



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

Murujuga Rock Art Monitoring Program: Independent peer reviews of Monitoring Studies Report 2025 and supporting documents

Department of Water and Environmental Regulation
August 2026

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August 2026

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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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Background

The Murujuga Rock Art Monitoring Program (the program, MRAMP) monitors, evaluates and reports on changes and trends in the integrity and condition of the Murujuga rock art. The program seeks to determine whether the rock art is being subject to accelerated change; specifically, whether anthropogenic emissions are accelerating natural weathering, alteration or degradation processes. The program underpins the Environmental Quality Management Framework (EQMF) which supports the management of environmental quality to protect the rock art on Murujuga. Through this framework, timely and appropriate management responses can be implemented by the Western Australian Government and stakeholders to emerging issues and risks.

The program is a joint initiative of the Murujuga Aboriginal Corporation (MAC) and the Western Australian Government, delivered by WSP and Curtin University.

Reports and documents produced as part of the program are subject to independent peer review by experts of national and international standing in relevant disciplines. Independent peer reviewers are engaged by the Department of Water and Environmental Regulation (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 share joint responsibility for oversight of the program, with MAC providing cultural knowledge and heritage expertise.

This document consolidates all independent peer reviews undertaken for the following documents:

- *Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2025* (the 2025 Technical Report) (Section 1)
- *Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Update 2025* (Section 2)
- Murujuga Rock Art Monitoring Program: Methodology Statements (Section 3).

All comments presented are those of the independent peer reviewers and do not necessarily reflect the views of the Department or MAC.

1 Murujuga Rock Art Monitoring Program: Monitoring Studies Report 2025

1.1 Dr António Batarda Fernandes

Document Title:	Murujuga Rock Art Monitoring Program: Draft Monitoring Studies Report 2025
Document:	COPP21065-REP-G-109
Reviewer:	Dr António Batarda Fernandes

Item No.	Section	Peer reviewer comment Document revision: B Date: 21/10/2025	Curtin MRAMP team response Date: 5/01/2026	Peer reviewer close-out comment Document revision: C Date: 27/02/2026	Curtin MRAMP team close-out comment Date: 20/04/2026
1	General / overarching comments	Overall, actions carried out, and results of the study seem to be on track in the proposed reaching overarching objectives. Moreover, what was noted in comment #3 denotes that an important adjustment and "improvising" capacity on the part of its researchers (see also comment #4) which is arguably important to enhance the scope and end results. But do see issues raised in comments # 8, 9 and 11.	Thank you. Taken as a comment.	-	✓
Executive summary					
2	iv	<i>"There are suggestions in the literature that black carbon (from both natural and anthropogenic</i>	A reference to Dorn's work has been added.	Ok	✓

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		<i>sources) may contribute to delamination of rock surfaces."</i> No references?			
3-Monitoring studies undertaken in the field					
3	Section 3.2 - (p. 37)	The inclusion of Group D samples in the study is very good news given the potential to further refine measuring anthropogenic impact, namely due to industrial activities, on rock surfaces.	The Group D samples were intended as an independent verification of the granophyre porosity findings. At this stage it is not proposed as an ongoing monitoring approach, as the preference of the scientists and the Ngarda Ngarli is to use non-destructive techniques wherever possible. Furthermore, the technique cannot be applied to rock art panels. The permeametry approach is being evaluated to see if it has sufficient sensitivity as a non-invasive technique to measure a surrogate of porosity (permeability) as a long-term monitoring approach for rock art panels.	Ok, I agree with the approach, namely regarding the correctness of using of "non-destructive techniques wherever possible"; let's see if permeametry can be reliably used.	Taken as a comment.
4-Laboratory analysis of field samples					
4	Section 4.2 – (p. 89)	<i>"During sample preparation, the billet for</i>	Agreed! Taken as a comment.	Ok	✓

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		<i>MB02_GPH fractured, and a thin chalky discontinuous orange patina was observed on the fracture surface."</i> One cannot avoid noting how often is the scientific method assisted by chance!			
5	Section 4.2 – (p. 106)	<i>"The timescales that link the initiation of porosity increases to eventual flaking and spalling are not yet well understood, but could range from decades to thousands of years."</i> Hopefully, this will be another issue which this study will shed light on.	Agreed! Taken as a comment.	Ok	✓
6	Section 4.2 – (p. 108)	<i>"(b) Flaking on rock art panel on the Dampier Salt Lease. Images taken 4th July 2024."</i> Are the green patches inside the flaking holes resulting from colonization, namely by lichens? If so, it is interesting to note that flaking holes show such colonization while engraved lines, shallower,	The darker-appearing patches inside the flakes are not obviously biological and are a consequence of a change in relief within the flaked patch. This has been clarified in the caption.	I agree with the adding of this clarification to the text, as the darker patches in the photo do suggest, at least, from afar, to this reviewer, being of biological colonization.	Taken as a comment.

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		for sure, do not...			
7-Statistical analysis of data from each component study					
7	Section 7.6 – (p. 197)	<i>“The purpose of (2) is to obtain complementary information with much greater sample size, by using the cheaper and non-destructive measurement of permeability as a surrogate for porosity (Section 3.2.4.3).”</i> This is critical for overall success of the study!	Yes, agreed. Refer comment 3.	Ok	✓
8	Section 7.12 – (p. 302)	<i>“Data to study emissions in relation to industrial activity is not currently available, so has not been pursued in this report.”</i> I’m somewhat puzzled by this statement, namely considering what is addressed in Appendix C. What precisely is meant by this passage?	Detailed study of sources and/or source apportionment is not within the scope of the project. The emissions from the historic power station and pellet plant were studied as these were historic sources not captured elsewhere. We have requested (and generally received) information from Industries for any abnormal events which may be observable in real-time monitoring.	So, shouldn’t the noted passage be altered, according to the given explanation?	The section has been amended to read: “Apart from historic Ramboll (2022) models for 2014 and our own estimation of DPS and associated emissions from 1966-2010, data to study emissions in relation to industrial activity is not currently available, so has not been pursued in this report. (It should be noted that MRAMP scope is intended to establish safe levels of air quality to protect the rock art, leaving source apportionment and any required emissions reductions

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					as a task for regulatory bodies)."
8-Associations between variables					
9	Section 8.1 - Deposition versus passive samplers	<i>"Some black carbon is due to natural sources such as bushfires, however the majority appears to be anthropogenic."</i> Resulting from? Human set bushfires? Industrial emissions?	The dominant sources in the region are shipping emissions, land-based diesel engines including rail and (intermittently) flaring by the industrial plants.	Ok, should that be noted in the text?	This is actually section 9.1. Text has been added to the end of the sentence to clarify "(e.g. from sources such as gas flares, shipping and land transport)."
Appendix B Photogrammetry					
10	B-18	<i>"(https://play.google.com/https://play.google.com/)."</i> Link not pointing to the mentioned app.	The hyper link has been fixed	Still does not point to the mentioned app.	The hyper link now points to the "Magnifying Glass and Flashlight" app.
11	B-51	<i>The imaging has been carried out without consideration for cultural meaning and it is expected that many of the images will need to be removed from the archive once the conditions have been catalogued. This issue has already been discussed with the MAC Heritage Research Manager who will receive the completed archive and advise on what may remain. There is</i>	All imaging will be retained by MAC and will remain available for future research. Presentation of certain images to a general audience may not be culturally appropriate.	Ok, so images will be kept?... Isn't it worth amending the text on that regard?	The text has been amended to: "The imaging has been carried out without consideration for cultural meaning and it is expected that many of the images may need to have viewing or access restrictions imposed. This issue has already been discussed with the MAC Heritage Research Manager who will receive the completed archive and advise on on cultural protocols in

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		<i>no need to keep any of the images as it is only the nature of the deterioration that is of interest.</i> Won't this imagery be worth keeping for comparison purposes in the future?			consultation with the Circle of Elders. All images and data will be retained."
12	B-51	<i>"Vandalism"</i> A search on previous reports reveals that there are only fleeting references to vandalism in the whole study. This begs the question: is vandalism an issue at Murujuga? Perhaps it is relevant that the final report includes data and analysis of the subject, if indeed it is occurring. After all, even if not of industrial origin, it is an anthropogenic threat to the rock art, if, again, presently occurring.	Vandalism is not within the scope of the project and is dealt with separately by MAC and the relevant authorities.	Ok. Perhaps that should be mentioned then if the reference to vandalism is to be kept.	Text added: "* It is important to note that although vandalism may be detected and reported to MAC and appropriate authorities, vandalism is not within the scope of impact that the MRAMP study is designed to detect or prevent."
13	B-52	<i>"by the author"</i> Who is?	Mr Andrew Thorn was the primary author of this appendix.	Ok	✓
14	B-70	<i>"This may be as infrequently as once every five years, once the preliminary analysis over</i>	Addressed at comment 11.	See answer to comment 11	Addressed at comment 11.

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		<i>the first 3–5 years shows no substantial alteration.”</i> See comment #11			

1.2 Professor Geoffrey Michael Gadd

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Document:	COPP21065-REP-G-109
Reviewer:	Professor Geoffrey Michael Gadd

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1	General / overarching comments	This is a very extensive report and presents results from the first three years of research undertaken to investigate air pollution effects on the Murujuga rock art. The report is also underpinned by several other previously published strategy documents. The report contains a very large amount of data spread throughout the main component sections as well as in an extensive set of Appendices. Some of the latter are written in the same manner as main component sections which seems unusual. There are also extensive sections dealing with statistical analysis of data and associations between variables. It is clear	We appreciate the comments. We have attempted to add clarifying text where possible to link the key sections and findings. However more clarity will hopefully be given in the final report once planned syntheses (combined or linked analyses) are complete. With respect to the response to any adverse conditions, those levels are determined in a separate EQC (Environmental Quality Criteria) document. The protocol for monitoring and assessing against those conditions is detailed in a further document MPDR	OK. It seems there is much to do for the final report. OK Please correct sentence?	Taken as a comment. Clarified to: The protocol for monitoring and assessing against those conditions is detailed in a further document MPDR (Monitoring Program Design Report).

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		that a large amount of work has been carried out in some of the main component areas and large datasets and analyses collected. However, I did find the report difficult reading because of the sheer volume of data included, and for areas at the edge of my expertise it was difficult to see what the main conclusions were from the findings in the various sections. There were no succinct readily understandable summaries of what the results meant in relation to the stated research goals or aims of the overall project in the various experimental sections. The final summary was also lacking in this respect. It is evident that many studies are ongoing while the microbiome aspects of the study were not reported in detail in this report and as stated are dependent on ongoing and future research. As mentioned in previous reports, there is little	(Monitoring Program Design Report). The response is then a matter for the regulator (Government) and possibly MAC.		

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		evidence provided of integration so far between the main organic, inorganic and microbiome components. Perhaps this is still dependent on the ongoing/future research and analyses. The main important conclusion from this report and the work carried out is that the monitoring procedures and analyses used are effective for monitoring changes in the rock art over time and could accurately show any changes in the airshed that may lead to adverse conditions. What steps that might be taken should adverse conditions occur have not been discussed in detail.			
Executive summary					
		The concept of “acceptable” levels of air pollution is difficult. The ideal state would presumably be no air pollution but I suppose that this is not possible. Are “acceptable” levels informed by criteria devised by various government or official bodies or solely	While we agree with the view that no air pollution is the ideal scenario, from a toxicology or dose/response perspective, there should be a level at which there is no measurable response. This level, combined with a suitable uncertainty factor,	The questions are not directly answered so I am inferring that “acceptable” levels are those which will be entirely devised from this research programme. But there must be some published governmental or other guidelines about air pollution that must help	It is indeed expected that acceptable air quality criteria for rock art protection will be set as part of this project. The Government of Western Australia and in some cases the Australian (Federal) Government are the

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		derived from the results of this research programme? Should future monitoring reveal unacceptable levels of air pollution, what steps would be taken? Would sources be closed down? Who would enforce any preventative measures? Air quality – stated that “sufficiently high concentrations of CH₂O or O₃ at ground level could be expected to impact the microbiome or accelerate reaction rates or oxidation processes on the rock surface” but note this is a speculative conclusion in the absence of experimental confirmation. pH and rainfall – stated that lower pH is due to microbial activity, but this is also a speculative conclusion and not confirmed by experimentation (see also later).	should permit “acceptable” levels to be established. Noting there is no part of the planet which is not exposed to some level of air pollution and that many sources are “natural” - e.g. wildfires. A primary goal of this project is to establish such levels. The implementation is a matter for the regulatory authorities. Regarding O₃ and pH, agreed these are speculative conclusions which we are planning to test via the microbiome chamber studies. However, Wang et al. (2021) found that <i>Blastococcus</i> became more abundant under high O₃ exposure. https://doi.org/10.1016/j.scitotenv.2020.142134	deliberations from this research programme. Who are the regulatory authorities? Any detail about them? Local, national etc.? To repeat some questions - Should future monitoring reveal unacceptable levels of air pollution, what steps would be taken? Would sources be closed down? I assumed the O₃/pH studies were already planned for ongoing work. The paper does indeed show some influence of O₃ on microbial communities with added complexities regarding N supplementation.	regulatory authorities for air quality and industrial activities. Ambient air quality guidelines are established and monitored in each state for “criteria air pollutants” (https://www.dcceew.gov.au/environment/protection/air-quality/air-pollutants). These are based on World Health Organisation (WHO) or other relevant guidelines for Human health (We have included these in the EQC document (for NO₂ and SO₂, no established limit exists for NH₃). As the guidelines are set based on human health evidence, they have limited applicability to rock art weathering. By our assessment, our monitoring data demonstrates that the levels of NO₂, SO₂, O₃ at Murujuga are well below the guideline limits for human health, however the dust/particulate (PM₁₀) limits

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					are exceeded. In the case of the levels set by the MRAMP EQC, it is planned that these will be integrated into an Environmental Quality Management Framework (EQMF, https://www.wa.gov.au/government/publications/murujuga-rock-art-environmental-quality-management-framework) and this will in turn specify corrective actions if exceedances of MRAMP EQC occur. The regulatory authorities are the same as those mentioned above. It is understood that the recent UNESCO World Heritage status adds additional reporting requirements and international scrutiny of any exceedances. Yes these O₃ studies were planned, we also are monitoring pH effects on the system due to exposure.

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1-Introduction					
	Section 1.1. Background	No comments.			
	Section 1.2. Objectives of the MRAMP monitoring studies	It appears there is some overlap between some of the stated research goals, particularly 3,5,6 and 7. Some research goals have not been included in this report in any detail, e.g. 9, soil monitoring.	We agree they are interrelated. Some of these goals could possibly be condensed. However, they separately cover identifying relevant emissions sources and types, relevant environmental conditions for peak emissions and measurement of these pollutants in proximity to the rock art. Re soil monitoring. Soil samples were taken during the initial rounds of fieldwork for microbiome and potential isotopic analysis. Soil microbiome analyses were reported previously. Isotopic analysis (Pt. etc.) as a means of determining anthropogenic weathering were discontinued as the approach is technically feasible but would require too large a rock surface sample.	OK but I still think they should have been condensed: differences are not clear as they stand. OK	These have been revisited and goals 3 and 5 consolidated into a new 3, with the original 5 deleted. ✓

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	Section 1.3. Scope of this report				
	1.3.1.	Re the chamber studies, it is stated that “microbial toxicology tests have been completed” but these are not included in this report. Further, I assume that this refers to any effects of the supplied air pollutants on the colonizing microbiome rather than toxicology testing in the usual sense, i.e. application of known pollutant concentrations to pure microbial cultures in a variety of standard procedures where effects on viability can be quantified.	Due to various constraints on document preparation, cutoffs for data must be imposed. This statement was highlighting that some of the planned microbial chamber studies have been undertaken, however after the cutoff date for inclusion in the current report. The initial microbiome studies were indeed relatively classical toxicology studies to assess the response (e.g. viability) to the species and pollutant being studied.	OK, so these results will be included in the final report?	Yes.
	Section 1.4. Abbreviations and definitions				
	1.4.3. Glossary	Biofilm – EPS is the correct abbreviation, not EPSs. Biomarkers – only the biomarkers relevant to ancient environments can be termed “molecular fossils”. Extracellular polymeric substances – it is not just bacteria that can produce	“EPSs” has been changed to “EPS” Biomarkers – molecular fossils removed. Bacteria corrected to microorganisms. Microbial metabolites corrected.	OK	✓

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		these. Widely occurring throughout eukaryotic microorganisms. Microbial metabolites – anything produced as a result of metabolism is a metabolite and can include substances that are not utilised for growth particularly those that are excreted from cells. Microcolonial fungi – this refers to a specific group of fungi growing in microcolonial form and usually thick walled and pigmented. Not all fungi growing on/in rocks and minerals that are important in rock biodeterioration/mineral transformations are microcolonial fungi. Weathering – “Mineral formation” is a better term than “mineralisation”, the latter term being widely (and erroneously) used by soil scientists/microbiologists to mean organic matter decomposition. Also there is no need to use “degradation” at all here.	Microcolonial fungi corrected. Agreed that MCF aren’t the only fungi important to rock mineral transformations – but that has no bearing on defining MCF in the glossary. Edited as per suggestions. The term “degradation” has however been retained as this is MAC’s preferred terminology to refer to acceleration of natural weathering processes due to anthropogenic activity/emissions.		

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2-Study methodologies, verification of competency, and quality assurance					
	Section 2.1. Elaboration of methodologies described in the MSDCA plan	No comments.			
	Section 2.2. Personnel and verification of competency	Re training of the MAC rangers, would any possible monitoring of microbiome development be included in the Rock Art Surface Monitoring credential?	The rock art panels need to be monitored with non-destructive methods. At this stage we don't have any non-destructive ways to monitor the microbiome of the rock art panels. We are looking at mounting patinated rocks to the air quality monitoring stations that can be used for microbiome monitoring long-term. We have trialled non-invasive microbial sampling techniques such as swabbing, but have determined they have lower accuracy/recovery compared to the dremel technique. The rangers have received initial training in both techniques however. Long term microbial monitoring in locations near the rock art panels (where	OK	✓

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			the sample rocks were collected) may be an option if required.		
	Section 2.3. Quality assurance	No comments.			
	Section 2.4. Review of Literature				
	2.4.1.	Broadening of the literature review scope is a good development.	Agreed. Taken as a comment	OK	✓
	2.4.2.	This includes discussion of previously published work on Murujuga rock samples and complementarity to the work in the current project, but this highlights several deficiencies in the published study regarding sample size, statistical significance, and assessment of microbial communities. It appears the published work is of limited value at best to the current work programme.	Agreed. Taken as a comment	OK	✓
3-Monitoring studies undertaken in the field					
	Section 3.1. Field monitoring generally				
	3.1.2.	There are considerable gaps in the field monitoring campaigns especially for	Vegetation sampling was primarily undertaken to obtain a representative	OK	✓

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		microbiome and vegetation. Is any future field monitoring planned or are the aspects described as not being included in Table 3.1.1. sufficient for the work envisaged?	sample of vegetation for organic chemistry and chamber combustion studies. No further collection is planned. The original study design for field microbiome sampling had two planned campaigns before the sample rocks were removed from the field for the other (/lab) analyses. Ongoing sampling of the rock microbiome in field and from AQM rock cubes may be proposed in the finalised study design.	OK I think that this would be a useful approach to undertake. I would have thought this would be an integral part of the study and not “proposed” at this later stage of the project.	✓ Noted and agreed.
	Section 3.2. Inorganic geochemistry fieldwork				
	Fig. 3.2.1.	Some indication what the different coloured symbols mean would be useful.	Figure 3.2.1 redrawn and legend provided	OK	✓
	Section 3.3. Surface change analysis (spectroscopy) fieldwork	No comments.			
	Section 3.4 Microbiome	Some added detail on what microbial samples and how	Details added	OK	✓

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	collection fieldwork	collected would be informative.			
4-Laboratory analysis of field samples					
	Section 4.1. Organic geochemistry laboratory analysis				
	4.1.4.1.	A) The major compounds identified were saturated and unsaturated fatty acids of biotic origin. What about other common biotic compounds like polysaccharides, phenolics etc. or are these not amenable to the extraction method used? B) Presumably the extraction methods used cannot distinguish between cellular constituents or excreted products?	A) <i>We identified other common biotic and abiotic compounds in generally in low abundances such as sterols, terpenes/terpenoids and phenolic compounds. No polysaccharides were determined as the analysis would have required a different preparation of the samples. We only determined mono-disaccharides and anhydro-saccharides. Section 4.1.4.1 has been edited.</i> B) This is correct.	OK but polysaccharides can be very important in microbial biofilms, and associated rock/mineral interactions. OK	Agreed, polysaccharides are important in microbial biofilms but as we stated before, we could not determine them in the polar fraction with our method. Hence, we only reported on mono- and disaccharides - building blocks of polysaccharides. ✓
	4.1.5.1.	Polysaccharides can also be derived from microbial sources. Can these be distinguished from those of plant origin? There are marked differences between many of them.	<i>At this level of analysis, the origin of the identified pyrolysis products derived from polysaccharides cannot be distinguished between microbial and plant origin.</i>	OK	✓

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	Section 4.2. Geology and inorganic geochemistry laboratory analysis				
	4.2.4.6.	Oxalate and formate detection has suggested the involvement of microbial activity, oxalate being particularly associated with many rock-inhabiting fungi. There is no speculation regarding the preceding mineralogical analyses as to whether any of the minerals detected may have been derived wholly or partly from microbial activity, e.g. Fe and Mn oxides, which can be a result of microbial action. Is it concluded there is no biotic involvement here? As oxalate was detected, was any evidence of oxalate minerals found or any oxalate-mediated mineral dissolution?	We are looking further into the association between the microbial community and the ratio of Mn and Fe in the patina. The current scientific consensus is that microorganisms play a large role in the increased concentration of Mn and Fe in the patina Oxalate minerals were not observed and oxalate-mediated mineral dissolution was not recognised, but this wouldn't be a process that could be unambiguously recognised in the field. This has now been stated in the text (see section 4.2.5.1)	OK OK	✓ ✓
	4.2.5.	Apart from brief referral to oxalate and formate in the Discussion (which is a repetition from Section	The ongoing microbiome work largely concerns the metagenome data and the chamber studies. We will be	So this would again be presented in the next report?	Correct.

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		3.2.4.4.), there is no other mention of any potential microbial involvement or linkage with any relevant ongoing or future microbiome work.	able to look at the genomic functions that the MAGs contain and link them back to the geological data we have of the patina samples they were from		
	Section 4.3. Microbiome laboratory analysis				
	4.3.1.	The objectives stated are to use molecular biology and bioinformatics tools to characterise the microbiome community structure and potential activities. There is no mention of the use of culture-based methods and laboratory experimentation with microbial isolates which has been taking place and scheduled in future work. Should the objectives be amended to take account of the fuller breadth of approaches used?	The additional objective of using representative bacterial and fungal pure cultures in the chamber study has been added	OK	✓
	4.3.4.	The results presented reveal a relatively restricted overall bacterial and fungal community composition which fits in with the “extreme” nature of the environment under study as well as the apparent	Agreed	OK	✓

[illegible]

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		revealed representative species that are commonly found in such habitats which confirms the validity of the experimental analyses.			
	4.3.7.	Fungal isolates 1-5 are mentioned but only the two species (1 and 5) are described here. What were the other isolates? Were only 5 apparently different fungal species isolated?	The original SAB plates had multiple fungal colonies, I subcultured them which led to five fungal isolates. Subsequent subculturing led to the loss of three of the isolates, they were no longer growing on SAB plates. So we were left with two that are very happy on SAB and I did DNA extractions on these, which will also be used in the chamber study	OK	✓
	Section 4.4. Chamber exposure (accelerated degradation) testing				
	4.4.1.	There is no mention of bioweathering in the objectives. Is this an omission or is it thought that bioweathering might not be occurring in these tests? If microbial growth and activity is enhanced through elevated pollutant levels	Yes we are hoping to be able to differentiate between physiochemical and biological weathering. Using rocks with the microbes and rocks without the microbes, exposing both to the pollutants and seeing if we can measure if the microbes	OK. Good luck!	Thanks

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		then this could become more significant. Is it possible to differentiate physico-chemical weathering and bioweathering?	are leading to increased weathering.		
	4.4.3.	The organisms selected seem highly appropriate for the purposes described but are described as being “included” in a wider list. Is this list available? Re the unknown fungal isolates – are these different to the two species identified in a previous section, i.e. <i>Toxicocladosporium</i> and <i>Myromaecium</i> ? If they are different, can they be identified? The <i>Spissiomyces</i> genus features strongly in several community analyses – is this organism an appropriate organism to investigate also?	No wider list of bacteria are being used. Yes the fungal isolates are the ones you have mentioned. This was an oversight as I wrote this section before the sequencing data came back. I edited the text to reflect these isolates. A <i>Spissiomyces</i> isolate would be ideal to use in the chamber study however we have failed to isolate it.	OK. I see.	✓
	4.4.3.1.	Methodology states that live/dead cell counts are taken but is the single staining protocol briefly described here effective for both bacteria and fungi? I recall that different procedures may be	The methodology has since changed slightly. Instead of using cell counts, we measure the ‘volume’ and intensity sum within that volume of the syto9 and PI fluorescence signals and record the intensity sum.	OK, so cell counts are no longer used. In that case how is the confocal protocol you indicate used to interpret colonization? Presumably bacteria and fungi cannot be distinguished using this? I	The inoculations of the rocks with the bacteria and fungi are done separately. So, no need to distinguish between bacteria and fungi. Additionally, the confocal imaging still allows you to see the cell counts and

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		necessary because of differences in, e.g. cell wall composition, membrane permeability etc. For fungi, how are “cell counts” possible or achieved? Only those growing as yeast-like forms may be considered as amenable to cell counts. “Counting” a filamentous fungal growth form has always been debatable. Can this aspect be clarified? The last sentence “Post imaging, the same protocol will be used as the inorganic methodology to assess the extent of mineral leaching and pH changes.” Is a little ambiguous – presumably it refers to “as in the inorganic methodology” rather than the imaging protocol.	This is common methodology in confocal microscopy	am not familiar with the use of confocal microscopy to “substitute” or inform about microbial numbers or density.	visualize the cells. The intensity sum is just a more robust metric to measure to determine live dead ratios between images.
5-Air quality monitoring network operation					
		No comments.			
6-Spatial mapping and surface modelling					
	6.3.1.	It is noted that the focus will switch to micro-scale simulations so as the work develops will there be any integration with the Microbiome team and their findings/hypotheses?	Yes this will be a collaborative effort. The main focus will be as stated, to understand moisture retention and gas diffusion to better understand time scales for	OK. So results obtained will be included in the final report?	Yes

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		Moisture retention, porosity and gas diffusion are important determinants of microbial colonization and growth.	microbe/rock/environment interactions.		
7-Statistical analysis of data from each component study					
	7.1.10	The stated incorporation of microbiome response effects from the freshly-cut granophyre rocks would be an essential and very good integrative step in the overall programme.	Agreed. Taken as a comment.	OK	✓
	7.4.2.	The large pH differences between the field campaigns are ascribed to three possible influences, i.e. seasonal effects, biotic activity and methodological problems. In the discussion it is stated that analysis of possible seasonal effects was inconclusive, but there is no further mention here of the possible influences of biotic activity (despite rainfall being highly influential) or methodological artefacts on the differences observed.	We are still attempting to understand the influence of biotic processes on rock surface pH and for this reason have recently incorporated oxalate analysis from field pH water samples. We expect to include more in subsequent reports.	OK	✓
	7.6.	Can some indication of actual sizes/dimensions of pores and channels etc. be provided? This is usual, at	This is an interesting point. We will investigate if we can extract an estimate of connectivity or cluster size	OK. I think this might be straightforward to get an estimate.	Acknowledged.

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		least in studies of microbial colonization of rocks and minerals with which I am familiar. Such information would inform whether microbial colonization was possible or restricted if the pores were too small. I cannot envisage how the results might relate to microbiome issues the way they are presented here.	(of pores) from the image analysis.		
	7.8.	This is a very brief account. Could further details have been provided of the results obtained using the Dremel method?	The sequencing results for the samples that had successful DNA extractions using the dremel method is not complete so nothing can be added.	OK	✓
	7.9.1.	There is mention that a more in-depth analysis of cyanobacteria may be informative regarding the production of certain compounds (also mentioned in 7.9.2.). Has this aspect been included in ongoing/future microbiome studies? It is mentioned that the gymnosperm markers are unlikely to be derived from gymnosperms at the time the rock was formed (p225) so is it appropriate to keep	An in depth genomic analysis of the metagenome assembled genomes isolated from the patina samples will be included in the final report. These results include many cyanobacteria MAGs. We will be identifying potential productions of different compounds genomically, tying that to abundance data and environmental data.	OK	✓

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		using this term in the report to describe these compounds? The text implies the plausible source as cyanobacteria.			
	7.10.-7.16.	These sections contain an extremely large amount of data and descriptions therein but I cannot discern any readily understandable overall conclusions in them (including the summary Key Findings 7.16.). The topics are outside my specific areas of expertise so such conclusions/summaries would have been useful.	The summaries at the end of 7.16 have been revised to aid clarity.	OK	✓
	7.16.1.	It is stated that a newly realized problem was that each individual unit of sample of rock was re-used in different experiments. Apart from mentioned “crossover events” regarding geochemical investigations, would not such re-use have affected any microbiome investigations and therefore would require reconsideration of these?	The summaries at the end of 7.16 have been revised to aid clarity.	OK	✓
8-Associations between variables					
	Sections 8.1.-8.6.	Like many of the individual Section 7 components,	The summaries in section 8 have been revised to aid	OK	✓

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		these sections contain a large amount of data analyses but I cannot discern any simple to read conclusions as to what the findings mean regarding the overall project aims and hypotheses relating regarding influence and effects on the rock/rock art (apart from demonstrating the thorough assessment and validity of the monitoring procedures employed).	clarity.		
9-General summary					
	9.1.	The initial summary paragraphs describe what procedures have been carried out without including and definite or speculative conclusions about what the results indicate in relation to the overall aims of the project. <u>Air quality</u> – it is noted that air quality remains good and that there is no evidence that organic air pollutant species are affecting the rocks/rock art. The suggestion that CH₃O or O₃ could impact the microbiome or accelerate	It was agreed with DWER and MAC that the year 3 report would largely be a summary of progress and definitive conclusions and data synthesis would be included in the final/year 4 report. As the reviewer notes, a number of chamber studies and data synthesis studies related to the microbiome remain outstanding. As mentioned above, given the Wang et al. (2021) we have ordered blastococcus cultures and have O₃	OK but not very useful for reviewers. OK No I meant CH₄ and CO₂ as this is referred to para3, p448. (yes CH₂O/HCHO has been discussed)	Taken as a comment ✓ We do not have any plans to do CH₄ or CO₂ exposures as the ambient levels only experience intermittent short-term peaks above

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		chemical and redox reactions would need experimental validation, so is this aspect included in future work? It is written that the CH₄ and CO₂ spikes are expected not to affect the microbiome but, as stated, further investigations are required to check this. It seems that the microbiome aspect of this project is dependent on many ongoing and future investigations which are hopefully achievable within the project time frame. <u>pH and rainfall measurements</u> – It is stated that the pH levels on patinated rocks tend to be acidic and “appears to be primarily due to microbial activity” although no data has been presented in the report that confirms this. There is a reference to this in 7.4.6. but this only refers to a 2005 publication. <u>Chamber accelerated weathering studies and interim EQC</u> – it is noted that EQC development is dependent on ongoing	experiments planned for all species. We cannot test every species present in the environment and may need to be guided by literature studies or observations from field datasets. We have discussed CH₂O/formaldehyde (I think the reviewer meant). The rock surface pH comments are based on no other plausible explanation (i.e. lack of correlation with other mechanisms and some evidence (both our preliminary work and literature) showing the presence of oxalic acid and other acidic organic metabolites. However as noted, further confirmation is needed.	OK	global background levels which have been assessed by the research team to be highly unlikely to produce a geological or microbial response, ✓

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		microbiome studies.			
	9.2.	The main conclusion is that the monitoring procedures and analyses employed are sufficient to provide early warning of adverse atmospheric changes. It would be useful to state how the conclusion(s) from this report align or fulfil the 11 research goals/objectives stated in Section 1.2. and how any incomplete goals will be achieved. As mentioned previously, should future monitoring indicate serious adverse changes in atmospheric pollution, what will happen? Would there be any mechanism to identify and mitigate or shut down the offending source(s)?	Section 9 has been revised along with this and other reviewer comments. The year 4 report and associated EQC documents will propose both an ongoing monitoring plan and EQC. As well as periodic review to incorporate any additional information from other studies and synthesise new monitoring data. The EQC and monitoring/review is intended to be developed into an Environment Quality Management Framework (EQMF) by DWER and MAC for implementation. It is understood this would in turn be linked to any necessary action to limit emissions.	OK OK (some earlier comments about this)	✓ ✓
References					
		An extensive list – no other salient comments.			
Appendix A Organic geochemistry – additional tables and figures					
		No comments.			
Appendix B Photogrammetry					
		This contains a vast amount	This section was primarily	OK	✓

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		of information and written in the style as some of the previous sections in the main body of the report. I am unclear therefore why it is included as an appendix rather than in the main report.	written by the spectrometry expert (Andrew Thorn) to address previous comments and discussion from the statistical reviewer. It was an editorial decision to place this in an appendix.		
Appendix C Historical emissions due to industrial activities					
		No comments.			
Appendix D Statistical analysis – additional tables and figures					
		This also contains a very large amount of information. Presumably any key conclusions/findings have been incorporated in the main report sections?	This section contains additional analyses which are referred to in the main document where appropriate, however, were deemed less important so were placed in an appendix to reduce clutter in the main report.	OK	✓

1.3 Dr John Henstridge, Data Analysis Australia

Document Title:	Murujuga Rock Art Monitoring Program: Draft Monitoring Studies Report 2025
Document:	COPP21065-REP-G-109
Reviewer:	John Henstridge, Data Analysis Australia

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1	General / overarching comments	This report represents the progress made in the third year of the project. As such it is “only” a progress report but it comes at a time when the contents of final report must be taking shape. In that light it is evident that substantially more progress will need to be made if the final report is to present a reasonably well established monitoring plan. - The project has a number of loosely connected areas, some relevant to understanding the processes of change on rock surfaces and some more oriented into measuring changes in the environment or rock surfacers themselves. (Perhaps one exception	As stated, this document is intended to report progress in the scientific studies, not the environmental quality criteria (EQC) or the ongoing monitoring plan which can be found in separate documents. It was agreed with DWER that this report would be largely limited to a continuation of the analyses in the year 2 report, with more complex data fusion and analyses in the year 4/final report. A key focus in preparing this year 3 report was the independent confirmation of the subsurface porosity, which has been undertaken. However, it is challenging to identify the cause of what is	I appreciate the clarification of the intended scope of this Report. I remain concerned that there is substantial work to be done between this Report and the finalisation of the project, effectively over the next six months and hope that this can be done. However, I must accept the assurances that this is in hand. A general comment about the revised draft is that it appears much closer to a finished Report. It is clear that many revisions have been made.	Largely taken as commentary and advice and we agree in principle. We agree substantial analysis work remains to be done, the statistical team are working hard to ensure that it is completed on time. Thank you. Now that the full data sets

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		to this is the spectrophotometry that attempted to measure objectively change in the rock art directly in a manner consistent with the human eye.) • This was a sensible approach as it avoided making too many assumptions as to a monitoring process when there was still much unknown. • It should never have been expected that all these areas would be successful as so much was unknown and the monitoring has to take place in a difficult environment. • By now I would have expected that some of these areas would be appearing to be successful whilst other less so. For example, the chamber studies reported in 7.1 appear to have made little progress. • Hence it is a time when a scheme for the likely	most likely a long-term / cumulative effect. The interim EQC and monitoring (MDPR) documents detail a more comprehensive suite of parameters to be monitored, including pH, with a weathering index model that combines all relevant parameters that can influence pH. However, despite historic acid rain, it appears that any impact is most likely to be driven by the microbiome, and this work is a key focus of the remaining studies (microbiome chamber studies and more detailed (data fusion) analyses of field samples). With respect to rock/art and rock surface monitoring, all current methods (pH, permeametry, spectrometry, photogrammetry) need to undergo analyses to determine if the signal-to-	A final general <i>statistical</i> comment is that outliers are likely to continue to affect this work, an inevitable outcome of doing difficult measurements in a difficult environment. This observation has been reinforced through the MRAMP team responses and suggests the adoption of an overarching “outlier strategy” may be appropriate. The global adoption of Spearman correlations is a step in this direction. Other issues to be addressed include when and how to Windsorise, how to handle BDL values and possibly censoring.	have been received for all instruments, a thorough QA/QC is being undertaken, including a review of outlier handling, with a view to consistency across all report sections.

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		recommended monitoring would be taking shape, some areas scaled back and the effort for the remainder of the project would be validation of this plan and the establishment of benchmark levels. Ideally this should be of the form of statistical understanding of connections between pollutants and rock surface degradation, and statistically valid measurement processes for the relevant pollutants. I see no sign of this inflection. As it is, the report presents much data but tends to be weaker than expected on meaningful analysis of the data and drawing inferences from the data. For what it gives that is new compared with what was presented in previous annual reports it is probably much too long.	noise ratio from repeats is sufficient to measure any change on realistic time scales. With respect to the chamber studies, the switch to single gas exposures (and initial focus on NO₂ - which has the weakest effect) meant that variability and error sources that were not previously apparent, became more pronounced. The revised microbiome chamber studies design is rapidly producing interpretable results.		

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		Greater thought needs to be given to what is presented and how from a statistical perspective. For example: • Presenting variances in tables is almost always confusing unless part of an analysis of variance where the focus is proportions of variance explained. The standard deviation has the units of the original measurements and hence is strongly preferred where possible. • There is inconsistent use of exponential formats in labelling graphs. Having a zero label on a log scale is a sign that things are wrong. • Plots and maps should only be presented where they help understanding. Many pages of such figures with little accompanying text are of doubtful value. Fewer plots, more carefully chosen and more thoughtfully presented would be better.			

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		- Exploratory data analysis is important in a project like this but is not an end in itself. Pages of boxplots can be useful for exploration, and it is expected that many of these will show nothing. But what is important is what was found and how it might impact upon future monitoring. - A more coherent approach to outliers is probably worthwhile – these are clearly a major issue across much of the data collected, presumably due to the difficulty in the data collection. My comments below highlight issues with specific sections but I am also concerned about what is not in the report. Since so much work remains to be done I have tried to focus on more critical aspects and statistical issues that may carry across to a somewhat different report.			

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Executive summary					
		I note that the executive summary states that “many opportunities remain for increased integration and analysis of combined datasets”. I agree strongly with this comment.	As noted above, this is a key focus for the year 4 /final report.	Agree	✓
1-Introduction					
	Section 1.1 - Background				
2-Study methodologies, verification of competency, and quality assurance					
	Section 2.1 - Elaboration of methodologies described in the MSDCA plan				
3-Monitoring studies undertaken in the field					
	Section 3.1 - Field monitoring generally				
4-Laboratory analysis of field samples					
	Section 4.1 - Organic geochemistry laboratory analysis				
	4.2	On pages 104-105, there	This has been rewritten and	The rewording is a great	Thank you

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		appears to be an inconsistency between the two scenarios of the effect of washing rock surfaces on the pH, and the statement that the observation that pH did not decrease after rain suggesting support for the second scenario. To me it would not support the second scenario.	now reads "The analyses showed that the pH values neither increase nor decrease after rain, so measurements presented in this report (Sections 3.2.4.4, 3.2.4.5, and 4.2.4.6) do not unequivocally support either hypothesis. The lack of a pH increase is considered strong evidence that acidic salts deposited on the surface and precipitated within the rock subsurface are not the main cause of pH values less than 6. However, the lack of a pH decrease is not strong evidence for an absence of microbial activity. It is possible that the response time for acid production by microbial activity after rain is different to the time-gap between rain and sampling, or that near-detection values of oxalate and formate, and the lack of a time series for oxalate and formate sampling, limited the information available from these measurements. Further work, potentially with	improvement and is now clear.	

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			improved sampling protocols (e.g., longer equilibration times, greater rock:water surface areas), is necessary to better support links between pH and microbial activity. Such work is beyond the scope of planned activities for year 4, but it is recommended to form part of the ongoing research program."		
5-Air quality monitoring network operation					
6-Spatial mapping and surface modelling					
7-Statistical analysis of data from each component study					
	7.1	While in one sense it is not critical, this section does not appear to state what the response variable is – what effect is being measured in the rock samples. I appreciate that a reference is made to another document for the description of the study, but it does make it unnecessarily obscure. The response is presumably the total of the	The introduction to section 7.1 has been rewritten to include more information on the experiments, as well as the response and how it was calculated. New text: "Detailed background information on the chamber studies is provided in the Monitoring Studies Report	The rewording and the revised figures are major improvements. My concern about possible calibration artifacts does not appear to have been responded to – hopefully this is not a problem.	Thanks Calibration artifacts should not be present in the ICP-AES measurements undertaken by the Chem Centre as they are well qualified, and NATA accredited in the technique. It is however possibly an artifact of the fact that we

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		change in Ca, K, Mg and Na in the water used to “rinse” out released ions. While this is not unreasonable, it probably requires further development – simply being strongly positively correlated does not in itself say it is best. I suggest either a logic based upon the chemistry/geology or some form of statistical optimality, perhaps using canonical correlation with the predictive variables. Finally, the normalising the response in each graph to a maximum of one means that there is no way of comparing the effects of exposure to different gases. As acknowledged with the text in 7.1.10, the experimental design seems particularly poor here. It would be interesting if the batch effect was attempted to be modelled in the data to see if this could correct the poor design choice? One might expect a somewhat linear or roughly quadratic effect which could be	2024 (Curtin University, 2024c). In brief, rock cube exposures are conducted for a six-day incubation period, with air pollution (either from hydrocarbon combustion or bottled gases) actively introduced at the commencement of the exposure on day one for one hour and then (if repeated) at 24-hour intervals for up to six days. After the active exposure period, the concentration inside the chamber is progressively reduced over the subsequent 23 hours, due to chemical reaction or diffusion in the chamber. For all exposure scenarios, incubation and UV light exposure are continued for six days to permit secondary reactions to occur. Rock cubes are rinsed in a defined quantity of ultra-pure water, then given a “bath” in ultra-pure water for 30 minutes both pre- and post-exposure. Pre- and post-exposure samples and blanks are analysed by the ChemCentre using	The rewording has placed greater emphasis on the unexpected nature of the findings, with the response to gas concentration not being monotonic. It also acknowledges the experimental problems, particularly those associated with re-use of rock samples.	are measuring very low concentrations of the analytes of interest – operating at or very close to the detection limit of the technique. As there is no Figure 7.1-18, I will assume this comment refers to 7.1-8. This figure was intended to show a possible hypothesis of the depletion of reactive surface minerals on the rock with multiple exposures. If this is indeed the case, then the revised approach of using freshly cut rock cubes each time will overcome this. This figure does however use the vegetation combustion exposure data. There are other sources of variability in this particular experiment as we are aiming to produce a homogeneous blend of multiple vegetation species and combust it under the same conditions, which can be challenging for heterogeneous solid fuels. However, such variability is

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		incorporated to the model. Othe oddities include the inconsistent selection of pointwise confidence intervals (e.g. 95%, 75% and 50%). The use of boxplots as well does not provide a high degree of clarity of the distribution of the data. Showing the actual data point would be more helpful to get an understanding of the sample size. Most seriously, the actual results as evidenced by plots such as Figures 7.1-3 and 7.1-6 seem so poor as to raise the question as to why the plots were presented. The “fitted” curves for these plots appear to be so dominated by artifacts and are so inconsistent with the expected monotonic relationship that the confidence intervals are simply not believable. Several of the plots (Figures 7.1-9 to 11, 13) have the lower quartile (the bottom of the boxes at the same level	Inductively Coupled Plasma-Atomic Emission Spectroscopy (ICP-AES) to determine the concentrations of leached elements. For further details, see Section 7.1 in the Monitoring Studies Report 2024 (Curtin University, 2024c). The experiments presented in last year’s report (Curtin University, 2024c) comprised ten exposure rounds (batches). The dose-response relationships between the chemical elements Calcium (Ca), Potassium (K), Magnesium (Mg), and Sodium (Na) when exposed to different doses of a 50/50 diesel/natural gas condensate mix were explored. Experiments using diesel, heavy fuel oil (HFO), and ammonia (NH3) were also undertaken.” And “From last year’s report (Curtin University, 2024c), multivariate analysis	However it does not fully consider the possibility that the experiments have a major flaw. I look, for example, at Figure 7.1-18 and ask why the batch should have <i>any</i> effect in an ideal experiment. Depending upon the precise design, if it was not fully balanced for batch then it is possible for this to affect the dose response curve estimate. Any final analysis should involve (a) a presentation of the experimental design and (b) model fitting to allow for dose, batch and carry over effects simultaneously.	unlikely to induce a monotonic response as shown, therefore the rock cube reuse effect is the most likely, which has now been solved. The final report will include the full experimental design used for all chamber studies. Statistical analyses will account for the variables specified in (b) as well as any other known or expected effects.

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		across all groups. This looks highly suspicious, suggesting a calibration artifact. Is this the case? My understanding is that these chamber studies are central to the setting the Interim EQC. Overall this section needs fresh data based upon a design signed off by the statisticians <i>before</i> the actual experiment. This should probably use fresh cut rock samples, not reusing them unless it is appropriate to consider interactions between chemical species. If they are reused then the design should be balanced in this respect. Fortunately this may be possible since it is entirely laboratory based, not requiring further fieldwork.	indicated a strong positive correlation between the concentrations of Ca, K, Mg, and Na, all in units of mg/L. For simplicity, the sum of these concentrations was modelled. The preferred measure of the response is therefore defined as the difference in the sum total of the assayed concentrations of Ca, K, Mg, and Na, expressed in mg/L.” The suggestion of using canonical correlation is noted. A more detailed analysis of these data will be presented in the year 4 report. All figures (7.1-3-7.1-16) have been redone without peak-normalisation. Additionally, GAMs were used to calculate the smoothed mean response (best model selected based on AIC) and all confidence intervals have been changed to be consistently 95%. Data points have also been added to the plots.		

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			A more detailed analysis, including testing for batch effects, will be presented in the year 4 report. Additional chamber studies are currently in progress as the results presented here indicate that re-using rock cubes has a large effect on the response. New experiments will only use freshly cut granophyre rocks, allowing more accurate determination of exposure effects. The basic design of these additional experiments is identical to the original, the only difference being the fresh-cut-rock requirement. As in the previous experiments, the statistical team provides a new randomised design for positioning the freshly cut granophyre rocks within the chamber for each gas exposure experiment.		
	7.2	This section appears to summarise the problems encountered in getting the two instruments (JAZZ and	During each campaign, the expert spectroscopist (Mr Andrew Thorn) emails the field colour data every few	I accept the clarifications and corrections. The additional information	Thank you Yes the calibration problems

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		HDX) working satisfactorily. It claims that these problems have been largely sorted out but there is no statement about how repeatable the measurements are beyond Figure 7.2-1 which suggests that there may still be major problems. Figure 7.2-1 means that comparisons are being made across campaigns, so it is difficult to distinguish between possible changes in the rock surface, changes in calibration and simple problems in repeatability. I am sure the team has data that might address some of these issues, but it is not presented. The differences in results between the instruments is not really addressed. How is it intended to come up with a “right” result with the two instruments? Is there an understood cross calibration? Finally, I have assumed that what is termed “predicted”	days to the statistical team. Thus, we can identify any problems in the data that haven’t been identified by the R shiny program. Typically, the expert and rangers can remeasure the site/s that have been identified to have problems. Through this process, we have identified three new faults that were not identified in the year 2 report. These new faults have been stated in Appendix D-1 of this report. The R shiny program, which detect faults on the fly in the field, was updated for every campaign with the new faults identified. All data, including faulty data, is stored on level zero of Nextcloud. Another csv file is created that excludes the faulty data, and these files are stored on level one of Nextcloud. These “clean” csv files have been used in the analysis for the year 3 report and will be analysed for the year 4 report. In the Rev. B version of the	suggests that the calibration problems are largely solved.	have been solved.

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		results actually means "estimated". It would be good to use less confusing terminology.	report, Section 7.2.1 stated that "functional Analysis of Variance (fANOVA) was used to model any long-term trend in field colour". However, upon reviewing the original code (just before we received the reviewer's comments), we discovered that the analysis performed was a generalized additive model (GAM) rather than fANOVA. The team member who conducted the original analysis has since left the project, and this discrepancy was only identified during the revision process of their R scripts after they left. To address this, we have now implemented the fANOVA approach as originally intended. The result of the analysis is a curve (trend) showing the relationship between percent reflectance and wavelength. Section 7.2.1 (Section 7.2.2 in Rev. B) is the same with some minor corrections. The rest of Section 7.2 has been rewritten as follows: Section 7.2.2 details the statistical		

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			methodology used to evaluate the similarities/differences between the JAZ and HDX instruments; Section 7.2.3 provides new results; and Section 7.2.4 provides clear conclusions that the JAZ instrument is more variable than the HDX but there was no difference in the trends (mean reflectance curves) between two instruments. We believe this provides a more rigorous assessment of instruments and campaigns, and it aligns with the methodology described in the report. These new analyses explicitly address the differences in results between the instruments so that we are now confident in using the HDX to measure the rock surface colour in the monitoring phase. However, as stated in the conclusion, rock-surface colour monitoring can only be conducted during cooler months, as the temperature		

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			and humidity is too extreme in summer (Garrbarn). There have been no changes in calibration. As stated in Appendix B-1 of the year 1 report: "The final regime adopted since the March 2023 campaign is to recalibrate after each target, with a two-minute rest prior to recalibrating and a 15-minute rest after five targets." Calibration consists of taking: 1) reference spectra, where an instrument takes a measurement of the Spectralon (white diffuse reflectance standard) with the lamp turned on; 2) dark current is another measurement of the Spectralon but with the lamp turned off. As stated in the year 2 report, both the reference and the dark current measurements taken by the HDX are checked by the R shiny program. If the reference trend is flat, it is deemed faulty. If the dark current trend is NOT flat, it is deemed faulty.		

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			Table 7.2-1 had a mistake in the total number of measurements taken by JAZ and HDX during campaign 10. Rev A numbers were actually the total number of JAZ and HDX measurements taken during campaigns 8 to 10. These numbers have been corrected. Thank you for the suggestion to use “estimated” instead of “predicted”. We agree that the term “estimated” is clearer in this context. We have revised the text and Figure 7.2-1, to replace “predicted” with “estimated” when referring to the GAM-fitted values. Figure 7.2-1’s new caption is “Estimated percent reflectance vs wavelength (nm) for (a) AQ03_GPH, (b) AQ04_GPH, (c) AQ11_GPH and (d) EX06_GPH for campaigns 1 to 10.”		
	7.3	This section is largely exploratory. It does highlight a lack of consistency	The purpose of this section has been clarified at the beginning with the first two	I accept the rewording – it is much clearer.	Thank you

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		between the IVL and Radiello devices, but does not comment on a way forward. In this section and throughout there is inconsistent rationale and use of either Spearman or Pearson correlation tests. It should be decided on the what the rationale will be, and that should be consistent throughout.	paragraphs replaced with the following text: “Woodside measures pollutant gases at five sites that are situated close to the Woodside properties. IVL passive samplers (previously Fermi) were used to measure pollutant gases from September 2021 to November 2022 while Radiello passive samplers were used from December 2002 to the present. That is, both sampler types were deployed by Woodside between September 2021 and November 2022. The Monitoring Studies Report 2024 (Curtin University, 2025) showed a scatterplot of the Woodside IVL passive sample data for five sites. In the final study report, a detailed analysis that will potentially include the air quality monitoring data from industry will be presented. In order to ascertain if the industry data (Radiello) is comparable to the MRAMP data (IVL), an exploration of the correspondence	Consistency in using Spearman correlations is appreciated.	Thank you

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			between the IVL and Radiello industry data is presented here.” Additionally, the final sentence was replaced with: “Based on this analysis, it appears that the Radiello data for NO₂ and NH₃ can be used in the advanced analysis as the correspondence with IVL measurements is reasonable. However, further analysis of the relationship between the Radiello and IVL samplers for SO₂ and HNO₃ must be undertaken in the final MRAMP monitoring studies report before these data can be included.” All correlations presented throughout the report are now Spearman correlations only (changes noted in each relevant section).		
	7.4	This again is largely exploratory and hence somewhat superficial. It highlights substantial differences between	As noted above, all correlations in the year 3 report have been changed to be Spearman correlations (see new Figures 7.4-12 to	Good that correlations are now consistent and robust. This underlines the need for consistent approaches to	Thank you Yes agreed about consistent

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		campaigns but except for the first does not investigate or suggest reasons for the differences. The correlation analysis is obviously affected by substantial outliers but the non-robust Pearson correlation is used. (The fact that relationships, if significant, do not seem to be linear suggests that correlations might not be appropriate.) Given that the outliers for at least CI⁻ were likely due to a malfunctioning instrument, removal of outliers should precede any serious analysis.	7.4-14). Closer investigation of the CI⁻ values has revealed that they appear to be data entries made in the field, but they are below the detection limit. These low values observed in Campaign 10 have therefore now been removed and all figures updated to reflect this (see below for more detail).	outliers. It is good that a possible reason has been found and action taken.	approaches to outliers. See above comment.
	7.4.2	I am puzzled when comparing Figures 7.4-2 and 3. The extreme outliers in Figure 7.4-3 (with values of about 7 and 8) do not seem consistent with the data in Figure 7.4-2. Has there been a rescaling? Figure 7.4-5 does not appear to be referenced in the text and it is not clear what if any conclusions should be drawn from it.	Figures 7.4-1 and 7.4-2 had the y-axis restricted to the interval (2,9), hence two unusually large values in campaigns 9 and 10 were not visible – these observations are the cause of the outliers in Fig 7.4-3. The y-axis scale has now been changed to the interval (2,14), allowing these points to be seen. Closer investigation of these two points has led to their being	Explanation accepted. See my above comment on outlier treatment.	Thank you Noted

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		The layout of these figures with seasons represented by rows and year by columns is good but some discussion on what it is telling us would be good.	removed from further analysis, likely due to an operator error. Text has been added to the end of the first paragraph of Section 7.4.2: “Also, two unusually high pH values were noted in campaign 9 (11.69) and 10 (13.5), which are most likely due to an unknown operator error.” Additionally, a paragraph discussing Figure 7.4-5 has been added: “The spatial-smoothing plots in Figure 7.4-4 show that, although pH values vary considerably between campaigns, spatial variability within each campaign is generally much smaller. Figure 7.4-5 removes overall temporal/campaign differences, highlighting spatial patterns likely driven by geological or site-specific factors. For most campaigns, adjusted pH values range between –1 and 1, indicating relatively minor deviations from the		

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			campaign median. Only Campaigns 1, 8, and 10 show higher deviations. Hence, temporal/seasonal differences are the dominant factor in the variation of pH between campaigns."		
	7.4.3	Note my comment about outlier Cl⁻ values above. I am curious as to whether the spatial analysis in Figure 7.4-8 includes these outliers. The Feb-Mar 2025 panel looks very different, but I cannot comment on any statistical significance. A finicky point, but in Figures 7.4-6 and 7 the inclusion of zero on a log scale is an achievement.	Figures 7.4-6 and 7.4-7 have been updated with the low Cl⁻ outlier values removed. Likewise Figure 7.4-8 has also been updated, noting that the spatial plots use winsorisation for extreme values. Additionally, campaign numbers have been added to the spatial plots throughout 7.4 to facilitate comparisons with the boxplots. The y-axes on Figures 7.4-6 and 7.4-7 have been updated to remove zero. We conclude that there is no systematic spatial trend between the different campaigns, that is, the pattern of Cl⁻ concentration differs for each campaign.	Accepted.	Noted
	7.5.2	I have to take issue with the redundancy analysis (RDA)	We thank the reviewer for this comment. As a	I agree with the decision to drop the RDA for campaign	Noted

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		and the comments that they suggest little difference between most campaigns. The statement that the results for campaigns 8 and 9 are “broadly consistent” based on Figures 7.5-1 and 2 is not obviously correct as I see many points of difference between these two plots. The analysis across all campaigns (Figure 7.5-3) suggests that there are substantial differences between campaigns. This is a classic situation where it should be possible to apply a statistical test. This is important as consistency across campaigns (or at least differences being understandable) is a requirement for ongoing monitoring.	consequence, we re-examined the sample sizes for each rock type in campaigns 8 and 9. We found that in campaign 9, several rock types had an insufficient number of samples to support a robust RDA. We have therefore removed the RDA for campaign 9 and only presented campaign 8. Last sentence in the first paragraph of Section 7.5.2 has been changed to: “In this section, RDA was applied only to campaign 8. There were an insufficient number of pXRF samples in campaign 9 to perform an RDA (see Table 3.2-3). Specifically, some rock types were under-represented - only one BAS site and two GRT sites were measured in campaign 9 using the pXRF.” A more formal analysis across campaigns will be included in the final year report.	9 and with the changes in wording. This illustrates two relevant points – the need to consciously give data set sizes and to give formal (if only approximate) analyses so that the readers time is not wasted on insignificant issues.	Agreed
	7.5.3	I make a similar comment	Noted. Again, a formal	Ditto	Noted

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		about this section, where the effect of campaign is not really addressed as being acceptable or not. The spatial comments are vague. I can appreciate that there is lots that could be looked at here so formal inference requires care, but it is not clear what the reader should take away.	analysis will be included in the final year report. For reference, a quick and simple two-way ANOVA of the SiO ₂ :Ti ratio by campaign and rock type indicates that there is no significant effect for campaign, only rock type, reflecting what we see in the boxplots.	Reassuring that campaign is not significant.	Thank you
	7.6	This section is unique in that it follows up last year's findings (which had suggested an issue of heightened porosity between Dampier townsite and Withnell Bay, an area that was closest to possible sources of emissions) with both more data collection and a revised analysis. The revision of the analysis highlighted a coding error in the previous report, leading to an understatement of statistical significance. Hence this Report presented both the revised analysis of last year's data and a full analysis of the extended data set.	We thank the reviewer for their kind remarks on the analysis conducted in this section. Unfortunately, covariates are not readily available to be included in the model. In the year 2 report, an elevation covariate was explored in relation to porosity but was not found to be significant and therefore was not considered here. With respect to the bandwidths used in the spatial plots, the reviewer is correct. The 2km value was the starting point, and the exact values used in each	Agree with changed wording.	Noted

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		It was good that suggestive results from last year led to further data collection to clarify findings, showing the benefit of a flexible multi-year study. The finding with the extended data set (section 7.6.5) is carefully written and has the appropriate statistical tests to ensure that multiple hypothesis testing pitfalls are avoided, meaning that the statements on significance should be taken seriously. While does not prove causation, it does highlight a potential relationship that should be further investigated. I regard the coding error for last year's report to be an unfortunate but, in this case, a minor issue. It appears to have only affected the figures displaying the significant areas, not the maps displaying p value levels and it was in a conservative direction. (Indeed, I should have seen the conflict between the two maps.) However it does	case is stated in the corresponding figure captions. The text on page 201 has been updated as below: “Nadaraya-Watson spatial smoothing (explained in Appendix F.1 of the Monitoring Studies Report 2024 (Curtin University, 2024c)) was applied using an isotropic Gaussian kernel with an initial bandwidth of 2 km.” Page 201 (7.6.3) last sentence of paragraph has been reworded as: “A porosity value of 0.2 is substantial (indicating that 20% of the volume is occupied by voids). Figure 7.6-4 shows there are differences in porosity values for different spatial locations, which result in substantial variations in the spatially smoothed porosity estimates seen in Figure 7.6-5.”		

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		highlight how errors can so easily creep into code. Other points: Were any other covariates trialled within the model that might explain the variability? This global spatial variation may be explained by something apart from distance from the putative emission sources. Perhaps this has been considered, but it is not obvious. Page 201 (7.6.3): It is specified that the Bandwidth is 2km, and then in the next sentence it states the bandwidth selection is semi-automated. It should state here maybe that 2km is the starting value if that is the case? As seen for the graphs in this section, the final bandwidth ends up either being 1.5km or 2.5km. Page 201 (7.6.3): The line - "The smoothed porosity values range between 0.007 and 0.218 (porosity 0.7% to 21.8%). A porosity value of 0.2 is substantial (indicating			

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		that 20% of the volume is occupied by voids), so the spatially smoothed component-of-variation is substantial.” Is difficult to interpret in the sense that the “, so ...” should be attached to the first sentence (the 0.2 being substantial does not influence the component-of-variation).			
	7.7	An initial observation is that the permeameter measurements using the rock cubes (7.7.3) are much less variable than the in situ measurements (7.7.2). Furthermore, the in situ measures are highly skewed, with some very high measures. This suggests that different aspects of the rock are being measured. For example, the in situ measures are likely to be affected by the rock surface (which might be more porous than the deeper layers) or the seal on the permeameter works better on a cut surface as on the rock cubes (which can lead to apparently high porosity).	Agreed, there is a known issue with an improper seal leading to higher porosity values, likely not observed when assessing the flat surface of the rock cubes. As such, the lack of experience of the technician in the field is also a factor. Spatial plots of the rock-cube permeability measures will be added in the year 4 report. At this stage, there are no porosity data available for rock cubes.	Accept.	✓

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		The discussion (page 212) concerning the apparent similarity between the maps of permeability (Figure 7.7-4) and porosity (presumably Figure 7.6-9) does hint at a connection, although one has to wonder whether the comparison is so simple given the problems of the in-situ measurements of permeability being compared with the “deeper” measurements of porosity. It would be useful to have a similar spatial analysis based on the rock cube permeability measures that might be less affected by surface effects.			
	7.9	The analysis here is very exploratory, without statistical tests to back-up what is being said. For example, saying that “The fluoranthene/(fluoranthene + pyrene) ratio is symmetrically distributed across the peninsula, with no specific clusters of higher values observed, particularly near industrial areas” should be backed up by a spatial	Agreed. This data has now all been received, and more substantial statistical analyses will be conducted as part of the year 4 report. Figures 7.9-2 and 7.9-5 have been updated to exclude 0 values (below the detection limit)	Accept.	✓

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		model looking at the distribution of the points and finding that the mean is constant if using a gaussian process. The absence of substantive statistical analysis means there is not much I can comment on.			
	7.10	This section is also largely exploratory (and voluminous). It covers deposition on an almost monthly basis across 21 sites. My comments are primarily on the data as presented in the plots. Note that most of these are limited in what they present as there are three principle variables – time, location, chemical species – and plots of just two are always unsatisfying. A modelling approach is I note that in a number of the plots (such as Figures 7.10-7 to 10) the lower bound of the box plots for SO_4^{2-} and NH_4^+ being the same across the plot suggests a cut off or detection limit issue. I would be concerned if this is the	This section is largely exploratory because only the dry bucket samplers have been analysed at present. Quality checks are ongoing for the wet and mist bucket results. Once completed, a more detailed analysis will be conducted, including modelling where appropriate. Figures 7.10-11 and 7.10-12 have been updated so that all x-axis ordering is consistent. Re detection limits. We have attempted to employ the best available analytical techniques which offer the lowest detection limits. However the environmental concentrations of some	Accept the changes made. The problems of detection limits is difficult to get around. Scientific convention is often that estimates below the detection limit are meaningless, a point that I do not always agree with. When doing a more formal analysis the use of a censoring approach may be appropriate.	Noted Thank you for this suggestion, we will take it under consideration.

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		case, especially for SO_4^{2-} and NH_4^+ which may be anthropogenic and may relate to possible damage to rock surfaces. I am not sure what to take away from the plots in Figures 7.10-11 and 12 as the x-axis ordering is always different.	species are typically very low (compared to typical levels in other urban/rural areas) during the last few years of measurements.		
	7.10.5	This has a more detailed analysis of sites AQ07 and AQ08 that are essentially co-located. Ideally these two sites should give essentially the same results. This is illustrated in Figure 7.10.14 where they are clearly positively correlated. Table 7.10-1 gives summary statistics for each of the chemical species considered. In many respects this contains results of little interest – it is obvious the distributions will be non-normal (so the Shapiro-Wilk tests are of little interest) and the Wilcoxon test subsumes the Levene test, albeit with lower power. What is missing are the correlations	Table 7.10-1 has been updated with the Spearman correlation coefficients, and the Shapiro-Wilk and Levene tests have been removed. Final sentence added to the end of 7.10.5: “Together, the above results indicate that the measurement process is consistent and reliable.”	Agree.	Noted

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		that would give some idea of how much of the data is signal and how much is noise. It would be nice to see a conclusion along the lines of “this shows that the measurement process is sound” if that can be said. The comments are otherwise somewhat anecdotal.			
	7.10.6	This contains ten pages of plots that are not easy to follow since they do not line up months in the year so that seasonal patterns can be identified. The text comments upon some such patterns but I have trouble seeing them in the data. There is a need for a more innovative approach to displaying this data. As part of this it would be useful to present in parallel the meteorological data as it appears that this may explain some of the variation observed.	We agree that the layout of the plots is difficult to interpret and have investigated improving them so that seasonal patterns can be more easily identified. More innovative approaches, including incorporating meteorological information will be considered for the year 4 report.	Agree	Noted
	7.10.7	Pertinent to the meteorological data is the rainfall pH data considered here. This seems very	Rainfall pH is only one component of deposition pH, given the environment mainly experiences dry	Accept	Noted

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		preliminary at this stage. While this seems very important, it is not clear where it is leading.	deposition. Deposition pH has been incorporated in the separate EQC document and the Weathering Index model – which will likely form the main EQC.		
	7.11	This effectively repeats the previous section but for data collected using a different mechanism. My comments are consequently the same. Whilst it was said this was deployed to allow comparison with the deposition sampler, no such comparison appears to be done.	Please see the comments on Section 7.10. Corresponding changes have been made to this section. That is, Figures 7.11-9 and 7.11-10 have been updated so that all x-axis ordering is consistent. Table 7.11-1 has also been updated with the Spearman correlation coefficients, and the Shapiro-Wilk and Levene tests have been removed. A concluding sentence has also been added to state that the co-located stations appear to be measuring the same quantities: “That is, the above results indicate that the measurement process is consistent and reliable.” Scatter plots comparing bucket samples and total deposition samples are presented in Section 8.6.2. A	Accept.	Noted

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			more detailed analysis will be presented in the year 4 report.		
	7.12 – 7.14	Again these sections are largely descriptive and exploratory. Where is the statistical analysis which could have been provided here similar to the porosity analysis in section 7.6? Even though the data is spatial-temporal and more complicated than in 7.6, you would still be able to do the similar Monte Carlo methodology for a certain timespan to determine p-values. Doing a more complex spatial-temporal model would also be of interest.	More advanced analyses will be presented in the year 4 report when all results from the last field campaigns have been received.	Accept.	Noted
	7.12	This section on passive samplers is essentially descriptive and while it uses a number of standard statistical measures, it does not attempt to draw any inferences from the data..	Agreed, the analysis presented is mostly the same as that for the year 2 report. Two new analyses – 1. ANOVA short-scale analysis, and 2. consensus rank and variation to compare spatial patterns, have been included but should be viewed as preliminary at present.	Accept.	Noted

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	7.12.3	The discussion of correlations could be made clearer. In particular, the correlations in Table 7.12-2 are spatial correlations whereas those in Table 7.12-3 are spatial-temporal correlations. The correlations themselves are difficult to interpret – those in the second table are generally smaller but more significant, due to the reduced averaging involved. Similarly the plots in Figures 7.12-10 and 11 are difficult to interpret and indeed I am not sure they help tell a story. For example Figure 7.12.10 tries to illustrate relationships with sinusoidal seasonal pattern but ignores trend and higher harmonics. It barely scratches the surface in terms of exploration while at the same time not being of any inferential value.	We have clarified in the text and table captions how the correlations in Table 7.12-2 and 7.12-3 were calculated. Table 7.12-2 Caption: “Spearman rank correlations between gases for spatial data. For each spatial measurement location, the average gas value was determined across all sampling periods for each gas from all available IVL passive sampler assay results. Each table entry shows the correlation between gases for these average spatial values.” Page 312 paragraph 2 first sentence is changed to: “Table 7.12-2 presents the Spearman rank correlations between different gas species for spatial data averaged over all sampling periods.” Table 7.12-3 Caption: “Spearman rank correlations between gas species for	Clarifications are a much needed improvement.	Thank you

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			spatial temporal data. For each spatial measurement location and each month, the average gas measure was determined. Gas measures were assigned to the month in which the majority of the sampling period took place. Each table entry shows the correlation between gases for these average spatial monthly values.” Page 312 paragraph 3 first sentences are changed to: “Table 7.12-3 shows the Spearman rank correlations for the spatial-temporal (monthly) data. In this analysis, many of the gases are significantly correlated, but the correlation estimates are generally small (less than 0.5). More correlations may be significant here as the data are averaged over a smaller time period. These results may reflect the temporal component in the gas distributions.”		

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			Paragraph 4 of 7.12.3 has been updated as follows: “The correlations in the spatial-temporal data (Table 7.12.3) include the effect of seasonal patterns and other variability over time, whereas correlations in Table 7.12.2 are more directly reflective of spatial gradients of gas concentration. The stronger correlations found in the annual average data suggest that spatial location is an influential factor in predicting gas concentration. The weaker correlations found in the spatial-temporal (monthly) data have several possible causes, including the small number of observations from which the monthly data is calculated and the higher variability of this data.” Figures 7.12-10 and 7.12-11 have been moved to Appendix D with a short		

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			paragraph included in the main text: “The above analyses do not attempt to explicitly take into account the temporal or spatial effects. An RDA analysis was also undertaken and is reported in Appendix D-5-3. This analysis showed that explicitly allowing for time of year or location predictor variables does impact correlations between gas variables. The RDA analysis that included time of year explained 44% of total variation, whilst the RDA with location only explained 12.6% of the total variation. Different predictor variables, e.g. distance and bearing to port, and accounting for non-linear relations could improve the underlying model and will be explored in the final year report.” More advanced analyses will be presented in the year 4 report.		

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	7.12.4	I do not understand Table 7.12-4. I expected the “Residual SD/Mean” to be $\sqrt{(\text{“Residual Variance”})/\text{“Mean of Means”}}$, but it is not. Also presumably the column is just the Coefficient of Variation? Figures 7.12-12 to 15 all appear to have a curved upper frontier to their scatterplots which appear to correspond to one of the paired measures being effectively zero. If so, this suggests a problem of the passive samplers simply not working a proportion of the time. Is this so? If so, is there a discussion of this problem? A statement along the lines of “10% of tubes appeared to not function” would be much more helpful.	Residual SD is calculated as the mean of the SD at each location. Residual Variance is calculated as the mean of the Variance at each location. Hence the Residual SD is not equal to the square root of the residual variance for this definition. The ratio here is the Residual SD divided by the mean of means. The above definitions have now been included in the second paragraph of section 7.12.4 as follows: “Table 7.12-4 reports the gas species and VOCs summary statistics for the duplicate pairs of observations for sampling periods for 2023 and 2024. The second column is the mean of means, obtained by calculating the mean of each duplicate pair at each site and sampling period, and then taking the mean across all sites and periods. The third column is the residual variance, which is calculated as the mean of the variance	Accepted. Regarding Figures 7.12-12 to 15, the fact that for a significant proportion of the data one observation is BDL (effectively zero) while the other one is far above this level remains problematic, in that it suggests a problem in measurement, as if the instruments sometimes fail to register. Again, a possible outlier issue.	Noted Agreed, see comments above.

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			at each location for each sampling period. The residual variance provides estimates of the intrinsic variability of the measurement technique and is the primary goal of this analysis. The fourth column is the residual standard deviation (SD), which is calculated as the mean of the standard deviation for each location and sampling period. The fifth column is the residual SD divided by the overall mean. This value describes the size of the remaining variability in the data relative to the average measurement. The last column is the proportion of residual variability, which is the residual variance divided by the total variance. This value indicates the proportion of the overall variability that is due to residual (within-site and within-period) variation.” With respect to Figures 7.12-12 to 15, thank you for pointing this out. At least two measurements were		

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			required to calculate a difference or variance per site; otherwise, the variance was set to NA and the point excluded from the plot. If a BDL result was returned, then the value was set to half the detection limit. These curves appear to occur when only one of the measurements in a pair was BDL. Explanations have been added at the appropriate points in the text.		
	7.12.5	This compares the results for the stations AQ07 and AQ08 that a physically adjacent and hence measurements at each are replicates. This gives valuable understanding about measurement errors and repeatability. The analysis of variance and similar methods are key to this. Table 7.12-6 demonstrates an attempt to do this methodically even it is a bit simplistic. The issue of whether distributions are normal seems to be driven	This section has been split into different parts, with the introductory paragraph reflecting this delineation: “An important consideration in statistical analysis of spatial data is the magnitude of short-scale variability. Section 7.11.5 of the Monitoring Studies Report 2024 (Curtin University, 2024c) explains that air quality monitoring stations AQ07 and AQ08, which are approximately 25 metres apart, are used to examine the short-scale variability. At	The changes clarify what is being done, particularly emphasising the initial nature of the analysis. The use of non-parametric methods largely overcomes the issues of outliers.	Thank you

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		by outliers. Furthermore the use of p-values for significance of the differences (in mean) between the replicates does not take into account multiple comparisons. Perhaps of more concern is the possibility that a more complex analysis (such as a higher order ANOVA) might be required, rendering the details in Table 7.12-6 essentially irrelevant.	the time of preparation of this report, IVL assay reports for gas and VOC species were available for the sampling months from June 2023 up to and including December 2024. This section provides an initial graphical comparison of gas observations from the two sites, followed by an update of the exploratory analysis determining the magnitude of short-scale variability. Finally, there is an analysis to determine if gas observations differ significantly between stations, firstly without considering temporal effects for all gas and VOC species. Then for gas species where a second order analysis is possible, the results of a two-way ANOVA by station and season is presented.” Table 7.12-6 has been simplified to include only the results of the non-parametric Kruskal-Wallis test for differences between the medians of the measurements for the two		Noted

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			co-located stations (AQ07 and AQ08). Only for Toluene was a significant result returned. No adjustments have been made for multiple testing at this stage. Other results have been moved to the Appendix.		
	7.13	This is largely descriptive, although it does highlight where some further investigations may be appropriate. It also reports numerous instrument problems. Note that this data is all time series (something possible with powered sites) and hence I was surprised to see the absence of time series exploratory methods being used. For example, some investigation of the autocorrelation or spectrum can give insights on appropriate sampling frequency (I recognise that the missing segments complicate this but do not make it impossible.) I particularly note that on page 348 there is discussion of transient peaks in	More advanced statistical analysis, including time series, will be included in the year 4 report for those instruments with sufficient, good quality data available. We have recently received information on specific events at Woodside. In the year 4 report, we will include at least exploratory analysis of the Woodside events (peaks). Figure 7.13-4 has been updated so that seasons are in order, as have the legends for Figures 7.13-22 to 27 and 30.	I look forward to Year 4 Report. The possible association with events at Woodside (and other possible emitters) is potentially a good source of information, effectively tracking spikes. Good that figures have been fixed.	We have dark smoke events that occurred at Karratha Gas Plant & Pluto LNG Park from July 2021 to March 2025. Yes it is planned to compare these events with gas species spikes. Thank you

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		detected gases such as SO₂. Since these may relate to specific events at the process plants or shipping, it seems appropriate to explore these further. The transient nature is not so much a problem but rather an opportunity for understanding possible causality. Why in Figure 7.13-4 are the <i>seasons</i> presented in alphabetical order? In one sense this is a minor issue but it should have been picked up at an earlier stage. Fortunately in other plots <i>months</i> are done in temporal order and in some cases seasons are as well. The legends for several plots Figures 7.13-23 and 27 have similar problems.			
	7.14	This section utilising low powered sensors also provides time series so it may be appropriate to use some time series approaches. Figures 7.14-1 and 3 use different y-axis scales for	Agreed, time series methods will be investigated for the year 4 report. Plots have been updated to use consistent y-axis scales within each Figure to enable comparisons.	Again, I look forward to this in the Year 4 Report. While individually the sensors may be less reliable, together and over time they may be as effective as higher cost sensors.	Thank you

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		each site making comparison difficult. Overall this is all very descriptive, with significant problems with outliers and instrument problems.	Low powered sensors are known to have significant interference from temperature, humidity and other gases. As “early warning” of any adverse events or condition is highly desirable, we remain hopeful that appropriate calibrations can be developed which will permit low powered sensors to be used for this purpose in some capacity.		
	7.16	With the exception of 7.16.7, this discussion of results makes many informal “findings” that are of unknown statistical significance. Phrases such as “no consistent spatial clustering is observed” deserve a bit more rigour behind them.	Agree – much of the analysis presented here is still exploratory by nature. Text has been edited to use “no visual evidence of spatial trend” instead of “no evidence of spatial trend” etc. to highlight that, in most cases, no formal analysis has been performed as yet.	Acknowledge and look forward to this in the Year 4 Report.	Noted
	7.16.1	Agree, but also point out the urgency of getting this design done and possibly reviewed before further chamber studies are attempted.	Agreed. Text at end of 2nd para of 7.16.1 has been edited to: “The statistical team has made changes to the experimental design of these studies, in particular, to ensure that each individual rock unit is used only once. The new experiments are	Acknowledge. It would be good if details of the experimental designs old and new are provided.	As requested, yes the old and new experimental designs will be presented in the Year 4 Report.

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			currently under way, and results presented here should be viewed with caution."		
	7.16.2	Some statement is required regarding the statistical power in measuring change with the current instruments.	We have not yet considered this aspect – measuring change over time. Instead, in this report we focused on the comparability of the two instruments, and the variability of their measurements. Being able to detect change (over time) will be considered in the final year report.	I look forward to this in the Year 4 Report	Noted
	7.16.7	This is a very important finding. While it appears to be statistically very significant, I recommend that further work to verify this in a statistically sensible manner be considered so it cannot be written off.	Agreed (assuming 7.16.6 is meant). This is currently under consideration by the statistical team.	Acknowledge. Since the finding is so important and will come under scrutiny, it will be important that the data set used in the analysis and the analysis itself be fully available for review.	As per the contract, data are available on request, and the analysis method is currently being written up for journal submission. Code will be available from the authors, either directly, via github or an R package.
8-Associations between variables					
	8	This section does contain more formal statistical analysis of certain relationships. On page 447 "distinct associations" are referenced without making it clear whether or not that is a statistical statement.	The word "associations" has been replaced by "relationships" to clarify that the statement is non-statistical. Noted – it is generally stated in the relevant section of the report but does depend on	Acknowledge	Noted

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		A general comment is that it would have been helpful if it was clearer just what data is being considered. For example, are they daily or monthly averages/totals? I suspect mainly daily, in which case the time series nature of the data may need to be considered.	the data in question (e.g. passive sampler data is only monthly).		
	8.1	Some of the relationships between deposition and passive samplers are described as “strong”, based upon correlations of 0.44 to 0.67. This is strong only in that the correlations are strongly statistically significantly different from zero. However this means that only 20% to 40% of the variation in one is explained by the other. The testing for the correlation not being zero is beside that point as you expect a correlation based upon two samplers measuring what are supposed to be closely related quantities. The real question is whether the correlations are high enough to give confidence in the results.	The analysis presented here should be viewed as qualitative. All correlations presented have been updated to be Spearman’s only. As such, there was only one found to be statistically significant. Further analysis is clearly required in the final year report to reconcile the differences observed between the different measurement techniques. The inclusion of the “mist” and “wet” bucket results may be of aid also. Agree with the comment that correlation analysis could be included in other parts of Section 8, this section should be considered a work in progress at this stage.	Few readers would assume that reporting of correlations is intended be purely “qualitative”. If that is the case, it should be made clear.	Apologies, a better word to use would be exploratory.

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		As pointed out on page 432, this correlation analysis is likely to be too simple since it ignores seasonal effects. Note that the other parts of Section 8 do not have any correlation analysis – why is not clear.	Further analysis will be presented in the final year report.		
	8.5	The RDA (redundancy analysis) is used in an attempt to understand relationships and while this is exploratory, it is difficult to know what weight to place on the results. Some statement of statistical significance would be appropriate. In Figure 8.5-1 wind directions are introduced as a categorical variable. In some other places in the report sines and cosines are used as a way of representing directions - some consistency would be useful at this stage. However I also acknowledge that each of the sites is a different direction from each of the possible sources so wind direction itself might not	At present, no weight or statistical significance should be given to the findings of this section – it is exploratory only and should be viewed as such. It will most likely aid our consideration of which variables to incorporate in more advanced analyses for the final year report. Agreed. Use of a more unified measure of wind direction, or else explicitly stated reasoning for alternative(s), will be included in the final year report.		

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		be the best approach.			
9-General summary					
	9	The summary is essentially not statistical.			
References					
Appendix A Organic geochemistry – additional tables and figures					
Appendix B Photogrammetry					
		It is clear that a lot of work has been done on the photogrammetry and the systems are now starting to show results. Figure B-4-2 is most reassuring as it does actually show a reconstructed 3D surface. That said, the document is still difficult to read and to determine just how things are being done. For example, in trying to understand Table B-3-1 I managed to track down actual equipment that was likely to be similar to that used:	The following examples have been added to the document to give the reader more illustrative examples to provide context for the selection.	It seems as if I largely worked out the equipment correctly. The description of Option 3 or (c) should make it clear that while it uses a 4 times objective, it is used to give a magnification closer to 1.5 times. Others might have the experience to judge whether this is a good use of the objective.	Taken as a comment. Taken as a comment.

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		1. The Olympus SZ40 is a stereo microscope:  2. The “DSLR with 100 mm macro lens” possibly referring to a macro lens on a full frame DSLR (several of the major manufacturers produce such a lens for that format). If so, I can understand why it is so heavy – I use a Canon system like that myself.	Example Option 1: Olympus SZ40 with a camera mounting option.  Example Option 2: Canon EOS7D with a 100mm F/2.8L EF Macro Lens  Example Option 3: DSLR fitted with a 4x microscope objective lens and adaptor mount. The actual system used (with light ring) can be seen in figure xxx  Example of Option 4: A 51Mp microscope camera fitted with zoom lens and control from a computer or external device.  Note that while 4 is an option, it does suffer from reliance of multiple devices, and when running instruments remotely using a phone or computer, heat and reliability was often found to be an issue over the all-in-one system provided by option 3.		

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		3. The “DSLR with microscope objective” is using an APS-C format camera (probably Nikon) with the objectives using an 80 mm back focus (instead of the design back focus of about 160 mm). Note that for this the width of the area imaged (that is, in the object plane) is likely to be 5 mm with the 10x objective, not the 3 mm stated. Knowing that this is being used at such a short back focus would raise concerns about image quality although it is not automatically unsuitable. 4. The 51Mp digital camera with zoom” is likely to be a standard industrial digital microscope such as (these are almost generic devices from China):			

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		 Such a combination is quite reasonable (depending upon the quality of the individual components), it is using the optics as designed and is potentially lighter weight than the optics of Option 3, It is frustrating having to do so much searching to determine just what the options considered were. It seems that option 3 was the only one followed through seriously.			
	B-3-3	It is interesting that reasonable quality outcomes appear to have been	P1: The small sensor size does allow for a good depth field in this instance.	P1: Acknowledge. Modern phone cameras are surprisingly good and are	P1. Agreed and taken as a comment.

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		achieved with the Samsung 24 ultra phone. I suspect that the very short focal length and hence small lens size gives a good depth of field that compensates for what might be considered “consumer” optics. It is also a demonstration that software can overcome what would previously been considered a physically less than ideal apparatus in terms of camera placement. I have some concerns about the actual image quality, especially as the 200MP resolution is likely to be well beyond the diffraction limits of the optics. I still have concerns about the longevity of the smartphone solution. Monitoring of the rock surfaces is a 10+ year exercise and current smartphones are unlikely to be supported over such a period. New models are likely to have different optics, different sensors and different software. At the very least it would be	“Consumer optics” have improved in terms of mobile phones (optics and onboard processing), and there are articles showing good comparisons if the distortions can be adequately modelled (better software and better manufacturing processes). • Patonis, Photis. "Comparative Evaluation of the Performance of a Mobile Device Camera and a Full-Frame Mirrorless Camera in Close-Range Photogrammetry Applications." <i>Sensors</i> 24, no. 15 (2024): 4925. • Patonis, Photis. "A Comparative Study on the Use of Smartphone Cameras in Photogrammetry Applications." <i>Sensors</i> 24, no. 22 (2024): 7311. • Abdelhamed, Abdelrahman, Stephen Lin, and Michael S. Brown. "A high-quality	clearly a cost effective package of features. However they are designed for a rather different purpose and are still limited by the physics of diffraction and colour, even if the lenses are perfect. The 200MP claimed resolution exceeds diffraction limits and is clearly designed to achieve other purposes such as image stabilisation. P2. The response considers the issue of the photogrammetry software whereas my comment was more about the phone software. It will be necessary to “preserve” a phone and its spare from software updates if the project is to ensure long term consistency.	P2. This will be considered, however device change may be possible to accomplish through a field cross-validation approach against the D750 as discussed below. If the primary goal is to detect (rock art) surface morphology changes due to additions (or more likely) deletions from the surface, then a change of phone/camera may impact minimum feature detection and create difficulties for long term data fusion, but

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		interesting to see if the next year's model phone, or one from a different manufacturer, gives consistent results. As a general comment, I would prefer to see a component based approach where there is choice of lens, imager, physical setup and software so when a change is forced it is not the complete system. Overall for the development of the phone and other systems, some effort will be required to fully automate the process.	denoising dataset for smartphone cameras." In <i>Proceedings of the IEEE conference on computer vision and pattern recognition</i>, pp. 1692-1700. 2018 Jasińska, Aleksandra, Krystian Pyka, Elżbieta Pastucha, and Henrik Skov Midtby. "A simple way to reduce 3D model deformation in smartphone photogrammetry." <i>Sensors</i> 23, no. 2 (2023): 728. This is especially seen in Geology (morphology) and Heritage field, especially when you consider the additional recording aspects have also improve (GNSS positional and GIS field notations). In truth, the 200MP in the Samsung is probably introduced so that lower resolution images could be created using pixel binning and on-board processing (to		would likely not render the approach invalid.

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			account for lighting and image noise), but this would also (possible) introduce additional processing artefacts. Diffraction limit of the raw image may be a concern with a theoretical maximum sampling resolution being approximately 0.47 μm @550nm wavelengths (less than the pixel size), but the resolution will be more on the order of 1.14 (Rayleigh formula) and we are probably at the limit of resolution for this sensor size without the use of machine learning to get around the physical limitations. Dainty, J. "Fundamental imaging limits of smartphone cameras." <i>Asian Journal of Physics</i> 33, no. 3&4 (2024) 165-166 P2: Most photogrammetry software is fairly consistent (most use Structure from Motion for pose estimation,		

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			and Semi-Global Matching for dense reconstruction, although some use least squares matching instead for this step), and the data could be re-processed to ensure consistency between versions. In terms of new sensors and optics, if the distortions can be adequately modelled, the solution should be comparable. Any new sensor introduced would also be benchmarked against the existing. While a component-based approach may be preferred, it does come at a cost of more failure points and setup requirements, and the smartphone is rapidly being adopted at a recording device because of its complete package (and integration of other recoding functions). P3: Agreed, but it this also represents a small form and complete package to capture information easily and		

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			repeatable in the field. Main issue would be ensuring capture methodology stays consistent.		
	B-4	The comparison of the phone and a more conventional DSLR camera (Nikon D750 with 70mm lens, not exactly one of the options discussed earlier). In essence, while the two approaches used different software and cameras, they were conceptually very similar, with the phone being potentially an easier and cheaper solution. The DSLR camera result was taken as “ground truth”, although that is not explicitly justified. It does however form a useful comparison.	The Nikon D750 was not an option used in the field, but it was on hand for the testing and used for the comparison (being a full frame camera and high aperture lens). Conceptually, they capture method was different as the DSLR captured the rock surface using multiple orbits around the area of interest. This was to get a strong geometry with highly convergent images to solve the camera parameters and reconstruct the surface (but at a lower resolution because of the distance to surface). The phone instead used a more regular grid spacing at a constant offset to the surface (more like for mapping with a drone, but at a closer distance). While the processing is similar, the capture techniques were different with the DSLR	A “proper” camera such as the D750 as a reference is appropriate, with more precise optics and better colour depth. It would have been useful to see how the four options considered each compared with such a standard.	Taken as a comment, however we will consider a field comparison against the D750 method for the next campaign.

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			design to give good geometry solution, and the phone design to give a higher and consistent resolution of the reconstructed surface. Barazzetti, L. "Network design in close-range photogrammetry with short baseline images." <i>ISPRS Annals of the Photogrammetry, Remote Sensing and Spatial Information Sciences</i> (2017): 17-23. Would prefer to use the DLSR as the comparator data set as multiple captures of the surface showed stable results between them. It also follows the more conventional (or adopted) method of image geometry to ensure good geometric reconstruction. That was why it was chosen as the "ground truth", along with the fact it is more generally accepted as the way to do things.		

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	B-5	This section explores using mobile phone imagery to obtain colour measurements. This is an interesting idea, but one that is unlikely to be effective (as indeed the author acknowledges in his conclusions. As well as my general concerns about mobile phones as a long term monitoring option, they have the significant limitation of using only three colour filters of the standard Bayer filter arrangement, each of which is broadband. This simple three dimensional system means that the measurements would be very sensitive to the spectrum of the illumination. By comparison, proper spectrophotometers work with a large number (typically >30) narrow bands, each of which is effectively calibrated independently. The one intriguing approach to this is the extent to which the problem of precisely replicating the location and size of each measuring point	The comment is correct, that this limited due to the sensor, and the limit number of (broad) colour bands. The calibration and conversion from the RGB space to Lab*. If a different mobile system was used, it would need to undergo a comparison between the old and new devices. This could be done using feature point matching but would need a high resolution (spatially) resolution. This comes at the cost of a high spectral resolution with narrow bands. While the system stays stable, it can be used for detecting changes (and large-scale changes such as spalling) if not precise measurement of colour.	It appears that there is agreement that proper colour measurement requires using a proper spectrophotometer. Indeed, the experience of both MRAMP and the previous CSIRO team is that it is hard enough with dedicated spectrophotometers. The use of imagery to detect non-subtle colour changes associated with spalling is a good idea, but probably it is too late to develop at this stage.	Agreed We agree that colour based approaches can be challenging. All MRAMP data will be retained and retrospective analyses could be applied during the planned 5-year review cycles. We have considered that micro and macro-scale analyses of spalling and fracture will be undertaken at intervals from the micro and drone photogrammetry datasets respectively.

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		for colour measurement could be addressed by an image that captures a larger area and then identifies the actual point through software. However the difficulty in finding an image sensor with colour resolution probably renders this impractical.			
Appendix C Historical emissions due to industrial activities					
Appendix D Statistical analysis – additional tables and figures					
	Appendix D	The criterion for placing these results in an appendix rather than in the main body is not clear to me. Indeed it is not clear just why some of these results are presented. For example Figure D-4-1 presumably shows an outlier issue (although the outlier ratios all close to 0.1 is suspicious) and the scale is such that nothing can really be seen. No commentary is provided. Overall, my comments about the main section of the report apply here.	The central analyses and findings (if any) are presented in the main body of the report. The Appendix contains additional exploratory analyses (e.g. using meteorological rather than Murujuga seasons) for reference. They were moved to the Appendix to reduce clutter and repetition. Agreed. Figure D-4-1 has been removed as it was not referenced in the main body of the report.	Noted.	✓

1.4 Associate Professor Dr Ron Watkins

Document Title:	Murujuga Rock Art Monitoring Program: Draft Monitoring Studies Report 2025
Document:	COPP21065-REP-G-109
Reviewer:	Dr Ron Watkins

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	General / overarching comments	I have found the 2025 report in general to be well-written and clearly presented. Once again there is considerable improvement over the previous report in these respects: I have not registered a typo or spelling error throughout, and the size and clarity of the figures with few exceptions is very good. I have made suggestions for improvement of the text, where it might improve the ease of scientific understanding. Where these are merely syntax, I have pre-fixed my comments accordingly. The timing of this report may	Noted and appreciated. Likewise, improvement suggestions are always well received.	Thanks.	✓

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		largely account for much of my overall censure. In the areas of my prime responsibility as a peer reviewer - Geology and Inorganic Geochemistry - the activity is largely post the investigative/developmental stage and has been one of monitoring. The broad criticisms I make with a view towards the pending formulation of the Final MRAMP Report in 2026.			
1		Presentation of information on individual components of the research/monitoring I find rather fragmented. Where results are reported, often by means of very "busy" figure diagrams, there is frequently little or no interpretation. Deferment of most interpretation to Section 7 is unfortunate. Whereas statistical interpretation is undisputably valuable and necessary in such a large-scale monitoring program, it has in this report been to the detriment of more focussed	It has been the preference of the statisticians to confine most of the analysis and discussion to Sections 7 and 8. This was also to comply with the project requirements that all analysis (and interpretation) be signed off by the lead statistician. However, we have revised the report where possible to add clarity.	That's interesting ✓ noted	Noted ✓

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		simple reasoning of individual sets of results. Both, of course, are required.			
2		In the area of my prime responsibility as a peer reviewer, the main delivery of analytical results is by way of extremely numerous Figures. I have criticised in past reviews the paucity of annotation on diagrams and the acute brevity of the figure captions. I was told this was due to a directive from DWER. In the current Report, the outcome is disastrous. Time and again, as a reader, one has the impression that the authors have said "here are the results, see what you can make of them"! The need to refer back to multiple earlier reports and to flip between each Figure and the little amount of explanatory information there may be	The Figure captions constraints were to conform to the WA government style guides. However, we have reviewed the captions and annotations and supporting text. It was agreed that this report would largely be an update of the year 2 report. I.e. largely the same methods with the benefit of additional data. Given the size of the year 2 report and the volume of documents produced by MRAMP to-date, we have chosen to refer to generally refer to previous documents rather than reproduce data.	✓ OK	✓ ✓

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		within the body of the text, in order to make sense of the Figure is both awkward and time-consuming and inevitably hinders understanding.			
3		In light of the main goal of MRAMP, I am surprised that locations of potential anthropogenic sources of airborne emissions are not shown on any of the maps/figures. Shipping lanes and the railway and roads are not depicted, nor are the industrial sites. While the importance of the atmospheric emissions is stated in the text, the only precise information as to their location appears late on in the Report e.g. " <i>King Bay and ranging from Dampier town and Rio Tinto's 2-Mile Rail Maintenance Yard in the south to Withnell Bay and Cowrie Cove in the north</i> ". There would be great benefit in having a simple Figure, early in the Report showing the geographical outline of Murujuga with clear	A new Figure (1.1-1) has been added to Section 1 which depicts major industrial activity, monitoring and sample locations (on separate subfigures to avoid clutter). Major landmarks of Dampier, Karratha and King Bay are also shown. The Industry locations have also been overlaid on Figure 7.6-10 (updated porosity map) for context.	✓ Excellent. The new figure is most useful and the use of three sub-figures does, indeed, avoid clutter. Replacing the two solid black dots on (c) with the same cross symbols for towns used on (a), or vice-versa, would be icing on the cake! ✓	✓ Solid black dots now used for towns in all figures ✓

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		depiction of population, transport, and industrial sites. In addition, the Figure could usefully depict labelled rock collection locations, as these are not otherwise presented in the current Report.			
4		Close to my heart, yes, but nevertheless of genuine importance, I must make a general comment on the delivery of petrological information in the Report. The MRAMP researchers have performed an enormous number of petrographic and inorganic geochemical analyses, using an impressive array of instrumentation. In the reporting of petrological and geological investigations, specifically mineralogy and porosity, I feel there has been an over-reliance on the instrumental techniques at the expense of optical microscopy. The value of SEM/BSE imagery, and associated TIMA, cannot be denied, especially in investigation of the most	This is taken as a comment and as advice. I couldn't agree more with Ron on the value of petrography and the pitfalls of over-reliance on automated techniques. However, it is very hard to find postdocs with strong petrography especially when a suite of other skills are also essential. By necessity these factors are reflected in the report. As lead on Inorganic Geochem, it is my (KE) responsibility to pick up any issues that relate to the primary objectives of the project. However I don't see it as within scope to repeat work unless it's within those primary objectives related to weathering of the rocks.	✓ as it was given. All I can do is repeat that the absence of an experienced optical mineralogist /petrologist doing petrographic analysis is unfortunate in a research program of the scale and profile of MRAMP that so depends upon the textures and minerals of the rocks and patina. I understand what is said and do not suggest KE must repeat all petrographic work undertaken by others! However, I do not see how incorrect identification and/or misinterpretation of features of the rocks and their weathered surfaces, as occur in the Report, fall outside the primary objectives of MRAMP.	✓ Agreed. I hope that we have corrected the issues and certainly any that relate to the final interpretation.

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		finely-crystalline rocks and the weathered rind/patina. However, the availability of such techniques does not devalue the effectiveness of the petrographic microscope. There are instances in the Report where intricate interpretation of equivocal SEM/BSE images is attempted, when the optical photomicrograph clearly shows the answer. In particular, the exclusive use of TIMA in identifying minerals and in modal analysis is misguided, leading to otherwise avoidable imprecise mineral designation.			
5		As Item 4 above, and as I have commented upon and corrected in previous reports, the petrographic description of rocks and minerals is less than perfect, and on occasion both naive and incorrect. Recognition of the importance of porosity in attaining the main goal of MRAMP, has served to	This is taken as a comment and advice and my comment (KE) above also relates to this.	It is indeed advice and my response above also relates to this.	Advice taken on board.

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		highlight the need for quality petrological and mineralogical reporting, including the recognition and genesis of voids. I strongly suggest more critical consideration be given to the detailed petrography/mineralogy when producing the Final report.			
6		Because I can't help myself, a word on the use of "data" and not that it is or isn't plural. At least in the Geology and Inorganic Geochemistry components of the Report, there is an almost exclusive use of "data". Too often the nature of the data is ill constrained and the misconception that data and results are synonymous abounds. A datum is simply any item of information; more specifically according to the Oxford Dictionary "a thing known or assumed as a fact, making the basis of reasoning or calculation". A modern definition of data	Taken as advice. There is a 'house style', which we try to stick to for consistency across the report. However, we can discuss Ron's suggestion for future reports.	✓ I have criticised inconsistency within the earlier reports, so must applaud efforts to irradiate it. Consideration of my suggestion that "data" not be used as a 'catch all' term is appreciated.	Thank you

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		"the quantities, characters, or symbols on which operations are performed by a computer" may be the key to my questioned over-use in the report. There is room for both constructs, but "results", where applicable, is more informative.			
Executive summary					
7		Page ii, Para 2, line 4: "Moving forward" is unscientific journalise at best. Suggest "In future". Omit "data" (it is redundant when repeated in the next line). A timeline for the Final Report and transition to on-going monitoring would be usefully mentioned here. [it is given later].	"Moving forward" has been changed to "In future" We cannot omit "data" because "data fusion" is a statistical analysis that integrates multiple data sources Approximate timeline added to this paragraph. "... final report (due mid 2026), before the program transitions to ongoing monitoring following peer review and publication."	✓ OK fine ✓	✓ ✓ ✓
8		Page ii, Para 5, line 5: I cannot see how this "approach" alone will "ensure" the long-term protection of the rock art? Rather it will form a foundation for future legislation and governance.	"ensure" changed to "form a foundation for"	✓	✓

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9		Page ii, Para 5, line 8: [syntax] Is there such a word as "learnings"?	The reviewer is of course correct, this is management jargon that crept in via the editorial process. It has been replaced with "knowledge"	✓	✓
10		Page iii, Para 3, line 3: Add "inorganic" before "geochemistry".	Added	✓	✓
11		Page iii, Para 3, line 5: "good" when unqualified is to be avoided. It can be omitted here without any loss of meaning.	Omitted	✓	✓
12		Page iii, Para 5, line 4: No need for a reference here in the Executive Summary but see general comment above regarding location map.	Maps have been added to chapter 1	✓	✓
13		Page iii, Para 5, line 8: In accordance with the style of this paragraph, you should insert (HNO ₃) after nitric acid.	Added	✓	✓
14		Page iii, Para 5, line 18: re. "accelerate reaction rates or oxidation processes on the rock surface" - meaning is unclear. Accelerate reaction rates of what? Is there possibly a typo and "or" should be "of"? If so, I suggest: "accelerate	Text amended to "... could be expected to impact the microbiome via established biocidal effects (at high concentrations) or accelerate oxidation reaction rates on the rock surface."	✓	✓

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		oxidation reaction rates".			
15		Page iv, Para 1, line 4: change "which will be analysed" to "and will be analysed"	changed "which will be analysed" to "and will be analysed"	✓	✓
16		Page iv, Para 2, line 3: omit "on average"	Omitted	✓	✓
17		Page iv, Para 2, line 4: "across all data" - precise meaning is unclear . You must state specifically which data? [see later comment on the overuse of "data"].	The text has been amended to: "The average rainfall pH is 7.4 and the deposition sampler dry pH is 6.7 for all measurements (years 1-3) presented in this report. This contrasts to previous measurements in 2005-2009 by CSIRO (2010) which show mean and median rainfall pH <5.6 for this period." Note the addition of more CSIRO rainfall pH data to relevant graphs.	✓ ✓ noted	✓ ✓
18		Page iv, Para 3, line 1: What is meant by 'live' surfaces and how do they differ from patinated surfaces?	This was intended to emphasise the expected presence of viable microbes on the surface. As opposed to patinated rock samples used in chamber or AQM studies which may be	OK	✓

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			rendered sterile.		
19		Page iv, Para 3, line 4: It would be instructive to give a pH value in brackets after "a clear alkaline signal".	Text changed to: "Measurements on the surface of bushfire-exposed rocks in the second year of studies showed a clear alkaline signal (as high as pH 8.9), which reverted back to (slightly acidic) baseline levels over 9-12 months following the exposure."	✓	✓
20		Page iv, Para 3, line 7: Have you measured the pH of sea spray at Murujuga? Spray is more likely to be "weakly alkaline", while marine aerosols (finer than spray) can have a very wide range of pH.	Limited measurements of seawater and sea spray from Murujuga have been in the range of 7.5-8.5. The reviewer is correct that Marine aerosol pH can vary (and is challenging to measure accurately) however can encompass a wide range and fine/ultrafine aerosols have been reported to become highly acidic. The text has been amended to: "... as well as sea spray, which could range between acid and alkaline."	✓	✓

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21		Page v, Para 2, line 8: add a comma after "weathering than NO ₂ ," - it is required for correct meaning. Also line 8: "important to weathering" is rather vague. Suggest: "effective in promoting weathering".	Comma added. Edited as suggested	✓ ✓	✓ ✓
22		Page v, Para 4, line 4: Replace "amounts" with "numbers".	Replaced "amounts" with "numbers".	✓	✓
1-Introduction					
	Section 1.1 - Background				
23	1.2	Page 2, Para 5, Line 8: Change "samples" to "sampled".	We could not find this specific instance; however the section has been proofread and revised.	OK - fine	✓
24	1.4	Page 7, Table 1.4-1: UNESCO appears twice.	Duplicate removed and UV moved to correct location.	✓	✓
25	1.4	Page 8, Table 1.4-2: P is Phosphorus. Although I have found an example of the spelling "phosphorous" for the element in the literature, it is unconventional. "Phosphorous", as used repeatedly in the Report is an adjective meaning <i>having characteristics of phosphorus</i> .	Corrected.	✓	✓

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2-Study methodologies, verification of competency, and quality assurance					
	Section 2.1 - Elaboration of methodologies described in the MSDCA plan				
26	2.2.3.1	Page 19: Para 6, dot point reproducibility: The sentence does not read well. I assume a verb is missing. Possibly add..... different samples are taken under the same experimental	Amended as suggested	✓	✓
27	2.4.2	Page 21, Para 2, line 2: "acid rain" is loosely used as it more accurately refers to precipitation with measured pH of < 4.5. line 2: change "(5.0)" to "(pH 5.0)" Whereas the set-up of the experimental chambers may be to approximate the pH of rain, it is the pH of H ₂ O in the environment of the rock surface that with other factors (e.g. biome) ultimately determines the	Agreed. "Acid Rain" has been placed in inverted commas in the text now for this reason. Changed to "pH 5.0" Also agreed.	✓ ✓ ✓	✓ ✓ ✓

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		rate of weathering of the rock.			
3-Monitoring studies undertaken in the field					
	Section 3.1 - Field monitoring generally				
28	3.1.2	Page 23, Para 4, line 1: Fieldwork campaign dates - omit "s".	Corrected	✓	✓
29	3.2.3	Page 27, Para 4, line 4: 25 mm? presumably diameter? Clarify.	Yes. Added.	✓	✓
30	3.2.4.2	Page 31, Para 2, line 3: An exercise to compare the performance of the two electrochemical field instruments is mentioned but not described or the results even briefly discussed. Reference is made to Figure 3.2-1 which also gives little clue to the overall nature of the exercise [see comment Item 1].	Comparison exercise described and discussed in detail – see section 3.2.4.2.	✓ but note: In the modified text Page 33, line 38 (2035) <i>pHpH</i> ? Omit pH.	✓ The second pH has been omitted.
31	3.2.4.2	Page 33, Figure 3.2-1: There is no key to the coloured points. Caption is not at all informative and there is no explanation of what it shows. The label	Figure replaced that includes legend and location to replace the time series numbers. New caption reads "Comparison of repeated measurements using the	✓	✓

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		"time series" gives no indication of the time of measurements that are merely said to have been taken during two weeks of Campaign 10.2 The schedule of the "time series" is vital to know, e.g. one per day over 12 consecutive days?	WP90 and new Ranger equipment for: (a) pH; (b) ORP; (c) Electrical conductivity (EC). Measurements made during weeks 3 and 4 of campaign 10. Each pair of measurements sampled the same solution and the measurements took place within five minutes of each other." Table added to provide the date information	✓ all good	✓
32	3.2.4.3	Page 34, Para 2, line 6: Can the variations in permeability resulting from varying temperature and humidity be quantified, such as to enable calibration for the effects? Relying upon "time of year" would appear to be quite inadequate given the considerable diurnal and daily changes in both factors.	Paragraph rewritten to clarify steps taken and the current state of knowledge regarding this perplexing change in values for the reference sample. New text reads: "Regular measurements of the reference material show a drop in permeability between campaigns 9 and 10. The reference material, a cut slab of dolerite from site AS02, was analysed three times each morning of the field campaign at the field accommodation. The total number of measurements for each	✓	✓

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			campaign ranges from 42 to 51. The average and standard deviations of these measurements made during campaigns 8, 9, and 10 are 7.3 ± 1.1, 6.2 ± 1.3, and 1.7 ± 0.6 millidarcies (mD). Consistent results within a campaign indicate that the permeability values are meaningful. The drop between campaigns 9 and 10 accompanied a commensurate decrease in field measurements, indicating that the cause does not relate only to the reference material. The manufacturers were consulted and, after some basic checks, responded that they are not aware of any hardware-related issues were could have caused the issue. Initially, it was thought that high temperatures and humidities may have affected the values, but preliminary analyses made in the Curtin laboratories prior to campaign 11, and three months after campaign 10, yielded similar low	✓ Fortunately the repetition appearing here is NOT present in the Comparison report, and the additional/amended text is fine.	✓

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			values (0.9 ± 0.2 mD, $n = 15$). The temperature and humidity under laboratory conditions are not high, so it seems unlikely that these factors were the primary cause of the change in observed values. Further, work on sandstones indicates that changes in relative humidity may account for changes in permeability of a factor of two, much lower than the factor of five observed between campaigns 8 and 10 (Meng, 1992). However, the addition of the campaign 11 data, which provides a full year of permeability monitoring, will confirm or refute this hypothesis. The full data set will be discussed in the final report. However, the addition of the campaign 11 data, which provides a full year of permeability monitoring, will confirm or refute this hypothesis. The full data set will be discussed in the final report. Until this issue is resolved, comparisons of		

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			measurements between field campaigns for which the reference material values differ should be undertaken with caution.		
33	3.2.4.3	Page 35, Table 3.2-5: The caption/headings of the table are quite inadequate. This is especially the case with regard to the statement in the text that "<i>temperature and humidity may affect the field permeability measurements</i>". Are T and humidity values not available for the numerous field measurements? They will surely be for the laboratory comparison. Is at least time of day of each measurement known? Adding mean and standard deviation values for each campaign is useful. In its current state, I question the purpose of Table 3.2-5? This needs to be made clear. If the values relate exclusively to the same dolerite standard, then it appears that the protocols/methodology were	Table removed; mean and standard deviations added to the text. Temperature is measured in the field and at the accommodation. The basis of the comment "it appears that the protocols/methodology were unsatisfactory in campaigns 8 and 9 but improved in campaign 10 when results were still significantly greater than recorded under controlled laboratory conditions" is unclear. Please clarify and I'll try to improve. I have added the text "Consistent results within a campaign indicate that the permeability values are meaningful." As well as "comparisons of measurements between field campaigns for which the reference material	✓ ✓ noted ✓ I apologise as my meaning was not clear. This issue has been the subject of a modified to text in the Comparison report and has been accounted for.	✓ Thank you ✓

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		unsatisfactory in campaigns 8 and 9 but improved in campaign 10 when results were still significantly greater than recorded under controlled laboratory conditions. Some basic explanation on the Table and in the text is required if any conclusion can be drawn as to the quality or significance of the tabulated values.	values differ should be undertaken with caution" to clarify the perceived quality and significance of the data.		
34	3.2.4.4	Page 36, Paras 2 and 3: I consider the statement of Point 1 is an over-simplification and may suggest a rather naive view of the complex chemical system that exists both during varying climatic conditions and in the procedure of measuring rock surface pH. For example, it assumes that the "acid" deposition remains on the rock surface and does so in a readily soluble form. It does not allow for previous infiltration of rainwater to the weathered layer, despite that where there is porosity such infiltration will exist, or	Change made as suggested. There is no assumption that the acid deposition remains on the rock surface – the water sampling interacts with the porosity below the surface. This has been clarified within the text "if low pH values are caused by acidic deposition, as suggested by Black et al. (2017) and subsequent researchers, then washing of the rock surfaces and infiltration of the rock substrate by rain will remove acidic salts. If soluble acidic salts were present before rain and removed by the rain	✓ ✓ clarification is fine Perhaps my comment was harsh. I have read the section in the Comparison report and any such implication, if present in the initial, is not present.	✓ Thank you Noted. Thanks.

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		have existed, to initiate the mineral alteration in that zone. Furthermore, most importantly, it implies that the authors consider pH to be synonymous with acidity, which it definitely IS NOT, in the likely weakly-buffered systems of heavy rainfall and the procedure of pH measurement. I suggest changing "will remove acid ..." to "may remove acid salts," as is the approach given in the following Point 2.	then the pH is likely to increase." I'm not sure where we imply that pH is equivalent to acidity. If this can be pointed out then I can alter the text.		
35	3.2.4.6	Page 36, Para 5, Dot Point Group C: It is not exactly clear here how samples of Group C were derived, e.g. collected from the location of equivalent Group A samples; from adjacent to precise site of previous sample removal; a split from the original Group 1 samples? Subsequent reading of the Report (Section 4.2.1) has revealed the required information, but I suggest that modification of the text is made <u>here</u> to	Clarified with the text "a suite of 11 billets of granophyre from group A. These provide effective duplicates of eleven of the group A samples"	✓ It seems that the final version in the Comparison report is a little different from this, but the meaning is now clear.	Thank you

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		clarify their source.			
36	3.2.4.6	Page 37, Table 3.2-6: Where pH is quoted to just one decimal point, it suggests a less precise measurement was made. If a second decimal point was recorded and "0" has been omitted, add the second decimal as it is significant.	pH observations changed to provide to two significant figures for all measurements	✓	✓
37	3.2.4.6	Page 37, Para 1: I cannot recall "remelted granite" used before as a rock type. It infers a specific genesis that the five main rock types do not. Some explanation is required if this unusual petrological term is to remain. Petrologically, a "coarse-grained dolerite" and a "fine-grained gabbro" are the same thing. I expect you are referring to your lithological types, in which case it would be correct to say either a "coarse-grained sample from a dolerite (dyke)", or a "fine-grained zone of the gabbro".	The qualifier 'remelted' has been removed because it does not have significance for these studies. The contact relationships are unclear for this sample, hence the terminology. I have changed the text as recommended and clarified that the contact relationships are unclear.	✓ ✓	✓ ✓
38	3.2.4.6	Page 37, Para 2, line 4: suggest change "rocks exposed on the walls" to	The term 'newly' could be misinterpreted so text changed to "so rocks	✓	✓

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		"newly exposed rock surfaces on the walls". line 6: omit "relatively".	exposed on the walls of the cutting since construction of the cutting have only been weathered over the last few decades" 'relatively' omitted.	✓	✓
39	3.2.4.7	Page 40, Figure 3.2-3: This Figure (locality map) in nowhere referred to in the text. The significance of the red rectangle is not stated in the caption.	Locality map is now referred to in the text. Caption modified to mention red square "The purple line shows the area accessed and assessed for sample collection. Samples were collected from within the red square."	✓ ✓	✓ ✓
40	3.2.4.7	Page 40, Figure 3.2-4: The figure's photograph is useful and should be referred to in the previous text (page 39, para 2) and/or its relevance/location be mentioned in the figure caption (I am aware it is referenced in later section 4.2.1, but the figure is well distant from that text).	Figure is cited in the text "Each of the five rock cubes is made of one of the five main lithologies that host rock art at Murujuga (granophyre, gabbro, dolerite, basalt, granite) (Figure 3.2-4)." Further info added to caption "Microbiome, pH, and PXRF data were collected from the rock cubes prior to campaign 9."	✓ ✓	✓ ✓
41	3.2.4.7	Page 41, penultimate para, line 3: [Syntax] Add comma	Comma after "instruments"	✓	✓

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		after instruments.			
4-Laboratory analysis of field samples					
	Section 4.1 - Organic geochemistry laboratory analysis				
42	Section 4.2 - Geology and inorganic geochemistry laboratory analysis	Some general comments:- In petrographic description of igneous (and metamorphic) rocks it is customary to discuss textures and minerals in terms of crystals with "grains" typically being associated with sedimentary rocks. This is not a hard and fast rule, with examples such as "a coarse-grained granite" rather than "a coarsely crystalline granite" being commonplace, but overall "crystals" rather than "grains" is to be preferred. In the specific case of the rock patina, where their formation may include the incorporation of wind-blown particles, differentiation between	Grains, as used here, is consistent with common usage in the journals that I edit, so it is retained for consistency with the previous reports. I appreciate the value of distinguishing between primary igneous grains and aeolian grains and the text has been checked to make sure there are no areas where there is ambiguity that leads to confusion. Have searched and placed for modes by area, as suggested. See comment 52. The font issue between numbered and un-numbered sub-sections is likely an issue in previous word conversion workflow for	Accepted, and your second point caters for my concern regarding aeolian grains ✓ OK ✓	Thank you ✓ ✓ ✓

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		residual igneous crystals and aeolian sand/dust grains is especially important. • See notes below on the unusual and inappropriate use of the term "modes by area" that occurs throughout this section. • In the sections 4.2.4, it may be an artefact of my computer display but the font of the un-numbered sub-headings (granophyre, dolerite, etc) appears equal or a little larger than that for the principal numbered heading (4.2.4.3 weathered rind, 4.2.4.4 Patina, etc). This makes navigating the report a little confused. • Since the rock groups, e.g. group A, are formally defined entities, I would suggest that "group" be capitalised throughout the report e.g. Group A.	documents. We now have agreed an improved workflow which should avoid these errors. All "group" (e.g. "Group A") has been capitalised throughout report.		
43	4.2.1	Page 51, Para 1, line 3: I thought the authors had	"fresh rock changed to "fresher rock"	✓	✓

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		settled for "fresher rock" - make change for consistency.			
44	4.2.1	Page 51, Para 2: The source of the Group C sample suite is well explained here (refer to previous comment (Item 35). I note that the text relating to Groups B and D is duplicated from the earlier section - thus is unnecessary?	More detailed descriptions of sample suites grouped together in Section 3.2.4.6 and that section is referred to here, removing the repetition	OK	✓
45	4.2.1	Page 52, Para 2, line 4: a verb is missing from this sentence - "to compare" or "to complement".	Changed as suggested	✓	✓
46	4.2.2.3	Page 53, Para 2: For the non-expert analyst, you might usefully point out that the HPIC is a solution-based method, and briefly describe how solutions were produced from the rocks.	Changed as suggested "Oxalate and formate were determined using ion chromatography on aqueous solutions produced by the same techniques used to produce samples for pH measurement (Section 3.2.4.5)." Additional information also provided in the relevant section on the field work. "Samples were analysed using a Dionex ICS 5000 high pressure ion	✓ perfect	Thank you

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			chromatography system. For both oxalate and formate, results were determined with reference to a calibration curve resulting from the analysis of standards prepared at concentrations of 0.2, 0.5, 1, 2, 5 and 10 mg/L of the target analyte."		
47	4.2.3	Page 54, Figure 4.2-1: (again) is the "fresh rock" in the turquoise-coloured box synonymous with "fresher rock", as adopted in the MRAMP program?	Fresh rock changed to fresher rock	✓	✓
48	4.2.4.1	Page 55, Para 2: importance of punctuation - in my experience the weathered rind is normally pale in colour so "The weathered rind is a pale, orange or brown coloured zone" is correct.	A comma has been added after "pale"	✓	✓
49	4.2.4.1	Page 55, Para 3, lines 3 - 4: the sentence "In addition, weathered rinds....." is superfluous and contradicts the previous sentence by implying that Group D rocks never show a weathered rind. The previous sentence stating	Text fixed "However, the Group D samples lack weathered rind and generally lack patina, with only a discontinuous chalky orange friable coating on parts of some surfaces."	✓	✓

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		that the weathered rind is "generally lacking" implies it is sometimes present. I suggest you omit the sentence.			
50	4.2.4.2	Page 56, Figure 4.2-2: the caption may be somewhat confusing, especially to readers inexperienced in BSE-SEM imagery. You should begin the second sentence of the caption with (a), as you do later for (b,c,d). It is not immediately obvious the relationship between the outlined red boxes. Your caption refers to the "red box" when there are two red boxes and the smaller red box defines an area, rather than location. It would make clearer if you write "The area in the smaller red box is shown at greater magnification in the larger red box". Alternatively, you could alter one of the rectangles in a different colour. Regarding panels b, c and d, while obvious to the authors, it may be prudent to	Caption rewritten so that the letter precedes the relevant information. The red box situation clarified. Captions to panels b–d clarified as requested. New caption reads "BSE-SEM images showing the variable nature of the transition from the fresher rock to the weathered rind in granophyre sample D001_GPH. Voids are shown in black, as is the region above the upper surface of the rock. (a) Area showing large sub-surface voids. The upper red box delineates the area shown at higher magnification in the lower red box; (b) Voids ranging from a few	✓ ✓ ✓	✓ ✓ ✓

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		state that the images depict three different areas of sample D001-GPH, and not the same area under different SEM conditions. You might usefully mention that the voids and porosity are shown in black matching the region beyond the exterior of the rock.	micrometres to 100 micrometres in size; (c) Image showing large quartz grains (dark grey mineral) surrounded by voids; (c) Unpatinated part of the surface showing a fracture, over 1 mm long, that penetrates the weathered rind."		
51	4.2.4.2	Page 57, Figure 4.2-3: Some brief explanation if required in the caption to explain the point of including this figure, other than it compares two quite unengaging views down a reflecting petrological microscope. What is the point? Is it that the two views of the same rock appear the same?! Are the multitudes of fine dark areas on the Group C sample, less pronounced on the group A sample, voids and porosity? It does appear that a large, roughly circular area of Group A may indicate exposure of the rock to an alternative procedure?	Caption clarified and now reads "A comparison of photomicrographs of Group A thin-section of sample AQ03_GPH (upper) and the group C billet of the same sample (lower image). Upper image shows darker central area that is less well polished than the upper left and lower right areas. Lower image shows a more homogeneous polish. Both images RFL PPL. Abbreviations: RFL: reflected light; PPL: Plane polarised light."	OK	✓

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		Without some brief account of what is visible, the figure is meaningless and can be omitted.			
52	4.2.4.2	Page 58, Para 2 and onwards: I contend that the frequent use of "mode by area" is inappropriate and most unusual. The mineral "mode" is an estimation, normally as a result of point counting on a 2-dimensional petrographic thin-section, of the <u>volume</u> % of the mineral in the rock. Thus "by area" is assumed in the mode. If there is reason for the thin-section not to be representative of a mineral's overall abundance, such as in a flow-orientated igneous rock or where there are preferential planes of micaceous minerals, this can simply be stated after the best estimate of the volume % is given. To introduce "mode by area" is to require it to be used universally at the expense of just "the mode" or "modal percent". It is entirely	'mode by area' changed to 'mode' throughout. Glossary updated to clarify that mode by area is equivalent to mode by volume except where a rock is strongly anisotropic.	✓	✓

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		superfluous, and potentially misleading.			
53	4.2.4.2	Page 58, Para 3, line 1: The sentence would be better as "The rock texture ranges from equigranular to porphyritic with highly variable microstructure within the fine-grained groundmass. (i.e. porphytic rocks have a fine-grained <u>groundmass</u> , whereas equigranular rocks do not). Note: an "equigranular " rock with fine-grain size would be termed aphyric or aphanitic.	Text changes as suggested.	✓	✓
54	4.2.4.2	Page 58, Para 3, line 10: The sentence "The radiating texture is similar to....." is a little perverse and possibly suggests the author is most familiar with high-T basalts. One doesn't have to invoke comparison with the spherulitic structures in basalt, to account for the fine devitrification mineral intergrowths that are common and characteristic of granophyres and generally termed granophyric texture.	Sentence "The radiating texture is similar to the spherulitic structures that are common in basalt." deleted as suggested. New text reads "These fine radiating mineral intergrowths are common and characteristic within granophyres and are interpreted as quench devitrification features that formed after rapid magma cooling"	✓ great	✓

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55	4.2.4.2	Page 58, Para 5, line 1: re. "of both types"? I assume you mean those having predominantly radiating or acicular structures within the fine matrix? You should elaborate on the two types, so I suggest you could say "the granophyre samples are commonly porphyritic displaying subhedral to anhedral albite phenocrysts (<500 µm) within a" Note 500 µm is <u>not large</u> even for a <u>granophyre</u> and might elsewhere be termed microphenocrysts.	Text modified along the suggested lines to read "Granophyre samples commonly show porphyritic textures defined by subhedral to anhedral albite phenocrysts (<500 µm), within a finer-grained matrix"	✓	✓
56	4.2.4.2	Page 58, Para 6, line 2: "These features" ... would appear to refer to the large unaltered quartz 'grains' lying between the patina and weathered rind. If "large", (you should state the size) the unaltered quartz crystals (rather than grains) are most likely residual after the weathering of the rock. I assume you mean the presence of secondary hydrothermal minerals epidote and chlorite. The	Text modified...Quartz is the least altered mineral. For example, in sample EA01_GPH, a cluster of quartz grains, up to 2 mm across, which are much larger than those found in typical fresher granophyre, weathered rind, or patina, occur at the surface of the sample. EA01_GPH. These quartz grains penetrate the matrix, are unstrained, and show abundant fluid	OK May read better by a change to the text in yellow, and omitting that in red.	✓ Change made as requested.

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		presence of quartz crystals between the patina and weathered rind, if primary (i.e. not wind-blown onto an accreting surface patina) are surely a surface weathering effect unrelated to any earlier hydrothermal event giving rise to fine quartz veins. It is usually possible to distinguish primary magmatic quartz from hydrothermal and low-T secondary quartz by difference in internal mineral appearance (such as crystalline strain, zonation, inclusions, etc.)	inclusion trails. Below the rock surface, the grain boundaries are relatively planar. Based on these characteristics, the cluster of quartz grains is interpreted as part of a quartz vein that intersects the current sample surface or as an microxenolith of granite. Such features would have formed prior to weathering. Embayments within the grain margins at or close to the surface of the rock are attributed to the effects of weathering, but these effects are much less than the pervasive weathering-related alteration that affects other minerals.		
57	4.2.4.2	Page 58, Para 6, line 4: "host" is unusual terminology for veins and, particularly, fractures - since these features are imposed upon the rock rather than being a primary component. Suggest you instead use "are cut by" or "display".	Changed as suggested "Other samples are cut by veins."	✓	✓
58	4.2	General comment on the petrographic description in	At the start of the	ü	✓

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		Section 4.2: Although the Murujuga rocks exhibit a degree of secondary alteration normal in 'fresher' rocks, one is essentially describing primary igneous textures. It would be useful to make clear wherever possible that both chlorite and epidote are secondary in nature, arising from alteration or former high-T igneous minerals during metasomatic or hydrothermal processes or a subsequent low-T metamorphic imprint.	granophyre section, added "Of the major rock-forming minerals, quartz is considered primary in that it would have formed from the magma during crystallisation. Feldspars would have formed from the magma but their composition and morphology was variably altered by the hydrothermal alteration. Chlorite, stilpnomelane, epidote, and, in most cases, the amphiboles, would have formed during hydrothermal alteration and are referred to as secondary." Similar paragraphs were added for the other rock types and chlorite and epidote are referred to as secondary throughout. I'm not sure this is all that useful given that there is no primary chlorite or epidote, so referring them to as secondary repeatedly seems to add words without	Sure you can. I merely advise 😊. I agree that given the revised paragraph (above), further use of secondary with regard to chlorite and epidote is unnecessary.	Instances of secondary removed other than those defining chlorite, epidote, stilpnomelane, and calcic amphiboles as secondary.

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			adding meaning. So I'm happy to remove if I can.		
59	4.2.4.2	Page 63, Para 1, line 1: again, "host" could be changed to "contain". line 4: microfractures could also be the result of blasting, as commonly seen in mined rocks, if the railway cutting was indeed blasted.	Changed to "contain" Altered as suggested "Other fractures do not contain minerals and are thought to have formed by mechanical stresses during blasting to form the railway cutting, or during sample preparation."	✓ ✓ excellent	✓ Thank you
60	4.2.4.2	Page 63, Para 2, line 4: [syntax]: add "in" before "the Monitoring Studies Report".	"in" added	✓	✓
61	4.2.4.2	Page 64, Figure 4.2-9: Some additional brief description should be added to the caption, or preferably some annotation made on the photomicrograph panels, to address the features - e.g. filled by secondary quartz; lined or filled by chlorite; unfilled fracture (resin).	Caption modified to read "Photomicrographs of railway cutting samples showing fractures and veins. (a, b) RT02_GPH. Fractures filled with iron oxides (dark and orange lines) or quartz (colourless fracture in (b)); (c) RT04_GPH showing opaque minerals within the sinuous fracture; (d) MB02_GPH showing a fracture that does not contain minerals. All transmitted plane polarised light"	✓	✓
62	4.2.4.2	Page 64, Figure 4.2-10: In	Arrow to patina shown and	✓	✓

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		the excellent photomicrographs (a) and (b), can the division between patina and weathered rind be distinguished? I imagine the patina is merely a very thin surface feature and the remainder is the weathered rind. If this is the case, a simple annotation (arrow) of the patina would be useful. If not, and the boundary between patina and weathered rind is elsewhere, an annotated line/trace should be added.	caption updated		
63	4.2.4.2	Page 66, Para 1, line 1: Reference is made to Figure 4.2-11 regarding chlorite, amphibole, or clay alteration but there is no annotation to show these? Rather plagioclase, not mentioned in the text, is shown! "Clinopyroxene" in the text is annotated as "Augite" on the figure: the non-geologist/mineralogist reader may well not recognise them as the same thing.	Chlorite, amphibole, clay, annotated on the image. Augite changed to clinopyroxene and text checked for augite throughout. Figure referred to where plagioclase is mentioned in the text.	✓ ✓ ✓	✓ ✓ ✓
64	4.2.4.2	Page 66, Para 1, line 2: You refer to Figure 4.2-12	Zoning shown with arrow, and Fe-Ti oxides are	✓	✓

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		commenting that "clinopyroxene adjacent to chlorite shows chemical zonation". However, this not mentioned in the figure's caption and there is no annotation to indicate chlorite or where chemical zonation is to be seen. It looks like it may be present in the two phenocrysts in (a). Such chemical or mineral zonation is a common feature in pyroxene phenocrysts in dolerite and basalt and is a primary igneous growth feature. The concentric zonation forms as T (and or P) falls during magmatic cooling and the pyroxene solid-solution changes to maintain equilibrium. The bright rims, as indicated by the yellow arrow, are reaction rims. These result from chemical disequilibrium most commonly occurring through hydrothermal or metasomatic reaction caused by late-stage disequilibrium between the	labelled. Origin of zoning text added (after paraphrasing).	✓ explanation is perfectly OK. However note a typo in line 2682 in Comparison report: "it's" should be changed to "its"	Thank you Typo fixed

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		phenocrysts and liquid. Similar rims may form as a result of low-T weathering as pyroxene breaks down. The environment of formation of reaction rims may be discernible by the type/composition of Fe oxides present.			
65	4.2.4.2	Page 66, Para 2, line 5: The mineral listing for the granite is confusing with respect to plagioclase. I believe I commented similarly in the previous Monitoring Report. I wonder if the "plagioclase" is as reported by the TIMA software? Albite is rightfully reported being the sodic end-member of the plagioclase solid-solution. Andesine, a second member of the plagioclase solid solution series is reported to be a minor mineral in the granite. However, you also report "plagioclase" as a major constituent of the granite. Since you recognise three separate plagioclase minerals, what is this third, abundant "plagioclase"?	Good point – the illogical list relates to the TIMA output. Andesine should be included with the non-sodic plagioclase and has been deleted as a minor mineral.	✓	✓
66	4.2.4.2	Page 66, Para 2, line 8:	Caption altered to state	OK	✓

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		Referring to Figure 4.2.13, you state the presence of microcline, yet in the figure caption you make no mention of microcline stating only "feldspar"? Given that plagioclase is a feldspar, I presume you mean "alkali feldspar" which encompasses the cross-hatched form, microcline? The use of different mineral terminology in the text and in figures, as I have commented upon previously with regard to pyroxenes, is unfortunate.	"Feldspar grains within the weathered rind show extensive sericite alteration and clay formation and it is unclear whether they originally formed as K-feldspar or plagioclase". Text altered to link K-feldspar and microcline "The K-feldspar shows cross-hatched twinning characteristic of the microcline form of K-feldspar."	✓ You could omit the text highlighted here in red. "form" could more accurately be changed to "polymorph"	Changed as suggested. "form" has been changed to "polymorph"
67	4.2.4.2	Page 68, Figure 4.2.13, as above: The plate should be annotated to indicate "smaller crystals of albite and microcline".	Albite and microcline are not individually recognisable on this relatively low magnification image. This information has been added to the caption.	✓ noted	✓
68	4.2.4.2	Page 69, Para 1, line 3: Replace "Murujuga gabbros" with "Murujuga gabbro samples" line 6: irrational use of the term "mode by area", as earlier.	Corrected Fixed as per comment 52.	✓ ✓	✓ ✓
69	4.2.4.3	Page 69, Para 2, line 7: The sentence beginning "Some grain boundaries are" is	Altered to state "Some grain boundaries show more	✓	✓

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		of little meaning. Having stated that clay forms by transition from fresher rock minerals, such as feldspar and pyroxene, it would be extraordinary if the clay was not heterogeneous.	intense colours, highlighting the highly heterogeneous nature of the fresher rock to clay transition."		
70	4.2.4.3	Page 69, Para 3, line 2: What is "altered chlorite" as a mineral? Chloritoid? Clay?	Clarifying sentence added "The term weathering-altered chlorite, or altered chlorite, refers to secondary chlorite that has partially altered to clay minerals during weathering. Altered chlorite shows a grain shape that is still typical of chlorite, and some areas show the characteristic anomalous birefringence. However, the composition is non-stoichiometric, reflecting weathering-related changes in chlorite composition.	OK	✓
71	4.2.4.3	Page 70, Figure 4.2-14: The caption talks of clinopyroxene which is annotated on Plate (c) as augite. Also, augite is altered to chlorite and plagioclase to sericite, so what exactly is "altered clinopyroxene" and	Caption simplified "Clinopyroxene and plagioclase are replaced by chlorite and epidote. Sericite is not visible on this image. Labels modified to be consistent. Altered	✓ OK	✓ ✓

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		"altered labradoritic plagioclase"?	clinopyroxene is retained as a term because grains of the alteration minerals are small and overprinted by Fe-Ti oxides, so they cannot be labelled individually.		
72	4.2.4.3	Page 71, Table 4.2-1: Suggest the caption would read better as "Estimated mineral volumes for granophyre samples from Group A as determined from modes measured by TIMA". In the table, Fe clay/Patina requires definition as it is present in the fresher rock. Also, what is Fe-poor_Patina? "AL-P_clay" should be "Al-P_clay"? Correct the spelling "clynopyroxene". What is the reasoning of including Ankerite to Pyrite in the mineral listing when none exists?	For this table and equivalents, caption changed to "Summary of mineral modes for Group A granophyre samples estimated by TIMA" Section on TIMA classification inserted into the 'general comments' section. The TIMA uses a mineral characterisation library to estimate the modes of the different minerals within each sample, based on the elemental composition of each analysed pixel. Many mineral groups, such as plagioclase and chlorite, show variable compositions and these can be split, or grouped, by the user. For this report, stoichiometric	Fine ✓ Note: "sample" should be changed to "samples" - this typo persists in the Comparison report.	✓ Typo corrected (sample to samples)

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			chlorite, amphibole, and clinopyroxene of all compositions are grouped and referred to as chlorite, amphibole, and clinopyroxene, respectively. An exception is K-altered chlorite, which refers to mineral that is most consistent with chlorite, but shows a non-zero K content, consistent with the presence of micrometre-scale illite alteration below the spatial resolution of the TIMA. Plagioclase is split into a sodic end-member referred to as albite, and a group referred to as plagioclase that includes all of the more calcic compositions (andesine, labradorite). The clay minerals are commonly fine-grained and show highly variable compositions, so a number of classifications were set up to capture Fe-rich clays, which are common in the patina but may also occur within the weathered rind (Fe-	✓ noted ✓	✓ ✓

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			Clay/Patina). Iron-poor clays, which are present in the patina, are classified as Fe poor_Patina. Similarly, Mn-Patina and Fe-Ca-K_Clay classifications were set up to capture Mn-rich material within the patina and Fe-, Ca-, and K-bearing clays. In some samples, a P and Al association is sufficiently common that a classification for "P-Al concretions" was set up to capture this material, which may relate to interaction between the patina and weathered rind, and bird excrement. These clay and patina classifications do not refer to a single mineral, but rather to a range of composition that characterises different regions within the patina and the highly altered parts of the weathered rind. Spelling of clinopyroxene fixed.	✓ ✓ ? Is this simply information ?? Not sure to where/what this refers?	✓ This is in response to the comment about ankerite and pyrite. The text around 'high mode of unclassified' was included in error. Apologies.

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			AL-P clay changed to P-Al concretions Modes less than 0.01% removed from the tables and note provided in the caption. These low modes do not provide meaningful information. The high mode of unclassified for the Group A samples		
73	4.2.4.3	Page 71, Table 4.2-1: Further to the comment above, what is the significance of the category [Unclassified] being in brackets when it is not in the subsequent tables 4.2-2 to 4.2-4. An obvious observation that is not mentioned is that the modal percent of the [Unclassified] is considerable (unlike in the other tables) and is the 4th largest component of the rock!	Brackets removed. It is interesting that 'unclassified' is so high for the Group A granophyres. This will be addressed as part of the reprocess of all the TIMA data.	✓ OK noted	✓ ✓
74	4.2.4.3	Page 72, Table 4.2-2: Similar comment as above	Caption updated as per comment 72.	✓	✓

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		on improvement of caption. Anhydrite, Barite, and Chalcopyrite are listed despite there being none. Could it be that traces of <0.01 % were recorded? If so, this should be stated in the caption.			
75	4.2.4.3	Page 73, Table 4.2-3 and previous similar tables: I believe some clarification of the mineral terminology (structure of individual terms) in these tables is needed. e.g. What is the distinction between say "Fe-Ca-K_Clay" and "Fe-Clay"; what is meant by "Fe-Clay/Patina" with a backslash in contrast with "Mn-Patina"?	Mineral classifications and terminology clarified in the 'General Comments' section of section 4.2. See comment 72 for added text.	✓	✓
76	4.2.4.3	Page 74, Figure 4.2-4: The inclusion of this figure and wording in the caption is highly questionable. As expected of rock collected in a modern railway cutting, there is NO weathering rind or patina. However, the table purports to show its modal composition. Low and	Excellent point. Right hand two columns removed.	✓	✓

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		behold, it has the same composition as the fresher rock! The caption seems designed to further the illusion by stating the "the zone referred to as weathered rind + patina may include some fresher rock". Too right it does, approximately 100%! All I can guess is that someone had the task of performing modal analysis on granophyre of each rock type, and there being none for the Group D sample, analysed the recently exposed exterior. Other than out of a misguided quest for completeness, I see no reason to include the two right-hand columns in this table, or at least remove the heading "Weathered rind + Patina". Whatever is decided, clearly the caption needs to be re-written to account for the nature of a Group D rock.			
77	4.2.4.3	Page 75, Para 1, line 1: What is meant by Fe-clay/patina?	See response to item 72	✓	✓

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78	4.2.4.3	Page 75, Para 1, line 4: Change "and shows irregular boundaries" to "is anhedral". Does it show evidence of magmatic corrosion or is it partially altered at low-T?	Changed as suggested. Sentence added to address Ron's question "Irregular margins on clinopyroxene may reflect resorption under magmatic conditions, alteration during hydrothermal alteration, or modification by weathering. Clinopyroxene within the fresher rock is commonly subhedral to anhedral, consistent with some pre-weathering modification. However, clinopyroxene within the weathered rind is substantially more embayed and irregular in shape than that within the fresher rock, consistent with substantial weathering-related grain shape modification."	✓ ✓ very good	✓ Thank you
79	4.2.4.3	Page 76, Para 1, line 3: After ... "the railway cutting" add "where the weathering is absent". line 6: [syntax] add missing bracket before "Figure 4.2-15)".	Added "'where the weathering is absent". Missing bracket not found. Likely an issue in previous word conversion workflow for documents. We now	✓ OK understood	✓ ✓

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			have agreed an improved workflow which should avoid these errors.		
80	4.2.4.3	Page 76, Para 1, line 9: "fill gaps between" is not an expression normally employed in petrographic description and hints at an improbable mode of formation. The clays have replaced fine matrix or have grown at the expensive of former crystalline minerals.	Text modified to "and form massive replacive aggregates or pseudomorphic replacement of matrix minerals, phenocrysts, and secondary hydrothermal minerals."	OK	✓
81	4.2.4.3	Page 76, Table 4.2-6 and also subsequent Table 4.2-7: What is meant by the category "The rest"? This is hardly a formal term and has no place in a scientific report. How does it differ from "Unclassified" ?	The rest refers to mineral with very small modes that weren't included in the tables. These small proportions have been added to 'unclassified' for simplicity.	✓	✓
82	4.2.4.3	Page 77, Figure 4.2-15: As with previous similar figures, the plates cry out for some annotation and/or more detailed description in the caption. e.g. in plate (a) how is one supposed to distinguish between quartz and microcline? Not everyone is familiar with BSE. It would also be useful to outline a crystal boundary	Caption modified to clarify which phase is which. "SEM-BSE images of granite sample D003_GRT. K-feldspar shows the highest BSE signal (palest grey). Quartz and albite show a similar BSE signal (darker grey than K-feldspar) but albite is typically more porous,	✓	✓

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		- are we looking at a 'large' crystal showing characteristic graphic texture (that looks to be the case in the far top-left and top-right of the view) or an overall fine intergrowth? In plate (c) similarly some annotation is required: e.g. indicate a crystal boundary? It is particularly necessary to indicate a void, or voids, since no external mounting medium is shown in the view. In plate (d) the bright specs are presumably Fe-oxides? Given the critical importance of porosity in the MRAMP, annotation and depiction of crystal boundaries and possibly enlargement would be most useful.	altered, shows a slightly higher BSE signal, and is typically less well polished than quartz. (a) intergrown quartz and albite (darker grey) and K-feldspar (lighter grey). (b) weathered rind characterized by formation of voids within albite (darker grey) and K-feldspar (lighter grey). (c, d) voids formed within albite (darker grey) and K-feldspar (lighter grey) create extensive microporosity.” Without going back to the SEM and finding the patch I can't 100% guarantee getting quartz and albite the right way around so I've put the general criteria in the caption. I've highlighted some voids on (c) and labelled Fe-Ti oxides on (d).	✓ understood ✓	✓ ✓

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83	4.2.4.3	Page 78, Para 1, line 1: What do you mean by "topography". Figure 4.2-14 that is referenced, shows photomicrographs from a standard petrographic thin-section that is more or less of uniform 30 microns thickness. Any apparent variation in the apparent elevation of the mineral surfaces is termed "relief" and is principally due to differences in refractive index (RI).	Clarified - The weathered rind of sample DO02_GBR shows a strong relief, where relief refers to the difference in elevation between the crests and troughs that define the surface of the patina on the thin section.	? From this response, I believe I was mistaken in querying your use of "topography". As it was used in description of a petrographic thin section, I thought you were referring to the apparent visual topography created when observing a thin section by variations in the refractive index (RI) of different minerals. This visual effect is termed (optical) relief. Variation in the height of a surface, such as in a landscape, is, of course, termed topography and also (physical) relief. I believe in the original script you may have been referring to this physical feature as shown by the margin of the rock in thin section. If this was the case, there is no need to change the text, save that "topography" could be replaced by relief, as done in your response, or by "roughness". A suitable text might read: " "The weathered rind of	Change made as suggested and sentence split for clarity "The weathered rind of sample DO02_GBR, where cut by the plane of the thin section, shows a strong relief. In this context, relief refers to differences in the height of the crests and troughs that define the surface of the patina on the thin section."

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				sample DO02_GBR shows a strong relief, where cut by the plane of the thin section."	
84	4.2.4.3	Page 78, Para 3, line 10: [syntax] add "are" after clinopyroxenes.	Added	✓	✓
85	4.2.4.3	Page 79, Figure 4.2-16: In line 4 of the caption "dissolution"?? Omit. In line 5 of the caption I suggest change of "on exsolution lamellae" to "along planes of exsolution". i.e. on the boundaries of exsolution lamellae, as is usual, rather than "on" (within) the lamellae.	Dissolution omitted. Change on line 5 implemented as suggested.	✓ ✓	✓ ✓
86	4.2.4.4	Page 80, Para 1, line 1: I suggest changing the first sentence to "Patina refers to a thin, distinctly coloured, layer that forms..... ", since patina is more than simply a discolouration.	Change implemented as suggested.	✓	✓
87	4.2.4.4	Page 80, Para 1, line 3: "black specks" is preferable to "black dots".	Change implemented as suggested	✓	✓

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88	4.2.4.4	Page 80, Para 2, line 1: syntax]: The sentence may read better as "Patina is well-developed in eight of the 16 granophyre samples in Group B (....."	Change implemented as suggested	✓	✓
89	4.2.4.4	Page 81, Para 1: Finally, the meaning of the terms such as Fe-patina and Mn-clay are explained. This should have been done earlier where these terms first appear.	See response to comment 72. Text here left as a reminder and summary.	OK	✓
90	4.2.4.4	Page 81, Para 3, line 1: [syntax] Sentence reads better as "On the dolerite, patina is thin or is locally absent."	Change implemented as suggested	✓	✓
91	4.2.4.5	Page 81, Para 5, lines 1 and 2: Suggest you move the bracketed list of samples to after the second sentence. Thus "and one granite (BU02_GPH,).	Change implemented as suggested	✓ noted	✓
92	4.2.4.5	Page 82, Figure 4.2-17. As the source of the rock samples of group D has been described, the photomicrograph can show NO patina or weathered rind as these are generally defined. Neither could have these effectively developed	There is a dark line on the surface of (b) that may represent the ochreous surfaces. These ochreous surfaces may have developed on fractures deep in the weathering profile, but regardless of when or how	Understood – see suggestion below. “..... and absent or almost absent patina” might be changed to “....but presence	I’m reluctant to add ochreous, which hasn’t been defined or explained, or to provide an interpretation without substantiation, but I have modified to “no weathered rind and thin discontinuous orange patina”, which I hope covers

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		in the short (geologically) period of 60 years.	they formed, they are there, so I've changed the caption to "no weathered rind and absent or almost absent patina"	of a modern ochreous surface"	all bases.
93	4.2.4.5	Pages 83 - 87: Figures 4.2.18 - 4.2.22. The point escapes me for including these five scans. The captions purport them to show micro-CT data but what data? None are evident and the captions give absolutely no clue as to the utility of these five figures or any reasoning as to why they are presented. The fact there are five, suggests some comparisons are to be made? Unless the significance of the figures can be illuminated by additions to the captions, I suggest they be omitted from the report.	I agree that the value is relatively low but they do show the different grain sizes and I've added some words to that effect. "These preliminary results are promising; the images show the differences in grain size in the different rock types, with finer grains within the granophyres, coarser grains within the gabbro, and potentially a single grain within the granite. Analysis of the micro-CT scans is ongoing and will be presented in the final monitoring studies report." I have also added this info to the captions with the words "In panels (b) to (d), individual grains are revealed by changes in the grey scale of the image." except for the granite, where the text now reads "In	I recognise your belief that analysis of micro-CT scans may be valuable. The changes made are all good.	Thank you

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			panels (b) to (d), individual grains are not visible, possibly because the sample consisted of a single grain."		
94	4.2.4.6	Page 88, Table 4.2-9: Given the results in the table are in mg/L it is evident that the determination of the three analytes was performed on solutions. The method, HPIC? should be stated in the caption. As NO details of how the solutions were derived from the rock surfaces have hitherto been provided in the text, or in the table's caption, the values for analyte concentration and detection limits is meaningless, in terms of the amount of each analyte on the rock.	See response to comment 46 where the method of collection is provided in more detail. This method is referred to in the text. Method stated in the caption.	✓ noted ✓ ✓	✓ ✓ ✓
95	4.2.5.1	Page 89, Para 1, line 4: [syntax] alter text to "next eight months" as the convention in this and the previous report.	Changed 8 to "eight"	✓	✓
96	4.2.5.1	Page 89, Para 4: Pt 2: The information here is questionable and the evidence base for the suggested hypothesis is	There are a lot of statements here, some of which are not consistent with our observations at	Your response is fully understood and accepted. My statements were based more on general field outcrops of these rock types,	Taken as a comment

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		counter intuitive. Firstly, on an outcrop scale, granites more characteristically exhibit exfoliation while being formed predominantly of anhydrous minerals quartz and felspar (admittedly accompanied by smaller amounts of mica and amphibole). A major factor in exfoliation structure is the uniformly massive nature of granite (and granophyre) and lack of fracturing, relative to the more jointed nature of basalt and dolerite. The more fissured nature of basalts frequently leads to spheroidal weathering which is akin to larger scale exfoliation. Secondly, weathering is an on-going continuous process that prior to human intervention would have been at a reasonably steady rate. If this is the case, one could expect the present basalt/dolerite surfaces to show evidence today of more exfoliation fractures	Murujuga over multiple field seasons. For example, we don't see that granites show more exfoliation than the other rock types, based on observations of flaking. In addition, the granophyres are commonly strongly jointed. The dolerites tend not to crop out over large areas, except at Watering Cove, but the level of jointing there is not noticeably greater than that of the other rock types. Additionally, the patina on granophyre is often thicker than that on gabbro but the gabbro patina can look thicker because of the undulating surface. Spalling and exfoliation may affect the patina only, and the patina and weathered rind. This is an important distinction that needs further observation to characterise at Murujuga. Finally, the idea that rate of weathering	not specifically on the exposures at Murujuga which I have observed only over a matter of days. Further, my understanding of relative patina development (thickness) may be biased by having spent more time at 'classic' rock art and rock type exposures as considered by local specialists. Your adjustment to the text (point 2) is valuable.	Thank you

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		and more enhanced weathering rinds than the gabbro which is not the case. i.e. one cannot arrest the weathering process so that amazingly all evidence of chemical weathering (the weathered rind) and physical breakdown (flaking, spalling) is conveniently removed. Thirdly, compositionally and mineralogically dolerite and gabbro are equivalent, different only in grain-size and degree of crystallinity (basalt, but less so dolerite, commonly contains a fine-grained mesostasis after original glass). One could argue that the uniformly finer-grained nature of dolerite and basalt make them less pervious to water and atmosphere and therefore the weathered rind is less well developed. From my modest field experience of Murujuga rock exposures, I think it is apparent that gabbro	is inversely related to grain size is contrary to the majority of studies, where mineral dissolution is proportional to surface area and surface area increases as grain size decreases. Given these findings, the logic that Ron presents is difficult to reconcile with the observations. However, I accept that the causal relationships are likely more complex than presented in point 2 and I've adjusted the text to reflect this. Point 2 now reads: “Dolerite, granite, and basalt typically show thin or absent patina, whereas gabbro and granophyre typically show well-developed patina. Patina is thought to be formed from externally derived material at rates that are not thought, currently, to be rock type dependent (Dorn, 2024). If formation rates do not depend on the		

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		exhibits the greatest (thickest) development of the weathered rind, which is significantly thinner than that generally developed on basalt, dolerite and granophyre. Unlike, dolerite, gabbro contains significantly greater amounts of hydrated and secondary minerals, that the gabbro, suggesting that mineralogical and chemical considerations control the rate of weathering rind development, without inciting exfoliation as a specific critical mechanism.	rock type then differences in patina thickness record differences in the frequency of flaking, spalling, or other patina-loss that relate to the characteristics of the underlying material. Such characteristics may include, but are not limited to: the rate of dissolution of minerals in the underlying rock; lithology-dependent strength or thermal property differences between the weathered rind, patina, and fresher rock; grain size or morphological differences; lithology-dependent changes in volume as weathered rind forms from fresher rock; microbial community-dependent differences in weathering rind formation; and pre-existing joint and fissure intensity and geometry. Further work is necessary to better understand the relative importance of these factors."		
97	4.2.5.1	Page 90, Figure 4.2-23:	Change implemented	✓	✓

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		State in the caption the lithology shown in the photograph, presumably granophyre.			
98	4.2.5.1	Page 91, Para 4, line 10: Give reference for the porosity measurement workflow.	Year 2 report cited.	✓	✓
99	4.2.5.1	Page 91, Para 5, line 5: To prevent the small chance of ambiguity, you could change "the detector switch" to "change of detector".	Done as suggested.	✓	
100	4.2.5.1	Page 92, Figure 4.2-24: The most immediately striking difference between the two plates is the overall colour from violet/blue (Group A) to purple/red (Group C). The key text is rather small and the equally small colour boxes make differentiation between similar colours difficult. Although different detectors were used for each plate, as far as I can discern, the colours for individual minerals in the key are consistent between the two plates. If this is the case, what might explain the different overall colouration, given that the rock billets	This image was selected to illustrate the systematic differences found in estimated modes that would reasonably be expected to be the same. This has been clarified in the caption to read " Group A analysis performed with pulse-TOR detector and mineral characterisation library optimised for this detector. Group C analysis performed with EDAX detector and mineral characterisation library optimised for this detector. The striking change in colour, from blue at the top to purple at the	✓ Understood. Text to clarify is Excellent.	Thank you

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		were cut adjacently from the same rock? This could usefully be explained in the caption. The texture and mineral relationships are well seen. The different order in which the minerals are listed in each key is unfortunate when eyeing the colours in the small boxes? Why is this? Presumably it is not the order of abundance, since that should be equivalent in each plate.	bottom, reflects a difference in the calculated modes of quartz and alkali feldspar.” The point is also made more explicit in the text “For example, sample AQ03 from Group A shows a predominantly blue colour in the TIMA map (Figure 4.2-24, upper panel), whereas sample AQ03 from Group C shows a predominantly pink/purple colour in the TIMA map (Figure 4.2-24, lower panel). Blue and pink/purple represent quartz and K-feldspar, respectively, so the images imply differences in the mineral modes that is implausible given that the polished thin section (Group A) and polished billet (Group C) were made from the same rock. Causes of the difference were investigated and two possible causes were identified.” It's unfortunate that this	OK Whole-heartedly agree both with regards to your openness and the pitfalls of over-reliance on automated techniques. ✓	✓ Thank you ✓

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			finding doesn't reflect particularly well on us. However, I think it is important to have it out there as an illustration of the potential pitfalls of automated mineralogy. Legend redone to show the most common phases at a better font size and in a consistent order.		
101	4.2.5.1	Pages 93 - 96, Figures 4.2-25 - 4.2.28: I viewed these figures for some time questioning why I was having difficulty is interpreting them. I subsequently realised that my first instinct in questioning the differences in mineralogy between the four rock groups was to compare the heights of the columns in each set. This was my error as the differences in modal mineralogy of the rocks is to be found in comparing heights of columns of the same colour between each set of columns. The design	Changes made as suggested, except that x-axis label changed to 'minerals' because they aren't modes on the x-axis.	✓ and, of course, you are correct in using "minerals"	Thank you

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		of these figures is clearly optimised to show difference in the amounts of each mineral in each rock of each group A - D. I have revisited the figures a number of times and still have difficulty with them, but am willing to put this down to my intellectual frailty, as the figures are in each case clearly presented. I would be interested to know if other reviewers or readers share my difficulty. Whether this is so, or not, I believe modifications could be made to clarify what the histograms depict. Such modifications entail: 1. move the colour key for the rocks from below the x-axis to the main area of the diagram (upper right) and make it larger; label the X-axis as "mineral modes"; and title each figure in the captions as "Variations in volume % of individual minerals between the four rock sample groups".			
102	4.2.5.1	Page 97, Figure 4.2-29: The small difference in	Difference in size of upper two graphs corrected.		

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		dimensions between the upper two graphs could be corrected. The significantly different scales and limits of scale between the two lower graphs is far more detrimental to their interpretation. Whereas technical reasons for differences in the modes between the group A and group C are given in the text, nothing is stated in the caption in support of these or other explanations for the differences. The graphs show some very interesting information deserving of interpretation, of which I can find none in the text or in the figure caption. Just one example is the appreciable difference in the modal % of quartz (+/- 8%) in the fresher rock of the rock groups A and C. Isn't this worth accounting for in two rock samples purporting to be equivalent (adjacent sections). There is a lot of other comparison to be	Differences in lower graphs necessary to show the range clearly. Differences in modes mentioned briefly in the captions with the text "Differences may relate to changes in analytical techniques or preparation artefacts." Differences in modes addressed as part of answer to comment 101.	OK ✓ ✓ but I believe you mean comment 100?	✓ ✓ yes

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		made - surely the aim of the Figure - that goes begging, and throws light on the actual purpose and value of including the large number of figures in this section of the Report.			
103	4.2.5.1	Page 98, Figure 4.2-30: I have revisited this figure a number of times suspecting that I have missed the point somehow. Unfortunately, it remains a puzzle to me. It would be useful to state in the caption that the two views - optical photomicrograph and SEM-BSE image - are of precisely the same thin-section with no change such as re-polishing. It appears to me that <u>both</u> areas circled on the photomicrograph contain a very fine aggregation of crystals (the left most aggregate has the appearance of calcite, although this is not confidently recognised from the figure reproduction). The caption tells that the area depicted in the right most	Caption rewritten to clarify "Comparison of optical photomicrograph (transmitted plane polarised light) (left) with SEM-BSE image (right) of sample BM03 (group D). The two views are of the same area of the same thin section without repolishing or other treatment between the time of acquisition of the two images. The circled area on the left of each image is green in the plane polarised light image (left panel) and consists of chlorite. However, in the SEM-BSE image (right panel), this area appears black and would be identified, incorrectly, as porosity by the porosity measurement algorithm. The circled area on the right	✓ all good	✓

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		circle would however be identified as a void from the SEM-BSE image. Lines 3 - 6 of the caption seem to state both that the area in the right most circle represents a poorly-polished area "rather than a void" and that the same area "does not contain a mineral" and is a void? Unless I am going slightly mad, there is something amiss here. Given the critical importance of porosity to the stated aim of MRAMP, I suggest the caption be rewritten carefully and most clearly.	hand side of each image is colourless in plane polarised light and black in cross polarised light (not shown). This area does not contain a mineral. It appears black in the SEM-BSE image (right hand panel) and would be identified correctly as porosity."		
104	4.2.5.1	Page 98, Paras 1 - 5 with reference to Figures 4.2-31 - 4.2-35: Reconciliation of the statements made in paragraphs 1 - 5 with the five figures is extremely difficult and is rendered particularly so by the like-appearance of the figures and by the absence of any guidance in the brief and uninformative captions. The reader is left to switch pages back and forth multiple times and, in my case, scribbling	• Captions of Figures 4-2-21 to 4-2-35 modified to state whether the images relate to samples that show polishing artefacts or not. • Method of identification of polishing artefacts stated in the text "These differences motivated a close comparison of the Group A and C samples using optical and SEM-BSE imaging Dark areas	✓ ✓ ✓	✓ ✓ ✓

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		notes on each of the figures. How much anguish could be save if the captions stated the perceived character of the rocks included - e.g. "...of sample rock AQ06_GPH that shows no polishing artefacts ...". Given my poor opinion of this passage in the report, I will not go into great detail on the arguments made. I shall point out the following: - it would be good to be told how the relative degree of polishing and hence the quantity of polishing artefacts was determined, since there is potential for circular argument. I presume it was independent of the TIMA results and based upon optical inspection of the polished surface? - possibly mineral hardness plays a role, and not exclusively the phyllo-silicate structure of the minerals. - since one seeks to assess proportional	on the SEM-BSE images were located and examined using optical microscopy." - Role of mineral hardness mentioned in the strategies to improve the techniques: "Development of polishing protocols that minimise damage to sheet silicates such as chlorite and softer minerals, which may also be plucked or damaged by polishing." - The scales on the graphs are selected to show the mineral modes of the particular rock as clearly as possible. Qualitatively, if the dots are far apart then there is a problem and if they are close together then one is reassured. The intention is not to compare the extent of the artefacts. The data could be provided in table form but given that	✓ Perhaps this is worth mentioning in the caption? Check that it is not With a little more explanation of the figure, as above, a Table is probably unnecessary. Good – like minds! Thoughts how this might be rectified given the importance of porosity in the MRAMP findings. Probably too late to address in the last year of 'research'.	These words added to the caption of Figure 4.2.29 and 4.2.31 to 35 "Scales were selected to show the mineral modes of the specific rock as clearly as possible. If the dots are close or superimposed than the two different sample preparation methods provide near-identical results. If they are far apart then some kind of analytical or preparation-related artefact is inferred." Table not provided. Not too late to address. We remain optimistic that consistent results can be achieved.

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		(percent) differences between the rock samples, it is a further complexity to contend with the variable scales between individual graphs. - The sentiments expressed in paragraph 5 are entirely justified. The implications of the recently found problem relating to variations in polishing have not be satisfactorily explored in the current Report. That the porosity estimates made in the previous report may have been over-estimated to some degree is of concern, given the pivotal importance of porosity in discussion of the risk of industrial emissions to the rock art. - Clearly the reliance on instrumental measurements, such as TIMA, to measure porosity, and mineralogy, with the seeming relegation in	the aim is to identify a problem that can then be fixed we chose not to. This table can be include if requested. - Dot point 4 taken as a comment - Dot point 5 taken as a comment and I agree completely.		

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		importance of optical microscopy, can be problematical [as previously commented upon in Item].			
105	4.2.5.1	Page 105, Figure 4.2-36: The intent to make the locations of the five rocks is clear, but the use of the large stars is unfortunate. These over-sized symbols are not best chosen to indicate map locations. I can make out a small central star within each star but any geographic markers at that point are obscured. Is it an illusion, or what is the reason that a single square symbol (AQ-A1) is infilled? What is meant by "Grid Boundaries" on the key?	Large stars replaced by red fill to the existing squares. 'Grid Boundaries' removed from the key and replaced by 'Legend'. Legend modified so that the partly infilled blue square matches the MAC office legend item. Red squares added to the legend. Sixth sample added (see comment 106).	✓ all items positively addressed	Thank you
106	4.2.5.1	Page 106, Figure 4.2-37: There is no key to the symbols and no explanation in the caption of their significance. Once again, the choice of the over-sized stars and their positioning, is flawed. Since they appear	Plot redrawn as suggested and sample modified to clarify. New caption reads "lot of porosity values for the Group A granophyre samples from Monitoring Studies Report 2024 (Curtin University, 2024c). The	✓ It correctly reads "Plot .." Check	✓ The caption has been revised to remove "Plot of" as beginning of caption.

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		on the main body of the diagram, it is natural to think they represent individual points on the X-Y plot, yet I have discerned they are nothing but indicators of the five rock samples of interest. If I am correct, their positioning is quite bizarre, since they partially obscure some of the data points. While there are 6 samples indicated by the star symbols on the diagram, only 5 are shown on the previous location map - where sample RS03 is not allotted a star. In all, this is a poor diagram and should be redrawn, Use of the stars is unnecessary and distracting. One could, for instance, indicate the 6 samples alternatively by showing the sample number in bold font, with possibly also a bolder vertical line.	names of samples that clearly show polishing artefacts are shown in bold and a dashed line highlights the porosity values for these samples. The different symbols are used to provide a visual separation between the different values. There is no systematic relationship between the presence or absence of a polishing artefact and the absolute average porosity value." RS03 added to the map.	✓	✓
107	4.2.5.1	Page 106, Para 6: The points 1 and 2 are a precise duplication of those on Page 36 of the report. Please refer	See response to comment 34. Points 1 and 2 rewritten to		

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		to my earlier comments relating to point 1.			
108	4.2.5.1	Page 106, Para 1, Line 1: Something very wrong in this argument! The statement seems to oppose, not support, the 'hypothesis' of point 2. There is no decrease in pH so there is no evidence for rainfall to spur microbial growth leading to lowering of pH.	Rewritten to clarify "The analyses showed that the pH values neither increase or decrease after rain, so measurements presented in this report (Sections 3.2.4.4, 3.2.4.5, and 4.2.4.6) do not unequivocally support either hypothesis. While the lack of a pH increase is reasonably strong evidence that acidic salts deposited on the surface and underlying surface were not the main causes of pH values less than 6, the lack of a pH decrease is not strong evidence for an absence of microbial activity. This is because near-detection values of oxalate and formate, and the lack of a time-series for oxalate and formate sampling, limit the information available from these measurements. Further work, potentially with improved sampling protocols (e.g., longer equilibration	✓	✓

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			times, greater rock:water surface areas), is necessary to better support links between pH and microbial activity. Such work is beyond the scope of planned activities for year 4, but is recommended to form part of the ongoing research program."		
109	4.2.5.1	Page 106, Para 1, line 4: I wholeheartedly support the statement that further work on sampling protocols for the determination and understanding of rock surface pH and chemistry at Murujuga is essential.	Agreed. Taken as a comment.	✓	✓
110	4.2.5.1	Page 106, Para 3, line 3: [syntax]. The sentence may read better if the duplication of "flake(s)" is removed. Suggest " Weakening of the weathered rind increases the probability that patina, with or without underlying weathered rind, will flake or spall from the rock surface".	Change made as suggested.	✓	✓
111	4.2.5.1	Page 107, Para 4, 2nd dot point: [syntax] "An extended suite of samples has	Changed "have" to "has"	✓	✓
112	4.2.5.1	Page 108, Figure 4.2-38: the	The spalling fish is not		

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		photographs of rock flaking are impressive. It is, however, unfortunate that they are from the Dampier Salt lease, since the effects of salt-induced corrosion of the rock surfaces is an additional factor. While the whole of Murujuga is affected by marine aerosols and/or spray, a natural occurrence, the local presence of airborne particulate salt in the immediate vicinity of the salt ponds has been of concern. It would be useful to know how close to the ponds the photographed flaking rocks are? To avoid any unnecessary conjecture, it would be prudent to state in the caption the location of the photographs with respect to the salt ponds. Alternatively, photos of flaking distant from the salt workings could be sought - they do exist.	located particularly close to the evaporation ponds. This is clarified in the caption, which now reads "Flaking on non-rock art panels on the Dampier Salt Lease. (b) Flaking on rock art panel on the Dampier Salt Lease. Site of photographs is 650 m from the ocean and 1.8 km from the nearest salt pond. Flaking is also seen commonly elsewhere on Murujuga."	✓ fine	✓
	4.4				
113	4.4.1	Page 118, Para 2, line 1 "implemented"? meaning is not clear to me.	The paragraph has been amended to read: "The studies in point (b) will	✓	✓

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			include analysis of the same inorganic geochemical weathering products as in (a), in order to account for biotic/abiotic interactions (e.g. additional weathering due to organic acids)."		
114	4.4.1	Page 118, Para 3: The referenced document COPP21065-PRO-G-117 Murujuga Rock Art Monitoring Program could usefully be dated. It is not clear whether it includes the new protocol to eliminate the re-use of cut samples.	Methods documents contain a revision number and revision date, however this is not included in the citation as it is a live document and it is expected that the latest/current version is cited. The latest revision is Rev 3 on 02/06/2024. However the reuse of rock cubes (or otherwise) is not specified in the document. It will be updated in the next revision.	✓ understood	✓
115	4.4.3	Page 120, Para 1, line 1: [syntax] change "however" to "but"	Changed	✓	✓
5-Air quality monitoring network operation					
116	5.3	Page 124, Figure 5.3.1: Nice photo but to add knowledge of the "suite of instruments" some annotation on the photo or direction in the caption is required of the	Additional information has been provided in the caption to identify visible instruments.	✓	✓

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		actual instruments that are visible.			
117	5.4	page 127, Figure 5.4-1: As above, the figure could be made more relevant by some annotation of the apparatus and instruments visible on the roof.	Additional information has been provided in the caption to identify visible instruments.	✓	✓
118	5.6	Page 129, Para 1, line 2: This is an example of the uncontrolled use of the word data (See also previous comment (Item ##)). Data are simply items of information. In this report they are most commonly "results", and this word is better constrained and preferable. Here, "The data of the field deposition samplers" could just as easily be their dimensions, manufacturer's name, colour, age, etc. What is meant appears to be results of quality and composition of the material collected.	Terminology updated to use the term results in place of data.	✓	✓
119	5.6	Page 129, Para 1, line 3: I believe "3-bucket deposition samples" should correctly read "3-bucket deposition samplers" ?	Changed to "3-bucket deposition samplers"	✓	✓

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6-Spatial mapping and surface modelling					
120	6.1	Page 133, Para 1, dot point 1: Sentence does not read correctly. Change "incorporation" to "incorporating" ?	Changed to "incorporating a region"	✓	✓
121	6.2	Page 133, Para 4			
7-Statistical analysis of data from each component study					
	7.1				
122	7.1	Page 136, Para 1, line 2: Use of "dose-response" relationship necessitates that the reader is told what the experiment was and how the response was arrived at. True, reference to the 2025 Monitoring Report, is made, but a minimum of explanation is required to allow the figures to be comprehensible in the context of the current report.	We have reworded the opening paragraphs of this section so that there is more information on the experiment included. New text: "Detailed background information on the chamber studies is provided in the Monitoring Studies Report 2024 (Curtin University, 2024c). In brief, rock cube exposures are conducted for a six-day incubation period, with air pollution (either from hydrocarbon combustion or bottled gases) actively introduced at the commencement of the exposure on day one for one hour and then (if repeated) at 24-hour intervals for up to	✓ noted ✓ all good	✓ ✓

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			six days. After the active exposure period, the concentration inside the chamber is progressively reduced over the subsequent 23 hours, due to chemical reaction or diffusion in the chamber. For all exposure scenarios, incubation and UV light exposure are continued for six days to permit secondary reactions to occur. Rock cubes are rinsed in a defined quantity of ultra-pure water, then given a “bath” in ultra-pure water for 30 minutes both pre- and post-exposure. Pre- and post-exposure samples and blanks are analysed by the ChemCentre using Inductively Coupled Plasma-Atomic Emission Spectroscopy (ICP-AES) to determine the concentrations of leached elements. For further details, see Section 7.1 in the Monitoring Studies Report 2024 (Curtin University, 2024c).		

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			The experiments presented in last year's report (Curtin University, 2024c) comprised ten exposure rounds (batches). The dose-response relationship between the chemical elements Calcium (Ca), Potassium (K), Magnesium (Mg), and Sodium (Na) when exposed to different doses of a 50/50 diesel/natural gas condensate mix were explored. Experiments using diesel, heavy fuel oil (HFO), and ammonia (NH ₃) were also undertaken."		
123	7.1.1	Page 137, Para 1, line 3: "physiochemical" is a third spelling of this word in the Report.	Changed to "physicochemical"	✓	✓
124	7.1.2	Page 138, Figure 7.1-1 (also Figures 7.1-2 to 7.1-5): This figure, as presented, is unintelligible. The caption gives no indication of what comprises the "cumulative response", or how the units are derived. The caption talks of "concentrations" but of what, and how were they	We have also reworded/reordered the last two paragraphs of the introduction to clarify what is meant by response in this section. New text: "From last year's report (Curtin University, 2024c),	✓	✓

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		measured?	multivariate analysis indicated a strong positive correlation between the concentrations of Ca, K, Mg, and Na, all in units of mg/L. For simplicity, the sum of these concentrations was modelled. The preferred measure of the response is therefore defined as the difference in the sum total of the assayed concentrations of Ca, K, Mg, and Na, expressed in mg/L. A summary of the data and figures for each element is provided below. In each case, plots of concentration response against average dose, duration of exposure, and order of exposure given by batch number, are shown. Brief observations are provided in each case, then key findings arising are summarised to guide future experiments.” Captions have also been updated throughout to	✓	✓

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			remove "normalised".		
125	7.1.2	Page 139, Figure 7.1-2: The caption fails adequately to explain "Frequency" is diagram (b) and, again, briefly how Ca+Mg+Na+K was measured.	Caption is updated to be: “(a) Boxplots of the change in Ca + Mg + Na + K concentrations after exposure to a domestic gas/diesel mix, broken down by rock type; and (b) histogram showing the frequency distribution of these changes. The average dose of NOx and SO2 are 161.509 µ g/m3 and 16.783 µ g/m3, respectively.”	✓	✓
	7.4				
126	7.4.1	Page 167, Para 1, line 1: "measuring acidity (pH)" should be changed to "measuring pH" to remove the common misbelief that pH and acidity, as scientific terms, are synonymous. They are not and you have not measured acidity.	Agreed and changed.	✓	✓

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127	7.4.1	Page 167, Para 3, line 2: Sentence is incomplete "..... could be explained either by startup effects (such as OR what?	This sentence has been changed to: "This discrepancy could have been explained either by startup effects (such as operator learning curves), revisions of technique, and deterioration of reagents." The bracket was in the wrong place.	✓ OK. I've had the same problem with my newly installed battery!!	✓ ✓
128	7.4.1	Page 167, Para 4, line 1: "these new data", but data is not best used here. Suggest "...in the third year of studies and the results have been added"	The recommended change has been implemented.	✓	✓
129	7.4.1	Page 169, para 2: This paragraph, which is not easy to understand, epitomises the problems and complexity of using pH as a measure in elucidation of the overall chemical environment of the rock surfaces. The implication/conclusion that the rock surface is dynamic with respect to pH is clearly correct. The very means by which the rock surface pH is measured is dynamic and little work has been done on	Agreed. Taken as a comment.	Fine	✓

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		understanding the controls on the measured pH. Water on the rock surface is very poorly buffered whether rainfall or deionised water from the laboratory. It is true that the pH of water in equilibrium with atmospheric CO₂ has pH ~5.6, but rainfall influenced by marine water can approach seawater pH of ~8.1. Furthermore, pH of seawater aerosols can vary down to pH 4 or less for short periods after formation. Accordingly, the full range of pH measurements shown in Figure 7.4-2 could be explained by rainfall alone. I strongly agree with the statement in line 6, that pH, as recorded in the past and current studies, is little understood and thus a poor indicator of the geochemical environment of the rock surface. A more complete picture of the degree and types of acidity, their sources, and both inorganic and biochemical reactions occurring in the water upon			

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		and beneath the rock surface is very much required.			
130	7.4.3	Page 173, Para 7, line 1: While the local influence of the sea is an obvious subject for study at Murujuga, I believe the question posed for future study regarding possible Cl ⁻ increase near to the sea, is broadly known. Studies have shown that increased levels of salt are generally recorded up to 10 km inland, and may extend to ~30 km or more. That Murujuga occupies a coastal archipelago on a largely open coastline experiencing seasonally strong winds, should ensure that Cl ⁻ is enriched throughout the area, and probably to a spatially consistent extent.	Agreed. Suggestion for further research deleted.	✓	✓
131	7.4.6	Page 184, Para 1, line 1: I respect your effort in interpreting the rock cube pH data, but it is perhaps unwise to overplay small differences. as for "Some evidence", the difference in surface pH between the	Added "However, these results are close to within error of each other and individual measurements would be difficult to attribute unambiguously to a rock type effect."	OK	✓

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		gabbro and granophyre cubes is trivial and individual results are indistinguishable at 1 standard deviation, which may not be readily achievable during field measurement.			
132	7.4.6	Page 184, Para 3, line 3: omit "both" - to make sense of subsequent ", respectively"	Deleted "both"	✓	✓
133	7.4.6	Page 184, Para 4, line 3: To suggest that clinopyroxene dissolves rapidly (in the surface environment) is likely to paint a totally false picture in the minds of readers unfamiliar with mineral stability and geological time scale. Possibly change to "more susceptible to chemical decomposition during long periods of weathering"	Changed as suggested, and added "Rock dissolution consumes protons, increasing pH, and dissolution of the minerals found in mafic rocks consumes more protons than dissolution of the minerals found in felsic rocks. However, if mineral dissolution were the first-order control on the pH of the rock cubes, then the highest values would be expected on the fine-grained basalts, which are expected to dissolve faster than the gabbros, based on grain-size--effective surface area relationships. This is not observed."	✓ The additional text is OK but I retain the reservation, discussed earlier, that mineral grain-size is not the sole factor constraining rate of dissolution.	✓

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134	7.4.6	Page 184, Para 5, line 3: As the previous comment, "The ready dissolution " should be changed, possibly to "The relatively faster decomposition" Note that the formation of voids in the high-T minerals will, almost certainly, not be simple aqueous dissolution but will involve intermediate formation of relatively easily dissolved secondary minerals.	Changed as suggested. And agreed on the secondary minerals.	✓ ✓	✓ ✓
135	7.6.1	Page 197, Para 7, line 5: "This year" is inconsistent with elsewhere in the Report where the year, 2025, is used. The complete sentence may be improved as "In 2025, using improved knowledge of the distribution of exposed gabbro, 16 sample rocks were collected to reduce gaps in the spatial coverage".	Sentence changed to: "In 2025, using improved knowledge of the distribution of granophyre, 16 sample rocks were collected to reduce any gaps in the spatial coverage."	✓	✓
136	7.6.1	Page 198, Figure 7.6-1: As an example of how the visual effects of figures may be enhanced to improve their impact and ease of understanding:	The figure has been updated to include many of your suggestions.	OK Comment of improved figure	✓ Black dot now used for towns

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		- the crosshair symbol is difficult to recognise at the scale of an A4 page. A black dot is simpler, standard for a town location, and will contrast with the coloured data points. "Karratha" could be added for clarity. - lightening of the grey background might allow easier distinction of the coloured symbols; blue of the NA crosses could be darkened to contrast with the tone of the green dots. - simple addition to the caption to read " <i>NA</i> indicates locations of the two samples" saves the reader having to search for non-existent others. - Eaglehawk Island could be labelled on the figure, given its significance in the statistical treatment.			
137	7.6.1	Page 198, Table 7.6-1: the caption could usefully state the type of images e.g. petrographic	Caption has been updated to be: "Number of SEM images of	✓	✓

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		photomicrographs, SEM, etc.	thin-sections, and number of distinct spatial locations, in sample groups A and B for the porosity study. "NA" indicates locations where sample images were unusable." Line 1 on page 198 also updated to be: "Table 7.6-1 shows the number of distinct spatial locations and the number of scanning electron microscope (SEM) thin-section images obtained in each group."	✓	✓
138	7.6.1	Page 199, Figure 7.6-2: The vertical scale should rightfully be labelled "Average Porosity Index" The dotted vertical locating line could be made somewhat bolder (see comment on subsequent diagrams).	Figure has been updated to incorporate your suggestions.	✓	✓
139	7.6.2	Page 199, Para 1, line 1: As with previous comment, "last year's report" might be avoided and replaced by the number/year.	Text replaced by: "Section 7.6 and Appendix F.2 of the last year's report (Curtin University, 2024c)"	✓	✓

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			described techniques for inferring the porosity of rock, and especially for inferring the variation in porosity as a function of depth below the rock surface. This is done from microscope images of thin sections of sample rocks. In last year's report (Curtin University, 2024c), these techniques were applied to 99 images of thin sections of granophyre. For this report, the same techniques were applied to the additional granophyre samples."		
140	7.6.2	Page 200, Figure 7.6-3 (and subsequent Figure 7.6-4): The perspective view is refreshing and generally works well. Given the juxtaposition of data circles from different locations, it may be worth making the fine, dotted vertical location lines more distinct. In light of the important discussions of the relationship of rock porosity with industrial emissions,	Both figures have been updated, but main sources of emissions have not been added as these would obscure the results.	OK	✓

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		how useful would it be to mark locations of the main sources of the emissions (industrial plant, railway and shipping routes) on at least one of these perspective diagrams? Addition of the two labelled town sites would not over-clutter the Figure.			
141	7.6.3	Page 204, Figure 7.6-6: This figure, in particular, is impressive. I am not a statistician, but if the statistics are sound, this figure is highly expressive. Note: as I stated in the first section of this review sheet (Item #), an accompanying figure showing precise locations of potential sources of aerial emission would be most useful.	Agreed, and yes, the statistics are sound. As stated elsewhere, we have provided approximate locations of industrial facilities/activity and therefore emissions. However, providing "precise" locations is challenging and not within the scope of the project. Most fixed industries have multiple potential emissions sources, shipping and rail emissions cover a large and variable spatial area and there is evidence that emissions sources and composition have changed over time.	Noted. ✓ Good point	✓ ✓
142	7.6.4	Page 205, Para 6, line 4: "elevated porosity" would	Agreed, the sentence has been updated to the		

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		benefit from clarification - elevated in comparison to what? Possibly "there is statistically significant evidence of elevated porosity in a region situated on King Bay relative to the remainder of the monitored study area".	following: "The overall conclusion from the previous data analysis remains: there is statistically significant evidence of elevated porosity in the region centred around King Bay relative to the remainder of the monitored study area."	✓	✓
143	7.7.2	Page 211, Figure 7.7-1 and text: Surely there is need to mention the large number of higher mD values recorded particularly, but not exclusively, from granophyre in campaigns 8 and 9. As these are discounted as outliers, and are noted in the text as erroneous, some brief explanation that has been considered should be given - e.g. instrumental or procedural errors? This is in light of the statement that "this appears to be a promising approach for field monitoring"	Agreed. The following text has been added: Page 212 first paragraph: "The data mostly ranged between 1 and 1000 mD, and there were some very large values recorded. For instance, there were many such values in Campaign 8 due to an imperfect seal issue."	✓	✓
144	7.7.3	Page 217, Figure 7.7-7: I feel that this figure is the ultimate product of the trend	We have now combined Fig 7.7-6 and 7.7-7 into one figure and added the	OK	✓

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		in the Report to present results on a diagram as a prime means of presenting data from the monitoring program. The absence of any direction or even brief interpretation of the diagram in the caption, together with little or no ability of the diagram to condense, rationalise, or relate the data, ensures that the Figure provides little or nothing to the reader that would not be provided by a table of numerical results. Please refer also to my general comment, item \$. While on the subject of the figure, it does not represent a "scatterplot" in the usual meaning of the terms and what is the significance of the consistent maximum recorded mD at ~ 7.5?	following discussion about the results. "Figure 7.7-6 shows the boxplots of rock cube permeability measurements by AQM site, in campaign 9, with the different rock types indicated also. The AQM sites that had the highest permeability measurements were AQ01, AQ03, AQ11, EX06, and EX09 powered station. Whereas the AQM sites with the lowest permeability measurements were AQ02, AQ10, and EX02. Each site displays a wide range of permeability, reflecting local heterogeneity in rock properties. Some patterns emerge — for example, GRT tends to cluster at lower permeability values, whereas GBR tends to cluster at higher permeability values. No clear overall trend in rock cube permeability variation across sites is evident; some sites (e.g., AQ08-1) show more consistent permeability, while others	✓ ✓	✓ ✓

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			(e.g., EX07) display greater variability.” Apologies for the confusion, the figure (now 7.7-6) is of the winsorised mD values. That is, extremely high values were replaced with less extreme values (See Appendix F, Monitoring Studies Report 2024, Curtin University, 2025). This information has now also been included in the caption: “Boxplots of the winsorised unpatinated rock cube permeability measurements for each AQM site, coloured by each rock type.”		
	7.8				
145	7.8	Page 219, Para 2, line 1: I think what is meant would be better conveyed by "In accordance with the study design, large-scale microbiome sampling was not performed in this year". My apologies if I have interpreted this wrongly i.e giant sized microbiome samples were collected? I am unsure whether "this	Text changed to: "In accordance with the study design, large-scale microbiome sampling was not performed during year 3."	✓	✓

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		year" refers to the calendar year 2025, or the year of fieldwork?			
146	7.8	Page 219, Para 4, line 4: re. "however, the Dremel method was successful" in doing what? Clarify with addition of e.g. securing a workable volume of microbiome?	Clarification added	✓	✓
147	7.10.7	Page 263, Para 1, line 1: References to previous studies and commentary are required. Some notable studies, referenced elsewhere in this Report, discussed primarily rock surface pH rather than rainfall.	This section has been revised to include additional references and the 2007-2009 CSIRO rainfall measurements.	✓	✓
148	7.10.7	Page 263, Para 2, line 11: It may be incorrect to claim "acid rain" occurring at Murujuga. A long established, widely used, definition of acid rain includes pH <4.5 since a pH close to 4.5 can be produced under natural conditions. The term acid rain is conventionally associated with anthropogenic industrial air pollution and the high levels	The discussion on "acid rain" has been moderated and it is mentioned that the Bednarik and Neumann definitions of acid rain are not the general convention. However, some measurements from 2005 and 2008/9 were below pH 4.5. We agree with your comments on pH vs acidity	✓ noted	✓ ✓

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		of acidity produced by mineral acids, but not other components that induce low pH but only weak acidity. In a nutshell, as commented upon previously, one must not be side-tracked into believing pH and acidity are the same thing. To assume highly acid conditions from low pH measurements can be dangerous.	and also the importance on what is occurring on the rock surface, rather than an inert collector.		
149	7.10.7	Page 263, Para 3, line 10: While this novel method may be effective in collecting a water sample, I would want to question whether that sample equates to wet deposition. Clearly the environment (P,T) of the A/C condenser is likely very different to that of the point of of raindrops and other unknowns, such as presence/effect of Hepa filters, and analyte concentration in the condensate reservoir, may come into play. A clear example of 'further work is	Agree. This was an exploratory approach and we had no expectation of the results. It is plotted with the rainfall data purely as it was a wet deposition approach with no water added (unlike the dry deposition samplers) and the sample workflow via the UWA labs was the same as the rainfall samples.	OK	✓

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		required'.			
150	7.10.7	Page 264, Para 2, line 2: You are correct in stating that definitions for acid rain vary and also that the onset of acid rain is generally stated a ~ pH 4.5. This not arbitrary, but represents the lower limit of pH that can be produced without the addition of strong mineral acids, such as H ₂ SO ₃ and HNO ₃ that form almost exclusively from industrial precursors. Consequently, I cannot agree that "Values below this (pH 5.6) can be considered acid rain".	Agree, however Bednarik and Neumann have both considered values this high to be "acid rain".	Shame on Bednarik and Neumann! 🙄	Agreed.
151	7.11.2	Page 267, Para 8, line 2 onwards:	We have read the section and it appears OK	✓ Sorry, it appears I miswrote.	✓
152	7.11.2	Page 269, dot point 2, line 2: "slightly similar to" ??	Text changed to "approximately the same as"	✓	✓
153	7.11.2	Page 269, dot point 5, line 2: "respectively" to what ?	Text changed to "In February and March 2025, the median pH of the total deposition samples was 6.3 and 6.7, respectively, ..."	✓	✓
154	7.11.5	Page 282, Para 3, dot point 8, line 1: The sentence "NH ₄ ⁺ total deposition rates were BDL (black colour) for every	Amended to: "... total deposition rates were BDL for one or more months across most locations."	✓	✓

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		month on at least one island" seems to be a rather random remark. What is the significance?			
	7.16				
155	7.16.1	Page 421, Para 4, line 10: Indeed, these issues may require further investigation by the MRAMP team, but the principles and protocols of serial experiments are pretty well established. Whereas, progressive experiment duration may be usefully applied to individual rock samples, a protocol involving non-linear time sequence, and the dual use of an individual sample in different experiments/analysis, can be considered bizarre and is theoretically unsound.	Agreed. Additional sentences added here: "The statistical team has made changes to the experimental design of these studies, in particular to ensure that each individual rock unit is used only once. The new experiments are currently under way, and results presented here should be viewed with caution."	✓	✓
156	7.16.1	Page 421, Para 5, Line 2: "110 times more important to weathering" is rather vague. Suggested alternative is "110 times more potent in chemical weathering"	Amended as suggested	✓	✓
157	7.16.2	Page 422, Para 1, line 1: [syntax] "The majority of the predicted percent	This section of the report has been rewritten after 7.2 was updated.	OK	✓

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		reflectance <u>was</u> "			
158	7.16.3	Page 422, Para 2, line 2: [syntax] Change "sampler" to "samplers" line 4: Any suggestion(s) as to why there is no correlation?	This section has been rewritten to read: "Section 7.3 compares the data from the Woodside IVL and Radiello passive samplers. NO ₂ measurements from both IVL and Radiello passive sampler are strongly correlated and NH ₃ measurements are moderately correlated between the two passive samplers, indicating that this data can be used in an advanced analysis together with the MRAMP IVL data. However, SO ₂ and HNO ₃ measurements show no correlation between the two passive samplers, potentially due to detection limit differences (to be investigated further)."	✓	✓
159	7.16.5	Page 422, Para 6 and 7. I have harboured misgivings about the past application of pXRF in MRAMP. The Report concludes that it is a useful tool able to provide reproducible results for the	Taken as a comment. The pXRF was initially used to confirm rock ID as stated, however was continued to determine if it was sensitive enough to examine spatial	It was meant as a comment and your conclusion is no surprise.	Thank you

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		surface elemental composition of rocks at Murujuga. However, I am yet to learn exactly what this "usefulness" could be usefully applied to? The past direction appears to have been towards recognition of the main rock types exposed in the study area, something that is readily achievable without the use of such instrumentation. As I have commented upon in previous reviews, the investigation of elemental ratios, such as for SiO₂ vs Ti, appears to have been based upon a 'let's try it and see' approach rather than towards any petrogenic or mineralogical reasoning. Without evidence that the pXRF could be used to determine variations in patina/weathered rind composition, I concur that the technique needs no further proofing.	trends or change over time. This does not appear to be the case.		
160	7.16.5	Page 422, Para 8: It would be very nice to have reference to a map showing	A map has been added to section 1 and overlaid on the porosity map.	✓ Very well addressed	Thank you

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		the locations of industries. See Item # at the beginning of this document.			
161	7.16.8	Page 423, Para 5, line 1: Replace "Analysis" with "Analytical"	Done	✓	✓
162	7.16.9	Page 423, Para 8, line 1: Just to confirm the use of P in a sequence that is otherwise of ionic species. From P, I would assume it is total elemental P (inorganic and organic), and not P ³⁻ or PO ₄ ³⁻ .	This terminology is as per the deposition graphs/data sections which are analysed by the ChemCentre using ICP-MS. It is a measure of available Phosphorus. Their terminology is "Phosphorus - Filterable"	OK. Total soluble P and therefore bioavailable.	Yes.
163	7.16.9	Page 424, Para 1, line 5: To my knowledge both iron rust and bird guano (pH > 8.5) are initially alkaline when deposited. They become acid as a result of microbial breakdown. Would there be opportunity for such biochemical activity prior to sampling and analysis?	Our understanding is that both can span acidic and alkaline when deposited. For iron rust this appears to be related to the form of iron and for guano this appears to be related to the diet of the animal (carnivore guano reported as consistently acidic). Our measurements on the rock cubes in the AQMs show a decrease in pH of 0.5-1.0 when obvious/fresh guano is present. We have undertaken XRD of rust samples from AQMs and they span a range of iron compounds. We are	✓ Fine, and thanks.	Thank you

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			undertaking studies on the contribution to pH. Obviously as with many substances, pH can change over time though various reaction mechanisms.		
8-Associations between variables					
	Section 8.1 - Deposition versus passive samplers				
164	8.1.1	Page 427, Para 3, line 2: Possibly here, "precipitation" could be replaced by "rainfall".	Done	✓	✓
165	8.11	Page 427, Para 3, line 3: The samplers are not collected periodically. Rewrite as "The contents of both the passive"	The text is largely correct as-is. The entire passive sampler or bucket is exchanged every 4 weeks for a new one. In the case of the buckets there are 2 sets for each location and position which are changed every 12 months for new buckets. They are then processed (the sample collected) under laboratory conditions. The text has been amended to: "Both the passive samplers and 3-bucket	OK understood	✓

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			sampler buckets are collected (exchanged) periodically,.."		
9-General summary					
	9				
166	9.1	Page 449, Para 3, line 3: [syntax] For correct meaning add a comma after "microbial communities,"	Done	✓	✓
167	9.1	Page 449, Para 5, dot point 3, line 3: [syntax] "levels appear" omit "s".	Done	✓	✓
168	9.1	Page 450, Para 2, line 2: Change "observed" to "measured" or to "recorded".	Done	✓	✓
169	9.1	Page 450, Para 3, line 2: Change "delamination" to "rock surface delamination".	Done	✓	✓
170	9.1	Page 450, Para 4, line 1: [syntax] "data reveal" - omit "s".	Done	✓	✓
171	9.1	Page 450, Para 7, line 1: Has "live surface" been used previously in the Report? If not, what is meant by this term?	This was intended to emphasise the expected presence of viable microbes on the surface. As opposed to rock samples used in chamber or AQM studies which may be rendered sterile.	OK (dealt with previously)	✓
172	9.1	Page 450, Para 7, line 6: Re. "as well as (neutral) sea spray". Unless this has been	Discussed earlier. Text changed to "...as well as sea	OK	✓

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		measured at Murujuga, the pH of sea spray is most likely weakly alkaline approaching seawater (~pH 8.1). If finer aerosols are present, pH could vary considerable down to pH ~4.0. (see earlier comments)	spray (which may range from acidic to slightly alkaline)..."		
173	9.1	Page 451, Para 4, line 1: [syntax] "are progressing on schedule, however, are not yet"	Done	✓	✓
174	9.2	Page 452, Para 2, line 2: The sentence beginning "The additional year of research .." makes no sense. Revise text.	Agreed. Text amended to: "The additional year of research and analysis presented in this report, however, clarifies some key aspects of results and relationships observed in the Monitoring Studies Report 2024 (Curtin University, 2024c)."	✓	✓
175	9.2	Page 452, Para 3, line 1: you might elaborate on "the minor error in one of the analyses... ". Does this refer to the poor polishing or the error in the statistical analysis? Did it involve only one analysis?	Clarified to: "The porosity findings have been reconfirmed using a second subset of data, which produced a significant porosity area when the (Curtin University, 2024c) plot is corrected to a true 95 % confidence value. Some minor/random errors in	✓ Note that "in" and "were" should be inserted as highlighted here. These small errors are present in the Comparison report.	✓ "in" and "were" have been added

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			sample preparation for porosity analysis were also detected, however were determined not to influence results."		
176	9.2	Page 452, Para 4, line 1: "good" when referring to air quality needs constraining and should be avoided when used alone. Alternative example is "... remains good in relation to accepted health standards	Amended to: "The overall air quality remains below the Interim EQC Guideline levels established following the second year of studies last year."	✓	✓
177	9.2	Page 452, Para 5, line 1: Replace "... add further support to the hypothesis .." with "support a hypothesis"	Done	✓	✓
178	9.2	Page 452, Para 6, line 4: replace "to previously unhealthy conditions" with "to previous conditions of concern for damaging rock surfaces".	Done	✓	✓
References					
Appendix A Organic geochemistry – additional tables and figures					
Appendix B Photogrammetry					

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Appendix C Historical emissions due to industrial activities					
179	C-4	Page C-4, Figure C-1-1: The green coloured category is not shown in the key.	The green colour is in the legend. This is likely an issue in previous word conversion workflow for documents. We now have agreed an improved workflow which should avoid these errors.	OK understood.	✓
180	C-4	Page C-4, Para 1, line 7: [syntax] Change "presenting" to representing"	Done	✓	✓
Appendix D Statistical analysis – additional tables and figures					

2. Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Update 2025

2.1 Dr António Batarda Fernandes

Document Title:	Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Update 2025
Document:	COPP21065-REP-G-110
Reviewer:	Dr. António Batarda Fernandes

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1	General / overarching comments	The weatherability index is a good proposal towards the aims of the study but do see following comments.	Thanks		
5-Combined weatherability index					
2	p. 13	I believe that the second chapter in page 13 could be more appropriately phrased since as it is conveys the idea that rock art only results from anthropogenic weathering of the patina, while, as already established, I believe, there are motifs achieved by pecking or abrasion that more deeply cut in the rock, well beyond the patina.	Paragraph amended to read: “Weathering of rocks at Murujuga is integral to creation of the rock art. The contrast between the dark rock surface, or patina, and the paler weathered rind that underlies the patina, is only possible because of weathering that formed the patina and the weathered rind. Various engraving techniques have been used	Ok	✓

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			at Murujuga, however all will penetrate the patina layer and some may reach the fresh(er) rock underneath. However, weathering after rock art engraving may have a negative impact on rock art. Changes in colour may change the contrast of the art, and destabilisation of the patina, flaking of the weathered rind, or a range of other physical changes, could lead to loss of the petroglyph. Conversely, regrowth of the patina layer in the engraved area (as has occurred on what are believed to be the older petroglyphs) may obscure shallower engravings."		
3	p. 18	"Dissolution or rapid regrowth of the patina would have negative impacts for rock art preservation, but is not considered here for two main reasons:" Although agreeing with the 2 reasons given, this is a major handicap for the study. And shouldn't the methods to counter this be discussed	A strategy to counter this limitation is proposed in the Y3 report in the plan for monitoring porosity on page 106. We've drawn attention to this on p18 of the EQC report, and also to Section 5.6.1 of the EQC report, where the incorporation of data from	Ok	✓

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		here?	the patinated billets is discussed. New text reads “For these reasons, it is difficult to predict the impacts of atmospheric gases on patina in a quantitative way using existing information. However, observations of the patinated rock billets, which will be mounted in the AQMs (Curtin University, 2025c), will provide detailed annual observations of the well-characterised patina, reducing uncertainties related to inter-sample variation. It is possible that this information can be integrated into the <i>W</i>/ model, enabling consideration of this critical component of the rock surface.”		
4	p. 25	“rock type information was obtained from the heritage database” Which database? Moreover, CAMS	Clarified to state “the UNESCO World Heritage listing descriptions” CAMS typo corrected	Ok	✓

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		corresponds to <i>Copernicus Atmosphere Monitoring Service</i>			
5	Figure 8, p. 28	How do the variations relate with estimations for World Heritage Sites? And why do values vary (so much)?	Variation in context of WH sites discussed. New text reads “It is interesting to compare variations of this magnitude to the variation amongst World Heritage sites (Figure 8). <i>W</i> / values of around 0.5, similar to those calculated from granophyre at the driest times of year, are calculated for the Brandberg National Monument Area in the arid Namib Desert, which is ranked second lowest of the thirty sites considered. A <i>W</i> / value of around 3, similar to those calculated for basalt at the wettest times of year, are calculated for petroglyphs within the archaeological landscape of Tanbaly, Kazakhstan, where petroglyphs are hosted by clay-cemented sandstone, which weathers slightly slower than basalt but the rainfall, SO ₂ , and NO ₂ concentrations are higher than those at Murujuga.”	Ok. Just spotted a typo in the way “Siega Verde” is spelled in p. 30. It should read “Siega” and not “Sierga”	Fixed

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			Variation in values discussed; new text reads “Most of the variability relates to rock type and precipitation; the consistent difference of around 0.75 units between granophyre and basalt is explained by differences in rate-controlling mineral and grain size. The difference of around two units between January 2024 and September 2024 relates mostly to changes in precipitation rate, with temperature playing a smaller role. Changes in gas concentrations are relatively small, so they do not induce large changes in <i>WI</i> .”		
6	p. 34	“The paucity of rock art on basalt may relate to the relatively fast weathering of this lithology.” This is an empiric-based statement, or collected data already allows proposing this?	The statement is based on the calculations. This is clarified with new text “According to the <i>WI</i> calculations, basalt is the fastest weathering lithology at Murujuga (Figures 9 and 11). Basalt is predicted to weather rapidly because its grain size is small, maximising the surface area term, and dissolution of this lithology is controlled by the	Ok	✓

Item No.	Section	Peer reviewer comment Document revision: A Date: 01/11/2025	Curtin MRAMP team response Date: 02/02/2026	Peer reviewer close-out comment Document revision: C Date: 10/02/2026	Curtin MRAMP team close-out comment Date: 23/04/2026
			rate of dissolution of clinopyroxene at pH values less than 6 (Table 3). Clinopyroxene is the fastest-dissolving of the rate-controlling minerals at Murujuga.”		
7	p. 35	“The WI model is sufficiently versatile that it can be used to assess the impacts of climate change on rock art at Murujuga and elsewhere.” Will it be possible to use elsewhere? Personal experience suggests that methods to measure and evaluate weathering are so site-specific that use elsewhere requires major or complete overhauls so that it becomes an altogether other model...	There is always a balance between increasing complexity, which renders models more site specific, and increasing general applicability. We have tried to build a model that has general applicability, but future refinements, such as the integration of a term for the microbiome, may render it less generally applicable. This has been stated in the text.	Ok	✓

2.2 Professor Geoffrey Michael Gadd

Document Title:	Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Update 2025
Document:	COPP21065-REP-G-110
Reviewer:	Professor Geoffrey Michael Gadd

Item No.	Section	Peer reviewer comment Document revision: A Date: 05 November 2025	Curtin MRAMP team response Date: 02/02/2026	Peer reviewer close-out comment Document revision: A Date: 06 March 2026	Curtin MRAMP team close-out comment Date: 23/04/2026
1	General / overarching comments	The report is clearly written and presented and reiterates some of the background information included in the previous interim EQC report. It contains summaries of the various methodologies and protocols used, focussing on the development of the weatherability index and its application. This interim EQC report has been significantly revised since the previous interim EQC report due to protocol changes and awareness of further background weathering information. Further progress on EQC development is dependent on ongoing and future work the aim of which is to produce a single weatherability index which	Thankyou. Taken as a comment.	OK	✓

Item No.	Section	Peer reviewer comment Document revision: A Date: 05 November 2025	Curtin MRAMP team response Date: 02/02/2026	Peer reviewer close-out comment Document revision: A Date: 06 March 2026	Curtin MRAMP team close-out comment Date: 23/04/2026
		combines all the investigated pollutant species. The report contains a significant amount of data, as well as in an Appendix. Specific comments and questions are itemised below.			
1-Overview					
	1.1.	Mention is made of “microbes which cement the minerals (in environmental dust) to the rock surface” and is included in areas of ongoing study. However, this aspect has not been specifically mentioned in future work so is it a part of the future microbiome work?	It is the current view of most of the scientific team that microbes are integral to any processes which have occurred on the rocks since the onset of industrial activity. As such, the microbial chamber studies are a key focus of the remaining work and development of final EQC.	OK. So this should be included in the described future work.	Noted
	1.3.	Fig.1. mentions <i>in situ</i> remedial work – what is the nature of this? The NOEC and LOEC terms are derived from ecotoxicology which refers to effects of environmental toxicants on living organisms. Can they be adequately applied to rock/mineral systems? Have they been applied in	This figure is taken from the cited government document and is not produced by the MRAMP researchers. It was prepared before the current project started. In-situ remedial work may be intended to refer to remedial work to reduce emissions, rather than remedial work for rock art preservation.	OK, but it is still unclear what it is (“may be intended to...”) and seems of little relevance to the current project. OK	MRAMP researchers are not the authors of the document that the figure has been taken from, as such we cannot definitively state what the text refers to as that is a question for the (government) authors. However we believe the figure is important as it explains the intended application of EQC within the EQMF framework.

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		other studies of rock weathering? Do they take account of longer-term effects? i.e. there may be no discernible initial or short-term effects for a given event but over time there could be due to the rate of chemical reactions, microbiome development and influence etc.	The terms NOEC and LOEC generally refer to a biological response so they are not generally used in rock weathering studies. However, dose response models, which is the approach used for Murujuga, have been used elsewhere to establish thresholds for damage to rock-based heritage by atmospheric pollutants. Text added "Dose-response relationships have been used to establish thresholds for pollutant concentrations in other situations where rock-based heritage may be at risk from particular gases within the atmosphere (Bai et al., 2025; Broomandi et al., 2022; Visone et al., 2025). Dose-response curves are based on the assumption that there is no effect below a threshold, at which point there is a measurable response, which triggers action. The threshold is	OK	✓

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			observable change, and this may be independent of the timescale over which change occurs.		
2-Methodology for EQC development					
		No comments.			
3-Updated interim EQC					
	3.1.	As stated, it is noticeable that the WHO guidelines for NO ₂ and SO ₂ are markedly lower than the interim standard EQC values, and that the WHO values will therefore be applied where available. In that case, does this not negate to some extent the work that has gone into devising an EQC at least for NO ₂ and SO ₂ ? Could the WHO values have been applied overall in the absence of a new EQC? Is some differentiation envisaged in EQC application between residential or non-populated areas?	The text and recommendations in this section have been revised in response to other reviewer comments. The project scope is to set EQC specific for the protection of rock art. The implementation of the EQC and the choice to apply competing (WHO or equivalent West Australian) health guidelines is a matter the relevant policy and regulatory authorities.	OK OK	✓ ✓
4-Monitoring against the Interim EQC					
		No comments.			
5-Combined weatherability index					
	5.1.	As well as the factors listed, industrially-emitted gases (and other pollutants) could	Agreed. The first paragraph has been clarified to add this	OK	✓

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		also impact on rock art because of effects on the microbiome which in turn could influence bioweathering.	and state that the weathering index does not (yet) include bioweathering.		
	5.2.	1 st paragraph. Free-living and symbiotic microorganisms are also significant in rock weathering. As mentioned in previous review exercises, bioweathering is the accepted term for weathering processes mediated by living organisms or their products.	“free-living and symbiotic microorganisms” has been added after animals Again noted. We will endeavour to use this term. It has been added to Section 5.3	OK Good. This point has been raised ever since the project started.	✓ Noted
	5.3.	Microbial processes can indeed enhance weathering rates as stated (although no further details are provided here) and in some cases can be the most important agents of biodeterioration of rock and mineral-based substrates although perhaps not at Murujuga based on the evidence provided so far. There is a voluminous literature on the subject in years after 1997. The cited background literature appears quite dated (as also in later sections) and there is no inclusion of	Agreed and this also may be the case at Murujuga. A more recent reference has been added and the term bioweathering incorporated into the text. The year 3 studies report contains a more extensive reference list.	OK	✓

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		possible biotic involvement or contribution to the conceptual model. The model only includes geological and atmospheric factors and primarily focusses on rainfall pH.			
	5.4.3.	As mentioned previously, there is no inclusion of biotic influence on mineral dissolution rates here, and the largely dated cited research publications do not refer to this aspect. It is now realised by the geological/geochemical community that this may be a significant influence on overall weathering, and some work has shown that the contribution of microbial dissolution has been seriously underestimated in certain systems. Does the evidence obtained so far from the microbiome work indicate that biotic processes are insignificant and can be ignored or is any appreciation of such a role dependent on further/ongoing research? Protons are indeed important in mineral dissolution, as well	It has been clarified in Section 5.1 that bioweathering is not (yet) included. It is aimed to include bioweathering in future revisions and this may indeed be the dominant weathering mechanism at Murujuga also.	OK It therefore should be included.	✓ Noted

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		as ligand-mediated dissolution by e.g. citrate and oxalate, which can be significant in some rock/mineral types depending on biotic activity.			
	5.5.1.	What could be the reason(s) why calculated pH values were lower than measured values?	One reason that calculated pH values might be lower than measured values is interactions with clay particles in the atmosphere. This is now discussed in the text "One possible reason for the discrepancy is that rainwater may have interacted with clay particles within the atmosphere, increasing pH (e.g., Broomandi et al., 2022). The effect of particulate matter on rainfall pH was investigated by Broomandi et al. (2022) for UNESCO world cultural heritage sites in Eastern Asia. Rainfall pH values calculated with and without the particulate matter term derived by Broomandi et al. (2022) were compared (Figure 6b). Inclusion of the PM term increases calculated pH values by up to 0.1 unit, but not to values as high as those observed. Further work	OK	✓

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			is necessary to establish whether a different PM term is more appropriate for Murujuga, and to assess the impacts of other possible contributors to the difference between observed and calculated pH values.		
	5.5.2.	Fig.6. What is the explanation for the increases in the weatherability index after 01.10.2024?	The calculated weatherability index includes a precipitation term so weatherability tracks precipitation. This has been explained as part of the response to comment 5. of A. Batarda	OK	✓
	5.5.3.	There is no discussion about the interpretation of Fig.7. What are the implications or significance of this summary for Murujuga? Does the WI fit in with what is known about the current state of these monuments?	Discussion of Fig. 7 provided as part of the response to comment 5 of A. Batarda.	OK	✓
	5.6.1.	It is suggested that a term be included to simulate effects of microbially-mediated mineral dissolution. This depends on work in progress: so far there has been no data produced regarding microbial bioweathering, Is obtaining data for such a term feasible given the remaining timescale of the project?	Chamber studies involving the microbiome are underway and the results will be used to attempt to develop a microbial term for the WI. These efforts will be described in the Y4 report. However, as the reviewer states, the causes and effects of microbial growth	OK	✓

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		It is not just gases that may influence microbially-mediated mineral dissolution but also other organic and inorganic pollutants and particulates that may be present in atmospheric emissions. Many publications are available in this area beyond the cited 2009 review.	are complex, and the multi-factorial nature is likely to prove challenging. See response to comment 3 of R. Watkins.		
6-Summary					
		It is stated that some of the criteria air pollutants (CAP) are unlikely to affect the rock/microbiome. However, ozone and particulate matter are listed among the CAP pollutants. In the Year 3 report, the importance of considering ozone levels is stated (p355) and that the “long range transport of ozone cannot be ignored” (p356). It is also stated that O ₃ could be expected to impact the microbiome or accelerate chemical/redox reactions on the rock surface (iii, p450). It is well known that particulate matter can impact microbial colonization and activity on rock/mineral	Ozone (O ₃) experiments are planned for the microbiome chamber studies. As stated in year 3 studies report and revision comments to the same reviewer, we have recently found literature (Wang et al. (2021) https://doi.org/10.1016/j.scitotenv.2020.142134) that <i>Blastococcus</i> was observed to become more abundant with O ₃ exposure.	OK. No answer about particulate matter or reconsideration of CAP.	✓ Apologies. This was an oversight. We are undertaking experiments with particulates and considering all CAP apart from Lead, for which there are no known (significant) sources currently.

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		surfaces. Should there be reconsideration of these CAP therefore?			
7-Glossary and abbreviations					
		Biomarkers – biomarkers can refer to substances found in all kinds of habitats, and are not only degradation products but can include products of biosynthesis, cell structural components etc. The term “molecular fossils” appears only applicable to the study of ancient environments.	The term has been changed to Geochemical biomarkers and the definition amended. The reference to molecular fossils has been removed.	OK	✓
		Microbiome – not all microbial eukaryotes are unicellular, e.g filamentous algae	Unicellular has been replaced with microbial	OK	✓
		Microcolonial fungi – this refers to a specific group of fungi growing in microcolonial form and usually thick walled and pigmented. Not all fungi growing on/in rocks and minerals that are important in rock biodeterioration/mineral transformations are microcolonial fungi.	Amended to clarify: “Some representatives of colonies of fungi...”	OK Unsatisfactory statement. See simple definition here and elsewhere in other responses. An adequate definition is in the Report glossary.	✓ Corrected to the same definition as the report glossary.
8-References					
		The bulk of reference support to this document is from relatively recent Curtin or	A more recent reference has been added. The year 3 studies report contains a	OK	✓

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		Government body publications. The rest of the geochemical/geological references appear quite dated. The two references that pertain to specific biotic/microbial processes are dated and superseded by many other relevant publications.	more extensive reference list.		
A-Appendix					
		No comments.			

2.3 Dr John Henstridge, Data Analysis Australia

Document Title:	Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Update 2025
Document:	COPP21065-REP-G-110
Reviewer:	Dr John Henstridge (Statistician)

Item No.	Section	Peer reviewer comment Document revision: A Date: 5 November 2025	Curtin MRAMP team response Date: 02/02/2026	Peer reviewer close-out comment Document revision: Date: 14/3/2026	Curtin MRAMP team close-out comment Date: 22/04/2026
1	General / overarching comments	This Update attempts to derive reasonable EQC values for Murujuga through a combination of experimental data, historical data and theory, particularly thermodynamics. In doing so it gives revised EQC values for a number of gas species. As a statistician I am not able to comment upon the detail of the thermodynamical arguments, although I can follow them and they seem reasonable. I do have reservations about the data driven aspects, namely: The chamber studies exposing rock samples to relatively high concentrations of certain gases are reported in the	All chamber studies were statistically designed with the exception of the dose sequencing. They were based on a very well designed apparatus and approach for human toxicology testing https://www.sciencedirect.com/science/article/pii/S2215016121003514 . Preliminary tests indicated that rock cube reuse was appropriate and did not influence results. Indeed it permitted the microscopy confirmation of weathering. The greatest issue with the chamber studies is that they necessarily operate close to the measurement limits of analytical techniques – for reasons of available time and to minimise dose. The switch	I appreciate the team's statement that they have developed an approach that attempts to incorporate both empirical studies of the effects of pollutants (the chamber studies) and a scientifically logical thermodynamic model to give the best possible EQC estimates. I also appreciate that further chamber studies are being conducted. However I remain concerned about the quality of the chamber studies, since the initial work appears to have been poorly planned, with an experimental design that did not incorporate appropriate balance in regard to reuse of samples and had poor choice of exposure levels. (I note	Laboratory simulations of environmental exposure are inherently a simplification of real environmental systems and it will be impossible to explicitly investigate all possible co-exposures and interrelationships. However we will investigate all that can reasonably be studied within the time available. Any others may remain topics for further recommended ongoing research and review (the latter is built into the EQMF framework), As stated in other comments, the chamber studies initially reused rock cubes as (a)

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		third annual report. It is clear that those studies have not been well conducted, with a lack of proper experimental design, very inconsistent results and strong suggestions of interactions between various gas species in their effect on rock surfaces. The historical data is also of very mixed quality. Collectively, these issues mean that the interim nature of the proposed EQC measures must be stressed. The proposal of an EQC should ideally also include statements on how it is to be measured. If, for example, the monitoring of gas concentrations is prone to a high rate of error, then how a possible exceedance of an EQC is acted on may be quite different than if it is measured with higher accuracy. That said, the development of the weatherability index based upon a model that attempts to	to gaseous NO₂ tests, which appears to have a very weak effect on rock weathering, gave the opportunity to further improve the design for the chamber studies. As has been added to Section 7.1 in the year 3 report: The year two report chamber studies predominantly utilised a methodology which exposed rock cubes to cooled, diluted combustion gases, consisting of air (N₂ and O₂), CO₂, CO, NO, NO₂, SO₂, H₂O, VOCs, trace gases and soot particulates. The method also utilised a UV lamp to assist with NO to NO₂ conversion (oxidation) and generate O₃ through reaction between NO₂ and VOCs. Additionally, the water vapour, temperature and UV would have been expected to induce reactions to convert many of the gases to their respective acids. The exposure of rock cubes to this cocktail of gases, droplets and particles produced an apparent exposure effect. This	that none of the reports provided to date include the experimental design, so it is not possible to fully understand the extent of the problems.) This has led to results presented in Section 7.1 of the 2025 report that lack credibility. I acknowledge that the reuse of samples is now regarded as a mistake and the further work will be largely (totally?) with fresh samples. I hope that this together with proper choice of exposure levels rectifies the shortcomings. This still leaves issues regarding the non-measurement of symbiotic effects and the new issue of the importance of interactions with microbiome effects. The former will need careful design and analysis for the remaining chamber studies which I understand is being done. Overall the high level of uncertainty in the choice of EQC levels remains	preliminary testing found no effect of reuse and (b) multiple exposures on a single cube were required to detect visible weathering using electron microscopy. The single gas exposures (particularly NO₂) allowed us to observe the error induced from rock cube reuse due to the weaker effect. The revised methods have addressed these experimental shortcomings and will be fully detailed in the next report(s). It is typical in toxicology to apply "safety factors" to account for interactions or effects which have not been or cannot be measured. This can be in terms of scaling factors of 10-1000 (e.g. see Illing (1991) https://academic.oup.com/annweh/article-abstract/35/6/569/133419) or more sophisticated methods (e.g. see Pennington (2003) https://www.sciencedirect.com

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		capture the physical and chemical processes is a major step forward.	effect was measured as leached elements post exposure, which could be attributed to weathering of minerals within the rock. The magnitude of this effect was determined, for NO_x (NO+NO₂) + SO₂ as these are the dominant gaseous air pollutants at Murujuga and were believed to be the most reactive and relevant exposure species at the time. Preliminary year two chamber studies explored the effect of reusing rock cubes over multiple exposures and determined this method was acceptable (and preferred as it was the most efficient use of rock samples, reducing the need to collect additional samples, which is culturally undesirable). The experiments also utilised all rock types, which reduced the number of replicates of each rock type per experiment. The year 3 report chamber studies planned to differentiate the effects of		/science/article/pii/S0147651302000891). To date we have used simple scaling factors as per the first paper, however more rigorous methods may be used for the final EQC if deemed appropriate.

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			NO₂, SO₂ and other air pollutants through direct exposure to individual (bottled) gas species. NO₂ was selected as the first exposure as it is the most abundant gaseous air pollutant at Murujuga and has received the most attention in previous literature. However, both our own work and literature cited in this report has now identified that NO₂ likely has only a very weak effect on rock weathering. Commencing single gas exposure experiments with NO₂ highlighted methodological deficiencies in the chamber studies, namely, rock cube reuse, the number of replicates per rock type and exposure scheduling, which have now been addressed and overcome. This has resulted in a decrease in the measured effect determined for NO₂ and to a lesser extent SO₂. The thermodynamic/ weathering model appears to		

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			be a suitable means of combining abiotic effects. However it appears increasingly likely (from both field analysis data and preliminary microbiome chamber studies) that the most likely mechanisms for observed effects is through interactions between air pollutants and the microbiome. These will be thoroughly explored in conjunction with the statistical team for the development of the next/final EQC and or weathering model. The assessment against EQC, measurement approach and uncertainties are described in a separate Monitoring Program Design Report (MDPR) and associated methods documents.		
1-Overview					
	1.1	Page 1, last paragraph. This references some of the results from the chamber studies but does not, in my opinion, properly present the limitations of the current	See above. Paragraph amended to include: “In this process, the weaker effect of individual gas exposure required some revisions to the chamber	See above.	✓

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		studies.	studies method in order to improve reproducibility.”		
2-Methodology for EQC development					
	2.2	Page 6, third last paragraph. I can appreciate that the factor of 10 to allow for synergistic effects might be in use in some places, but to assume that such a step is appropriate here is perhaps rash. I suspect there are examples of where the synergistic effect is more than tenfold. I suggest the new chamber studies actively explore this.	This is being examined in current chamber studies.	Acknowledged.	✓
5-Combined weatherability index					
	5	Overall I find this section to be a major new contribution to the study program, one that has the potential of systematising much of the work done. For example, weatherability has the potential for identifying “canary” sites that might display changes before others. However I am disappointed by the apparent lack of statistical thought. This section contains many references to data from Murujuga without	Taken as a comment. The current index was finalised shortly before document deadline. Future iterations of the index will incorporate input from the statisticians (e.g. yr 4 report).	Acknowledged I look forward to the more complete analysis.	✓ Noted

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		consideration of just how accurate the data might be. This weakens the reliability of the index as it stands.			
A-Appendix					
	A-1	I find the plots here to be of limited use as they demonstrate that there are substantial differences between measuring sites, but do not present any form of uncertainty (error bars) on the individual measurements. I am left not knowing whether the exceedances are real or just “random”.	The boxplots are of passive sampler data, which is received as single values representing the (average) level detected over the previous 4 weeks, i.e. smoothed exposures. This means any exceedance is highly likely to be real as the analytical error on these measurements is very small and error bars would likely not be visible if included. However, measurements include random natural effects and may be influenced by factors such as (natural) dust deposition, guano, etc...	Acknowledged	✓
	A-2.1	The caption for Figure A-2-1 indicates the sites are ordered by their overall average concentration of NO ₂ . It is not clear why the second and third plots have the ordering reversed.	Ordering has been changed to retain the same site order for Figure A-2.2 and A-2.3 as per Figure A-2.1.	Acknowledged	✓

2.4 Dr Des Lord, Des Lord & Associates

Document Title:	Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Update 2025
Document:	COPP21065-REP-G-110
Reviewer:	D LORD

Item No.	Section	Peer reviewer comment Document revision: A Date: 28/10/2025	Curtin MRAMP team response Date: 02/02/2026	Peer reviewer close-out comment Document revision: B Date: 02/03/2026	Curtin MRAMP team close-out comment Date: 23/04/2026
1	General / overarching comments	This review was conducted on the Interim Environmental Quality Update 2025 report issued in September 2025. Development of EQC is one of the more important “outputs” of the MRAMP. Their progressive development through a series of reviews and reports is strongly endorsed. Of importance, the EQC report structure and content should be of a “stand alone” nature, with each successive revision providing further clarity and including all relevant information previously presented. However, this reviewer was unable to review the September 2025 report as an	Comments and feedback are appreciated. This document is intended to report the current development of the EQC, but not fully detail the scientific basis (contained in the studies report), nor is it intended to detail the monitoring against the EQC or the standardised methods, reported in separate documents. Some requested inclusions such as an executive summary are sensible however not specified in the contractual document structure.	Responses noted. Agreed, no need or expectation to present all scientific information. The task will be to succinctly summarise and present this information...particularly significant conclusions. This reviewer is not aware of any contractual requirements for the structure of the EQC document but will check with the agency responsible.	Taken as a comment, however we have taken the comments on board and will incorporate them in the year 4 EQC document.

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		independent “stand alone” document. Significant referral had to be made to the May 2025 report as well as several other reports, including the major Monitoring Studies Report Rev B issued in August 2025, to reach an understanding of the development of the EQC. Further, the 2 more recent EQC reports (May 2025 and September 2025) are similar in structure but substantially different in detail, for no apparent reason. For example, the May 2025 report provides details on the results of the important accelerated weathering studies, which influenced the interpretation and recommendation of EQC for pollutant gases. Further evidence of the study teams view of the importance of this matter is provided in the Yr3 Research Summary, where it is stated that “the identification of spatial patterns in rock porosity etc...are already shaping the development of Environmental Quality	We will take these comments and revision suggestions onboard and ensure that the subsequent/final EQC document is as complete and standalone as possible.		

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		Criteria". Little on this topic is provided in the September 2025 report with the September 2025 report dominated by a discussion on the proposed Weatherability Index. See further comments on the WI below. The September 2025 report presents a significant recommendation for the EQC, this being the recognition of SO₂ as the critical pollutant gas (with nitrogen containing gases of far less influence). Appropriate criteria are proposed and compared with measurements. This is a significant outcome. (Note: The table of equilibrium constants for the solution of relevant gases in water provides rationale for this observation, as does previous studies on acid rain that clearly identify SO₂ as the most significant pollutant gas for reducing the pH of precipitation.)			

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		Recommendations • The purpose and presentation of information of future EQC reports needs reconsideration. • The report needs to be “stand alone” and present the proposed EQC, their derivation, and the programme to monitor compliance. Appendices are useful for providing technical detail or discussion. • The team should be commended on their purposeful endeavours to develop EQC, with the awareness that the information needed to present a structured EQC document that will stand the test of time, is all there somewhere in among the technical reports, • An Executive Summary would be useful to “tightly” summarise the content. • This reviewer is of the view that there is little benefit in any significant			

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		revision of this document. It would be more useful to commence the preparation of what should be close to the final draft of the proposed EQC document			
1-Overview					
		The Overview is provided in 3 separate sections. The distinction in content between the first 2 sections is vague, both include general statements as well as specific technical comments, some of which overlap. Perhaps blend the two? Whatever, the team may wish to consider the inclusion of a clear statement in the opening paragraph concerning the purpose and content of this document. For example.. The MRAMP was established to determine whether accelerated weathering of rock art due to anthropogenic activities is occurring and to identify conditions for their protection. Management of the rock art will be undertaken using an EQMF and conditions needed to	The suggested paragraphs have been added and Sections 1.1 and 1.2 merged.	Noted	✓

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		protect the rock art will be stated as EQC. EQC will be clear, measurable and auditable. In the section Purpose. The statement is made “The information contained in this document supersedes the Interim EQC published in the Interim Environmental Quality Criteria 2025 (Curtin University, 2025a) released in April 2025. This document does not achieve this aim of being independent of previous drafts and other sources of information. See further comment in General above.			
		EQC Concept. Appropriate explanation of the EQC concept and the derivation of EQC	A statement has been added at the commencement of Section 1.2 (formerly 1.3): “Historically, EQC were developed in the marine science field and refer to benchmark levels that trigger management attention and action to protect ecosystem structure and function, serving as standards for assessing environmental	Noted and supported Benchmarks or selected yardsticks are used universally to measure and manage effects of anthropogenic activities. In the case for the protection of the rock art, the final EQC document (consequent to the establishment of an EQMF and Environmental Values for the areas) will also need to	We are unsure what offsets refer to in this instance? Likewise a buffer zone. The objectives of the project are to identify EQC necessary to protect any/all rock art, regardless of its location and proximity to industry or emissions sources. Although the world heritage

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			quality and determining regulatory limitations on contaminants.”	delineate potential areas of impacts, consider the relevance of designating a “buffer” zone close to sources of emissions, and perhaps even consider the issue of offsets.	inscription requires the protection of all rock art within the World Heritage boundary, the development of an EQMF and the practical implementation of EQC is a matter for government and beyond the scope of this project.
2-Methodology for EQC development					
		This section provides important information and technical data on the evolution of EQC. It has been of value to the reviewer. It is written in a “progress report” style and the expectation would be that these sections would be edited as needed when the final QC document is prepared. The rationale to identify SO₂ and not the nitrogen containing gases as the dominant influence on wet and dry deposition pH (and presumably total acidity) is well presented and supported with sound evidence. This is a major contribution to the development of appropriate	Taken as a comment	Noted	✓

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		EQC. Information on wet and dry deposition pH is suitably presented.			
3-Updated interim EQC					
		In the September 2025 report, there is little information or discussion on the chamber studies used to develop and modify the proposed criteria for SO ₂ and NO ₂ . The rationale is provided elsewhere (in other reports) but should be included here. The opportunistic sampling undertaken during/after the heavy rainfall in early 2025 provided valuable supporting information on ambient rainfall pH.	Consideration will be given for including this additional information in the final EQC document.	Noted	✓
4-Monitoring against the Interim EQC					
		Appropriate for the two major measures proposed as EQC, namely concentrations of atmospheric pollutants and the pH of rainfall. Appendices contain relevant monitoring results compared to EQC.	Taken as a comment	Noted	✓
5-Combined weatherability index					
		An interesting approach. If it is accepted that an EQC			

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		should have the properties of being clear, measurable, and auditable, and of a nature that remedial action can be directed at any selected component (e.g. SO₂ emissions). It is not apparent or clear how the WI meets these objectives. Whether this approach can be further “tuned” and be useful as an EQC is still to be demonstrated. The basis for the WI approach is provided in section 5.3 titled Conceptual Model, with a simplified diagramme (Figure 4) that presents the relationships that cause dissolution of minerals from the rock matrix due to lowered pH, and the subsequent processes leading to the calculation of a WI. To support the calculation of WI the report presents substantial valuable information on rock types, their crystal structures and potential for weathering at lowered pH levels, as well as thermodynamic relationships	Taken as comment mostly Correct Please see additional text on this in response to Comment 3. of A. Batarda Correct Agree in principle.	Noted Noted Noted	✓ ✓ ✓

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		for the solubility of gases in unbuffered water that can lead to lowering the pH of wet/dry deposition. But in the calculation of WI, numerous assumptions and simplifications are made, for example - The use of equilibrium thermodynamic expressions, as done above, cannot replicate speciation within the system of interest (section 5.4.2) - Experimental dissolution rates are up to several orders of magnitude faster than mineral dissolution rates in natural environments (e.g. Drever and Clow, 1995). (Section 5.4.3) - Dissolution or rapid regrowth of the patina would have negative impacts for rock art preservation but is not considered here.	Correct Other reviewers share these concerns. The limitations are discussed in additional detail, and the refinements to the model, and the data collection required for those requirements, are now discussed in additional detail in Section 5.6, see response to comment 3. of Ron Watkins. - Table 3 includes five rock types (gabbro/dolerite/basalt/granophyre/granite). Geological map added. - This will be looked at through monitoring of the patinated billets. See response to comment 3. of Ron Watkins. - As the primary goal of the project is the protection of the rock art, a means of accurately measuring any change in the rock art condition over time is highly desirable. The accuracy and sensitivity of spectrometric and	Noted. Changes in text in sections 5.1 as well as 5.6 noted - Noted and appreciated - Noted. Strongly supported. We all look forward to the outcome of this proposed work. - Noted. Approach strongly supported - This reviewer is of the view that it is the more appropriate value, rather than smaller number should be used. Measured values should take precedence. As a general rule, in numerical modelling, measured values are used to	✓ - Thank you - Thank you ✓ - As the reviewer suggests, measured values would normally be preferred if the measured and theoretical values are directly equivalent.

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		- Calculated pH values were less than the measured values, indicating that the calculation provides a conservative estimate of rainfall pH, and confirming that this technique can be used for comparison but not to predict absolute values to be used for comparison with the proposed EQC pH values. (Section 5.5.1) - A weatherability index of 4 may not have an instantly translatable physical meaning. However, if it increases or decreases then changes in weatherability can be used as a basis for management decisions. Please expand on how, as, if the WI index changes due to say SO₂ increases then it is the SO₂ EQC that would be used for any remedial action.	photogrammetric methods for this are still being examined before they can be included in ongoing monitoring. 4. The WI calculations could use theoretical or measured pH values. It makes sense to use the lower of these values. This has now been stated in the text. 5. Taken as a comment, but also worth noting that feldspars do not typically contain magnesium, chloride or sulphate.	calibrate theoretical models, not the reverse. 5. Noted with thanks	However, they are not directly equivalent in this case because the dry deposition measurements use an arbitrary amount of water and add that to a month's accumulation of dust but without equivalent cumulative NO_x and SO_x exposure. For this reason, in the case of Murujuga, the conservative estimate is preferred and we have clarified this and the reasoning, in the text. 5. ✓

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		- It is possible to devise a semi-quantitative description of weathering rates for different rock types that includes the impacts of first-order geological and atmospheric controls on chemical weathering. - This statement implies significant “assumptions and simplifications” which would not be suitable perse for an EQC These simplifications/assumptions clearly limit the use of the WI as described for use as an EQC. Some further general comment 1. Table 3 section 5.4.3 includes only 4 rock types although the text and table title indicates 5.....some clarification needed, Whatever, somewhere in the document there would be merit in providing a figure presenting the surface geology of the area. See			

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		for example figure in Burrup Rock Art (M Donaldson) with a description of the origin of the igneous rocks of the peninsula and recognising the distinction between intrusive and extrusive formations (relevant to weathering). 2. Addressing other aspects of weathering, namely development of porosity, and the relevance the weathered rind for rock art conservation and longevity is a fundamental consideration in setting EQC and would be anticipated to be included/recognised in some form in the development of a WI. 3. The study team has suggested that spectral measurements of rock art could form an integral part of routine monitoring, for consideration as indicators of accelerated			

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		weathering. Please provide additional background and rationale. 4. What is the benefit of calculating the WI using “theoretical” values of the pH of wet/dry deposition when reliable measurements of pH of these are available? ...other than to affirm that calculated values address the bulk, but not all, of the principal causes of pH reduction. Where reliable measurements are available, they would normally take precedence over calculated values. 5. Of general interest is the observation that the weathering trials show that the 6 main chemical species being “dissolved” from the rock matrix are: sodium, potassium, calcium, magnesium, chloride, and sulphate, these being largely derived from the felspars			

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		in the igneous rocks. In seawater, these 6 ionic species plus bicarbonate are the dominant ions in solution, while the ratio of the concentration of these ions in ocean waters is constant. This observation is an oblique confirmation that the weathering trials undertaken in this study have successfully reproduced the dominant weathering processes of igneous rocks that have been occurring for millennia.			
6-Summary					
		The current EQC include critical • air pollutants and • wet and dry deposition pH. These can be measured accurately. They meet the requirements of being “clear, readily measured, able to be audited, with all with standardised approaches”. The team should be commended on achieving this major outcome with sound information to justify the recommendations.	Taken as a comment.	Noted	✓

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		The relationship of matters such as rock porosity, the effects of patina and weathered rind on weatherability, and modifications of spectral signals for exposed rock art need further consideration.			
7-Glossary and abbreviations					
		Briefly assessed but noted to be incomplete. For example, the terms porosity and permeability are not included in the Glossary	A definition of porosity has been added. Permeability does not appear to be mentioned in the document.	Noted. Comment. As the concepts porosity and permeability have similar interpretations, a short discussion explaining each and their differences, and how they are measured would be of value.	Permeability has been added to the glossary. The distinction between the two terms and typical measurement techniques has also been added.
8-References					
		Appear suitable and complete	Thanks.	Noted	✓
A-Appendix					
		The information presented graphically in the Appendices provide an extremely valuable summary of monitoring data, and is presented well, being compared with proposed EQC	Thanks	Noted	✓

2.5 Associate Professor Ron Watkins

Document Title:	Murujuga Rock Art Monitoring Program: Interim Environmental Quality Criteria Update 2025
Document:	COPP21065-REP-G-110
Reviewer:	Ron Watkins

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1	General / overarching comments	As with other MRAMP documents recently reviewed, this report overall is well-presented and free of the typographic and syntax errors that were commonplace in some earlier documents. I give my criticisms in a positive manner, and those referring to syntax and writing style are merely suggestions that may improve the text.	Thankyou. Taken as a comment.	✓ Corrections and modifications are well done with just a few that might be improved with further brief attention: 29, 32, and 36. The last mentioned <u>definitely needs to be addressed.</u>	Noted and addressed below.
2		I would direct the authors to the common definitions of Pollution verses Contamination in Environmental Geochemistry. Contamination is an amount of a substance above that	Noted and this is a valid argument, however we have chosen to utilise the terminology used in the air quality field nationally and internationally. E.g. see. https://www.dcceew.gov.au/environment/protection/air-	✓ OK. Point accepted.	✓

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		which would normally be present without external enhancement (usually through human activity). Since pollution is most commonly defined as a degree of contamination that causes a measurable detrimental environmental effect (normally health), the broad use of "pollutants" when referring to industrial and other emissions at Murujuga is to pre-judge their environmental significance.	quality/air-pollutants#criteria		
3		While I applaud the will of the authors to arrive at an all-encompassing Weatherability Index (WI) for Murujuga EQMF, it is, as recognised, a most complex undertaking. The currently proposed (interim) model is fraught with gross simplifications and choices where practicality/possibility is the over-riding criterion. This renders such an all-engrossing formula liable to be at best too imprecise/insensitive and at worst incorrect and	We recognise the limitations of the approach and agree that the risks are significant. However, the EQMF requires some kind of predictive and ideally process-based relationship that links rock weathering and atmospheric chemistry. We've added some text that we hope clarifies how seriously we are taking this issue "The overarching criteria for the WI as an EQMF is that it is suitable for use with an EQMF framework. That is,	✓ OK. Your argument is cogent and makes clear that severe limitations are understood and may persist to render the WI imprecise. I understand that reviews and refinement are scheduled to beyond the cessation of the strictly research phase of MRAMP.	Agreed and the decision to use the WI approach as the primary EQC/EQMF decision tool has not yet been made. The rock art strategy framework envisages 5-yearly review and improvement cycles at minimum.

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		misleading. While the WI is subject to refinement, I fear the opportunity to research and evaluate it satisfactorily will be too little. Certainly, there will be an on-going requirement to interrogate the results of its employment in EQMF and to develop and refine the model beyond the final year of research and testing.	the WI must be clear, readily measurable, and auditable, and it must be possible to devise standardised approaches for measuring and comparing the WI against the criteria. Rock weathering is complex, and many aspects are poorly understood or only semi-quantified, so it is inevitable that simplifications must be made to satisfy the criteria around practicality and transparency for EQMF components. To be clear, these simplifications are made when the information that would permit the testing and refinement required to support a more complex model does not exist. However, there is a risk that such simplifications may render the integrated formula too imprecise/insensitive to be useful, or even incorrect and misleading. This risk must be considered in the context of the value of		

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			an integrated formula, which is that rates of rock weathering can be compared across Murujuga for any set of air quality, temperature and geological input parameters. The effects of future changes can also be considered. These benefits are not available if only single parameters are considered (e.g., the concentration of a single gas species), and indeed, any model linking air quality to rock weathering encounters the same limitations around poorly constrained mechanisms and rates. Thus, while the limitations of the WI approach are acknowledged, and the specific uncertainties are discussed in detail below, the value of such an approach is considered sufficient to justify its consideration. The interim EQC described here is a starting point that		

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			will be improved as the data sets grow, if the WI model is implemented. For example, the WI model will be tested and refined: 1. When interim EQC are converted to EQC after submission of the Y4 report. At this point the microbiome and patinated chamber studies will be complete and this information can be integrated into the model. 2. On review of the EQC after two or three years. At this point in-situ monitoring of rock art panel pH and patinated rock located within the AQMs will have completed two or three cycles (Curtin University, 2025c), and will provide a more quantitative picture of real-time rock weathering that can be used to test, and then calibrate, the WI model. 3. In response to ongoing		

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			monitoring results including photogrammetry, which records flaking and other changes in rock surface condition.”		
4		Obviously, the veracity of the WI will become apparent during the Monitoring stage of MRAMP. My current thought, having reviewed this report, is that despite the multiplex of components addressed in the formula, the generalisation of the effects of so many, may make it impotent, relying on a very few measured variables, notably pH, as the crucial determinant. The need to rigorously study pH and other geochemical variables at Murujuga and to be able to reason with these individually, as well as part of a WI, is most important.	Agreed. Taken as a comment and addressed by the response to 3.	✓	✓
5		While the quest for a perfect WI is a holy grail, I consider it will not be gross factors, such as the five lithologies and broad models of mineral	This is an interesting point and it will be possible to interrogate these smaller scale variations using the	✓ Agreed.	✓

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		dissolution that will determine the impact of local anthropogenic emissions at Murujuga, but variations of a small, localised scale. Hence, minor-scale variance of texture, mineralogy, degree of natural weathering, as well as micro-environments, may be the determinants of the conservation risk of individual, and individual groups of, petroglyphs.	patinated slabs that will be placed in the AQMs. By completing a detailed textural analysis of the same rock on a yearly basis, we will be able to test these hypotheses. This has been highlighted in the interim EQC document (Section 5.6.1)		
6		I feel there has been absence of sufficient rigour applied to results inorganic geochemical analysis. Here I draw attention to one example, which is notable in having persisted into the present Report after having been commented upon in the previous MRAMP Report. I have noted it briefly where it is discussed as review point 12. In the text regarding the durability of individual minerals under weathering, it is concluded that plagioclase feldspar has the greatest susceptibility to	These are valid comments and we have considered and discussed this and will continue to do so. AI will likely be incorporated into the next round of revisions and analysis. As noted, the initial results found a high variability in AI. These were largely the results of combustion experiments. Engines (such as that used for the combustion experiments) contain a large number of aluminium containing metals, some of which will mobilise	✓ I am very happy with this positive response and the plans to interrogate the AI results to a greater degree in future.	Thank you

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		dissolution and, in the case of the laboratory weathering chamber studies, removal in leachate. The predominant element in plagioclase is Al. Aluminium is also a major constituent of the secondary breakdown products of feldspars and pyroxene - chlorite, epidote and clays - that predominate in each of the five major Murujuga rock types. Despite the abundance of Al, it is excluded from discussion of the results of the Chamber studies as made clear: "The concentration of Al showed variability that may be attributable to contamination by Al-bearing components in the experimental apparatus, so this element is not discussed further here". I find this sentence quite remarkable. In the two most recent MRAMP Reports, the reason given is that Al may be released from Al-bearing components of the experimental apparatus,	into the engine oil and also exhaust gases. https://www.alsglobal.com/-/media/ALSGlobal/Resources-Grid/Equipment-reliability/Technical-Bulletins/Oil-and-Lubricants-Addressing-aluminum-measurement-issues-in-engine-oil-analysis.pdf Now that we have largely switched to bottled gas exposures there is no reason not to utilise the Al data apart from consistency with the previous results. The reviewer raises some good points regarding analysing the ICP count data and we are likely to examine this.		

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		BUT these components and the source of the AI have never been described. Furthermore, analytical blanks where no sample rock is present in the chamber yielded no measurable AI. Assuming the blank experiments were performed correctly, this is clear evidence that AI occurring in the experimental leachates is NOT derived from the apparatus. The reason for its exclusion from discussion seems to be solely that the analysed concentrations of AI do not conform to a strict dose:response curve. I would argue that detailed consideration of the mechanisms for element release from rocks in the experiments would very likely produce variability in the results given factors such as the presence of AI in different minerals of different solubilities, and expected small-scale mineralogical heterogeneity in the rock, both in terms of mineral			

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		distribution and orientation. The source of the AI and its variable concentration in results of analysis are fertile grounds for a research program such as MRAMP, yet there is no evidence in the Report that basic interrogation of the ICP-AES analysis has been performed. For example, what is the background equivalent concentration (BEC) and precision of the AI response at the chosen analytical wavelength. Without such consideration, normal in a research study, there is no sense of the analytical precision for AI in the optical analysed range and, therefore, the veracity of the results. The Report states that the ICP-AES analysis was performed at the Chemistry Centre, a highly reputable establishment. However, the terms under which the Centre undertook the analysis is not clear. Was a Centre's research scientist involved in MRAMP or was			

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		the ICP-AES analysis performed on a routine basis commissioned by MRAMP? Whichever is the case, the Centre would have made available the basic analytical parameters and result output - point counts per second - from which the "variability" of the AI results, and the lack of variability in other element results, could be satisfactorily interpreted. It is necessarily speculative on my behalf, but I suspect the "variability" in AI is telling much about the release mechanisms of elements during the chamber experiments and the weathering of rocks at Murujuga. In which case, to simply exclude AI from the discussion, and further consideration in the MRAMP program, would appear very misguided.			
1-Overview					
7	1.1	Page 1, Para 3, Line 9: re. "which has more complex interactions" Than what? Doesn't read well. Clarify.	Revised to: "...which has more complex biogeochemical interactions compared to single gas species,"	✓	✓

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8	1.1	Page 1, Para 5, line 2: add "concentration" before "range"	Done	✓	✓
9	1.1	Page 1, Para 5, line 8: Possibly "heavy fuel oil (HFO)" is better than "shipping fuel" in the context of this sentence?	Done	✓	✓
2-Methodology for EQC development					
10	2.2	Page 5, Para 4, line 5: It is pertinent to mention here that the ICP-AES measurements are performed on solutions, not on rocks directly. Therefore, dilution factors apply according to sample preparation. The "detection limits" listed appear to be those "in solution", so can the reader assume that all measurements were performed directly on samples (e.g. rainwater) without dilution?	ICP-AES results reported here are performed on 15 ml ultrapure water samples used to do a pre/post exposure "wash" on each rock cube used in the chamber studies. Measurements were kept as a solution concentration as the paired pre/post measurements on identical water volumes are compared. If it is desired to determine dissolution rates these could later be calculated based on mass or surface area of the sample however this has not yet been undertaken. For field deposition samples (elemental analysis not reported in this document), ICP analysis is undertaken and this is calculated as a	✓	✓

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			deposition rate (mass per surface area). pH values reported here were measured undiluted for wet samples or using a defined quantity of ultrapure water supplied by the ChemCentre. The methods are all published in separate documents and it was decided not to reproduce the full method detail here.		
11	2.2	Page 5, Para 5: I note you have switched to quoting "LOR" from previously "detection limit" (such as LLD). The principle behind these two terms is different and some brief mention of this would be prudent.	LOR is used as it is the terminology used by the ChemCentre, however we also obtain raw data. LOR and LOD are now both included in the glossary.	✓	✓
12	2.2	Page 5, Para 5, line 5: I find the discussion on the interpretation of aluminium (Al) perplexing. As it may reflect a lack of rigour in the accounting for the results of ICP-AES analysis, I have commented in detail in my General comments - point 6.	Refer to comments above	✓	✓
13	2.2	Page 5, Para 6, line 1: The	Edited as suggested	✓	✓

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		use of "hosted" might give the impression that all released elements were within the primary minerals (bonded within the atomic crystallographic structure). This ignores those adsorbed onto crystal surfaces which may contribute significantly to the leachate. In the text "bound within or adsorbed externally" may be better.			
14	2.2	Page 6, Para 1, line 1: Do you consider that sanidine could be present in rocks of this age?! The high-T (volcanic) sanidine structure can be replicated in some low-T environments but this is not the primary sanidine mineral implied here.	Changed to "One of the polymorphs of alkali feldspar is the most likely source of" to avoid distraction from the narrative.	✓	✓
15	2.3	Page 7, Para 1, line 5: Re. "SO ₂ is much more reactive and diffusive than NO ₂ ." is not very meaningful unless the nature of the reactivity is stated. i.e. it more reactive doing what? Note that "more diffusive" is fully understandable.	Amended as suggested	✓	✓
16	2.3	Page 7, Para 2, line 8: Re. "are believed to have been most likely to be responsible	Done	✓	✓

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		for the elevated porosity found." ?? May I suggest "are most probably responsible for the increased porosity".			
17	2.3	Page 7, Para 2, line 10: "for limited benefit". I find this conclusion a little surprising since: 1. Previous SO ₂ emissions may have had a role in pre-conditioning the rocks for subsequent (current) decomposition given their greater reactivity; and 2. that SO ₂ from former energy plants and industry has been cited for the current distribution of increased porosity.	Agreed. The term "limited benefit" has been removed.	✓	✓
18	2.4	Page 7, Para 3, line 2: This sentence is rather vague for non-chemists. Henry's Law determines the solubility of gases in a liquid.	".. of gases in liquids" added for clarity.	✓	✓
19	2.5	Page 8, Note on Figure 2: I have commented in my review of the Summary document upon the rather indistinct reproduction of the colours on this same diagram. Here the orange and red coloured areas are	Noted	✓	✓

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		both distinct and true to their description, suggesting that the diagram elsewhere was 'washed out' as a result of its reproduction.			
20	2.5	Page 8, Para 1, line 4: Do you instead mean "calcareous strata" or "carbonate-bearing strata" as these may act to neutralise acidic rainfall?	Amended to "carbonate-bearing strata"	✓	✓
3-Updated interim EQC					
21	3.1	Page 9, Table 1: Does the unit $\mu\text{g}/\text{m}^3$ require refinement with addition of "at standard T and P" or "at 25°C"? The caption uses "IVL" which hasn't been defined previously in the text and doesn't appear in the Glossary.	STP specified IVL added to glossary	✓ ✓	✓ ✓
4-Monitoring against the Interim EQC					
22	4.2	Page 11, Para 1, line 2: I'm not sure about "for monitoring against", here and elsewhere in the document. Suggest the alternative "for monitoring and comparison with".	Amended as suggested	✓	✓
23	4.3	Page 12, Para 3, line 1: Does your intended meaning require the second	Done	I believe "no - removed" is meant. ✓	Yes. The second "average" word was removed.

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		"average"?			
24	4.4	Page 12, Para 4: I can understand the difficulty, but does this strategy enable the recognition and quantification of single events of very high emission of relatively short duration? Such incidences are commonly the prime driver of significant pollution events.	This section is more about a workable definition of an exceedance. All events would be investigated. An air pollution event of sufficient magnitude to induce a strong response in terms of monthly averaged pH would likely be measurable by real time instruments and it is hoped could be detected and acted on almost immediately.	OK - satisfactory response.	✓
5-Combined weatherability index					
25	5.4.1	Page 14, Para 4: line 1: change full stop to a colon - "involves two steps:"	Done		
26	5.4.2.1	Page 16, Para 4, line 4: Where "(square brackets)" is written twice, the second one is redundant and can be removed. Further, you should add a reference to "Equations 3 - 7", since square brackets do not feature in Equation 2 as currently referenced.	Done Modified "Activities are used in Equations 3 to 6 and subsequently."	✓ ✓	✓ ✓
27	5.4.3	Page 18, Para 1: As I have pointed out in other peer reviews, I believe it is more appropriate	We do understand that there is value in using a term that provides genetic information.	✓ A pragmatic response which I am sympathetic to.	Thank you

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		to use "crystals" rather than "grains" when referring to igneous and metamorphic rocks. Here is a particularly pertinent case since sandstone is described appropriately using "mineral grains". I say this in an advisory manner for the sake of clarity and convention, as the use of "grains" in description of igneous and metamorphic rocks is not unusual.	However, there is also the risk that the name is contingent on correct interpretation. Further, we are reluctant to modify the term, rendering this report inconsistent with the other reports, so we are retaining 'grain'.		
28	5.4.3	Page 18, Para 1, Table 3: I think there may be mistakes in the "minerals" column? Labradorite plagioclase does not characteristically occur in a granite; and albite (Na-plagioclase) is not typically a dominant component of granophyre. In both rock types the predominant feldspars are alkali feldspar (orthoclase, microcline).	We have clarified these matters in the text and removed 'labradorite' from the rate-controlling mineral for granite. Added text reads "While K-feldspar is typically more common than the plagioclase feldspars in unaltered granite and granophyre, the extensive hydrothermal alteration has converted much of the K-feldspar to albite. In addition, the plagioclase feldspars dissolve more quickly than K-feldspar. Rate-controlling	✓ ✓ A wholly acceptable response to both points. .	✓ ✓

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			minerals are summarised in Table 3."		
29	5.4.3	Page 19, Para 1" With the possible exception of some mono-mineralic rock types, e.g. quartzite, dunite, chalk, I would suggest that congruent dissolution is NOT the case. Consequently, to consider only the rare congruent dissolution would seem to be inappropriate and a gross simplification. To add this as a standardised factor to be applied ubiquitously to all (5) Murujuga rock types and to all sample rocks, whether or not, detailed mineralogical study has been undertaken, I find extremely dubious.	We agree that congruent dissolution is highly unlikely, as evidenced by the abundant clay minerals. However, we are looking for the rate-controlling steps that ultimately lead to rock disintegration. The clay minerals are rheologically weak and undergo continuous further alteration which is difficult to characterise and represent. However, their weakness means that they don't hold the rock together in the same way that crystalline minerals do, and the processes are faster than the dissolution of the primary minerals. Further, the clays weather out more quickly than the precursor minerals, based on the void-rich area at the top of many of the thin sections. This indicates that chemical weathering of the clay minerals is faster than chemical weathering of the precursor minerals – this is	✓ ✓ I appreciate your detailed explanation, but see below. I am not going to argue for further modification of your additional text, if you are happy with its inclusion. It is, however, lengthy and could be simplified and, thus, shortened, if you choose to do so.	✓ Noted. We will take attempt to simplify and streamline the text in the final (year 4) EQC document.

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			within the weathering profile and may relate to gradients in intensive variables such as pH within the profile as well as absolute dissolution rates. The decision to focus on congruent dissolution is justified by these factors. Additional text added to clarify. "Clays are a common product of incongruent dissolution. The widespread distribution of clays within the weathered rind at Murujuga indicates that incongruent dissolution operates to some extent within the weathering rind. For this reason, it is, on first sight, appealing to include incongruent dissolution within the WI approach. However, the rates and characteristics of the processes that form and subsequently modify clays are not well known and a semi-quantitative parameterisation of those processes of the type undertaken here is difficult to		

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			test. Further, clays are rheologically weak compared to their precursor minerals, so clay formation reduces the strength of the rock such that physical weathering of a clay-rich rock is fast relative to physical weathering of a fresher rock on Murujuga. Finally, SEM-BSE images of the outer layers of many of the rocks show that the lower parts of the weathered rind are richer in clay minerals than the upper parts, where voids are more common (Curtin University, 2025b). This indicates that the clay dissolution zone, which can be visualised as a volume that separates the region where clays are dominantly weathered out of the rock from the region where clays are more common than voids, advances more quickly than the mineral dissolution zone, which separates the fresher rock from the region where most of the fresher rock minerals have been replaced by clays. This observation indicates		

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			that chemical weathering of clays is faster than chemical weathering of the precursor fresher rock minerals."		
30	5.4.3	Page 20, Para 2, line 6: "permit promote"?? I suggest you omit "promote".	Done	✓	✓
31	5.4.3.1	Page 20, Para 3, line 5: To use relative surface area and duration of mineral dissolution alone in calculating the WI would seem, once again, to be a critically gross over-simplification. For example, ingress of water/atmosphere into a coarsely crystalline granite is likely to be greater than a fine-grained aplite since large crystal faces and fracture/cleavage within the larger mineral provides preferential pathways absent in a more uniformly fine-grained version of the same granitic rock. The presence of joints and the growth patterns of secondary minerals are just two more of the multitude of factors that that will determine the rate of weathering.	This point is acknowledged with the following text "This approach is a simplification. Features such as cleavage, non-spherical grain shapes, variably developed porosity, and fractures mean that the effective surface area is different to the calculated surface areas by an amount that is unknown. However, it is unlikely to be greater than the orders of magnitude differences in surface area that relate to grain size--volume relationships. However, it is possible to perform effective surface area measurements on the Murujuga rocks to reduce this source of uncertainty. This work will be recommended if the interim EQCs are retained in a form	OK. An excellent response and future directive.	Thank you

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			similar to that proposed here.”		
32	5.4.3.1	Page 20, Para 4, line 1: I appreciate you are presenting an illustrative example, but to hint that crystals in the Murujuga rocks might be approximated as spheres (i.e. equi-dimensional), is a very long bow to draw. An equivalent example might be that 1 gm of pyroxene (that would sit on a thumb nail) may have a surface area that might cover your palm, whereas the surface area of 1 gm of clay might cover a tennis court.	See response to comment 31. Also, text added “Further, the reactive surface area of minerals depends on the mineral itself. For example, the reactive surface area of a clay mineral is commonly higher than that of a pyroxene of equivalent grain size.”	I would replace "commonly higher" with "vastly greater" in the added text.	"commonly higher" was replaced with "vastly greater"
33	5.5.1	Page 21, Para 5, line 3: Re. "indicating that the calculation provides a conservative estimate of rainfall pH" requires clarification. The meaning of the term 'conservative' here in isolation is unclear. From Figure 5 it is apparent that you mean the estimate is of lower pH value.	Text clarified to read “provides a conservative lower estimate of rainfall pH”	✓	✓
34	5.5.1	Page 23, Figure 5: Add label 1/1/2025 on the x-axis of panel (a).	Done	✓	✓
35	5.5.2	Page 24, Para 3, line 3:	Done	✓	✓

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		change "It's" to "its".			
36	5.5.2	Page 24, Para 3: I do not necessarily agree with the perceived correlation of the order of weatherability (WI) and abundance of petroglyphs "to a large extent". Many other factors come into play, such as presence of large relatively flat surfaces, attitude of surfaces, degree of surface colouration and presence/absence of patina. Furthermore, such correlation might imply the ancient rock engravers had knowledge of the Weathering Index! Without that assumption, any relationship, if indeed present, is merely serendipity. Note also, that the described roughness of the gabbro rock surface (compared to that of granophyre) is somewhat mitigated by the presence of a more distinct surface patina. I'm unsure if the density of petroglyphs on each lithological type has been measured? The lower incidence of gabbro outcrops	Additional text added to clarify that there is no implication that WI was a factor in the choice of substrate for rock art. (As an aside, we don't think it an unreasonable suggestion but certainly unproven). The point of interest here relates more to the rate of rock art loss and preservation potential – hopefully this is now clear, new text reads “It is also interesting to compare the relative weatherability with the abundance of rock art on each rock type. Granophyre, which is the most abundant rock type, hosts the most rock art (Donaldson, 2011), whereas rock art is sufficiently rare that is difficult to find on basalt. Obviously, multiple factors contribute to whether a particular rock surface of a particular rock type was selected as the site for rock art production. For example, large relatively flat surfaces,	? The additional text is fine and, in general, adds clarification. However, while my comment (36) revolves around the relative abundances of petroglyphs on granophyre and gabbro, the two most abundant rock types in terms of outcrop, the revised text discusses granophyre and basalt. My understanding is that basalt outcrop is minimal at Murujuga and limited to narrow dykes and possibly the margins of gabbro intrusions. Some of my comments, such as surface roughness and more distinct patina, do not apply to the finer-grained basalt, while others, namely rock art is hard to find on basalt, is valid. I wonder if "gabbro" and "basalt" might have been transposed during the writing of the new text? If not, and a comparison of granophyre and the minor basalt was	Basalt was the intended rock type here. Comments on gabbro have been added and the text has been reorganised to read “It is also interesting to compare the relative weatherability with the abundance of rock art on each rock type. The distribution of rock art today reflects a combination of the rates of rock art production and loss. Multiple factors contribute to whether a particular rock surface of a particular rock type was selected as the site for rock art production. For example, large relatively flat surfaces, which are most commonly provided by granophyre, may have been attractive for rock art. In addition, the orientation of surfaces, degree of surface colouration,

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		and their preponderance in narrow valleys are factors. Given the sum of uncertainty outlined above, I suggest this paragraph would best be omitted from the document.	which are most commonly provided by granophyre, may have been attractive for rock art. In addition, the orientation of surfaces, degree of surface colouration, presence/absence of patina, position within the landscape, and proximity to resources such as water are likely to have been influential. However, the rate of rock art loss also affects the abundance of rock art on a particular rock type. So, for example, it is possible to speculate that high proportion of rock art on granophyre, and the low proportion on basalt, reflects, to some extent, the low WI of granophyre and the high WI of basalt."	meant, some reference to gabbro, as the second most widespread substrate to the rock art, is required.	presence/absence of patina, position within the landscape, and proximity to resources such as water are likely to have been influential. Indeed, granophyre, which is the most abundant rock type, hosts the highest number of rock art sites (Donaldson 2011). Gabbro is the second most abundant rock type, and host the second-highest numbers of rock art sites. Typically, gabbro is rougher than granophyre, making rock art production more difficult. However, gabbro also tends to have a thicker patina than granophyre, so that rock art could be inscribed deeply. In contrast, basalt has a smooth surface, but rock art on basalt is sufficiently rare that is difficult to find. The WI is directly related to the rate of rock art loss. Therefore, it is possible to

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					speculate that the high proportion of rock art on granophyre, and the low proportion on basalt reflects, to a greater or lesser extent, the low WI of granophyre and the high WI of basalt, as well as other factors discussed above."
37	5.6.2	Page 34, Para 4, line 4: modify text to read "... of the rate-controlling primary minerals at Murujuga".	Amended as suggested	✓	✓
7-Glossary and abbreviations					
38	7	Definition of "detection limit" as used in the text, as DL or LLD, could usefully be included.	Done	✓	✓
		IVL might also be included as it is not otherwise defined in the text.	Done	✓	✓

3 Methodology Statements

This section provides the independent peer review reports undertaken between April 2025 to May 2026 for the following component study methodologies (Methodology Statements):

Organic Geochemistry of Rock Surfaces, Dust and Particulates

The final methodologies will be incorporated into the standard operating procedures for the ongoing monitoring program, where relevant.

3.1 Professor Geoffrey Michael Gadd

Document Titles:	Methodology Statement: Organic Geochemistry of Rock Surfaces, Dust and Particulates
Document:	COPP21065-PRO-G-113
Reviewer:	Professor Geoffrey Gadd

Item No.	Section	Peer reviewer comment Document revision: 4 Date: 29 September 2025	Curtin MRAMP team response Date: 5 January 2026	Peer reviewer close-out comment Document revision: 4 Date: 23 February 2026	Curtin MRAMP team close-out comment Date: 20 April 2026
1	General	A detailed and comprehensive account of the methodologies used in the organic geochemistry analyses.	Taken as a comment.	OK	✓
1. Purpose and scope					
		The aim of this research is “to identify organic compounds present on rock surfaces with the potential to cause accelerated degradation of petroglyphs, either through interactions with specific minerals or the microbiome”. However, there is no detailed mention here of how the organic geochemistry work is linked to or integrated with the microbiome studies. There is also little mention of this in the main report.	As the reviewer would be aware, organic chemicals on the rock surface are either from biological or petrochemical origin. The goal of this work is to understand these relative contributions as far as possible. Noting that the identification of unknown compounds which may be degraded after prolonged environmental exposure, is one of the most complex aspects of organic chemistry. Links between the organic chemistry findings and other component studies are being	OK	✓

Item No.	Section	Peer reviewer comment Document revision: 4 Date: 29 September 2025	Curtin MRAMP team response Date: 5 January 2026	Peer reviewer close-out comment Document revision: 4 Date: 23 February 2026	Curtin MRAMP team close-out comment Date: 20 April 2026
			explored for inclusion in the following report.		
2. Description of tasks and/or activities					
		No comments.			
3. Equipment and consumables required					
		No comments.			
4. Required training					
		No comments.			
5. Detailed procedures					
		No comments.			
6. Additional information					
		N/A			
7. Other documents required					
		No comments.			
8. Risk management					
		No comments.			
9. References					
		No comments.			
Appendix A Organic geochemistry detailed procedures					
		No comments.			

3.2 Dr John Henstridge, Data Analysis Australia

Document Titles:	Methodology Statement: Organic Geochemistry of Rock Surfaces, Dust and Particulates
Document:	COPP21065-PRO-G-113
Reviewer:	Dr John Henstridge, Data Analysis Australia

Item No.	Section	Peer reviewer comment Document revision: 4 Date: 21 October 2025	Curtin MRAMP team response Date: 5 January 2026	Peer reviewer close-out comment Document revision: Date: 4 March 2026	Curtin MRAMP team close-out comment Date: 20 April 2026
1	General	This document has nothing that is directly statistical and hence I am unable to comment except to point out that some of the procedures appear to take samples from rock surfaces that may be difficult to replicate. Examples are the depth to which rock surface is prepared and how rock samples are broken up to expose surfaces. Failure to achieve consistency in such areas can lead to problems in long term monitoring.	Noted. Generally, the approach aimed to recover a similar mass from each sample where possible. The Dremel approach cannot sample the 'fresh-ish' (non weathered) rock well (due to hardness), so a rock with a thicker weathered rind will result in a higher sample mass per unit surface area. However, the technique essentially samples the region with softer/weathers or more porous rock, which is the zone most likely to contain biological or petrochemical compounds. In hindsight, relating the measurements to both	Acknowledge	Noted

Item No.	Section	Peer reviewer comment Document revision: 4 Date: 21 October 2025	Curtin MRAMP team response Date: 5 January 2026	Peer reviewer close-out comment Document revision: Date: 4 March 2026	Curtin MRAMP team close-out comment Date: 20 April 2026
			sample mass *and* sample surface area may have been desirable.		
1. Purpose and scope					
2. Description of tasks and/or activities					
3. Equipment and consumables required					
4. Required training					
5. Detailed procedures					
6. Additional information					
7. Other documents required					
8. Risk management					
9. References					
Appendix A Organic geochemistry detailed procedure					

3.3 Associate Professor Ron Watkins

Document Titles:	Methodology Statement: Organic Geochemistry of Rock Surfaces, Dust and Particulates
Document:	COPP21065-PRO-G-113
Reviewer:	Dr Ron Watkins

Item No.	Section	Peer reviewer comment Document revision: 4 Date: 21 October 2025	Curtin MRAMP team response Date: 5 January 2026	Peer reviewer close-out comment Document revision: Date: 4 March 2026	Curtin MRAMP team close-out comment Date: 17 April 2026
1	General	This document describes the individual methods and procedures employed in the organic geochemistry study within the MRAMP. There is occasional mixing of tenses with the text but all instructions are clearly explained and readily understandable. Accordingly, I need offer only a small number of minor possible improvements.			
1. Purpose and scope					
2. Description of tasks and/or activities					
2	2.5	Page 2, Para 3, line 5: "until all organic matter has been extracted from the powder". How is this determined?	Changed. Reads now "...the process is repeated until the solvent remains colourless."	✓	✓

Item No.	Section	Peer reviewer comment Document revision: 4 Date: 21 October 2025	Curtin MRAMP team response Date: 5 January 2026	Peer reviewer close-out comment Document revision: Date: 4 March 2026	Curtin MRAMP team close-out comment Date: 17 April 2026
3	2.6	Page 2, Para 4, line 1: "laboratory" is preferred to "lab"	Corrected	✓	✓
4	2.6	Page 2, Para 4, line 2: Sentence beginning "Sample (10g or less) is weighed " does not read well. Replace "and" with "together with".	Corrected	✓	✓
5	2.11	Page 3, Para 4, line 4: This may be better: "The compounds pass into the ion source of the mass spectrometer"	Changed	✓	✓
6	2.12	Page 3, Para 5, line : add "for" after "...used analytical technique"	Added	✓	✓
7	2.15	Page 4, Para 3, line 1: It is either a technique, or it is a device!	Changed. Reads now "The technique is used to determine trace levels of organic analytes in air.	✓	✓
3. Equipment and consumables required					
4. Required training					
5. Detailed procedures					

Item No.	Section	Peer reviewer comment Document revision: 4 Date: 21 October 2025	Curtin MRAMP team response Date: 5 January 2026	Peer reviewer close-out comment Document revision: Date: 4 March 2026	Curtin MRAMP team close-out comment Date: 17 April 2026
6. Additional information					
7. Other documents required					
8. Risk management					
8	8	Page 8, Para 6, line 2: Sentence does not read correctly - "includes identifies of"?	Corrected to "identities"	✓	✓
9. References					
Appendix A Organic geochemistry detailed procedure					