B	MRAMP team response Date: 27-08-2024 Revision: Rev C	Peer reviewer close-out comment Date: 09-02-2025 Revision: Rev E	MRAMP close-out comment Date: 12-03-2025 Revision: Rev 0
1	General	<p>This document in general is well presented. Some convoluted sentences and syntax errors, however, may lead to equivocal meaning on a topic where clarity is of the essence.</p> <p>I have a major question regarding the arguments made in Section 5.1 (Page 11):</p> <p>i) The Interim EQC for NO₂ and SO₂ were developed from combined combustion experiments. You state that the two gases "are likely to have a similar mechanism of action".</p> <p>Given that this statement has a critical bearing on the Guideline</p>	<p>We agree with these comments, however all experiments to-date utilised combined exposures from combustion processes. Until we have a sufficient body of NO₂ and SO₂ single gas exposure data, it was considered that this combined treatment was the best approach for contractually required interim EQC development at present. This will likely change in year 3.</p> <p>**Note, the EQC document has been significantly revised in response to other comments.</p>	<p>✓ OK</p> <p>Understood</p>	<p>✓</p> <p>✓</p>

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		<p>value to be monitored, it demands justification. On what basis was this concluded? Have you observed the same degree of effect on the rock surfaces by exposure to the gases individually, and in combination? I can think of various reasons that the two gases might behave differently. Whereas both are "acidic" and react with moisture to form strong mineral acids, the two acids have differing characteristics with regard to their ability to dissolve minerals and the products formed. One example is that nitrates formed by reaction are typically highly soluble in water, while sulphates are mostly less soluble or highly insoluble and may arrest reactions through armouring of the mineral surface. Another difference is the propensity for sulphuric acid to dissolve metal oxides, e.g. magnetite and ilmenite, and may be more corrosive to Mn-Fe patinas.</p>			
1		ii.) Choosing a combined Interim EQC that is deemed to be exceeded only if <u>BOTH</u> NO ₂	As these are guideline levels not standards, there is at present no "unacceptable" level		

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		and SO ₂ guideline levels are individually exceeded appears seriously flawed at this time, in light of my point (i)., above. Furthermore, as you have discussed, the use of HFO, a major source of SO _x , has been phased out in the vicinity of Murujuga, and present industry may be expected to emit very little. In contrast, NO _x may very well be emitted from persistent sources, such as fertilizer and explosives production, vehicle/ship exhausts and bushfires. I may have understood incorrectly, but under your proposed EQC, it seems that ANY degree of NO _x emission would be deemed acceptable, so long as SO _x emission was low?	that would require action. The intent is that guideline levels are reported on but without a commensurate standard level there would not be any required action under the MRAS framework. It is not expected that NO ₂ would massively increase, however you do raise an important point for consideration.	✓ Thank you.	✓
2	Section 1 – Overview	Page 3 Para 2 Line 1: Sentence beginning "The Interim EQC. doesn't make sense. Word or words missing?"	Corrected	✓	-
3	Section-2 Methodology for EQC	Page 4 Para 3 Line 1: Is the term "response-toxicology"	Although we are not explicitly assessing the ecotoxicology of the microbiome, the work will		-

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	developm ent	appropriate, since the Report does not venture into human or environmental health?	eventually do so. We are also using toxicology methods. The text has however been amended.	OK ✓	
4		Para 6 Line 11: "of from" ?	Corrected	✓	-
5		Page 5 Para 6 Line 3: Sentence beginning "Such events are unreliable in nature however on a long- ..." does not read well [some punctuation required]. Also "unreliable" for what? Replace with e.g. unpredictable, intermittent, sporadic?	Amended	✓	-
6		Page 6 Para 4 Line 7: If you begin a sentence with a conjunction "Hence", you require a verb: "Hence the decision was made to undertake"	Amended	✓	-
7		Para 5 Line 1: "Nor" should not be used to begin a paragraph [tack this sentence onto the end of the previous paragraph where the logic is retained.	Amended	✓	-

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8		Para 6 Line 4: " ... SO ₂ due to historically ..." [insert "to"	Inserted	✓	-
9		Line 8: Change to "to have peaked at more than twice the current levels".	Amended	✓	-
10	Section-3 Interim EQC	Page 8 Figure 2, and also Figures 3 and 4: What are the units of Cumulative response? Show on Y-axis and/or explain in caption.	Added. Cumulative response is normalised so unitless.	✓	-
11	Section-4 Ongoing EQC developm ent	Page 11 Para 1 Line 5: It would be prudent to qualify the role of geogenic dust/clay in ameliorating pH - "... on the rock surface and may also play"	Added	✓	-
12		Para 3 Line 3: You are correct in saying the patina weathering is a critical issue. It has a different geochemical and mineralogical character to that of the weathered rind and is a key to the visual preservation of the petroglyphs. You state that it will be considered in the development of future EQC. It would be useful to state briefly how it will be considered? e.g.	Yes correct. Added.	✓ Thank you	✓

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		through focussed chamber experiments on patinated surfaces?			
13		Line 4: "However" is linking two unrelated statements and should be omitted.	Omitted	✓	-
14	Section-5 Monitoring against the Interim EQC	Para 6: see my General comment above. Page 12 Figure 5: re. "IVL" in key on diagram should be clarified by change to "current instrument (IVL) measurement. If not, it should be spelled out in the caption. The caption is poorly written. It is the averaged measurements that are plotted. The EQC is drawn.	Amended	✓	-
15		Comment: would it not be instructive to show also the maximum recorded values, since these would constitute a dose, such as in the chamber experiments. It is not uncommon in environmental geochemical studies that it is the maximum occurrence of a contaminant that constrains the extent of the environmental impact. Do not the results of the short duration chamber	This is a reasonable suggestion. However the maximum dose in the chamber studies is usually much higher than maximum dose observed in the field, particularly using the passive samplers which produce monthly averages. It may be more prudent to compare peaks observed from the realtime air quality	✓ understood	✓

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		experiments, suggest that a short dose of NO _x or SO ₂ can effect a change in rock porosity?	instruments with chamber studies peaks.		
16		Page 14 Figure 6: The dashed Interim EQC line should appear in the key. The visual presentation would be improved if the colour of the DPS points was not the same as the EQC line.	The dashed Interim EQC line has been included in the legend	✓	-
17		Para 1 Line 5: The argument presented in the sentence beginning "When considering this ...: is not clearly evident. The sentence is long and convoluted. I believe "however" is used inappropriately connecting two unrelated statements. I suggest lacing a full stop after "peak" and starting a new sentence with "Furthermore".	Amended as suggested	✓	-
18		Page 15 Para 1 Line 1: Sentence beginning "This "over-accounting .." is rather awkward with unnecessary repetition. Removal of "due to the	Amended as suggested	✓	-

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		progressive introduction of emission reduction measures" results in no loss of information.			
19		Para 3 Line 5: Omit "as well" and insert comma after "since,"	Done	✓	-

4 Monitoring Studies Data Collection and Analysis Plan Amendment 2024

4.1 Professor Geoffrey Michael Gadd (2024)

DEPARTMENT OF WATER AND ENVIRONMENTAL REGULATION

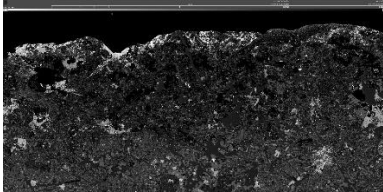
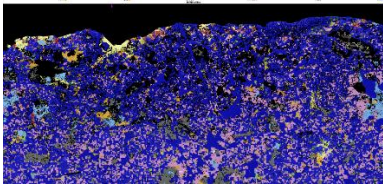
Independent Peer Review Comments Sheet

Reviewer: Professor Geoffrey Michael Gadd	Review Comments Sheet ID:
Document title: Murujuga Rock Art Monitoring Program: Monitoring Studies Data Collection and Analysis Plan Amendments 2024 [COPP21065-PLN-G-105_C]	
Document revision: Revision C (February 2024)	
Date of review: 26 February 2024	

Item no.	Section no.	Peer reviewer comment	MRAMP team response	Peer reviewer close-out comment	MRAMP team response to close-out comment
1	1	Introduction Clearly summarises background and rationale for the programme amendments.	Comment.		

Item no.	Section no.	Peer reviewer comment	MRAMP team response	Peer reviewer close-out comment	MRAMP team response to close-out comment
2	2.1.1	Plutonium isotope analysis I am not surprised that this has been discontinued. I have never come across this techniques in my experience albeit primarily bioweathering. As I queried in an earlier review “I do not understand the relevance of plutonium analysis or how this could provide any information on changes in soil chemistry and biology”. The research team has provided convincing justification for discontinuation.	Comment.		
3	2.2.1	Open Path-Fourier Transform Infra-Red (OP-FTIR) scanning campaign The proposal that the OP-FTIR measurements be reduced to a 1-3 week pilot-scale program using more portable equipment is valid and would reveal whether measurements above detection limits are possible with such deployment.	Comment.		
4	2.3.1	Passive sampling of atmospheric volatile organic carbon concentrations Since data from the BTEX suite of chemicals were below detection limits it seems that further application of the passive (BTEX) VoC samplers is pointless. It is unclear why the	The majority of measurements are below detection limits but not all. E.g. Toluene levels are above detection limits for most sites/measurements. The contract requires the study design (and therefore any	OK	

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		<p>opinion of the chief statistician is required in the absence of meaningful measurements.</p>	<p>changes) be approved by the Chief Statistician. The separate review by the DWER Air Quality Branch has recommended a minimum of 12 months of measurement before any decision to discontinue. We agree with this approach.</p>		
5	3.1	<p>Inorganic geochemistry rock surface porosity permeability investigations The correlation between the spatial pattern of NO₂ concentration and porosity is convincing. A query –how is it concluded that the voids were likely to have been previously occupied by feldspar minerals? Would mineral maps show nothing in the voids?</p>	<p>The voids show the same morphology and size as minerals within the fresher rock and the lower parts of the weathered rind, and locally, the margins of the voids are continuous with the margins of mineral grains. The mineral maps show that the voids are unoccupied by minerals. The images below show the voids (black) in the weathered rind at the top of the image and the fresher rock below. Figure 1 and 2 show the granophyric intergrowth between quartz and feldspar is visible in the fresher rock portion. This pattern becomes skeletal in the weathered rind and it is visible only by the remaining quartz.</p>	<p>I see, thank you for the explanation.</p>	

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			 <p data-bbox="824 475 1205 539"><i>Figure 1 – SEM Backscatter of granophyre thin section</i></p>  <p data-bbox="824 758 1205 815"><i>Figure 2 – TIMA Phase map of same region</i></p>		
6	3.1.1	<p data-bbox="414 837 797 933">Sample rock collection and laboratory porosity measurements.</p> <p data-bbox="414 941 797 1007">The proposed further sampling is convincingly justified.</p>	<p data-bbox="824 837 1055 869">Comment. Agreed.</p>		
7	3.1.2	<p data-bbox="414 1032 797 1096">Historical emissions inventory calculation.</p> <p data-bbox="414 1104 797 1230">This looks like a very useful addition and may provide valuable background information to inform the project.</p>	<p data-bbox="824 1032 1055 1064">Comment. Agreed.</p>		
8	3.1.3	<p data-bbox="414 1259 797 1323">Field permeability measurements.</p> <p data-bbox="414 1331 797 1390">Purchase of the Field Permeameter would be a useful</p>	<p data-bbox="824 1259 1055 1291">Comment. Agreed.</p>		

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		addition for this aspect of the programme.			
9	3.2	<p>Microbiome chamber studies workflow development What is meant by "...the Murujuga species are relatively novel to science..."? Organisms are either novel or not. Some organisms may clearly be novel, but most will be known genera and species.</p> <p>For what purpose is the use of green fluorescent protein (GFP) for the bacteria and fungi? It is unclear what the "pre and post exposure measures" (measurements?) are and what is being assessed, meaning it is difficult to judge the value of this approach within the programme. What is hoped to be determined? Is it just to visualize the organisms or to determine if certain organisms respond to certain stimuli? Where in the genome is the GFP protein going to be expressed? Without sufficient detail or justification I cannot reach a conclusion on this proposed addition.</p> <p>If the use of GFP and therefore the creation of a genetically-modified organism does require specialist containment</p>	<p>Apologies for the clumsy wording. The meaning of the text is that pure cultures of the dominant microbes found at Murujuga (which we will be utilising in the chamber experiments) are not on the "exempt dealing" list of the Department of Agriculture and Forestry and Fisheries (Australian Federal Government). Thus, we require import permits and must perform experiments with these microbes in a PC2 laboratory or possibly an Approved Arrangement laboratory, thus justifying the duplication of the workflows. These laboratories are routinely used by other researchers working with genetically modified and quarantine restricted organisms. We could culture and isolate samples from Murujuga, however we believe it would be more time consuming to do this than import pure cultures from facilities that have them available already.</p> <p>GFP: We undertook initial confocal microscopy tests on the rock samples from Murujuga</p>	<p>1. Apologies but I do not follow some of the reasoning. If you are not culturing and isolating samples from Murujuga, how do you know what pure cultures to import from elsewhere? Or is this information already known from previous studies?</p> <p>2. GFP. Thank you for the explanation. This aspect of work seems demanding with considerable methods development, so I accept that addition of specialized expertise is necessary.</p>	<p>1. We have 16S sequencing results from the two field sampling campaigns at Murujuga. The species proposed for the chamber studies are existing isolates of the most dominant genera/species detected in the 16S sampling. If culturing and isolating equivalent samples from Murujuga is deemed more efficient than importation, then the workflow will be revised.</p>

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		<p>according to legislation then this must be applied or this direction of research changed.</p> <p>I do not understand why short-term engagement of another senior researcher is required. Relevant expertise should already exist within a molecular microbiology research group. The expertise that the requested senior researcher should have is insufficiently specified – is it just to instruct how to apply the GFP system?</p>	<p>and found significant autofluorescence and binding of stains to the rock itself. Through GFP labelling of the pure cultures we will eliminate this issue, and we will be able to determine at what pollutant concentrations do we see if the microbes are actively growing or if cell death is occurring. I.e. we will be able to conduct highly controlled dose-response exposure studies with pre and post measurement of specific microbial cells/cultures and their response to the exposure.</p> <p>The outcomes of the chamber study we are hoping to achieve is as follows; determine what concentrations of pollutants are affecting the cells growth rates through CLSM, what changes in gene transcription is happening pre and post exposure through metatranscriptome sequencing. This data will help us understand what cellular responses these representative microbes have to the common industry pollutants.</p> <p>These studies will obviously represent a simplification of the full suite of inter-related community effects and Murujuga, however we believe this work is essential to assist</p>		

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			<p>with the development of exposure criteria/limits for the microbiome.</p> <p>We have not yet determined where in the genome the GFP protein coding gene will be inserted. This is where the addition of the senior researcher is essential. The current microbiologists are expert in environmental geomicrobial studies, however do not have sufficient experience in GFP labelling or some of the other workflows for controlled dose/response toxicological studies. Therefore we plan to include a senior researcher with extensive experience in culturing, GFP labelling, pre and post exposure workflows and toxicological studies to assist with design and method development (the labour for this component will not be extensive), as well as a more junior microbiologist from his team with similar experience to assist with the day to day laboratory tasks, as the current team is nearly at capacity.</p>		
10	3.3	Organic geochemistry investigation of hydrocarbon residues	The exact nature of the compounds are yet to be determined. So far it has been determined that they are	OK.	The technique is not GC-MS- it is GCXGCTOFMS

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		<p>Analysis of previous extractions have revealed the presence of “partially biodegraded hydrocarbons mixtures”. This finding therefore already reveals anthropogenic impact on the site and also, since biodegraded, the relevance of their presence to microbial activity. Therefore, the additional work requested is to identify the specific hydrocarbon compounds present which will add interesting knowledge but not affect the overall conclusion that there is an anthropogenic impact on the site. Will there be attempts at quantification since the term “measurable” is used? Can GC-MS data for chemical identification be correlated with amounts?</p> <p>I cannot comment on the labour time requested of 12 months for this. As I understand, the most difficult procedures are producing samples that provide strong spectra, and the subsequent assignation of the signals to specific chemicals which might be revealed by use of known standards but can be difficult and time-consuming for obscure intermediates.</p>	<p>complex organic mixtures which may be of petrochemical origin, however other options remain possible.</p> <p>The initial focus of the work is identification, however quantitative determination will be conducted where possible.</p>	<p>So how is this possible and if so, how would it be done? Can GC-MS data be correlated with amounts?</p>	<p>The 64 collected rock samples underwent a process of patina collection and solvent extraction for organic compositional analysis by one dimensional gas chromatography mass spectrometry (GC-MS). Organic geochemical analysis to-date has revealed highly complex organic mixtures extracted from some of the rock patina samples. These extractions appear to be partially biodegraded hydrocarbon mixtures which are likely to be from a range of mixed environmental sources.</p> <p>It is proposed to undertake two-dimensional gas chromatography – gas chromatography time of flight mass spectrometry (GCXGC TOFMS) analysis of the extracted samples in an attempt to deconvolute and identify the composition of these highly complex samples. This additional analysis does not require any additional sample collection, however due to the complexity of identifying complex hydrocarbons and the specific expertise required for such analyses it is expected to take up to 12 months labour.</p>

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					<p>If confirmed, the presence of such environmentally significant compounds incorporated in the patina of rock samples may / may not be related to the microbes identified on the patinas.</p> <p>Quantitative determination will be undertaken if possible, however it is challenging for such complex mixtures.</p> <p>Identification is the primary goal. Relative (semi-quantitative) proportional determination may be possible.</p>
11	3.4.1	<p>Use of a second MAX-DOAS Skyscan instrument</p> <p>This would enhance analysis and data acquisition and is sufficiently justified.</p>	Comment. Agreed.		
12	3.4.2	<p>Inclusion of NH₃ passive samplers at industry AQM EX-04</p> <p>The addition of duplicate NH₃ passive samplers would further enhance analysis and data acquisition.</p>	Comment. Agreed.		
13	3.4.3	<p>High-volume dust collection and laboratory analysis</p> <p>Installation of dust collectors would be an important addition to the programme. Airborne dust is a major source of</p>	Comment. Agreed.		

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		distributed mineral and organic particulates, including pollutants, as well as microbial propagules, and known to be highly significant for colonization and bioweathering of rock and mineral substrates.			
14	3.4.4	Additional calibration unit for real time gas analysers Provision of a second back-up calibration unit is a sensible precaution.	Comment. Agreed.		
15	3.4.5	Additional LiDAR units for particulate remote sensing Deployment of the additional LiDAR systems would enhance existing capabilities and enable more detailed characterisation of particulate matter and their sources.	Comment. Agreed.		
16	4	Summary This clearly tabulates the proposed modifications and additions to the research programme.	Comment.		

4.2 Dr John Henstridge, Data Analysis Australia (2024)

DEPARTMENT OF WATER AND ENVIRONMENTAL REGULATION Independent Peer Review Comments Sheet

Reviewer: Dr John Henstridge (Data Analysis Australia)	Review Comments Sheet ID:
Document title: Murujuga Rock Art Monitoring Program: Monitoring Studies Data Collection and Analysis Plan Amendments 2024 [COPP21065-PLN-G-105_C]	
Document revision: Revision C (February 2024)	
Date of review: 01 March 2024	

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1	General	<p>These comments relate to the proposed changes outlined in the document dated 16 February 2024 (the “Plan Amendments 2024”).</p> <p>(a) I review this as an experienced statistician with familiarity with environmental monitoring practices. I acknowledge</p>	Noted.	Nothing required	

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		<p>that there are many non-statistical aspects of the proposed changes that I cannot comment on.</p>			
2	General	<p>The Plan Amendments 2024 are stated to consider of scope reductions where activities were determined to be infeasible or of limited value, and scope extensions responding to new findings.</p> <p>(a) As a general comment, it is good to see that the Plan is retaining flexibility to respond to experience during the project.</p> <p>(b) That said, care needs to be taken that scope is not reduced prematurely based upon what might be seemingly negative albeit statistically insignificant findings. Hence it is important that the Chief Statistician be involved in these decisions to avoid selection biases in the data collection.</p>	<p>(a) Agreed.</p> <p>(b) We agree that this is an important issue. "Changing horses in midstream" carries the risks of selection bias and loss of statistical power. The statistical theories of sequential inference, and of variable selection in multivariate regression, give some guidance on how to make such decisions. The Chief Statistician will continue to be involved in such decisions.</p>	<p>An additional risk in changing direction is the delay in starting a consistent time series.</p>	<p>Agree</p>
3	2.1	<p>Activities to be discontinued from the MSDCA Plan</p> <p>I can appreciate the problems of sampling for Pu. The precision of such measures are typically limited by the count nature of</p>	<p>We agree with this assessment. Our decision is also based on the experience of the geochemistry studies, which encountered difficulty in obtaining sufficient mass of</p>	<p>Nothing required</p>	

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		<p>the measurement process and hence there is often a limit on how small physical samples can be.</p> <p>(a) I note that porosity measurement is recommended as a replacement. I comment on that below.</p>	<p>sample material from some rock surfaces.</p>		
4	2.2.1	<p>Open Path-Fourier Transform Infra-Red (OP-FTIR) scanning campaign.</p> <p>Replacement of the OP-FTIR instrument seems sensible and the carrying out of a pilot at a location where levels are likely to be in the detectable range is appropriate.</p> <p>(a) As it seems that detection limits will be critical in this assessment, the pilot should attempt to quantify just what can be done with this instrument.</p> <p>(b) The issue of longer term sighting will need to be considered after the pilot. At this stage it will be necessary to consider whether the practical detection limits mean the instrument will be able to contribute meaningful</p>	<p>Agreed.</p>	<p>Nothing required</p>	

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		measurements where they are needed.			
5	2.3	<p>Activities to possibly be discontinued if determined by the Chief Statistician to have insufficient value.</p> <p>The studies I have seen on the passive sampling tubes suggest that they are both highly variable in their measurements and have detection limit problems. It is highly appropriate for the Chief Statistician to make decisions on whether to continue with this approach, taking into account how the gap in the data will be handled.</p>	<p>The initial experiment comparing the different brands of passive sampling tubes, and the subsequent replicated study of the IVL tubes, were both undertaken in the field, so that the variance estimates reflected environmental signal variability as well as laboratory/technical error variability.</p> <p>Now that IVL passive sampler tubes have been deployed in the field in duplicate for each gas, it will be possible to estimate the laboratory/technical variability (conflated with nugget effect if any) using the paired differences. This will enable us to estimate the signal-to-noise ratio, which would enable a decision to be made on whether to continue deploying the passive sampler tubes.</p> <p>DWER have suggested that a decision be made after at least 12 months of data have been collected and analysed and we agree with this. The Chief Statistician will be involved in any such decision.</p>	Agree	

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6	3.1	<p>Inorganic geochemistry rock surface porosity permeability investigations</p> <p>The rock porosity measurements based upon thin sections have been analysed for their spatial distribution.</p> <p>(a) I note that the similarity of the spatial distributions in Figures 3-2 and 3-3 suggests a relationship with anthropogenic emissions being the causal link.</p> <p>(b) This would be more convincing if measures additional to NO₂ were given. I suspect that measurements of other emissions may have a similar spatial distribution, but that needs to be confirmed.</p> <p>(c) However, I agree that it is appropriate to infill some of the spatial data by extending porosity measures to some of the islands not currently covered.</p> <p>(d) I note the proposal to trial a new instrument, the TinyPerm 3. While this seems to be a possibly productive approach, it will be necessary to calibrate it</p>	<p>(a) Agreed. A correlation is clear.</p> <p>(b) This has been investigated using the data to hand. The spatial patterns of the air pollutants vary from each other considerably and also vary seasonally. Measurements to-date, as well as Ramboll modelling suggest that the spatial pattern of NO₂ would be likely to agree most closely with the spatial variation in porosity found. However, this needs to be investigated further, and causal mechanisms studied in detail. Such analyses will be undertaken.</p> <p>(c) Agreed.</p> <p>(d) Agreed. Measurement of permeability is a new approach, motivated by the preliminary findings about porosity, but of course permeability and porosity are different. Our view is that field measurement of permeability, being non-destructive and much easier to perform than the microscopic analysis of porosity, may serve as a useful surrogate or</p>	<p>It concerns me that other pollutants do not follow that of NO₂ and NO₂ is then used as a justification for the approach. This suggests that care should be taken to not overly target specific locations for additional sampling as the foundation for this targeting may turn out to be weak. Instead, a more general infill approach (including the proposed islands) may be more appropriate.</p>	<p>Locations for additional sampling of rock have been selected by an infill approach, guided by the geology. The primary aim is to reduce the prediction error of spatial smoothing, and this has been done by placing new sample locations in areas where the distance to the nearest existing sample location is large.</p> <p>The study team considers NO₂ to be particularly important because it is the most prominent component of industrial emissions at Murujuga by mass (amongst the gases considered) and because there are several plausible hypotheses that involve NO₂ reactions. The spatio-temporal pattern of NO₂ distribution was not a factor in the choice of additional sample locations. However, before committing to the chosen additional sample locations, the statistical team did use information about the NO₂ distribution to verify that the power of the study design will not be compromised.</p>

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		<p>against existing measures. The Chief Statistician should be involved in the plan for doing this.</p> <p>(e) The introduction of a new instrument at this stage may have an impact upon when baseline data begins to be established.</p>	<p>concomitant variable. This will need careful attention by the Statistical Team.</p> <p>(e) Agreed.</p>		
7	3.3	<p>Organic geochemistry investigation of hydrocarbon residues</p> <p>The discovery of significant complex organic mixture presumed to be from hydrocarbons is important both for the fact that it suggests a form of environmental pollution not previously considered and that it may affect other measurement processes.</p> <p>(a) My understanding is that the chemical analysis of such compounds by gas chromatography is a well-established technique and it should be possible to proceed with this as is proposed.</p> <p>(b) I cannot comment upon the time frame proposed but note that this is an important task that should be prioritised so that further</p>	Comment. Agreed.	Nothing required	

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		measurement plans can be drawn up if required.			
8	3.4	<p>Air quality monitoring This section discusses changes driven by the discovery of likely associations between PM10 measures and the pH on rock surfaces. This means that measurement of airborne particulate matter and NH₃ is now seen to have added importance.</p> <p>(a) Extending the network of AQM sites measuring NH₃ seems highly appropriate. I note that the choice of sites should reflect the understanding of sources and wind conditions, with a need to have controls.</p> <p>(b) The use of more samplers for particulate matter is also critical. The siting of these will be important and should reflect best understanding of the sources of particulate matter and wind conditions.</p> <p>(c) That said, I caution about making too many assumptions about the sources and travel of particulate matter and strongly suspect that just two more sensors as</p>	<p>(a) Noted/Agreed.</p> <p>(b) Each AQM has passive (dust) deposition samplers which are analysed every month for a wide range of parameters. As well as solar and reference-grade real-time dust monitors. However, the current passive samples are fully utilised in the existing workflows. This addition proposes the collection of higher mass samples using powered high-volume samplers. These samples can then undergo more detailed characterisation with improved detection limits for a range of organic and inorganic components. These samplers will also provide dust samples for the chamber studies.</p> <p>(c) Noted. Given the samplers are mains powered, deployment options are more limited, however DWER reviewers have also suggested a wider range of sites be considered. The deployment of these samplers will be designed</p>	<p>(a) Noted</p> <p>(b) I can understand that having higher volume dust collectors constrains their location to where they can be mains powered.</p> <p>(c) Noted</p> <p>(d) Noted</p> <p>(e) It is concerning that PM2.5 models are poor. This is a classic example of where local calibration of models is important.</p>	Agree.

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		<p>suggested might not be enough. There is a need to a more detailed plan to be developed and reviewed, at the very least, by the Chief Statistician.</p> <p>(d) The use of LIDAR to measure airborne particulate matter seems very attractive, although I note that the technology appears to have some challenges.</p> <p>(e) In particular several of the instruments proposed have limited range compared with the area of interest suggesting that either multiple instruments will be required or a plan for moving the instruments around will be needed. As some of the emissions are likely to be periodic and may be missed if the instrument is in the wrong place at the time of emission, there is a strong argument for multiple instruments. However, I am aware that they are likely to be expensive.</p> <p>(f) Cross-calibration of the LIDAR measures with other measures may be required.</p>	<p>and reviewed with the Chief Statistician.</p> <p>(d) The LIDAR instruments will aid with understanding horizontal and vertical profiles of particulates. Calibration against the reference dust monitors already deployed will be undertaken. Instrument availability and/or cost may prevent more extensive deployment or multiple sites, however the optimum location for deployment and duration will be designed in conjunction with the DWER Air Quality Branch and the Chief Statistician.</p> <p>(e) This issue with the Ramboll model was understood during the study design and therefore was accounted for in design. Based on significant characterisation work conducted on Pilbara dusts and ores (undertaken previously by members of the research group, independent of and prior to MRAMP), it is believed the main issue is that Pilbara geogenic dust includes a much greater PM_{2.5} fraction than other geogenic dusts globally. Therefore, the</p>		

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		(g) I note with some concern the statement in 3.4.5 that “Ramboll modelling massively underpredicted PM2.5 levels in the airshed”. This is of concern since the Ramboll modelling was a critical contributor to the design of the sampling program. The reasons for this underprediction need to be understood.	additional PM _{2.5} is expected to be associated (and spatially correlated) with PM ₁₀ .		
9	General	Overall, I agree with the amendments proposed but strongly suggest that they all require further development into an informed study plan or design.	Agreed.	I look forward to the study plan.	The amended or additional study designs will be performed and submitted at the same time as the Year two annual report, and peer reviewed before the work commences.

5 Methodology statements

This section provides the independent peer review reports undertaken between July 2024 to March 2025 for the following component study methodologies (Methodology Statements):

- Passive Air Quality Monitoring (AQM)
- Passive AQM Sample Collection, Maintenance and Context Observations
- Powered AQM Installation and Commissioning
- Powered AQM Operations, Procedures and Observations
- Organic Geochemistry of Rock Surfaces, Dust and Particulates
- Inorganic Geochemistry Laboratory Analysis
- Chamber Exposure Studies.

Several of these methodology statements were updates to previously reviewed documents and reviewers did not have any additional comments. The final methodologies will be incorporated into the standard operating procedures for the ongoing monitoring program, where relevant.

5.1 Professor Geoffrey Michael Gadd (2024)

Document titles:	Murujuga Rock Art Monitoring Program: Methodology Statements 1. COPP21065-PRO-G-117 Chamber Exposure Studies
Reviewer:	Professor Geoffrey Gadd
Date review commenced	June 2024

Item no.	Section	Reviewer comment 24-06 2024	MRAMP team response 27-08-2024	Peer reviewer close-out comment 11-11-2024	MRAMP team close-out comment 11-03-2025
COPP21065-PRO-G-117 Methodology statement: Chamber Exposure Studies					
	Version:	(COPP21065-PRO-G-117 Rev 3)	(COPP21065-PRO-G-117 Rev 4)	(COPP21065-PRO-G-117 Rev 4)	(COPP21065-PRO-G-117 Rev 4)
1	Section 5.2	<p>Although a significant aspect of the research programme, this area of (Micro) biological processes remains “under development”. Is there any information as to when this work will proceed?</p> <p>It is stated that two bacterial species and one fungus will be used, but no details are provided. What species of microorganism are envisaged?</p>	<p>The microbiological aspects of the chamber studies are currently being set up.</p> <p>We currently have <i>Chroococciopsis cubana</i> and <i>Rubrobacter spartanus</i> (from the DSMZ culture collection). We are working on sourcing fungal species; however we are finding it hard to source a <i>Spissomyces</i></p>	<p>OK but is it possible to give an actual date when these experiments will commence?</p> <p>OK but it is not surprising there is difficulty with the <i>Spissomyces</i>. The Dutch culture collection is probably the most likely source to possess it. There is negligible information/research on this</p>	<p>Mid-March 2025 the chamber experiments are going to commence.</p> <p>Yes agreed it is unsurprising <i>Spissomyces</i> is hard to find in the culture collection. It was the most abundant (apart from ASVs that were not able to be taxonomically classified). We have made</p>

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		<p>The microorganisms will be obtained from “suppliers”. What is the reason for this, instead of using isolates that were already obtained from the sample sites?</p> <p>How will the relevance of work with these pure cultures be</p>	<p>genus. The rationale for the choice of these particular bacteria is that we are trying to use representatives for the most abundant microbes we see in the amplicon data. We may also be using a <i>Geodermatophilus</i> sp. However we need to use an approved arrangement (AA) laboratory, which involves extra logistical issues.</p> <p>We have so many isolates from Murujuga that it would be a prohibitively large amount of work to keep them growing and to identify them.</p> <p>As shown by the abundance data, the microbial communities</p>	<p>relatively recently named genus. It is also an organism, like other related rock-inhabiting fungi, that is extremely slow growing and difficult in culture meaning that experimentation is difficult. Was this really the most abundant fungus detected or could there be other more manipulatable species that could be used?</p> <p>I do not understand why an approved laboratory is not readily available in the microbiology facilities since current regulations seem to require this for many microbes, e.g. many fungal spore formers.</p> <p>It would be usual to select and identify the most abundant/dominant species in studies like this which might be very few in number. Identification of small numbers of isolates is entirely feasible, especially with modern advances in molecular methods. Such organisms may be more relevant than strains obtained from culture collections.</p> <p>It is generally accepted that more extreme environments</p>	<p>efforts to source alternative fungi from the rocks in Murujuga, we currently have three isolated and are working on characterising them before using in the chamber study. We are doing the chamber study in a PC2 laboratory, which is typically fine for the majority of bacteria and fungi that are not pathogenic, however the import of <i>Geodermatophilus</i> species requires a 5.2 approved arrangement to work on which are not readily available.</p> <p>We have used this methodology on fresh culture samples selecting for fungi from rocks in Murujuga. Since we already have the bacterial strains ordered and ready to go we will utilise these in the chamber study.</p> <p>Agreed.</p>

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		<p>correlated with any observations or processes occurring on the natural site? Or is this not possible? If not, what is the eventual aim of such experimentation? Is it solely to possibly demonstrate what kind of processes may be occurring?</p> <p>I realise that confocal microscopy may be the means to visualise the GFP-labelled organisms, but seems not to be a useful technique for visualizing pre- and post-exposure changes in the organisms. What possible changes in the organisms are</p>	<p>on these rock surfaces are diverse and complex (as they usually are in many environments). To try to capture this complexity in this experiment is not viable.</p> <p>In a controlled experiment it is more appropriate to isolate a few of the most important taxa, and to investigate how they respond to typical pollutants. We concede that it is hard to extrapolate from such experiments to the natural environment, but it will be sufficient to see if these pollutants have an effect on the species selected and how it makes them interact with the rock substrate.</p> <p>We hope to compare control and exposed cultures on the rock with confocal microscopy to assess their viability and growth.</p>	<p>exhibit a lower diversity of species. However, this can still be complex and difficult to assess and study.</p> <p>Yes it is appropriate to isolate a few important taxa as I have often commented on. However this approach appears to be contradicted by the previous statements and others alluding to the difficulties of isolation and identification, obtaining organisms from culture collections etc. So from the team responses there appears to be an inherent contradiction in the project over this aspect. Can this be reconciled – will any natural isolates be used or not or will any experiments just use culture collection strains?</p> <p>I still do not see how this will be carried out. How is confocal microscopy carried out with the rock surfaces? I am not aware of any other studies using this for growth/viability of organisms on rock.</p>	<p>Due to the issues with sourcing Fungi we have new isolates that have been sourced from Murujuga rock samples. As we were able to order in abundant bacterial taxa that are in the sequencing data we will continue the chamber experiments with these genera.</p> <p>To clarify – we will not be using GFP labelled bacteria/fungi. We will be using live dead staining and confocal microscopy to understand how the</p>

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		<p>expected to be seen using this approach?</p> <p>I still do not fully understand the rationale of the GFP approach for investigating geomicrobial activities that may be occurring at Murujuga, especially in view of the pure culture work mentioned above.</p> <p>I am assuming the exposure will be to clean air (controls) and air containing the various substances of concern?</p> <p>It is stated that “the required sample size and number of experiments is still to be determined by statisticians as data becomes available”. As no data is forthcoming how is this possible? Is there any information about when data might become available?</p> <p>I do not understand the need for separate statisticians: those involved in the actual experimentation should be competent in statistical design and replication in their experiments or is this competency lacking?</p>	<p>Yes, clean air controls are included in the study design.</p> <p>Sample size/statistical power calculations will be undertaken once preliminary “range finding” studies have been completed.</p> <p>The project contract specifies that the statistical design, data analysis and statistical inference shall be performed to the highest international standard throughout all parts of the study. This high standard can only be assured by including qualified and recognised experts in statistical methodology on the project team. While the researchers involved in each component study can be</p>	<p>Rationale has not been explained.</p> <p>OK</p> <p>OK but can a time when such “range finding” studies have been completed be specified?</p> <p>OK. I see that the inclusion of statistical experts in the team would be essential if several new statistical techniques have been developed, presumably with the input of the statistical expert(s).</p> <p>I should mention that in our own previous work we often investigated microbiome connections with mineralogy (as</p>	<p>pollutants effect their viability in comparison to a control.</p> <p>Rationale – we will be able to assess what concentrations of pollutants are having measurable impact on the viability of the bacterial and fungal cells that have been inoculated on the rock surface.</p> <p>✓</p> <p>Initial range finding exposures will be commenced in early March 2025.</p>

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			<p>expected to have basic competence in the statistical design and analysis procedures that are widely established in their particular fields, this project involves investigating connections between observations made in different scientific fields (such as the connection between microbiome and mineralogy) which requires a wide range of advanced techniques. Indeed the project has developed several completely new statistical techniques to meet this challenge. Documentation and validation of new statistical techniques to the required standard is generally beyond the competence of all but a qualified expert in statistical science.</p>	<p>others have) in our multidisciplinary team where established statistical techniques were used, with the statistical knowledge and experience already embedded in the team.</p>	<p>The contract specifies statistical experts to check over and integrate the results from all the fields of study in this project.</p> <p>Yes, that is to be expected, and is the case here. The additional work by the stats team is a contractual requirement.</p>

5.2 Professor Janet Hergt (2024)

Document titles:	Murujuga Rock Art Monitoring Program: Methodology Statements 1. COPP21065-PRO-G-114 Inorganic Geochemistry Laboratory Analysis
Reviewer:	Professor Janet Hergt
Date review commenced	June 2024

Item no.	Section	Reviewer comment 25-06-2024	MRAMP team response 27-08-2024	Peer reviewer close-out comment 11-11-2024	MRAMP team close-out comment 11-03-2025
COPP21065-PRO-G-114 Methodology statement: Inorganic Geochemistry Laboratory Analysis					
	Version	(COPP21065-PRO-G-114 Rev 3)	(COPP21065-PRO-G-114 Rev 4)	(COPP21065-PRO-G-114 Rev 4)	(COPP21065-PRO-G-114 Rev 4)
1		p. i. It looks odd in the Table of Contents to have two subheadings called “Thin sections” under the same subsection. In fact, the subheading “2.3.3 Thin Sections” should be removed as it is not required (see below).	Table of contents has been fixed, see below	OK	-
2	2.2	p.1 It’s unclear why there’s a subheading 2.2.1 as everything in section 2.2 relates to thin sections.	“Micro and nano-characterisation” is now new section 2.4. Under this is “Section 2.4.1 Thin sections”.	OK	-

Item no.	Section	Reviewer comment 25-06-2024	MRAMP team response 27-08-2024	Peer reviewer close-out comment 11-11-2024	MRAMP team close-out comment 11-03-2025
3		p. 2 Correct the clumsy wording in the first paragraph.	"allow to obtain" changed to "allow the distribution to be determined" "In order to obtain" changed to "To obtain" "previous to the analyses" changed to "prior to the analyses"	Good	-
4	2.3.2	p. 2 Remove "only" as indicated	"only" removed	OK	-
5		p. 3 There appears to be a stranded piece of text at the end of this section. "Micro and nano-characterisation?" that should be removed. The subheading "2.3.3 Thin Sections" should also be removed here.	See above comment.	Good	-

5.3 Associate Professor Ron Watkins (2024)

Document titles:	Murujuga Rock Art Monitoring Program: Methodology Statements 1. COPP21065-PRO-G-114 Inorganic Geochemistry Laboratory Analysis 2. COPP21065-PRO-G-117 Chamber Exposure Studies
Reviewer:	Associate Professor Dr Ron Watkins
Date of review	17 July 2024

Item no.	Section	Reviewer comment 17 July 2024	MRAMP team response 27 August 2024	Peer reviewer close-out comment 11 November 2024	MRAMP team close-out response 18 December 2024
COPP21065-PRO-G-114 Methodology statement: Inorganic Geochemistry Laboratory Analysis					
	Version	(COPP21065-PRO-G-114 Rev 3)	(COPP21065-PRO-G-114 Rev 4)	(COPP21065-PRO-G-114 Rev 4)	(COPP21065-PRO-G-114 Rev 4)
1	General	Sections 1 - 4 of this statement satisfactorily outline the instrumentation and discuss why each method/instrument is used. Section 5 is essentially a methods/laboratory manual for each of the procedures. Note: I have reviewed in detail only those parts of Section 5 that relate to XRD analysis that have been added since a previous review.	Noted Noted	-	-

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2	p. 1	Para 3 Line 1: "Thin sections" [make plural].	Done	✓	✓
3	p. 2	Para 1: Line 3: "These analyses will allow to obtain the ..." change to "These analyses will allow us to obtain", OR alternatively, "These analyses will reveal the".	Changed to "These analyses will allow the distribution"	✓	✓
4		Para 3 Line 2: Change "crushed" to "powdered"?	Done	✓	✓
5	p. 3	Para 6 Line 1: re "sharpies", as a brand name may not be universally understood. Better is "marker pen" or "permanent marker".	Done	✓	✓
6	p. 5	Para 1 Line 2: " of powdered sample" [omit "s"].	Done	[no response from reviewer]	-
7		Line 3: A ring-mill is not employed to 'crush' a sample but to convert a crushed rock to a fine powder, as required by powder XRD. It is useful to specify the composition/type of high- purity rings, presumably agate.	Text corrected and now reads "P This technique requires 1 – 3 grams of powdered sample. A larger amount of rock is crushed using the ring mill within the School of Earth and Planetary Sciences, which uses WC rings. A sub-sample is then micronized at the XRD facility using a microniser, which uses agate pellets, provided by the XRD laboratory."	✓	✓

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8	p. 34	Para 1 and 2: An observation: running a normal-sized ring-mill with a mere 3g of rock chips would invite damage occurring to the rings or puck. Agate is especially susceptible to impact damage when sample volume is too little. Possibly a specialised miniature mill was employed?	The ring mill is used to crush the sample. Subsequently, a microniser is used to prepare a sub-sample of 3 g of sample for XRD. This has been clarified in the text." When the tube is dry, the 3 g of sample pre-prepared by the ring mill, plus the CaF2 powder, is carefully transferred to a micronizing tube, where the tube has been re-loaded with 56 agate pellets".	✓ Thank you for the explanation	✓
COPP21065-PRO-G-117 Methodology statement: Chamber Exposure Studies					
	Version	(COPP21065-PRO-G-117 Rev 3)	(COPP21065-PRO-G-117 Rev 4)	(COPP21065-PRO-G-117 Rev 4)	(COPP21065-PRO-G-117 Rev 4)
9	General	This document gives a satisfactory account of the apparatus employed. The methodology is detailed essentially in the form of an operating manual, along with working details, such as personnel and Health & Safety issues. All is presented clearly.	Thanks	✓	✓
10	Section 5.1.1 p. 2	Points (a) to (c): Does the preparation alter the rock surface at all? E.g. do lubricating oils from cutting saw change porosity/reactive surfaces, etc...? If so, what is done to ameliorate the effects?	The cutting saw is lubricated with water, so water may remove highly soluble materials. However, the cubes are placed in water in any case prior to the experiments, so highly soluble material is removed in any case.	Thanks for information	Ok Thanks

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			Text added "The rock saw used for sample preparation uses water as a cutting lubricant and so does not alter the rock except that highly soluble minerals may be dissolved, but these would likely be dissolved by step 5.1.1(b) in any case."	✓ good addition	
11	Section 5.1.2 p. 3	Para 3 Line 2: "where it is attached to" [insert "is"].	Done	✓	✓
12		Figure 5-2 On my copy for review, the cell culture dishes look distinctly ovoid, especially the inset. I'm sure they are round.	Yes, they are standard cell culture inserts and are circular.	✓	✓
13		Caption Line 1: If the culture dishes are round, 35 mm wide) should be changed to "(35 mm diameter)".	Amended	✓	✓
14		Line 5: Possibly "rock surface temperature" would be better.	Amended	✓	✓
15	Section 5.1.5 p. 5	Para 3: You have gone to a lot of very thorough and rigorous effort to expose rock surfaces to weathering agents yet are you only analysing trace elements liberated. Do you plan to do some other pre/post microanalysis of rock surfaces to	Yes, we are also doing this as detailed in section 7.1.5.4 and Figure 7.1-14. The detection of liberated elements is more sensitive than the imaging, so useful imaging can only be conducted after	✓ well explained	Thank you

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		detect physical evidence of surface alteration through this exposure, e.g. SEM imaging, porosity, or surface roughness techniques?	multiple or high dose exposures.		
16		Figure 5-3: Of what is the Biomass combustion chamber constructed? In the MRAMP Report, possible metal contamination from this apparatus is speculated, so its construction material could usefully be given described here.	The primary chamber is aluminium with silica fire bricks for the combustion surface. All downstream apparatus are stainless steel or aluminium and can be cleaned to prevent any organic contamination.	✓ clear but might this usefully be included in the text?	This has been updated in the text of the relevant document.