

Discovery of stromatolite formation in post-impact hydrothermal lacustrine environments and its implication for early Earth

Corresponding Author: Dr Jaesoo Lim

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr Lim,

Your manuscript titled "Discovery of stromatolite formation in post-impact warm lacustrine environments and its implication for early Earth" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Additionally, the following editorial thresholds should be met in the revised manuscript for further consideration in Communications Earth & Environment:

1. Provide stronger evidence that the stromatolites are biogenic, with clearer sediment and geochemical analyses to rule out abiotic origins.
2. Support claims of hydrothermal activity and meteoritic signals with independent and consistent evidence.
3. Clarify the timing of events and microbial processes, including uncertainties in dating and organic matter sources.
4. Reframe speculative links to oxygen production and the Great Oxidation Event as hypotheses rather than conclusions.

If additional work allows you to either incorporate or refute these criticisms, we will be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish

to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript, titled Discovery of stromatolite formation in post-impact warm lacustrine environments and its implication for early Earth investigates stromatolites found around the margin of a post impact lake. It is interesting to consider the role of impacts on early Earth in making habitable environments, however there a number of issues that must be addressed before this work is suitable for publication.

Overall, I find the paper to be well written, and the dating conclusions to be rational. The authors are (rightly) not using the dates as absolute, but attempting to demonstrate that the stromatolites formed after the impact. I am not an expert on the Os system, so I hope another reviewer can add insight to this portion, but the conclusions on this seem sound to me. The stromatolites incorporate the Os signal, indicating that they formed after the impact, and the ¹⁴C dating, which is admittedly fraught in a system like this, does support this conclusion, even with the reversals. I would not expect the reservoir effect of carbon in a lake this small to offset the dates by 10s of thousands of years. So I am with the authors on the idea that these stromatolites formed in a post-impact lacustrine setting. My issues (addressed below) stem more from the theories of stromatolite formation, and the DNA evidence

Major Issues

The stromatolite definition given is a common one, but not one used by the community when approaching a new site, as it is genetic and assumes that we know how the stromatolites formed, and that microbes were involved. When investigating a new formation, it is best practice to use the Semikhatov 1979 descriptive definition of a stromatolite - a laminated sedimentary structure accretionary away from a point or limited surface. Then, evidence must be presented that these

structures are biogenic. What was their paleoenvironment on the shoreline? Did they form at the sediment water interface, or from pore waters? Is there a high degree of inheritance? Are the fabrics all precipitated, or is there evidence of trapping and binding (such as detrital grains trapped beyond the angle of repose)? It appears from the thin section photomicrographs that there are differences in the grain sizes, from micrite to spar. Are some of these later pore-filling cements? Or were there changes in the environment that caused changes in crystal size during precipitation? Overall, much more detail is needed on the formation of these structures before we can even conclude that they are biogenic (see Grotzinger and Knoll, 1999; and Bosak et al., 2013, both in Annual Reviews of Earth and Planetary Sciences for good reviews of the issues surrounding stromatolite biogenicity). The forms studied here do resemble some forms from the Green River Formation, which may act as a good comparison (see Frantz et al., 2014, Palaeogeography, Palaeoclimatology, Palaeoecology).

Furthermore, the understanding of how stromatolites form is not well developed. I don't much blame the authors, as the model presented (eg, stromatolites need a lack of metazoans, high salinity, high pH) is what one would find in a textbook. However, the stromatolite community has moved on from this simplistic understanding. The decline of stromatolite forms in the rock record begins well before the diversification of metazoans, making their disappearance more of an open question. There is a notable switch in stromatolite texture along with the decline (See Frantz et al., 2015, Geobiology). Microbialites form in a number of different environments today, not just hyper restricted areas (see Dupraz et al., 2011, Encyclopedia of Geobiology; or Cooper et al., 2022 Journal of Sedimentary Research for examples). Shark Bay is the class example of a hypersaline lagoon stromatolite, but those forms are texturally very different from the ones presented here (and the lagoon is only hypersaline for a portion of the year, it still includes abundant metazoan, including the stalked diatoms that greatly impact the accretion of the stromatolites in that location).

Additionally, the carbonate chemistry that is called upon to explain formation is not well developed. For instance, the saturation index and its parameters are not mentioned. pH is not the master variable when it comes to these systems- alkalinity and DIC concentrations determine whether or not the SI favors precipitation. There is work done to show that calcium ion concentrations would be high enough. But this brings up the question- the waters are supersaturated with respect to calcite/aragonite, why is microbial metabolism needed to form a stromatolite? The microbes that are called upon in the DNA section- what do they do to SI in a highly productive, alkaline system? Sulfide oxidizers are not known to generate alkalinity – quite the opposite. Sulfide oxidation can produce hydrogen ions, which would negatively impact SI unless coupled to CO₂ fixation or some other process. Should we instead be looking at microbial mats acting as nucleation surfaces through the binding of ions to charged EPS? It is also necessary to know whether or not these forms are dominated by precipitated fabrics or trapped and bound fabrics. All of these issues need to be addressed further.

The DNA results also require more information. First of all, how can the authors be sure that this DNA is a primary signal from tens of thousands of years ago? The stromatolites do not appear to be a closed system, as they are found on the surface of cut valleys with intermittent water flow. That DNA could come from anywhere (see Petryshyn et al., 2021 Geobiology for a discussion of the possible meanings of genomic results from stromatolites). Is this active DNA? Relict? Populations from the post impact lake that have survived and thrived for 20ky?

The abundant communities highlighted in the paper are found in multiple environments. For instance, *Thiobacillus thioparus* is found in hydrothermal settings, but is also found in mine wastewater, soil, aquariums, and was once even pulled out of an ice core metagenome. Presence of this sequence alone cannot be used to say that this was a definitively hydrothermal environment. https://microbeatlas.org/taxon?taxon_id=90_421;96_1931;97_2322;98_2865;99_4650

The same can be said of *Annwoodia aquaesulis*
https://microbeatlas.org/taxon?taxon_id=90_421;96_11500;97_13918;98_17160;99_83977

and *Sulfuritortus calidifontis*:
https://microbeatlas.org/taxon?taxon_id=90_421;96_31105;97_39339;98_51170;99_73055

Instead of relying on DNA evidence to show that the stromatolites grew in a hydrothermal setting, other proxies could be used. For instance, high Mg/Ca or Sr/Ca ratios, REE profiles showing Eu anomalies, or even clumped isotopes if the sample has not been heated or recrystallized. These would be more robust than the DNA evidence given.

Minor Issues

The manuscript states that impact craters should be considered as possible active oxygen oases on early Earth, but the ties to the GOE are weak at best. The authors found green algae in samples, but the evolution of green algae is well beyond the GOE. What specifically about the stromatolites mean they were oxygenic? There do not appear to be gas bubbles in the thin section micrographs that could be interpreted as evidence of oxygen production. There is no work done to show why a post impact crater would be particularly suited to the evolution of oxygenic photosynthesis. This also ties to a larger problem, in that the stromatolites are assumed to be biogenic without much work done to show why.

There is a lack of detail about temperature throughout- what is meant by 'warm' water, or 'moderately' thermophilic? There needs to be more specificity.

Reviewer #2 (Remarks to the Author):

This study reports stromatolites from a subrecent impact crater lake as a analog site for early Earth habitats. The authors

investigate the sediments from this basin with a broad range of techniques, integrating petrography, mineralogy, (isotope) geochemistry, palynology, and microbial ecology. Combining this evidence, they derive a model of stromatolite growth directly linked with post-impact hydrothermal activity. Finally, they draw comparison to Archean lake systems, highlighting that stromatolite blooms in crater lakes may have contributed to the Great Oxidation Event.

The article is well written, presents novel results, and the breadth of data draws a comprehensive geobiological picture of this intriguing study site. However, some individual conclusions seem not straightforwardly supported by the evidence or require better explanation. I am, however, confident that these issues can be addressed in a revised manuscript:

1. Additional discussion on how the meteoritic signature inferred from c(Os) and 187/188 ratio is imprinted in the stromatolite would enrich the discussion in my opinion. As you acknowledge in L221, the stromatolites seem to preserve an even higher meteoric component than any other material in the basin (IB, SD, PL). This seems inconsistent with a simple admixture (implied by Fig. 5?) of these materials into a microbial mat, which should lead to an equal or lower c(Os) and 187/188 ratio, but certainly not higher. Is there some fractionation mechanism occurring during the incorporation in the microbial mat that is not explained here?

2. There is a substantial time gap between the impact event and the stromatolite growth. Linking stromatolite growth to the impact event, therefore, requires evidence for sustained hydrothermal activity in the basin. However, neither the geochemical nor mineralogical data seem like robust indicators of hydrothermal activity, as claimed. The relative enrichment of Ca and Sr may simply be a function of higher authigenic calcite formation that may not be related to a hydrothermal system at all, but e.g., exposure of fresh rock by the impact. It is unclear from the data in which mineral phase the sulfur enrichment is hosted, opening the possibility that substrate availability for sulfate reducers lead to temporarily higher pyrite formation. Thus, the causal link between hydrothermal activity and stromatolite growth remains tentative based on these data. Moreover, even accepting a hydrothermal system during Stage 1, there doesn't seem to be any evidence presented for its persistence through Stage 2 (e.g., highlighted in Fig. 4). In light of the microbiological data, the mineralogy and geochemistry could be considered consistent with a hydrothermal system, but it should be made clear that the latter do not work as independent lines of evidence and the model depends mainly on the DNA and palynology data.

3. Except for a recently reported example from the Paleoproterozoic (Kirkland et al. 2025, Nature Communications) the oldest accepted impact crater on Earth is ca. 2.2 Ga to my knowledge. This raises the question how exactly the findings presented here help us test the hypothesis of stromatolites in hydrothermally influenced crater lakes contributing to the GOE. I would appreciate some more concrete explanation of how studying this analog site will inform future research.

Minor comments:

L38: The term "trace fossil" does not seem to fit in the context of stromatolites.

L53: But only for post-Cryogenian times, because the absence of animals earlier would allow stromatolites also in other suitable environments.

L61: I agree that life in Archean impact craters may be an important and overlooked ecological niche, but readers unfamiliar with the early Earth may need a little more explanation on the significance of impact events during this time. A brief summary on this, perhaps drawing from the review by Lowe & Byerly (2018), may enrich the introduction.

L67: Could you be more specific as to what kind of rocks?

L91: This sentence seems to mix observation and interpretation. The presence of (former) microbial mats is not something observed in thin section but inferred from the observed wrinkly organic-rich laminae and overall morphology of the specimen. I suggest clarifying this.

L100: The term „microbial mat“ suggests a living microbial community, whereas your 14C samples derive from a fossil stromatolite.

L131: Above you write the post impact lake sediments are up to 65 m thick, here post impact lake sediments reach to 71 m. Please clarify.

L146: Could the age reversal also be due to the uptake of old inorganic carbon from carbonates by microorganisms, rather than reduced productivity?

L186: Does this also refer to ref. 23? It's not immediately clear to me.

L240: Could you provide a reference? Since it was not drier everywhere in the world during the last glacial, this may not be self-explanatory to readers unfamiliar with the paleoclimate of Korea, including myself.

L243: It might be useful to specify these time periods here to provide a more plausible link between the impact event and the much younger stromatolite growth.

L255: „Abundance“ would be more appropriate here than „concentration“, since the counts were not quantified.

Reviewer #3 (Remarks to the Author):

Summary:

The manuscript entitled "Discovery of stromatolite formation in post-impact warm lacustrine environments and its implication for early Earth" reports the finding of stromatolites in a paleo-crater-lake formed by a meteoritic impact of Quaternary age. New data include 14C and 187Os contents in the stromatolites and other sedimentary formations in the crater, complementing existing 14C and mineralogical data from sediment cores in the crater. Several claims are rightfully supported: e.g., stromatolites formed after the impact crater's formation and they recorded an influence of the impactor (shown by Os isotopes). While these are already important conclusions of the paper, multiple points of data interpretation are

not firmly demonstrated and discussed deeply enough in my view. Whereas the suggestion of stromatolite formation in relation with hydrothermal activity is plausible, the present study does not demonstrate the direct causal links between the meteorite impact and the stromatolites formation. The timing of events remains uncertain in my opinion, which does not help in constraining the post-impact history. The present data in no way tell us about the microbial processes/diversity that shaped the stromatolites and how they were influenced by the crater lake's chemistry. Hence, the study does not demonstrate how and why impact craters may have favored life development and O₂ production in ancient Earth. Rather it provides a new and interesting observation of stromatolites formed in a Quaternary crater lake. The study's outcomes should therefore be presented with more caution.

Major remarks:

Here I present my major remarks about the submitted manuscript; the line numbers in parentheses refer to my detailed comments further below.

About the form:

- * This study repeatedly refers to refs 25 and 26, such that it may sometimes be unclear what is new or not; some passages dwell on data from these refs too much, while others skip them too fast (see comment on line 133-134; 400-402).
- * The method is unclear on why different pre-treatments were applied before 14C analyses (e.g., comment l100).
- * Going back and forth with topics and arguments is detrimental to the flow of reading (e.g., comment l110-113, l138-141, l153-180). The text is sometimes unrepresentative of the subsection titles.
- * Imprecise cross-references (comment l414-416) and figure not representative of the text (e.g., comment l393)

About the content:

- * Description of the stromatolite sampling (possibly not in place?) (comment l86-89)
- * Hydrothermal activity not demonstrated but deduced (e.g., comment l28-29)
- * The 14C-ages are estimates but not clear-cut ages (e.g., comment l113-115; l305, l385-387)
- * The record of organic matter is not clearly identified as autochthonous or allochthonous (e.g., comment l141-142, l110-111, l137-138)
- * Interrogation about Os isotope results (comment l224-227)
- * The environmental changes that occurred after the crater's formation – hypothesized to favor stromatolite formations – are not investigated in depth and the causal links between the meteorite impact, hydrothermal activity, high pH/alkalinity conditions, and stromatolite formation should be expressed with more conditional phrasing. Putative related production of O₂ during the Precambrian should be expressed as perspectives and futures areas of research, rather than an established conclusion (e.g., 307-313; 320-322).

Detailed comments:

Abstract

Line 28-29: Hydrothermal activity is not firmly demonstrated in the text, but deduced, notably by comparison with another impact crater. The abstract should more accurately reflect that hydrothermal conditions were possibly met.

Line 30-34: The first sentence should be deleted in my view, because there was almost no mention of O₂ in the text. The authors use an implicit shortcut according to which stromatolites produce oxygen. It was not evidenced for the crater stromatolites studied here, and it was most likely not true for the entire Earth's history. The second sentence should also be rephrased with a much more conditional phrasing. The formation of Quaternary stromatolites in an impact crater on a planet already field with life is no evidence that meteorite impacts played a significant role in creating habitable environments on an "abiotic" primitive Earth; the present study does not fill this gap.

Main text

Line 40: Please delete the word "slowly"; it is hard to justify what is slow or fast. Stromatolites growing in concentrated solutions may have relatively fast-growing rates (e.g., Iniesto et al., 2021 – Env. Microbio. reports), some layers dominated by mineral precipitation may also relatively fast depending on the season (e.g., Arenas et al., 2019 – J. of Paleogeography), etc.

Line 42: Please rephrase considering that photosynthesis does not directly change alkalinity (CO₂+H₂O → CH₂O + O₂, and HCO₃⁻ to CO₂ + OH⁻, no change in alkalinity – 1 mol of HCO₃ for 1 mol of OH⁻). Instead, the pH increase resulting from photosynthesis may induce carbonate precipitation. By contrast, a microbial process like sulfate reduction strictly generates alkalinity. Possibly: something like "which induces an increase in alkalinity and promotes..."

Line 47-49: Include a cautionary word such as 'partly due' to account for the fact that stromatolites decline may relate to more than a single factor (see Grotzinger, 1990 in AJS; and cautions brought by ref 12, Riding, 2006). Also see Caumartin et al. (2023; GPL).

Line 50-51: I would suggest rephrasing such as: "These ancient fossils persist on the modern Earth; present-day stromatolites have notably been identified...". See comment below.

Line 53: Please use a less unidirectional formulation (for example, "...stromatolites may serve..."); or add at the end of the sentence, "although modern stromatolites are also found in open marine conditions (e.g., Reid et al., 2000)".

Line 59: "... Hapcheon impact crater, Kora, formed ~xx years ago (ref)". I would add the age of the crater already in the introduction since it was previously determined.

Line 62: I think a discussion about other impact crater stromatolites and/or microbial mats that have been reported so far is missing (e.g., in the Ries crater, see ref 2 Osinski et al., 2020; and Arp et al., 2013 in Meteorit Planet Sci).

Line 65: I suggest moving the 'Fig. 1' cross-ref to the end of the sentence.

Line 71: The type of sedimentary environment in which stromatolites developed is a relevant information for the present study. Could the authors develop a little on what the early post-impact sedimentary environments of the crater were?

Line 75: From the Fig. 1, it seems like the indicated transect is northeast-southwest rather than east-west.

Line 76-77: It is not very clear whether 20CR12 is another sedimentary core that contains other types of sediments than the

first 5 ones (it is shown with the same symbol on Figure 1 than the cores), or if it just corresponds to other deposits. I cannot see the 23HICO1 site on the figure either. Please clarify this brief presentation of the cores/samples.

Line 78-80: I would shortly inform what are the ages for these cores if they were published before.

Line 84: It seems to me that it's still the northeast rather than the east?

Line 86-89: Considering the images in Supp. Fig. 7, this description remains too elusive (please refer to my comment about the caption of the figure below in this document).

Line 89-91: Could these objects be referred to as oncolites? How could the authors introduce this notion? A mention of the objects they are growing on would also be quite relevant. Is there not some would in the middle that could be dated in order have a proper starting age of stromatolite formation?

Line 95: The supp. Fig. 9 requires close up views of the samples laminated structure such as in Fig. 2. These samples from Fig. S9 do not clearly resemble stromatolites at the meso/macro scale, and as it is, the figure does not really help identifying them as such. Similarly, Fig. S8 shows only the STR site; thus, microscale observations of the MSG samples are necessary.

Timeline of stromatolite Growth

Line 98: Considering the difficulties and discussion required to use radiocarbon ages warrants a conditional phrase here. I advise rephrasing as: "may be used to determine".

Line 100: Were there no other organic matter sources at all? It is difficult to assert that all organics come from autochthonous benthic communities (see for example, Petryshyn et al., 2021; Geobiology), especially based on optical microscopy pictures alone, and thus the formulation of microbial mats organic matter is misleading. The subsequent discussion even insists on allochthonous components. Hence, it calls for a broader description of the organics that may have been analyzed.

Besides, the method section describes that some of the stromatolites were only crushed and ground and analyzed without additional dissolution steps (i.e., analyzed as bulk material and not specifically as organic matter). This should be more explicit here. Please find complementary comments in the method section below.

Line 108: From Fig. 3 and Sup. Table 1 the age of 04_3 is 26070 cal yr BP and not 19880.

Line 110-111: "or organic materials"; I agree with the distinction made here, and this should be already settled before (see previous comment). In my opinion the authors should mention 'the organic materials' rather than 'microbial mats', possibly after defining what organic materials may consist of.

Line 111-112: Can the authors explain what can be observed in thin sections from Supp Fig. 8? This figure only shows images for a single stromatolite, and if it does not allow differentiating between autochthonous and allochthonous organics, I would delete this part of the sentence.

Line 110-113: This sentence is confusing. Because of the citations, we tend to believe that the inference made here comes from the literature, whereas it's actually only hypothesized by comparison with other crater sediments²⁶. This aspect is discussed in detail in the next paragraph and is detrimental for the comprehension of the text at that point. I would delete this sentence.

Line 113-115: I agree with the first part of the sentence, not really with the last part. (i) If the organics were allochthonous (e.g., from the SD) and they do not record logical age trends (i.e., as observed here, younger to older from the inside to the outside), how can we consider these ages to represent the formation ages of the stromatolite layers? I agree that 23390 yrs would represent a higher limit, but since organics may come from plants that are older the microbialite layers, the actual age of STR03_1 could be younger: anywhere from 23390 to 14660 yrs. Similarly, 14660 is a higher age limit but the actual age of STR03_6 could be younger. Hence, this age range (even approximate) does not correspond to the range of the stromatolite age of formation but to an approximate estimation of the boundary ages within which it grew.

(ii) If the organics were autochthonous in origin (which is likely for at least part of the OM from stromatolites), the age reversal trends suggest they recorded a DIC that depleted in ^{14}C faster than if it was influenced by radioactive decay alone. For example, if the lake was increasingly supplied by a source with "older DIC". In such a case, the ^{14}C ages given here would hardly be relevant for dating stromatolite growth periods. Were stable isotope compositions of carbonate C and O measured in the samples? These might help evidencing such trends.

Can the authors comment on these two possibilities i and ii (and adapt the text accordingly)?

Line 124-125: Can the authors explain how the method brings further information about the use of humic acid, humin, etc.? Possibly, the method in the supplementary could develop on where these different organics come from, and how they can be used for ^{14}C dating. Otherwise, "see Methods for details" can be deleted.

Line 132: Please inform on the exact depth? For comparison with the 71 m mentioned above.

Line 133-134: I would have guessed that there were less turbidite deposits as time went by after the impact crater's formation (considering a decrease of the crater's topography with time). The lines 133-134 build on the refs 25 and 26, but some further explanation is required for the present study to stand as itself (especially since this age reversal is critical in the present study too). How do the authors explain this increasing trend of gravitational turbidite deposits?

If one considers that lake sediments formed at most ~21 000 yrs ago, how do the authors interpret the absence of sediment deposition between the top of IB and the bottom of lake sediment (that is, an about 20000 years interval)?

Line 137-138: Please rephrase in a more straightforward way: "The age reversal of the STR03 stromatolite from 21,000 to 28 000 cal yr BP is similar to that recorded in Stage 1 of the lake sedimentary core 20CR05 (Figs 3 and 4)." Besides, it seems from (Table S1; Fig. 3) that the age reversal of this sample is from 23390 to 28 ka rather 21 to 28 ka.

Overall, this discussion suggests that the age reversal in stromatolites result from the same process as for the sediments (i.e., an increase of allochthonous OM). However, the Supp. Figure 8 does not clearly evidence such type of OM in STR03 but rather the opposite. How could this be consistent with the suggested increase in allochthonous OM?

Line 138-141: This phrase is repeating the information at the end of the former paragraph and could (should) be deleted.

Line 141-142: I suggest to use a more cautious phrasing. For example: "This similarity suggests that the stromatolite ... could have formed in the early post-impact lake environment."

Moreover, the second part of the sentence is confusing because it is not obvious from the above discussion that stromatolites primarily record an autochthonous production: (i) it is mentioned that they may trap organic material, (ii) their record of a reversal indicates they likely record an increasing allochthonous component as well. Hence, this comparison

between stromatolites and sediments does not hold, and I believe the inference of autochthonous aquatic productivity should be argued based on other (independent arguments). I do not think that ^{14}C ages can be used as direct arguments of OM's origin, especially for these stromatolites. The $\delta^{13}\text{C}$ of the organic components measured for ^{14}C may help differentiating such differences in OM sources.

Line 144: Following the previous comment, "For example" can be deleted here (replaced by "notably?").

Line 147-150: Considering the high location of the stromatolites in the impact crater, that is, very far from a topographic low and the centre of the crater (especially if they were transported down the valleys since their formation) (Fig. 1 and S7), how would these organics from slope deposits have affected the stromatolites? And even more so in a predominant manner?

Testing post-impact stromatolites

Line 153-180: The outline is confusing. Sediment cores as well as stromatolite ages have already been discussed in relation to the impact crater formation prior to this passage. This new section is called « testing post impact stromatolites » but starts with two long paragraphs about another sediment core. I believe dating of the crater's formation and subsequent lake sediments should be elucidated first and definitely, clearly separating and starting with the results from prior studies in a sort of context section. Then, new data allowing to refine the crater's dating should be presented, before stromatolites ^{14}C results can eventually be put in perspective with prior pieces of information. Finally, I note that this part was repeated in the supplementary.

Line 162-163: I suggest a rephrasing of this sentence for a better flow of reading. For instance: "These findings support the reported range of ages for syn-impact fragments of the Hapcheon impact event ($42,770 \pm 1,030$ cal yr BP) 26."

Line 168: Which core does this refer to? If it's 20CR05, Fig. 4 could also be referred to for a better visualization relative to the entire core.

Line 186-187: Please add a reference to this sentence.

Line 186-189: I would move this just before the last sentence of the paragraph in order to maintain the reading flow about ^{187}Os , and then comfort these inferences with platinum group and Rhenium.

Line 193-194: I suggest a rephrasing as: "These findings warrant further investigation about the possible incorporation of meteoritic material into the stromatolites found in the Hapcheon crater."

Line 198-199: The concentration should be given for each stromatolite, like for isotopic ratios just above.

Line 199: This formulation ('A recent study') is used many times throughout the text but is not necessary. A selection of places where it can be deleted (e.g., here) would help for a better flow of reading. As above for the ^{14}C ages, information about Os are quite intertwined here: first data on stromatolites, then target rocks in the crater, then "basic" theory on meteoritic material, then back to stromatolites, and again values of a target rock. This would be better for clarity to reorganize this.

Line 199-202: Why are these values not shown in Figure 5 like the other target rocks in the crater presented below? Could the authors explain why they used the other target rock for the binary mixing model?

Line 204-205: Could the authors be more specific about "approximately 0.126"? This seems imprecise: is it an average, on how many samples, from what types of meteorites? If it's only in general, should they provide a more comprehensive description of the range meteorites can show? Maybe a formulation like "Meteorites tend to exhibit..." would be more adequate.

Line 212-213: Why is the second stromatolite meteoritic component percentage not shown in the text?

Also, while the precision on $^{187}\text{Os}/^{188}\text{Os}$ is not low enough to recognize a trend through stromatolite layers (Supp. Table 3), the Os concentrations show neither a consistent increasing or decreasing trend. This could imply that the erosive flux of meteoritic material is not constant and doesn't follow any specific trend. Do the authors think this could be worth discussing in the text?

Line 224-227: Why is there a decrease of the meteoritic component with time (i.e., going up from impact breccias to lake sediment deposits)? One would expect that the initial breccias contained more meteoric component. Can the authors comment on this? Is this not also a bit contradictory with the ^{14}C ages which suggest a stronger authigenic lake component at the base of lake sediments?

Line 229-230: Please mention that it is a small meteoritic component: "...received a continuous – albeit small – influx of ..."

Impact craters as a cradle for stromatolite growth and implications for the early Earth

Line 236: "Modern stromatolites tend to form under extreme conditions, such..." (see a previous comment).

Line 237-238: suggested modification: "...high pH and high alkalinity are required to ensure supersaturation with respect to carbonate minerals."

Line 238-243: I think this is irrelevant here. The effect of cold/dry climate could be mentioned later, to reinforce the idea that hydrothermal activity might have been a prerequisite for form stromatolites considering such climatic conditions.

Hydrothermalism is much better introduced as it is later in the text. Furthermore, the transition to identify "past environments conducive to stromatolite formation" is sufficient, this passage at lines 238-243 should be deleted.

Line 247-248: This appears contradictory. Why would allochthonous inputs (from the detrital turbidite SD slope deposits) reflect the lake's water chemistry? Please rephrase.

Line 261: By in situ, do the authors imply as authigenic in the sediments during early diagenesis? I agree this is quite likely but how can they rule out a precipitation in the water column? In any case, the only presence of calcite (presumably authigenic) cannot simply be compared with stromatolite formation processes. The conclusion to infer would be that conditions favourable to calcite precipitation were met early during the lake sediments deposition, but it cannot be related to stromatolite that directly.

Line 274-276: I don't think this can be used as an argument since the XRD analyses seem to have been carried out only integrating these 5 phases. In other words, calcite must be showing an inverse trend compared to the sum of the other 4. This is not problematic with the comparison of Ca content from XRF data, but the covariation is less clear in this case.

Line 276-278: I do not see the causal link between this conclusion and the argument in the sentence before. The Fig. 4 and S10 suggest a dilution effect of the calcite by the terrestrial material, regardless of the intensity of calcite precipitation in the

lake or sediments, and even more independently of the stromatolite story (just above it is suggested that calcite precipitates in porewater sediments at the bottom of the lake, whereas stromatolites most likely were at the very top of the crater lake). The interval with more calcite suggests that it is the only moment when hydrothermal activity would have actually favoured carbonate (and thus stromatolite) formation. Because of the allochthonous inputs, however, it is hard to precisely date this interval, just as it is hard to know exactly when did the stromatolites grow. See previous comments (e.g., about lines 113-115; or line 305). Therefore, whereas one can suggest that this calcite-rich interval corresponds to the moment of stromatolite formation, I think that neither this interval or moment can definitely be dated and correlated in time with absolute ages.

Line 280: I recommend adding: "... substances (allochthonous influence) diluting the calcite precipitates in the sediments..."

Line 281-284: If we considered from the above discussion, that the younger 14C ages in Stage 1 either suggest that in situ productivity was higher during Stage 1 or that it was less diluted by terrestrial components than during Stage 2, how could the authors differentiate these 2 possibilities? Could they use Eu anomalies to characterize hydrothermal activity for example? Do they think that the constant 0 % calcite content during stage 2 even when terrestrial inputs are slightly decreasing is evidence for the absence of high pH / alkalinity conditions during stage 2?

Anyway, a possible increase of pH and salinity/alkalinity may be inferred at the base of the lake sediments, but the role of hydrothermalism should be proposed only then, and be further discussed and demonstrated below.

Line 286-288: Would it not be possible to recognize allochthonous vs autochthonous influences via DNA analyses?

Line 305: Since the humic substances may belong to both autochthonous and allochthonous organics (as seen for example with stage 1 and 2, respectively), the humic acid at 72m may represent a mix between old and young OM. Henceforth, how would the age of 20000 yrs be reliable? I would stick to referring to depth in the sediments rather than absolute ages, considering the uncertainties associated.

Line 307-313: As far it seems consistent that stromatolites formed during a period of higher pH/alkalinity in the lake, the dates should be presented with more caution. The 14C measurements do not allow giving fixed age formation ranges in my opinion. Thus, I recommend describing that such a chain of event is plausible, but will require further constraint on the timing.

Line 314-316: I would delete: "and to assess the crater as a habitat for stromatolites.", which is redundant and reads weirdly. In anyway, I would still specify "impact crater" because many microbialites have been described in volcanic crater lakes. This point could actually be mentioned to underline the present study's novelty. Yet, could the authors describe how different the present stromatolite samples are from that described in the Ries crater (Germany) (see ref 2 Osinski et al., 2020; and Arp et al., 2013 in Meteorit Planet Sci).

Line 318: This figure would fit well as a "highlights of the paper" figure, but does not bring new information to the main manuscript. Neither the geochemical conditions resulting from the meteoritic impact nor how it influenced the processes of stromatolite formation are explicated in Fig. 6. While the oxygen oasis is brought forward, it has not been discussed at any point before the very last lines of the paper. This figure may be deleted from the main text, or revised.

Line 320-322: A (slime) temporal link was evidenced between the crater's formation and the growth of stromatolites. Direct evidence of the causal link between the two is missing, or in other words, a discussion of the environmental conditions linking the impact crater formation to the stromatolites. The phrasing should thus be much more cautious, saying that stromatolites may indeed form within hydrothermal dominated impact craters. Connecting the present observations to early Earth requires even more caution. In no way were the diversity and role/function of microbes putatively building the reported stromatolites described. The flourishing of benthic microbial communities (possibly producing O₂, but this was not discussed/evidenced at all here) in a modern world already filled with life does not inform on what would have been the consequence of impact meteorite back then. Better understanding how this Hapcheon crater enabled stromatolite growth will be a first step in filling this gap.

Line 323: "should be considered active oxygen oases" It follows from my previous comment that such a conclusion can hardly be given. It should at best include a very conditional phrasing.

Methods and notes

Lines 358-361: This seems to present only part of the samples presented in the study.

Line 385-387: To add to previous discussion on the validity of 14C dating, STR03 sample has been described in terms of growth ages based on 14C data. However, this part of the method implies that it was analyzed from powders without HCl-treatment. As stated at lines 390-391, the HCl treatment should avoid a possible strong reservoir effect resulting from inorganic carbon. Then, how can the dates for this sample be interpreted in terms of periods of stromatolite growth? And could they not be affected by processes similar to what I described in point (ii) from the comment about lines 113-115.

Lines 390-392: Why was a different approach chosen for these samples? Could the authors explain their choice of treatment for each sample or at least why they used a different treatment for the different samples (e.g., 20STR03 vs. 04)?

Line 393: The Supplementary Figure 8 shows organic-rich laminae from the 20STR03 sample, which corresponds to... none of the samples mentioned just above. Such microscopic images should be provided for the HCl-treated samples.

Line 394: Please provide the time of reaction.

Line 400-402: The subsection title addresses the dating of stromatolites but lake/subaerial sediments are described here. Title or text organization should be adapted. Since a lot of data comes from the refs. 25 and 26, it would also be very useful to have a table or some text (at the start of the method for example) where one can find which data are new and which were already published.

Line 404-407: Could the authors inform approximately how much material of each category was retrieved for 14C analysis?

Line 414-416: This subsection presents IBs dating, why is there a reference to stromatolites from the Fig. 3? Also, it is strange that Supp. Table 3 is about samples that are not mentioned in this paragraph.

Line 417-445: These 3 paragraphs appear in the main text as well.

Figures

Figure 2: Which area of the stromatolites were ground? Roughly the area shown with dashed yellow circles? Please specify this information in the caption. In G, there is only yellow circle for 2 drilling locations?

From the text in the methods, it is said 14C analyses were performed on HCl-treated powders after micro-drilling (Fig. 2e, f) or alternatively after crushing some part of the stromatolites (Fig. 2a, b)? From the captions, it seems that all analyses were performed after HCl treatment. Please clarify this point. If there was no HCl treatment in Fig. 2a, b, please mention that the bulk powdered material was analysed and proceed similarly for all subsamples shown in this figure.

Figure 3: Similarly, the text in the method section indicates that only STR04 samples was treated HCl, but the caption does not make a distinction.

Figure 4: The figure is a bit crowded. I recommend, for example, to put the 'Stage 1, Stage 2, Stage 3' mentions with hydrothermal activity on the right side of the figure, as it's not striking at first glance, that the three coloured bands correspond to these three stages.

Figure 6: See comment about line 318.

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Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Dr Lim,

Your manuscript titled "Discovery of stromatolite formation in post-impact warm lacustrine environments and its implication for early Earth" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

Additionally, you must address the following points in the revised manuscript:

* The DNA evidence is not definitive. Please tone down the interpretation of the results to account for this comment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Mojtaba Fakhraee, PhD
Editorial Board Member
Communications Earth & Environment

Somaparna Ghosh, PhD
Associate Editor,
Communications Earth & Environment
Consulting Editor,
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors did a good job of addressing my initial concerns. The Raman mapping is a powerful way of looking at the incorporation of organic material, and satisfactorily showed that trapping and binding was aiding in the morphogenesis of the structure. The positive Eu anomaly preserved in the carbonates shows clear influence of hydrothermal activity.

The DNA evidence is still a little thin. I agree that the Raman shows that there was an active microbial community- but it can't be definitively stated that the OTUs found were that community. A sentence should be added to clarify that these results are not definitive, and may not represent the original microbiology present at the time of stromatolite formation. Otherwise, I am satisfied with the revisions

Comments

Last line of the abstract, and the dis- oxygen was not needed to make Earth 'habitable'. There were thriving anerobic microbial ecosystems long before the evolution of oxygenic photosynthesis and the accumulation of free oxygen on Earth. The way it is written now makes it seem like oxygen is a prerequisite for life on Earth.

Line 42: should read "Stromatolites, laminated sedimentary structures accretionary away..." are considered the oldest evidence of life. As these are technically trace fossils, and rarely preserve actual microbial body fossils.

Reviewer #2 (Remarks to the Author):

I have read the revised version of the manuscript by Lim et al. and was pleased to see that all my concerns have been addressed satisfactorily. Thus, I have no further comments.

Kind regards,

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Response to Reviewers

First of all, thank you all reviewers and editorial board members to evaluate our paper and for precious instrumental suggestions and comments.

We have tried to revise our text according to the comments suggested by the reviewers. We highlighted correction in red. The following is point-by-point response (*italic*) to the reviewers' comments with a list of changes to the manuscript text.

Reviewer #1 (Remarks to the Author):

The authors did a good job of addressing my initial concerns. The Raman mapping is a powerful way of looking at the incorporation of organic material, and satisfactorily showed that trapping and binding was aiding in the morphogenesis of the structure. The positive Eu anomaly preserved in the carbonates shows clear influence of hydrothermal activity.

The DNA evidence is still a little thin. I agree that the Raman shows that there was an active microbial community- but it can't be definitively stated that the OTUs found were that community. A sentence should be added to clarify that these results are not definitive, and may not represent the original microbiology present at the time of stromatolite formation. Otherwise, I am satisfied with the revisions

Answer: According to the comment, we revised the text by adding a sentence "It is worthy to note that these DNA results are not definitive, and may not represent the original microbiology present at the time of stromatolite formation, needing caution". Please see the revised text (lines 380-382).

Comments

Last line of the abstract, and the dis- oxygen was not needed to make Earth 'habitable'. There were thriving anerobic microbial ecosystems long before the evolution of oxygenic photosynthesis and the accumulation of free oxygen on Earth. The way it is written now makes it seem like oxygen is a prerequisite for life on Earth.

Answer: According to the comment, we revised the last sentence. Please see the revised text (line 26).

Line 42: should read "Stromatolites, laminated sedimentary structures accretionary away..." are considered the oldest evidence of life. As these are technically trace fossils, and rarely preserve actual microbial body fossils.

Answer: According to the comment, we revised the text. Please see the revised text (line 46-47).

Reviewer #2 (Remarks to the Author):

I have read the revised version of the manuscript by Lim et al. and was pleased to see that all my concerns have been addressed satisfactorily. Thus, I have no further comments.

Answer: Thank you all reviewers for valuable comments, which have made our earlier manuscript clearer and sounder in terms of biogenicity in stromatolite and interpretation on the hydrothermal influences in the post-impact crater.

Summary:

The manuscript entitled *"Discovery of stromatolite formation in post-impact warm lacustrine environments and its implication for early Earth"* reports the finding of stromatolites in a paleo-crater-lake formed by a meteoritic impact of Quaternary age. New data include ^{14}C and ^{187}Os contents in the stromatolites and other sedimentary formations in the crater, complementing existing ^{14}C and mineralogical data from sediment cores in the crater. Several claims are rightfully supported: e.g., stromatolites formed after the impact crater's formation and they recorded an influence of the impactor (shown by Os isotopes). While these are already important conclusions of the paper, multiple points of data interpretation are not firmly demonstrated and discussed deeply enough in my view. Whereas the suggestion of stromatolite formation in relation with hydrothermal activity is plausible, the present study does not demonstrate the direct causal links between the meteorite impact and the stromatolites formation. The timing of events remains uncertain in my opinion, which does not help in constraining the post-impact history. The present data in no way tell us about the microbial processes/diversity that shaped the stromatolites and how they were influenced by the crater lake's chemistry. Hence, the study does not demonstrate how and why impact craters may have favored life development and O_2 production in ancient Earth. Rather it provides a new and interesting observation of stromatolites formed in a Quaternary crater lake. The study's outcomes should therefore be presented with more caution.

Major remarks:

Here I present my major remarks about the submitted manuscript; the line numbers in parentheses refer to my detailed comments further below.

About the form:

- * This study repeatedly refers to refs 25 and 26, such that it may sometimes be unclear what is new or not; some passages dwell on data from these refs too much, while others skip them too fast (see comment on line 133-134; 400-402).
- * The method is unclear on why different pre-treatments were applied before ^{14}C analyses (e.g., comment l100).
- * Going back and forth with topics and arguments is detrimental to the flow of reading (e.g., comment l110-113, l138-141, l153-180). The text is sometimes unrepresentative of the subsection titles.
- * Imprecise cross-references (comment l414-416) and figure not representative of the text (e.g., comment l393)

About the content:

- * Description of the stromatolite sampling (possibly not in place?) (comment l86-89)
- * Hydrothermal activity not demonstrated but deduced (e.g., comment l28-29)
- * The ^{14}C -ages are estimates but not clear-cut ages (e.g., comment l113-115; l305, l385-387)
- * The record of organic matter is not clearly identified as autochthonous or allochthonous (e.g., comment l141-142, l110-111, l137-138)
- * Interrogation about Os isotope results (comment l224-227)
- * The environmental changes that occurred after the crater's formation – hypothesized to favor stromatolite formations – are not investigated in depth and the causal links between the meteorite impact, hydrothermal activity, high pH/alkalinity conditions, and stromatolite formation should be expressed with more conditional

phrasing. Putative related production of O₂ during the Precambrian should be expressed as perspectives and futures areas of research, rather than an established conclusion (e.g., 307-313; 320-322).

Detailed comments:

Abstract

Line 28-29: Hydrothermal activity is not firmly demonstrated in the text, but deduced, notably by comparison with another impact crater. The abstract should more accurately reflect that hydrothermal conditions were *possibly* met.

Line 30-34: The first sentence should be deleted in my view, because there was almost no mention of O₂ in the text. The authors use an implicit shortcut according to which stromatolites produce oxygen. It was not evidenced for the crater stromatolites studied here, and it was most likely not true for the entire Earth's history. The second sentence should also be rephrased with a much more conditional phrasing. The formation of Quaternary stromatolites in an impact crater on a planet already field with life is no evidence that meteorite impacts played a significant role in creating habitable environments on an "abiotic" primitive Earth; the present study does not fill this gap.

Main text

Line 40: Please delete the word "slowly"; it is hard to justify what is slow or fast. Stromatolites growing in concentrated solutions may have relatively fast-growing rates (e.g., Iniesto et al., 2021 – Env. Microbio. reports), some layers dominated by mineral precipitation may also relatively fast depending on the season (e.g., Arenas et al., 2019 – J. of Paleogeography), etc.

Line 42: Please rephrase considering that photosynthesis does not directly change alkalinity (CO₂+H₂O -> CH₂O + O₂, and HCO₃⁻ → to CO₂ + OH⁻, no change in alkalinity – 1 mol of HCO₃ for 1 mol of OH⁻). Instead, the pH increase resulting from photosynthesis may induce carbonate precipitation. By contrast, a microbial process like sulfate reduction strictly generates alkalinity. Possibly: something like "which induces an increase in alkalinity and promotes...".

Line 47-49: Include a cautionary word such as 'partly due' to account for the fact that stromatolites decline may relate to more than a single factor (see Grotzinger, 1990 in *AJS*; and cautions brought by ref 12, Riding, 2006). Also see Caumartin et al. (2023; GPL).

Line 50-51: I would suggest rephrasing such as: "These ancient fossils persist on the modern Earth; present-day stromatolites have notably been identified...". See comment below.

Line 53: Please use a less unidirectional formulation (for example, "...stromatolites may serve..."); or add at the end of the sentence, "although modern stromatolites are also found in open marine conditions (e.g., Reid et al., 2000)".

Line 59: "... Hapcheon impact crater, Kora, formed ~xx years ago (ref)". I would add the age of the crater already in the introduction since it was previously determined.

Line 62: I think a discussion about other impact crater stromatolites and/or microbial mats that have been reported so far is missing (e.g., in the Ries crater, see ref 2 Osinski et al., 2020; and Arp et al., 2013 in *Meteorit Planet Sci*).

Line 65: I suggest moving the 'Fig. 1' cross-ref to the end of the sentence.

Line 71: The type of sedimentary environment in which stromatolites developed is a relevant information for the present study. Could the authors develop a little on what the early post-impact sedimentary environments of the crater were?

Line 75: From the Fig. 1, it seems like the indicated transect is northeast-southwest rather than east-west.

Line 76-77: It is not very clear whether 20CR12 is another sedimentary core that contains other types of sediments than the first 5 ones (it is shown with the same symbol on Figure 1 than the cores), or if it just corresponds to other deposits. I cannot see the 23HICO1 site on the figure either. Please clarify this brief presentation of the cores/samples.

Line 78-80: I would shortly inform what are the ages for these cores if they were published before.

Line 84: It seems to me that it's still the northeast rather than the east?

Line 86-89: Considering the images in Supp. Fig. 7, this description remains too elusive (please refer to my comment about the caption of the figure below in this document).

Line 89-91: Could these objects be referred to as oncolites? How could the authors introduce this notion? A mention of the objects they are growing on would also be quite relevant. Is there not some would in the middle that could be dated in order have a proper starting age of stromatolite formation?

Line 95: The supp. Fig. 9 requires close up views of the samples laminated structure such as in Fig. 2. These samples from Fig. S9 do not clearly resemble stromatolites at the meso/macro scale, and as it is, the figure does not really help identifying them as such. Similarly, Fig. S8 shows only the STR site; thus, microscale observations of the MSG samples are necessary.

Timeline of stromatolite Growth

Line 98: Considering the difficulties and discussion required to use radiocarbon ages warrants a conditional phrase here. I advise rephrasing as: "may be used to determine".

Line 100: Were there no other organic matter sources at all? It is difficult to assert that *all* organics come from autochthonous benthic communities (see for example, Petryshyn et al., 2021; Geobiology), especially based on optical microscopy pictures alone, and thus the formulation of microbial mats organic matter is misleading. The subsequent discussion even insists on allochthonous components. Hence, it calls for a broader description of the organics that may have been analyzed.

Besides, the method section describes that some of the stromatolites were only crushed and ground and analyzed without additional dissolution steps (i.e., analyzed as bulk material and not specifically as organic matter). This should be more explicit here. Please find complementary comments in the method section below.

Line 108: From Fig. 3 and Sup. Table 1 the age of 04_3 is 26070 cal yr BP and not 19880.

Line 110-111: "or organic materials"; I agree with the distinction made here, and this should be already settled before (see previous comment). In my opinion the authors should mention 'the organic materials' rather than 'microbial mats', possibly after defining what organic materials may consist of.

Line 111-112: Can the authors explain what can be observed in thin sections from Supp Fig. 8? This figure only shows images for a single stromatolite, and if it does not allow differentiating between autochthonous and allochthonous organics, I would delete this part of the sentence.

Line 110-113: This sentence is confusing. Because of the citations, we tend to believe that the inference made here comes from the literature, whereas it's actually only hypothesized by comparison with other crater

sediments²⁶. This aspect is discussed in detail in the next paragraph and is detrimental for the comprehension of the text at that point. I would delete this sentence.

Line 113-115: I agree with the first part of the sentence, not really with the last part. (i) If the organics were allochthonous (e.g., from the SD) and they do not record logical age trends (i.e., as observed here, younger to older from the inside to the outside), how can we consider these ages to represent the formation ages of the stromatolite layers? I agree that 23390 yrs would represent a higher limit, but since organics may come from plants that are older than the microbialite layers, the actual age of STR03_1 could be younger: anywhere from 23390 to 14660 yrs. Similarly, 14660 is a higher age limit but the actual age of STR03_6 could be younger. Hence, this age range (even approximate) does not correspond to the range of the stromatolite age of formation but to an approximate estimation of the boundary ages within which it grew.

(ii) If the organics were autochthonous in origin (which is likely for at least part of the OM from stromatolites), the age reversal trends suggest they recorded a DIC that depleted in ¹⁴C faster than if it was influenced by radioactive decay alone. For example, if the lake was increasingly supplied by a source with “older DIC”. In such a case, the ¹⁴C ages given here would hardly be relevant for dating stromatolite growth periods. Were stable isotope compositions of carbonate C and O measured in the samples? These might help evidencing such trends.

Can the authors comment on these two possibilities i and ii (and adapt the text accordingly)?

Line 124-125: Can the authors explain how the method brings further information about the use of humic acid, humin, etc.? Possibly, the method in the supplementary could develop on where these different organics come from, and how they can be used for ¹⁴C dating. Otherwise, “see Methods for details” can be deleted.

Line 132: Please inform on the exact depth? For comparison with the 71 m mentioned above.

Line 133-134: I would have guessed that there were less turbidite deposits as time went by after the impact crater's formation (considering a decrease of the crater's topography with time). The lines 133-134 build on the refs 25 and 26, but some further explanation is required for the present study to stand as itself (especially since this age reversal is critical in the present study too). How do the authors explain this increasing trend of gravitational turbidite deposits?

If one considers that lake sediments formed at most ~21 000 yrs ago, how do the authors interpret the absence of sediment deposition between the top of IB and the bottom of lake sediment (that is, an about 20000 years interval)?

Line 137-138: Please rephrase in a more straightforward way: “The age reversal of the STR03 stromatolite from 21,000 to 28 000 cal yr BP is similar to that recorded in Stage 1 of the lake sedimentary core 20CR05 (Figs 3 and 4).” Besides, it seems from (Table S1; Fig. 3) that the age reversal of this sample is from 23390 to 28 ka rather than 21 to 28 ka.

Overall, this discussion suggests that the age reversal in stromatolites result from the same process as for the sediments (i.e., an increase of allochthonous OM). However, the Supp. Figure 8 does not clearly evidence such type of OM in STR03 but rather the opposite. How could this be consistent with the suggested increase in allochthonous OM?

Line 138-141: This phrase is repeating the information at the end of the former paragraph and could (should) be deleted.

Line 141-142: I suggest to use a more cautious phrasing. For example: “This similarity suggests that the stromatolite ... could have formed in the early post-impact lake environment.”

Moreover, the second part of the sentence is confusing because it is not obvious from the above discussion that stromatolites primarily record an autochthonous production: (i) it is mentioned that they may trap organic material, (ii) their record of a reversal indicates they likely record an increasing allochthonous component as well.

Hence, this comparison between stromatolites and sediments does not hold, and I believe the inference of *autochthonous* aquatic productivity should be argued based on other (independent arguments). I do not think that ^{14}C ages can be used as direct arguments of OM's origin, especially for these stromatolites. The $\delta^{13}\text{C}$ of the organic components measured for ^{14}C may help differentiating such differences in OM sources.

Line 144: Following the previous comment, "For example" can be deleted here (replaced by "notably?").

Line 147-150: Considering the high location of the stromatolites in the impact crater, that is, very far from a topographic low and the centre of the crater (especially if they were transported down the valleys since their formation) (Fig. 1 and S7), how would these organics from slope deposits have affected the stromatolites? And even more so in a predominant manner?

Testing post-impact stromatolites

Line 153-180: The outline is confusing. Sediment cores as well as stromatolite ages have already been discussed in relation to the impact crater formation prior to this passage. This new section is called « testing post impact stromatolites » but starts with two long paragraphs about another sediment core. I believe dating of the crater's formation and subsequent lake sediments should be elucidated first and definitely, clearly separating and starting with the results from prior studies in a sort of context section. Then, new data allowing to refine the crater's dating should be presented, before stromatolites ^{14}C results can eventually be put in perspective with prior pieces of information. Finally, I note that this part was repeated in the supplementary.

Line 162-163: I suggest a rephrasing of this sentence for a better flow of reading. For instance: "These findings support the reported range of ages for syn-impact fragments of the Hapcheon impact event ($42,770 \pm 1,030$ cal yr BP)²⁶."

Line 168: Which core does this refer to? If it's 20CR05, Fig. 4 could also be referred to for a better visualization relative to the entire core.

Line 186-187: Please add a reference to this sentence.

Line 186-189: I would move this just before the last sentence of the paragraph in order to maintain the reading flow about ^{187}Os , and then comfort these inferences with platinum group and Rhenium.

Line 193-194: I suggest a rephrasing as: "These findings warrant further investigation about the possible incorporation of meteoritic material into the stromatolites found in the Hapcheon crater."

Line 198-199: The concentration should be given for each stromatolite, like for isotopic ratios just above.

Line 199: This formulation ('A recent study') is used many times throughout the text but is not necessary. A selection of places where it can be deleted (e.g., here) would help for a better flow of reading. As above for the ^{14}C ages, information about Os are quite intertwined here: first data on stromatolites, then target rocks in the crater, then "basic" theory on meteoritic material, then back to stromatolites, and again values of a target rock. This would be better for clarity to reorganize this.

Line 199-202: Why are these values not shown in Figure 5 like the other target rocks in the crater presented below? Could the authors explain why they used the other target rock for the binary mixing model?

Line 204-205: Could the authors be more specific about "approximately 0.126"? This seems imprecise: is it an average, on how many samples, from what types of meteorites? If it's only in general, should they provide a more comprehensive description of the range meteorites can show? Maybe a formulation like "Meteorites tend to exhibit..." would be more adequate.

Line 212-213: Why is the second stromatolite meteoritic component percentage not shown in the text?

Also, while the precision on $^{187}\text{Os}/^{188}\text{Os}$ is not low enough to recognize a trend through stromatolite layers (Supp. Table 3), the Os concentrations show neither a consistent increasing or decreasing trend. This could imply that the erosive flux of meteoritic material is not constant and doesn't follow any specific trend. Do the authors think this could be worth discussing in the text?

Line 224-227: Why is there a decrease of the meteoritic component with time (i.e., going up from impact breccias to lake sediment deposits)? One would expect that the initial breccias contained more meteoric component. Can the authors comment on this? Is this not also a bit contradictory with the ^{14}C ages which suggest a stronger authigenic lake component at the base of lake sediments?

Line 229-230: Please mention that it is a small meteoritic component: "...received a continuous – albeit small – influx of ..."

Impact craters as a cradle for stromatolite growth and implications for the early Earth

Line 236: "Modern stromatolites tend to form under extreme conditions, such..." (see a previous comment).

Line 237-238: suggested modification: "...high pH and high alkalinity are required to ensure supersaturation with respect to carbonate minerals."

Line 238-243: I think this is irrelevant here. The effect of cold/dry climate could be mentioned later, to reinforce the idea that hydrothermal activity might have been a prerequisite for form stromatolites considering such climatic conditions. Hydrothermalism is much better introduced as it is later in the text. Furthermore, the transition to identify "past environments conducive to stromatolite formation" is sufficient, this passage at lines 238-243 should be deleted.

Line 247-248: This appears contradictory. Why would allochthonous inputs (from the detrital turbidite SD slope deposits) reflect the lake's water chemistry? Please rephrase.

Line 261: By *in situ*, do the authors imply as authigenic in the sediments during early diagenesis? I agree this is quite likely but how can they rule out a precipitation in the water column? In any case, the only presence of calcite (presumably authigenic) cannot simply be compared with stromatolite formation processes. The conclusion to infer would be that conditions favourable to calcite precipitation were met early during the lake sediments deposition, but it cannot be related to stromatolite that directly.

Line 274-276: I don't think this can be used as an argument since the XRD analyses seem to have been carried out only integrating these 5 phases. In other words, calcite must be showing an inverse trend compared to the sum of the other 4. This is not problematic with the comparison of Ca content from XRF data, but the covariation is less clear in this case.

Line 276-278: I do not see the causal link between this conclusion and the argument in the sentence before. The Fig. 4 and S10 suggest a dilution effect of the calcite by the terrestrial material, regardless of the intensity of calcite precipitation in the lake or sediments, and even more independently of the stromatolite story (just above it is suggested that calcite precipitates in porewater sediments at the bottom of the lake, whereas stromatolites most likely were at the very top of the crater lake).

The interval with more calcite suggests that it is the only moment when hydrothermal activity would have actually favoured carbonate (and thus stromatolite) formation. Because of the allochthonous inputs, however, it is hard to precisely date this interval, just as it is hard to know exactly when did the stromatolites grow. See previous comments (e.g., about lines 113-115; or line 305). Therefore, whereas one can suggest that this calcite-rich interval corresponds to the moment of stromatolite formation, I think that neither this interval or moment can definitely be dated and correlated in time with absolute ages.

Line 280: I recommend adding: "... substances (allochthonous influence) diluting the calcite precipitates in the sediments..."

Line 281-284: If we considered from the above discussion, that the younger ^{14}C ages in Stage 1 *either* suggest that in situ productivity was higher during Stage 1 *or* that it was less diluted by terrestrial components than during Stage 2, how could the authors differentiate these 2 possibilities? Could they use Eu anomalies to characterize hydrothermal activity for example? Do they think that the constant 0 % calcite content during stage 2 even when terrestrial inputs are slightly decreasing is evidence for the absence of high pH / alkalinity conditions during stage 2?

Anyway, a possible increase of pH and salinity/alkalinity may be inferred at the base of the lake sediments, but the role of hydrothermalism should be proposed only then, and be further discussed and demonstrated below.

Line 286-288: Would it not be possible to recognize allochthonous vs autochthonous influences via DNA analyses?

Line 305: Since the humic substances may belong to both autochthonous and allochthonous organics (as seen for example with stage 1 and 2, respectively), the humic acid at 72m may represent a mix between old and young OM. Henceforth, how would the age of 20000 yrs be reliable? I would stick to referring to depth in the sediments rather than absolute ages, considering the uncertainties associated.

Line 307-313: As far it seems consistent that stromatolites formed during a period of higher pH/alkalinity in the lake, the dates should be presented with more caution. The ^{14}C measurements do not allow giving fixed age formation ranges in my opinion. Thus, I recommend describing that such a chain of event is plausible, but will require further constraint on the timing.

Line 314-316: I would delete: "and to assess the crater as a habitat for stromatolites.", which is redundant and reads weirdly. In anyway, I would still specify "impact crater" because many microbialites have been described in volcanic crater lakes. This point could actually be mentioned to underline the present study's novelty. Yet, could the authors describe how different the present stromatolite samples are from that described in the Ries crater (Germany) (see ref 2 Osinski et al., 2020; and Arp et al., 2013 in Meteorit Planet Sci).

Line 318: This figure would fit well as a "highlights of the paper" figure, but does not bring new information to the main manuscript. Neither the geochemical conditions resulting from the meteoritic impact nor how it influenced the processes of stromatolite formation are explicated in Fig. 6. While the oxygen oasis is brought forward, it has not been discussed at any point before the very last lines of the paper. This figure may be deleted from the main text, or revised.

Line 320-322: A (slime) temporal link was evidenced between the crater's formation and the growth of stromatolites. Direct evidence of the causal link between the two is missing, or in other words, a discussion of the environmental conditions linking the impact crater formation to the stromatolites. The phrasing should thus be much more cautious, saying that stromatolites may indeed form within hydrothermal dominated impact craters. Connecting the present observations to early Earth requires even more caution. In no way were the diversity and role/function of microbes putatively building the reported stromatolites described. The flourishing of benthic microbial communities (possibly producing O_2 , but this was not discussed/evidenced at all here) in a modern world already filled with life does not inform on what would have been the consequence of impact meteorite back then. Better understanding how this Hapcheon crater enabled stromatolite growth will be a first step in filling this gap.

Line 323: "should be considered active oxygen oases" It follows from my previous comment that such a conclusion can hardly be given. It should at best include a very conditional phrasing.

Methods and notes

Lines 358-361: This seems to present only part of the samples presented in the study.

Line 385-387: To add to previous discussion on the validity of ^{14}C dating, STR03 sample has been described in terms of growth ages based on ^{14}C data. However, this part of the method implies that it was analyzed from powders without HCl-treatment. As stated at lines 390-391, the HCl treatment should avoid a possible strong reservoir effect resulting from inorganic carbon. Then, how can the dates for this sample be interpreted in terms of periods of stromatolite growth? And could they not be affected by processes similar to what I described in point (ii) from the comment about lines 113-115.

Lines 390-392: Why was a different approach chosen for these samples? Could the authors explain their choice of treatment for each sample or at least why they used a different treatment for the different samples (e.g., 20STR03 vs. 04)?

Line 393: The Supplementary Figure 8 shows organic-rich laminae from the 20STR03 sample, which corresponds to... none of the samples mentioned just above. Such microscopic images should be provided for the HCl-treated samples.

Line 394: Please provide the time of reaction.

Line 400-402: The subsection title addresses the dating of stromatolites but lake/subaerial sediments are described here. Title or text organization should be adapted. Since a lot of data comes from the refs. 25 and 26, it would also be very useful to have a table or some text (at the start of the method for example) where one can find which data are new and which were already published.

Line 404-407: Could the authors inform approximately how much material of each category was retrieved for ^{14}C analysis?

Line 414-416: This subsection presents IBs dating, why is there a reference to stromatolites from the Fig. 3? Also, it is strange that Supp. Table 3 is about samples that are not mentioned in this paragraph.

Line 417-445: These 3 paragraphs appear in the main text as well.

Figures

Figure 2: Which area of the stromatolites were ground? Roughly the area shown with dashed yellow circles? Please specify this information in the caption. In G, there is only yellow circle for 2 drilling locations?

From the text in the methods, it is said ^{14}C analyses were performed on HCl-treated powders after micro-drilling (Fig. 2e, f) or alternatively after crushing some part of the stromatolites (Fig. 2a, b)? From the captions, it seems that all analyses were performed after HCl treatment. Please clarify this point. If there was no HCl treatment in Fig. 2a, b, please mention that the bulk powdered material was analysed and proceed similarly for all subsamples shown in this figure.

Figure 3: Similarly, the text in the method section indicates that only STR04 samples were treated HCl, but the caption does not make a distinction.

Figure 4: The figure is a bit crowded. I recommend, for example, to put the 'Stage 1, Stage 2, Stage 3' mentions with hydrothermal activity on the right side of the figure, as it's not striking at first glance, that the three coloured bands correspond to these three stages.

Figure 6: See comment about line 318.