Dear Jack Middleburg,

Thank you for the opportunity to provide a revised submission of our paper to BG. The re-submission includes the mark-up draft of the paper, this cover letter and response to the reviews, the supplemental data section (unedited), along with the final copy for review. We have endeavoured to follow the advice of our reviewers. Many of the comments were highly insightful and we appreciate the feedback. The location of these edits is indicated in the specific itemized responses provided below.

Noting comments made by BGs editorial staff about the color choice to our figures, we have taken this opportunity to also revise the figures to make them clearer for those who are color impaired. This has taken a great deal of time and we struggled with issues of whether the changes would make the figures harder to read (especially if printed in blank and white). If these changes are not deemed an improvement, we would like to request specific suggestions on how to make them acceptable.

Lastly, we also took the liberty to add an acknowledgements section to the end of the paper that includes appreciation for your efforts and those of the three reviewers.

Sincerely,

Todd

Submitted on 13 Jun 2022
Anonymous referee #3

Anonymous during peer-review: Yes No
Anonymous in acknowledgements of published article: Yes No

Recommendation to the editor

1) Scientific significance Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?	Excellent Good Fair Poor
2) Scientific quality Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?	Excellent Good Fair Poor
3) Presentation quality Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)?	Excellent Good Fair Poor

For final publication, the manuscript should be

accepted as is

accepted subject to **technical corrections**

accepted subject to **minor revisions**

reconsidered after **major revisions**

rejected

Were a revised manuscript to be sent for another round of reviews:

I would be willing to review the revised manuscript.

I would not be willing to review the revised manuscript.

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Manuscript bg-2021-245

"The influence of near surface sediment hydrothermalism on the TEX86 tetraether lipid-based proxy and a new correction for ocean bottom lipid overprinting"
by Bentley et al.

Here the authors use novel GDGT date to determinine the potential overprint of sedimentary tetraether production on the TEX86 proxy and associated water temperatures near a hydrothermal vent system. The authors propose a way to correct for this. However, upon reading the manuscript it seems to me that this impact maybe be particularly important for hydrothermal vents. It remains unclear to what extent the processes as observed in this specific 'anomalous endmember' are more widely applicable and I am not convinced that a correction is generally needed. General statements such as those at the end of the abstract ("A model to correct the overprint signal using IPLs is therefore presented that can similarly be applied to all near-surface marine sediment systems where calibration models or climate reconstructions are made based on the TEX86 measure") or conclusion section are therefore not completely justified (see specific comments below). However, overall, I feel that the manuscript is generally well written, very interesting, and after some minor revision, can be published.

Comments:
Line 95 to 133. I question the need for these sections in the introduction. Both sections are about non-thermal influences/driving forces. This is not what the manuscript is about.

The section is quite important as a foundation to section 3.4. As such we have not removed this section, but we have considerably shortened the text. Additional comments to the end are provided below.

Figure 1. The quality of this figure needs to be improved. Suggest adding proper labels to the

indivual panels and changing the background colour of panel C to make the colours of the push core transect stand out a bit better. In addition, 'Photo' in the figure caption should be 'Photos'.

This figure (and all subsequent figures) have been revised. For figure 1, all recommended changes have been made. For the following figures, we have revised the color scheme to conform to recommendations provided by BG (https://helpx.adobe.com/photoshop/using/proofing-colors.html). The color scheme for samples follows the pallet below.

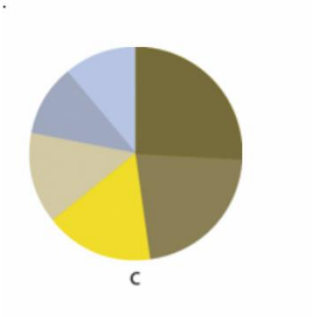

Table 1 (and 2). The table headings lacks detail. Suggest making clear that this is data from sediment push cores that were collected along a transect at the Cathedral Hill.

The correction is made.

Line 249 and 250. Remove 'then' (2 times).

Done.

Line 334. The abbreviation BIT, CBT and MBT are not introduced the first time used. In fact, if I am correct, they are only used once throughout the whole manuscript. I suggest removing these completely and change the sentence to ' …records such as TEX86 are based….'

The mention of these other proxies has been removed.

Figure 4. It remains unclear to me why the three panels are not of equal size? It currently suggests that panel A is of greater importance.

The figure has been revised and all panels are of the same size.

Line 489. For clarity and to avoid confusion I suggest that the authors make clearly that the clusters are grouped together based on their presence in unique thermal zones immediately and not a few lines later. In addition, it would be helpful if this information is added to the figure caption of figure 5 as well.

The suggested change has been made.

Line 498-501. Based on lipid analyses is there any evidence that would justify that the hyperthemophilic Methanopyrus kandleri could indeed contribute and would fall into cluster C?

We do not have lipid or genomic data to reasonably link the hyperthermophile. So as this is speculation, we have decided to remove the sentence.

Figure 5. What does the 'R2 = 0.42' refer to in Panel A?

The value was to indicate the overall sample calculation. This has been removed from the revised figure.

Line 587-588. Please make clear that this is only tested in this very 'anomalous endmember' environment (line 136) and has not been tested across more 'common' environments.

The sentence has been amended following the recommendation.

Line 625-627. I feel that this final statement is not fully justified, given that it is only tested on one very specific environment, and too speculative. The authors do not present a clear case why this can/should be extended to all near-surface marine sediment anywhere in the manuscript. In fact, the manuscript does not need a statement such as this at the end of the conclusion section. It contributes only little (if anything). I therefore suggest removing it completely. The last line of the abstract (lines 53-55) should be adjusted accordingly.

We respectfully disagree with this point. Section 3.2 details an exact case where TEX86 when applied to other "non-hydrothermal" Guaymas Basin sediments and does not accurately produce SSTs. For McClymont et al. 2012, the application of $U_{37}^{k'}$ did produce better matched temperature reconstructions for more modern satellite derived MASST. Also, as evidenced in the introduction, there are many instances where the proxy does not work. Based on past reported literature and the outcomes of our study, we feel the final sentence of the conclusion does merit an allowance of speculation on our part. We also feel the readers of BG would like to entertain that though in reflecting on whether the presented model could be a benefit to their own research applications.

Submitted on 22 Jun 2022
Anonymous referee #1

Anonymous during peer-review: Yes No

Anonymous in acknowledgements of published article: Yes No

Recommendation to the editor

1) Scientific significance Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?	Excellent Good Fair Poor
2) Scientific quality Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?	Excellent Good Fair Poor
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I read the rebuttal and the revised manuscript. Overall, the authors did a reasonable job in rebutting the comments and revising the manuscript though a number of comments of both reviewers were not incorporated in the manuscript, either because the authors did not understand or considered e.g. 'excessive' or 'obvious'. That is a bit disappointing to read as comments were made in an honest effort to improve the manuscript.

Having said that, I think the manuscript has improved and I have a final few minor points which I recommend to consider before the manuscript is published.

Key point no. 3: I think this statement should be nuanced. The correction can only be applied in modern systems and hence will only be indirectly of use for paleostudies.

We have changed the statement to read: "A diagenetic correction model is presented to remove overprinting artifacts in the TEX_{86} proxy."

Introduction. I asked this to be shortened as it is well known for quite some time that TEX86 is not an SST proxy. I see, however, no rebuttal to this comment and also no shortening of the introduction.

The introduction has been substantially shortened.

l.98-100. It has now been well established that MGII do not produce any GDGTs based on genetic studies. See Zeng et al., 2022, Nature Communications.

We have removed the sentence suggesting MGII may contribute GDGTs.

L 115. No reference given for this statement.

This sentence was removed during the revision of the introduction.

l. 307, 326. The fact that you report concentrations (or rates in line 307) in too many significant numbers in your companion paper is no excuse not to do it here. It simply is incorrect to report it in this precision. The Table 1 does a better job at this, so why not be consistent?

The correction has been made.

Line 588. Change measurements into values and add" .. in hydrothermal settings", i.e. you have demonstrated an impact of benthic microbial communities in this setting but not elsewhere (yet). I also commented previously that benthic AOM microbes also imprint the sedimentary signature and that this should be acknowledged. However, I may have missed this in the revised version.

We have changed the sentence to read: "Although, this study demonstrates the benthic microbial community can influence TEX_{86} values in anomalous, end-member environments; the above model has not yet been tested across conventional ocean shelf environments." We believe the previous mention of

AOM microbes imprinting is related to the Schouten et al 2003 study, which is referenced both in the above paragraph as well as the introduction of this paper. We also are of the understanding that AOM overprints are usually observed with archaeols, which do not factor into the calculation of the TEX_{86} proxy.