nature portfolio

Peer Review File

Iron-rich microband formation in marine sediments by hydrothermal iron cycling bacteria at Lucky Strike

Corresponding Author: Dr jeremie Aubineau

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

Version 0:

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 Aubineau,

Your manuscript titled "Iron-rich microband formation in marine sediments by hydrothermal iron cycling bacteria at Lucky Strike" has now been seen by 4 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. Should additional work allow you to address these criticisms, we would 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.

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In particular, please ensure that the revised manuscript meets the following editorial thresholds:

**Present a compelling and clear assessment of the extent to which the study makes a novel contribution to iron-rich microband formation through iron-cycling bacteria, and discuss your findings in the context of the existing literature.

**Fully clarify the type of BIFs that the study is applied to and resolve all discrepancies between the model and the BIF framework.

**Provide full justification for your methods and assumptions.

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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,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements
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For Manuscripts that fall into the following fields:
• Behavioural and social science
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• Life sciences
An updated and completed version of our Reporting Summary must be uploaded with the revised manuscript
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For your information, you can find some guidance regarding format requirements summarized on the following checklist: (https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This manuscript systematically describes the petrography, mineralogy, and geochemical signatures of some Fe-rich microbands from the Lucky Strike hydrothermal vent system. From these evidences, authors depict a comprehensive process of the formation of these bands, from microbial Fe(II) oxidation by zetaproteobacteria, to biotic and abiotic precipitation of ferrihydrite/Fe(III) minerals, and eventually to the early diagenesis of primary BIF sediments via dissimilatory Fe(III) reduction to form siderite and magnetite. These microbands share very similar characteristics with some microbands that we commonly see in Banded Iron Formations. Lastly, authors offer a depositional model of these microbands. They conclude that this study helps to delineate both biotic and abiotic processes that are potentially involved in BIFs formation; and explain some of the key but still disputable characteristics of BIFs, such as low organic carbon content.

While that this study will be of interests of deep-time geochemistry and geobiology communities, I believe this manuscript does not fully meet the publication criteria of the Communications Earth and Environment. Therefore, my recommendation is a rejection. My concerns are described and explained below.

My biggest concern is the lack of novelty of this study. The examination of the sediments from this particular study location is both new and well done. The mineralogical, petrographical, and geochemical analysis are indeed detailed and comprehensive. However, the discussions and subsequent conclusions from this study really lack novelty and thus provide only limited new insights. Some of the major conclusions from this study include: microbial Fe(II) oxidation is important for BIFs formation; dissimilatory Fe(III) reduction is coupled with anaerobic Corg oxidation and thus the microbands are organic lean, siderite and magnetite are secondary diagenetic minerals via DIR; and formation/deposition of Si-rich bands is contributed to both a restricted basin morphology and episodic hydrothermal venting. Most of these findings, or at least significant parts of them, are already well known and have been extensively studied from a plethora of previous studies. The following are some what I consider as key references regarding each of findings described above. Authors should go through some of them, incorporate them into the texts, and also discuss/emphasize the novelty of their findings compared to the following studies, which I think should be a vital part of this work.

Refs regard microaerophilic Fe(II) oxidizing bacteria in the context of BIFs
Chan, C.S., Emerson, D. and Luther III, G.W. (2016) The role of microaerophilic Fe-oxidizing micro-organisms in producing banded iron formations. Geobiology 14, 509-528.
Emerson, D., Fleming, E.J. and McBeth, J.M. (2010) Iron-oxidizing bacteria: an environmental and genomic perspective. Annu. Rev. Microbiol. 64, 561-583.
Kappler, A. and Straub, K.L., 2005. Geomicrobiological cycling of iron. Reviews in Mineralogy and Geochemistry, 59(1), pp.85-108.

Refs regard DIR in the context of BIFs
Konhauser, K.O., Newman, D.K. and Kappler, A., 2005. The potential significance of microbial Fe (III) reduction during deposition of Precambrian banded iron formations. Geobiology, 3(3), pp.167-177.
Posth, N.R., Konhauser, K.O. and Kappler, A., 2013. Microbiological processes in banded iron formation deposition. Sedimentology, 60(7), pp.1733-1754.

Refs regard Si bands and Si precipitation in the context of BIFs
Klein, C., 2005. Some Precambrian banded iron-formations (BIFs) from around the world: Their age, geologic setting, mineralogy, metamorphism, geochemistry, and origins. American Mineralogist, 90(10), pp.1473-1499.
Maliva, R.G., Knoll, A.H. and Simonson, B.M., 2005. Secular change in the Precambrian silica cycle: insights from chert petrology. Geological Society of America Bulletin, 117(7-8), pp.835-845.
Schad, M., Halama, M., Bishop, B., Konhauser, K.O. and Kappler, A., 2019. Temperature fluctuations in the Archean ocean as trigger for varve-like deposition of iron and silica minerals in banded iron formations. Geochimica et Cosmochimica Acta, 265, pp.386-412.

My second issue is the ambiguity of this study. After reading it a few times, I still have no idea what type of BIFs this study is applied to, and also what age(s) of BIFs authors attempt to relate this study to. I think authors should make it loud and clear at the beginning of this manuscript with these questions. Also, what makes this study unique? Hydrothermal systems have been extensively studied, including the Loihi Seamount.

My third issue is authors' attempts to use a MOR as an analogue depositional environment for BIFs in general. I can understand that some reactions authors mention here may be applied to BIFs. However, I am not fully convinced that MOR is a suitable analogue for BIFs deposition because the depositional environments of BIFs are not the same as the location where the Fe is sourced and no BIFs has been interpreted as being a MOR deposit.

The following are my detailed comments:

Line 34: There should be no fundamental mechanistic difference between the deposition of microbands and other parts (i.e. meso- and macro-bands) of the same iron formations at the same location.

Line 50-51: I would specify which type of BIFs, Superior-type or Algoma-type? Based on the tectonic setting of the study location, Algoma-type BIFs should be the obvious answer. These two types of BIFs are drastically different and authors should make it clear at the very beginning.

Line 56-61: This is implying that there was dissolved oxygen in the deep-water realm in the Archean, which has never been shown.

Line 93-116. Refer to my third point above.

Line 168: I would delete "for the sake of simplicity"

Line 174-175 Is the EDS analysis quantitative? Or is this a simple relative ratio? If it is the latter, then there is no enough evidence to state that there is only little Si in this scenario since Fe concentration is not quantified either.

Line 284-287 need refs here

Line 292 the widespread association…..has been….

Line 318 How about the pH of the hydrothermal fluids? It also controls the surface reactivity of Fe minerals. How does pH change with temperature for such complex fluids like hydrothermal fluids?

Line 302 I cannot see the implication and relevance of this section in this manuscript. The take-home message from this section is unclear and how it is related to BIFs is not explained.

Line 366 I would have to question if this 6 percent of P concentration would make a huge difference in primary productivity. What is the equivalent P concentration in ppm or M?

Line 427 please explain what do you mean by "hydrothermal plumbing circulation system". This is ambiguous and unclear. Also, does this mechanism work on continental shelves? If not, then this statement is overly generalized and can only be applied to a very specific tectonic setting.

Line 829 Figure 2. Both g and h, the micrographs of the stalks are blurry and difficult to read.

Reviewer #2 (Remarks to the Author):

Review of COMMSENV-24-2828: Iron-rich microband formation in marine sediments by iron cycling bacteria at Lucky Strike

Summary:
The authors present an interesting study in which they performed an in-depth mineralogical and (isotope) geochemical characterization of the Lucky Strike hydrothermal vent site. They combined mineralogical observations (e.g. XRD, FTIR, 57Fe Moessbauer spectroscopy and electron microscopy) with geochemical whole rock analysis (e.g. digestions, XRF and lectron microprobe measurements) and stable isotope analysis (C and N isotope systems) to link mineral deposition and transformation with previously established microbial activity. The authors suggest that the mineralogy observed at their sampling sites is the result of the metabolic activity of microaerophilic Fe(II)-oxidizing bacteria (zetaproteobacteria) coupled with the early diagenetic activity of dissimilatory Fe(III)-reducing bacteria. Finally, the authors put their work in the context of the deposition of Precambrian Banded Iron Formations (BIF) and conclude that the processes observed at the field site could potentially explain the presence of organic carbon-depleted Fe-rich microbands in BIFs.
The manuscript is certainly timely as it seems to move the interpretation of the BIF rock record forward by connecting (microbial) processes which so far have only been studied separately. Overall, the manuscript is very well written, interesting and relatively easy to understand. However, occasionally the language tends to be a little bit convoluted as I will later point out in my line-by-line comments. In general, the study seems to be conducted in a careful and diligent way and the methods employed are appropriate. Further, I did not find any major flaws in the interpretation of the resulting data. However, I do not have any specialist knowledge on stable isotope geochemistry and can, therefore, only offer limited judgement in this regard.
My major problem with this manuscript is the ambiguity when it comes to its contextualization. Major BIFs (Superior-type BIF) are generally believed to have been deposited over the continental shelf and are the result of a combination of (microbial) processes occurring in the water column, the soft sediment and later during low-grade metamorphism. By contrast, minor (and older) BIF (Algoma-type BIF) are generally associated with hydrothermal and volcanic processes. It is not quite clear which type of BIF the authors are envisioning their model to fit and there seems to be some disconnect between the interpretation of the study outcome and the consensus on BIF deposition (see further discussion below). My feeling is that this should be clarified before the manuscript can be published.
In conclusion, I think that this is a great paper. However, it needs some more polishing and re-contextualization before publication. I have added further comments below and hope that the authors may find them useful. If necessary, I would be glad to review this article again.

Major comments:
As already pointed out above, while everything seems to be in order regarding the data acquisition and interpretation, my major concern is its contextualization.
There are several reasons for this (see e.g. Klein, 2005; Bekker et al., 2014 or Konhauser et al., 2017, for references):

1) This work describes microbial mats that are closely related to a abyssal hydrothermal system and hydrothermal effluent. It is correct that the Fe source for BIF deposition is believed to have been hydrothermally sourced Fe2+, the primary minerals during BIF deposition are generally agree upon to have been deposited through water column processes (either in the form of Fe(II) or Fe(III) minerals). However, BIFs are not considered to have been formed through microbial mats (as implied in lines 392-395). Instead, as the authors correctly point out in line 62, one major theory for BIF deposition envisions a “water column-derived ferrihydrite rain” to create BIF.
While it is conceivable that microaerophilic Fe(II)-oxidizing bacteria would have contributed to the deposition of BIFs, it is unlikely that they did so as part of microbial mats. At least when considering the deposition of major BIF. Instead, they would have needed to be planktonic (see for example the work of Dr. C. Chan, University of Delaware) to be able to migrate along the redox gradient over the shelf area and not being dragged to the seafloor by the stalks and sheaths and associated minerals (see e.g. your line 28 or 291).
If the authors want to keep their work within the framework of the deposition of major (Superior-type) BIF, they may want to add a clarification to their discussion. Alternatively, the authors could instead put their model within the framework of the genesis of smaller (Algoma-type) BIF which are generally associated with hydrothermal systems and (submarine) volcanism. The authors seem to already partially have done that in that the BIF described in their reference 4 seems to fall under the Algoma-subtype. The authors could put their work within the framework of Algoma-type BIF and then speculate that under certain circumstances the same processes could be responsible for the genesis of Superior-type BIF.

2) Redoxcline and oxygen penetration depth in relation to depth of deposition.
If assumed that abyssal microbial mats formed by microaerophilic Fe(II) oxidizers indeed contributed to BIF genesis, we face another challenge: The depth of the redoxcline within the Precambrian ocean.
While there is still some ambiguity regarding the exact oxygen concentration leading up to and following the Great Oxidation Event (see e.g. Lyons et al., 2024), the way it is currently contextualized in the manuscript, it would require full-depth oxygenation of the early ocean. However, this is at odds with the requirements of other rock formations/ geochemical evidence of that time which generally require/ suggest that at least the deep oceans remained anoxic until some time after

the GOE.
It is not unlikely that microbial mats consisting of microaerophilic Fe(II)-oxidizing bacteria contributed to BIF genesis in some fashion. But I suggest that the authors carefully think about the way they contextualize their work. There are certain discrepancies between the proposed model and the sedimentological and geochemical framework of BIF deposition which require some clarification and reworking.
The obvious caveat is that there are no optimal modern analogues that perfectly resemble the Precambrian water column and sedimentary systems. In this regard the field site utilized in this study already represents a great fit. However, care should be taken to point obvious deviations out and make a good effort to explain/ discuss them.

Line-by-line comments:
Line 29-32: This sentence seems to be quite long and complicated. I suggest to either rephrase it or shorten it.
Line 38: What do you mean by "below wave action depth"? BIF are generally considered to have been deposited below storm wave base.
Line 42: "banded BIFs". That is doubling down on the name. Please remove the "banded".
Line 42/ 43: This should be "at THE time of deposition".
Line 48: Please define "deep sea BIF". Major BIFs have been suggested to have been deposited on continental shelves. Is that what you envision here?
Line 50: As I pointed out in my major comment, I am a bit uncertain about the connection you make between your "modern abyssal hydrothermal" field site. So again, what kind of BIF do you envision to be formed via this mechanism?
Line 69-72: This sentence is quite long and convoluted. I suggest simplifying it for clarity.
Line 77-79: You infer a lack of modern analogues for the Precambrian Ocean here. But there are a variety of lakes throughout Europe, East Africa and Southeast Asian which have been intensely studied and that are frequently used as modern analogues for the early ocean (e.g. Crowe et al., 2008; Llirós et al., 2015; Koeksoy et al., 2016; Friese et al., 2021; Roland et al., 2021; Vuillemin et al., 2018, 2023, etc.). I, therefore, don't necessarily agree with this statement. Please revise.
Line 80: Replace "poor" with "low".
Line 84: Regarding the organic matter content of BIFs please carefully double-check throughout the manuscript. You cite an organic carbon content of less than 0.5 wt.% here. However, at a later point you cite 0.8 wt.% (line 384). Please check for consistency.
Line 84-88: Again, this sentence is quite convoluted. Please simplify.
Line 89: I suggest replacing "paramount" with "ongoing".
Line 101-116: I am just wondering if this paragraph shouldn't go to the Materials and Methods as sth. like "description of field site"? You can still provide the most important information here but the way it is currently presented it seems to be too excessive for the introduction.
Line 144-148: This sentence is too long and convoluted. Please rephrase or break it up into shorter sentences.
Line 156/ 157: "at THE NTE site".
Line 179: "corals-like" should be singular.
Line 202: "vary considerably" instead of "considerably vary".
Line 203: Replace "whereby" with "where" and put "<0.4%" into parentheses.
Line 204: Again, put the P values in parentheses.
Line 235: Replace "highly differ" with "differ considerably".
Line 266: "subsequently". Remove.
Line 267: "Fe(II)-reducing bacteria". Shouldn't this be "Fe(III)-reducing bacteria?
Line 268-275: I don't understand the logic of these two sentences. Please rephrase for clarity. Something seems amiss here.
Line 305: "… transformation to lepidocrocite …". Please replace "to" with "into".
Line 306: Please replace "… our observations in Y3." with "… our observations at the Y3 site."
Line 309: Either "show ABUNDANT organic molecules" or "show AN abundance of organic molecules".
Line 330/ 331: Sentence following the "whereby". I would rephrase the second half of the sentence into "where the stalk-forming Zetaproteobacteria and organic molecules facilitate the nucleation of biogenic Fh."
Line 334: "texture" should be plural.
Line 347: Wording. Replace "copious" with either "abundant" or "high".
Line 395 and 396: Recent work by Rico et al. (2023) actually showed that at least the Fe(III)-reducing bacterium they utilized in their study was not able to oxidize complex organic matter and couple this to Fe(III)-reduction. Further, to my knowledge, Fe(III)-reducing bacteria generally prefer low molecular weight organic compounds and are usually dependent on fermenting bacteria to make complex organics available to them. I don't know if such bacteria are known to occur on your field site?
Line 396: Replace "small" with "little".
Line 413: "previously discussed" instead of "discussed previously".
Line 422: "aqueous Si" instead of "Si aqueous".
Line 423: "mineral" should be plural.
Line 424: It should be "THE deposition of BIF-like …".
Line 427: What do you mean by "hydrothermal PLUMBING circulation system"? "plumbing" seems to be the wrong word here. Please clarify.
Line 434-436: I am a bit confused here. How do you define biomass? Are you referring to the presence of actively metabolizing cells? Because in my mind "microbial filaments" should generally be part of the biomass. Please clarify.
Line 442: "It is suggested THAT this process points …".
Line 443: "proposed" should be "propose".
Line 444: Why are Si-rich bands absent from BIFs? They are one of the core components of BIFs and in fact quite abundant on any scale from micro- to macrobands. You mean that this explains the absence of Si-rich bands at your field site? Please revise and/ or clarify.

Line 522: “Prior TO analysis …”.
Line 523/ 524: Wouldn’t this high pressure and temperature result in the transformation of some of the Fe minerals? Please clarify.
Line 536/ 537: Why was specifically this velocity range chosen? Also, for identification of Fe(III) minerals 77 K would have been better. I assume the measurements were conducted at these temperatures due to the absence of a cryostat? Also, please define "room temperature". Assumed room temperature is 20°C, this should be ~293 K. Do these 7 K really make a difference for the measurements?
Line 560/ 561: Please rephrase. “… solutions diluted TO DIFFERENT CONCENTRATIONS.”

Figures:
The figures are very nice. However, I would personally prefer if the pictures/ photomicrographs in Fig. 1-4, 6 and 7 could be lined out with a black line to better distinguish them from the background. I am also wondering if the font size in Fig. 5 and Fig. 8 could be increased a bit? Some of the legends are quite small.

References:
Bekker, At, et al. "Iron formations: Their origins and implications for ancient seawater chemistry." Treatise on geochemistry. Vol. 12. Elsevier, 2014. 561-628.

Crowe, Sean A., et al. "Photoferrotrophs thrive in an Archean Ocean analogue." Proceedings of the National Academy of Sciences 105.41 (2008): 15938-15943.

Friese, André, et al. "Organic matter mineralization in modern and ancient ferruginous sediments." Nature communications 12.1 (2021): 2216.

Klein, Cornelis. "Some Precambrian banded iron-formations (BIFs) from around the world: Their age, geologic setting, mineralogy, metamorphism, geochemistry, and origins." American Mineralogist 90.10 (2005): 1473-1499.

Koeksoy, E., Halama, M., Konhauser, K. O., & Kappler, A. (2016). Using modern ferruginous habitats to interpret Precambrian banded iron formation deposition. International Journal of Astrobiology, 15(3), 205-217.

Konhauser, Kurt O., et al. "Iron formations: A global record of Neoarchaean to Palaeoproterozoic environmental history." Earth-Science Reviews 172 (2017): 140-177.

Llirós, Marc, et al. "Pelagic photoferrotrophy and iron cycling in a modern ferruginous basin." Scientific reports 5.1 (2015): 13803.

Lyons, Timothy W., et al. "Co-evolution of early Earth environments and microbial life." Nature Reviews Microbiology (2024): 1-15.

Rico, Kathryn I., et al. "Resolving the fate of trace metals during microbial remineralization of phytoplankton biomass in precursor banded iron formation sediments." Earth and Planetary Science Letters 607 (2023): 118068.

Roland, Fleur AE, et al. "The possible occurrence of iron-dependent anaerobic methane oxidation in an Archean Ocean analogue." Scientific reports 11.1 (2021): 1597.

Vuillemin, Aurèle, et al. "Metabolic potential of microbial communities from ferruginous sediments." Environmental Microbiology 20.12 (2018): 4297-4313.

Vuillemin, A., et al. "Authigenic minerals reflect microbial control on pore waters in a ferruginous analogue." Geochemical Perspectives Letters 28 (2023):

Reviewer #3 (Remarks to the Author):

The abstract of the manuscript is very well written and concise to a level not often seen in geoscience literature, and the authors should be complimented on that. The rest of the manuscript is also well-written and can be followed. The structure is appropriate and most of the arguments are backed by the observations and data presented. I agree with most of what the authors are proposing, and the work could be an example of where the principal of uniformitarianism could be applied, to some extent, to Precambrian BIF, where many previous works have concluded that this cannot be done. Where the presented work do not compare as well to Precambrian BIF, the authors mostly highlight this and provide reasonable explanations for the observed differences.

I do believe, however, that some facets of the manuscript can be improved. This is not so much related to the observations, data, and ideas proposed by the authors, but rather where they could clarify certain aspects and consider some other aspects. Below are my major comments on this manuscript that need to be addressed by the authors:
1. In lines 80 to 92 the authors provide some good literature background on why there is a lack of direct evidence for biological activity associated with BIF deposition, which I agree with. However, it is worth noting that some examples exist

where more direct potential evidence for biological activity associated with iron formation deposition has been documented. The examples include potential fossilized bacterial filaments in IF-associated jasper (Dodd et al., 2017), oncolites (i.e., spherical stromatolites) in shallower water GIFs (Smith et al., 2017; 2020) and stromatolites interbedded with IF (Smith, 2018).

2. Magnetite is by far the most common Fe-ox mineral in pristine (i.e., not outcrop) Precambrian BIF samples (e.g., Beukes, 1980; Smith et al., 2013; Li et al., 2017) but appears to be much less abundant in the samples studied. The authors take good care to try and explain the origin of the magnetite, but it would be good if they could mention and comment as to why magnetite over hematite preservation is favoured in Precambrian BIFs.

3. The authors make good comparisons between the C isotopes in carbonates where there is high TOC, and where there is low TOC, showing the latter isotopes to be more depleted in 13C, implying carbonate formation through organic C oxidation. However, overall, many of the 13C(carb) values are much higher than what has been observed in the geological record. Why are the values so much higher than for Precambrian BIFs (d13C(VPDB) of ca. -20 to -6 per mil) and Precambrian carbonate platforms (d13C(VPDB) of ca. 0 per mil)? Can the authors comment? I think their comparisons and conclusions are valid, but why do they show such high values even though they show the expected relative trend?

4. Lines 373-379: If I read this correctly, the authors want to imply that what they interpret as Si dislodgement during transformation of filamentous to more crystalline structures and an associated increase in Fe/Si ratio could explain the alternating Fe-rich and Fe-poor banding in BIFs. Is it therefore implied that the banding could be diagenetic? Is this happening at a scale in the studied material that is comparable to band widths in BIF? How would this fit with recent work that has been done (Bau et al., 2022; Mundl-Petermeier et al., 2022, Wainwright et al., 2024) that seems support that BIF banding is a depositional/primary feature? However, later in the manuscript in lines 405-428 it appears as if a primary, hydrothermal venting and Si oversaturation (i.e., a primary depositional process) plays a role in forming Si-rich bands. Are the authors implying that the Fe and Si banding could be both primary and diagenetic? I think this should be cleared up by explaining and delineating these processes better and how such banding could be distinguished when studying samples.

Below are more detailed comments and recommended corrections:

- Please note that minor improvements can be made with regards to grammar in some places, and this can be easily addressed with some editorial input prior to publication
- Line 42: “banded BIFs” means “banded banded iron formations”, which makes little sense. I would recommend deleting “banded” or changing it to “well-laminated” or something similar.
- Line 42: Change “chemical rocks” to “chemical sedimentary rocks”.
- Lines 42-44: References need to be added for this statement.
- Lines 64-65: Magnetite is the much more common Fe-oxide phase in pristine BIFs overall (Beukes, 1980; Li et al., 2017). These lines should be adjusted to convey this more accurately.
- Line 71: There is great debate about the exact timing of the GOE (ca. 2.50-2.19 Ga; e.g., Bekker et al., 2004; Gumsley et al., 2017; Warke et al., 2020; Poulton et al., 2021; Hodgskiss and Sperling, 2022), and the information in this line should be updated to provide a range, which would be better considered and more accurate.
- Line 105: Change “generation” to “deposition”.
- Line 128: Add “potentially” in front of “involved”.
- Line 267: Should this not be “Fe(III)-reducing bacteria”?
- Lines 322-324: Specify where the slightly larger Fh particles nucleate by adding “at the former” at the end of the sentence.
- Line 334: Change “texture” to “textures”.
- Line 339: Change “On the other hand” to “However”.
- Line 351: Change “since” to “because”.
- Line 353: Add Dymek and Klein (1988) as a reference along with Bau and Dulski (1996).
- Line 371: Add “was” in front of “lost”.
- Line 443: Change “proposed” to “propose”.

Kind regards,

Bertus Smith
Associate Professor
Department of Geology
University of Johannesburg

References:

- Bau M., Frei R., Garbe-Schönberg D.,Viehmann S.,2022: High-resolution Ge-Si-Fe, Cr isotopes and Th/U data for the Neoarchean Temagami BIF, Canada, suggest primary origin of BIF bands and oxidative terrestrial weathering 2.7 Ga ago. Earth and Planetary Science Letters 589, 117579.
- Bekker, A., Holland, H.D., Wang, P.-L., Rumble III, D., Stein, H.J., Hannah, J.L., Coetzee, L.L., Beukes, N.J., 2004. Dating the rise of atmospheric oxygen. Nature 427, 117–120.
- Beukes, N.J., 1980. Lithofacies and stratigraphy of the Kuruman and Griquatown Iron Formations, northern Cape Province, South Africa: Transactions of the Geological Society of South Africa, 83, 69–86.
- Dodd, M.S., Papineau, D., Grenne, T., Slack, J.F., Rittner, M., Pirajno, F., O’Neil, J.O. and Little, C.T.S., 2017. Evidence for early life in Earth’s oldest hydrothermal vent precipitates. Nature, 543, 60-63.
- Dymek, R.F. and Klein, C., 1988. Chemistry, petrology and origin of banded iron-formation lithologies from the 3800 Ma Isua supracrustal belt, West Greenland. Precambrian Research, 39, 247-302.
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Reviewer #4 (Remarks to the Author):

Review: Communications Earth & Environment
Iron-rich microband formation in marine sediments by hydrothermal iron cycling bacteria at Lucky Strike

Summary:
As presented in this communication, the authors studied microbial mats at the Lucky Strike hydrothermal vent field to develop a model of BIF deposition and mineral transformation. The authors aim to offer direct evidence for a mechanism BIF deposition by aerobic iron-oxidizing bacteria, particularly in light of the organic carbon-poor iron oxide/carbonate-rich bands found in these formations today. Mat samples were collected in situ from various sites in the region. Data related to the texture and mineralogy, bulk geochemistry, spatially-resolved chemistry (electron microprobe), and C- and N-isotope analysis of the mats is presented and discussed. This data is subsequently related to a microbial model of mineral formation and transformation in BIF based on marine Fe(II)-oxidizing Zetaproteobacteria.

The idea that the authors present is intriguing and not hard to get behind (a microbial mat model for mineral formation and transformation that also places some limitations on the preservation of organic carbon). However, at the moment, this manuscript requires much reworking to more convincingly lead the reader through the data available and the intricacies of the authors´ arguments. The mechanisms of mineral formation and transformation in the mats must be more in focus. I recommend major revision here, and make general and details comments below that I hope will highlight weaknesses and recommend some remedies.

General comments:
1. The authors are presenting a microbial mat model for BIFs here, but only superficially discuss the organisms forming these mats. It is clear that a large portion of the work on the microbes on site is presented in another manuscript to which the authors refer. Nonetheless, this manuscript, and the arguments brought forth and detailed in Fig 8, do not stand strongly without more information provided to the reader about the microbial players at the center of the story. Without this, how can the connection between the microbes and the data presented here be made? I highly recommend that the authors bring in more information about the microorganisms they are building a model on. It would be necessary to describe them, their distribution (at hydrothermal fields, but also in coastal regimes), and the type of minerals found in their association at other hydrothermal vent locations. I do not see why this has not been done, particularly as it seems that relevant data has been published – why not do this and cite the relevant work? But, in the end, the reader must be able to follow fully.

2. The vent sites are heterogeneous on many levels, as is seen throughout the results. This is, of course, part of working with environmental samples. But, it does not make the task of using environmental samples for the purposes of building a model for events in the far past any easier. Also, the authors wish to discuss mechanisms of mineral transformation across many sites. For this reason, the description of the field, the location of the sites, their relation to one another, all this must be as clearly communicated as possible. And, it must be consistent to the figures.

3. The authors describe Fe-rich crumbly mats and Fe-rich flocculent mats, but a more in depth discussion of what this means is lacking.

4. Why is Fig 8 only discussed in the conclusion? Is this not the central figure that should tie everything together and main

message? If so, then this requires the authors to guide the reader through more thoroughly. By the conclusion, the reader should feel a sense of déjà vu, or a restatement, but rather it is the first time the reader sees the main point of the manuscript. The authors must dedicate more of the discussion linking their evidence to this figure. It is currently lacking.

5. There are 3 points outlined in Lines 388-403 that are all conceivable in my opinion. However, I am not convinced that the manuscript, as currently written, clearly evidences these three points. I recommend that the authors revisit this writing to evidence these points systematically for publication.

6. Do the authors envision this mechanism they describe here as being important throughout the entire period of BIF deposition? If not, then what are the limitations? Can they speak to this in the manuscript?
Detailed comments:

Abstract, line 28: What is meant by “profuse production” … Is this more than usual for this microbe? This sounds this way. And if so, that what triggers this profuse production?

Line 42: banded BIFs…banded Banded iron formations, remove one. Same line “chemical rocks” what is meant? Chemically formed? Abiogenic?

Line 56: I do not agree that all previous models suggested only planktonic microorganisms. Mats have been discussed before.

Line 59: in oceanic, but also coastal photic zones.

Line 62: “ferrihydrite rain” – is a citation required?

Line 77: starting with “One reason…” I do not disagree, but I also argue that maybe there are multiple precursors and mechanisms – and that this is also the reason for the disagreements.

Lines 103-108, starting with “To assess…” What are the limitations of studying modern hydrothermal vents to understand the past? Can the authors please add some lines to address the potential limitations (along with the strengths) of the site as a proxy? In other words, how strong is this link?

Lines 122-123: Which are the dominant organisms in these mats? Please add as it should only strengthen the argument for the proxy.

Lines 125-130: Can the authors be more specific as to which analysis were used here to build this model of microbial mat development using this proxy?

Line 163-165: Can the authors cite similar interpretations?

Line 174: Do the authors mean the FeOx at the top and the bottom of the mat? Are the FeOx minerals in Y3 silica poor? Unclear here.

Line 190: ending with “not identified” Anywhere? Or at which mat sites? Unclear.

Line 194-206: How can we interpret this heterogeneity in a useful way across time and space here, where SiO_2 is discussed as well as later on where C and N systematics are discussed? What does the variation in the sites mean for the model they are describing? The authors must provide some more guidance in the discussion to strengthen the efficacy of their model.

Line 210: “the above results”…which results are meant, specifically?

Line 253: “consistent with” but not indicative of. Can the authors speak to this?

Line 258-259: This is way too late and way to subtle to be the first mention that Zetaproteobacteria are foundational to the mats in these samples. The manuscript needs a massive reframing for it to stand alone. And there needs to be more images, perhaps of these organisms or a description of what their mineral forming mechanisms are – here to build this bridge to the past as the authors wish.

Lines 258-275: The interpretation of C- and N- isotopic signatures here is challenging. At best, this is due to the large disparity in temporal and spatial scales between the processes under study. Unless the authors really develop the discussion to lead the reader deftly through, I fear the reader is left unconvinced of the author’s conclusions.

Line 273-275: Starting with “Instead, …” this sentence is very difficult to follow.

Line 277-283: Perhaps a conceptual drawing here would help to bring home the point.

Line 287: after “nucleation sites” – figure citation?

Line 299: Figure? Reference?

Line 306: “in agreement with our observations in Y3” Only at that site? Why? And why not the others? There is a need to improve the ability to understand differences and similarity between the sites for the reader.

Lines 314-316: Starting with “Further, our mineralogical…” This is interesting! Can the authors provide evidence for this here and discuss? There is more focus needed in general in evidencing the mechanisms proposed. How does this organism drive precipitation and mineral character/transformation in the mats, etc.

Lines 322-326: Again, there is a lack of conceptual drawing to distinguish the dominant controls at the sites that drive the variation seen within the model proposed.

Lines 337-342: Starting with “Goethite...” What drives or suppresses this transformation in these mat environments?

Line 348: What is “all evidence”? Can the authors be specific?

Line 354: “indicates that the δ13C-carb values are controlled by alternative mechanisms” How? Please explain, as rather central to the argument the authors bring.

Lines 360-365: This is a long and confusing sentence. Please reword.

Line 366: “FeOx-rich layer compared to the Fh-rich layer” The terms are not making sense. Is Fh not ferrihydrite? Is that not an FeOx?...

Line 368-372: I am happy to read this interpretation. I agree.

Line 373-379: This information again is coming so late in the manuscript for a paper on the role if microbial mats in the deposition of BIF…And again, I would like to see a conceptual image showing this idea, rather than just a description.

Line 405, Section Formation of Si-rich bands: this is an intriguing concept, but not sufficiently deconstructed here yet.

Line 443-444: “the striking absence of Si-rich bands in BIF”…What do the authors mean by this? I don´t agree.

Figure 1: The font in the labels for the sample sites are very hard to read due to the red color and the small size. These labels could also include the acronyms uses throughout the manuscript here as in the caption. The consistency will help keep the reader on track. Please change.

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 Aubineau,

Your revised manuscript titled "Iron-rich microband formation in marine sediments by hydrothermal iron cycling bacteria at Lucky Strike" has now been seen by our original 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.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. Specifically, please tone down your claims of novelty, acknowledging previous research and uncertainties. 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.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor
Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

After carefully reviewing authors’ responses to my comments and the revised manuscript, I believe that this manuscript may be suitable for publication following consideration of the minor comments and edits outlined below. Authors adequately addressed my major and minor concerns, made sufficient and more convincing changes to the manuscript, and more importantly highlighted the geological context and novelty of their work. Their revisions, especially the enhanced discussion of microband formation in the context of Algoma-type BIFs as well as their detailed analysis of their samples, will offer some valuable insights for our community. Lastly, I would like to thank the editor for the opportunity to review this work and thank authors of this manuscript for their contribution.

Minor comments:
Line 330: Should be Fe(III) reducing bacteria

Line 507 Fermenters also produce H2 to support C fixation for Fe(III) reducers…I would probably rephrase this sentence as such: fermenters also produce H2 to facilitate Fe(III) reduction and oxidation organic C molecules for Fe(III) reducers. The point here is that "C fixation" implies autotrophy, which is mostly not true for Fe(III) reducers.
Line 556 How about pH. The solubility of Si is also heavily dependent on pH and it also happens that hydrothermal fluids tend to have different pH from background seawater. In this case, the pH of hydrothermal fluids is significantly lower than ambient seawater. This increase in pH up to ambient seawater pH of 8 could also increase the solubility of Si and thus inhibit Si precipitation. I wonder if authors have considered this factor, other than temperature. If so, why pH is not considered a primary factor here.

Reviewer #2 (Remarks to the Author):

Review of COMMSENV-24-2828A: Iron-rich microband formation in marine sediments by iron cycling bacteria at Lucky Strike

Summary:
The authors present a revised version of a previously submitted manuscript (COMMSENV-24-2828). They performed an in-depth mineralogical and (isotope) geochemical characterization of the Lucky Strike hydrothermal vent site. They use this data in combination with previously published data on the microbial community composition to suggest a model of microbially driven Fe(III) mineral precipitation [microaerophilic Fe(II)-oxidizing bacteria (zetaproteobacteria)] coupled with subsequent early diagenetic mineral transformation by dissimilatory Fe(III)-reducing bacteria at their sampling site. Finally, the authors put their work in the context of the deposition of Banded Iron Formations (BIF), specifically Algoma-type BIF. They conclude that the processes observed at their field site explain the presence of organic carbon-depleted Fe-rich microbands in these Algoma-type BIFs, processes which potentially also extend to the much larger Superior-type BIFs.
I find this iteration of the manuscript much improved over the initial submission, especially since the authors went back and, (1) refined their model for the deposition of Fe-rich microbands in BIFs, (2) narrowed-down and streamlined the relevance of their findings and, (3) now provide additional/ refined (supplemental) information (e.g. microbial community composition) and discussion (mineralogy, microbial mats and Algoma-type BIFs) relevant to the content and context of the paper. My previous comments have been addressed to my satisfaction, and I do not find any major problems regarding language, logic or context within the paper. My only remaining major issue is that the supplementary tables that the authors frequently refer to seem to be missing (at least I did not see them among the reviewer files) and I would appreciate it if the authors could still add these.
In conclusion, I stand with my initial assessment that this is a great paper. Except for the missing supplementary tables I only have various minor suggestions and I recommend a minor revision following which (at the editor's discretion) the paper should be fit to be published. If necessary, I would be happy to review this manuscript again.

Major comments:
1) Supplementary tables. The supplementary tables S1-6 which the authors frequently refer to in the main text and throughout the supplemental information seem to be missing. Based on the context in which they are cross-referenced in, they seem to contain important supplemental mineralogical, geochemical and modelling results. I would, therefore, kindly ask the authors to either add these tables to the supplemental information or make the respective files available along with the remaining manuscript.

Line-by-line comments:
Line 39: Replace "clues" with "key information".
Line 39/40: Last part of the sentence. Replace "… from which they formed and iron cycling in the early oceans." with "… from which they WERE formed and ABOUT THE iron cycling in the early oceans."
Line 44: Replace "deep-seawater" with "deep sea".
Line 45: Add an "appear" after "sporadically".
Line 51: Remove the "consequently" at the beginning of the sentence.
Line 52: Second half of the sentence after the comma. I don't quite understand what you mean by this. Please rephrase for clarity.
Line 54: Replace "…, formed when thick ice sheets deoxygenated seawater, …" with "…, WERE formed when thick ice sheets RESULTED IN THE DEOXYGENATION OF THE seawater, …".
Line 57: Replace "Overall" with "Generally". Remove the "content" after "Fe".
Line 60: Replace "were" with "was".
Line 69: Replace "insufficiently explored" with "understudied".
Line 77: Remove the "the" before "microaerophilic".
Line 78: Remove the "the" before "Zetaproteobacteria".
Line 84: Replace "consist predominantly of" with "are dominated by".
Line 85: Remove the "respectively".
Line 88: Beginning of the line insert "rather" before "than". Replace "in" before "oxidant-depleted" with "under".
Line 90: Sentence following "… prior to the GOE." Replace current start of the sentence with "Based on this theory, FeOx in Archean BIFs are …".
Line 93: Add a "mineral" between "stable" and "phases". Also, maybe give one or two examples of such stable mineral phases.
Line 95 & 96: Replace the current sentence with "Consequently, there is currently no consensus on the dominant Fe mineral precursor phase to BIFs.

Line 99: Replace “the proper” with “appropriate”.
Line 102: It is not entirely clear to me at this point if you use the abbreviation “FeOx” only for crystalline Fe(III)-bearing mineral phases such as hematite and magnetite (as previously introduced) or if you also include poorly crystalline Fe(III) mineral phases such as ferrihydrite. I am asking because the context in which this abbreviation is used in this sentence seems to indicate that it also extends to the poorly crystalline minerals that are usually formed by Fe(II)-oxidizing bacteria.
Line 107: Replace “On the other hand” with sth. else since this formulation is usually used in a fixed combination with “on the one hand”.
Line 111: Replace “massive” with “extensive”.
Line 120: “along cracks and fractures on various seafloor lithologies”. Remove the “seafloor” and move this part of the sentence after “mats” at the beginning of the line.
Line 131: Is there part of the sentence missing after “Y3”? Otherwise please add a “.”.
Line 151/ 152: This sentence lacks some connection to the previous sentence/ remainder of the paragraph. Is there something missing here?
Line 190/ 191: This definition of FeOx minerals contrasts with the one you provide in the introduction. Please correct and/ or modify.
Line 207: “silica phaseS” instead of “silica phase”.
Line 220: Replace “whereas” with “and”.
Line 228: I suggest replacing “predominant” with either “general” or “overall”.
Line 237: Replace “remarkably” with “considerably”.
Line 246: Replace “and” with a comma.
Line 248/ 249: I suggest putting the values for TOC and total N in parentheses.
Line 262: Remove the “The” at the beginning of the sentence.
Line 263: “grow” instead of “growing”.
Line 264: I suggest splitting the sentence by replacing the comma at the end of the line with a full stop and then start the next sentence by adding a “They” before “use”.
Line 266: I would put this equation in a separate line to make it stand out more.
Line 267: The d13Corg values provided here differ from the ones in table 2. Which are the correct values?
Line 269: It should be “Fe(III)-reducing bacteria”. Add a “the” before “Fe cycle”.
Line 273/ 274: Shouldn’t it be “Lo’ihi seamount mats in the Pacific Ocean” instead of the way it is currently written here?
Line 274: Add a “the” before “RubisCO”.
Line 280: Add a “that” before “the latter”.
Line 282: Switch the permille number and “d13Corg values” around.
Line 285: Replace “because of” with “due to”.
Line 285/ 286: This part of the sentence doesn’t seem to be quite right. Can you please confirm/ rephrase for clarity?
Line 289: I suggest replacing “and” with “…, resulting in …”.
Line 292: Add an “a” before “much lower”.
Line 298: Replace “like” with “similar to”.
Line 299: Add a “which are” before “believed”.
Line 312: Add a “However” before "surface coating”.
Line 313: Remove the “nonetheless”.
Line 317: Remove the “commonly”.
Line 335: The transition to the mentioning of BIF microbands is rather abrupt and does not necessarily follow logically. I would rather suggest that you pick this finding up again at the start of the section on microbands in BIFs (line 388).
Line 340: Add a full stop after "products". Start next sentence with "For example at the Y3 site, where ...". Remove the “the” before “stalk-forming”.
Line 350: “… this is supported …” instead of “supports”.
Line 391: Maybe add a reference to your statement regarding <0.8 wt.% Corg in BIFs. Especially since it was already brought up in the initial round of reviews.
Line 393: Consider using a synonym for “propositions” here. You already use the word “proposition” in the previous sentence.
Line 412: Consider replacing “easily” with “readily”.
Line 417: Add an “all” before “BIF types”.
Line 439: Replace “IN the water column” with “FROM the water column”.
Line 449-454: This is a very long sentence. I suggest splitting it.
Line 456: Remove the “content”.
Line 460: Replace “first” with “initial”.
Line 468: Is “although” the correct word to use here?
Line 470-472: I don't quite follow the connection here. This whole section is on the contribution of benthic mats to Algoma-type BIFs and then you list two of the most enigmatic Superior-type BIFs as examples? I don't follow the trail of thought here. Please clarify.
Line 492-495: See my previous comment. This is the connection that was missing earlier. I suggest moving the example you make above to this part of the section.
Line 500: Consider replacing “shows” with “suggests”.
Line 519: Replace “insights” with “light”.
Line 521: Replace the “to” at the beginning of the line with “and”. Replace “help” with “helpS”.
Line 562: Replace “into details” with “in detail”.
Line 578: “Peak indexing was performed …” instead of “Indexing peaks were done …”.
Line 621: Add a “the” before “GET”. Replace “An approximate …” with “Approximately …”.

Figures:
Figure 2: The photomicrographs in panels (g) and (h) look a bit blurry. Do the authors have higher resolution images of these?
Line 941: "as suggested by XRD". You mean there are no discernible iron mineral reflexes in the corresponding diffractogram? I feel this is not necessarily very intuitive. Do you have corresponding Moessbauer data? I would probably rather refer to these instead of the XRD data.
Figure 3: The blue squares and the blue arrow in panel (b) are barely visible. I suggest making the lines thicker.
Figure 4: I suggest putting individual letters denoting panels in the figure description in parentheses to make them easier to identify (e.g. lines 963 and 964).
Figure 5: The axis labels of the diffractogram for the South Isabel sample are not the same font size as the labels for the other panels. Please correct.
Figure 6: The orange arrow in panel (d) is barely visible. I suggest making it thicker.
Line 981: Insert an "are" before "usually".
Line 984: Add "arrow" after "orange". Put the reference to panel d in parenthesis.
Figure 7: I suggest labelling the individual panels with letters for better reference.
Line 1002: "only are" should be "are only".
Line 1003: "All microbial metabolisms …" should be "Not all microbial metabolisms …". Remove the "not" in line 1004.

Tables:
I would make the text in the top row (the one describing the columns) bold to make it stand out better.

Supplementary information:
Line 53: "located at 32 m to the XXX". There is a description missing here.
Line 68: Add a "down" after "slows".
Line 70/ 71: Doesn't this also depend on having the appropriate (opposing) gradients of Fe(II) and O2?
Line 75: You have these two terms in italics in the main text.
Line 82: I suggest writing "Fe(II)-oxidizing" instead of "Fe-oxidizing".
Line 85: I suggest writing "Fe(III) oxyhydroxides" instead of "Fe oxyhydroxides".
Line 86: Replace "make" with "form".
Line 95: I would rather write "… mat communities were composed of …" instead of how it is currently written.
Line 133: What does "MIR" stand for?
Line 137: Is it possible to somehow separate the "cm-1" from the references.
Line 207: Add e.g. a "However" at the beginning of the sentence to indicate that you are restricting the extent of the previous statement.
Line 216: Replace "is" with "remains".
Figure S1: The various markers for the different mineral phases in the lowermost panel make it extremely busy. Would it be possible/ justifiable to only include the markers for the 3 most dominant/ characteristic reflexes?
Line 268: Is this information on the chlorinity essential? You don't/ barely refer to it throughout the main and supplementary text.
Figure S4: Is it possible to make the legends bigger?
Line 277: Replace "remarkably" with "considerably".

Reviewer #3 (Remarks to the Author):

Dear Authors

Thank you for all the effort and time put into addressing the reviewers' comments. I believe all of my comments have been addressed to a satisfactory degree.

I also read the other reviews and your comments and responses to those, and from what I can ascertain most of these have been addressed as well, leading to a much improved product.

There are not further major comments from me.

Kind regards,
Bertus Smith

Reviewer #4 (Remarks to the Author):

The authors have made significant edits to the manuscript in line with my previous comments. There are just a few smaller points that I address below, so that once the comments are addressed, I can recommend only minor revisions.

Reviewer #4 (Remarks to the Author):
Summary: As presented in this communication, the authors studied microbial mats at the Lucky Strike hydrothermal vent field to develop a model of BIF deposition and mineral transformation. The authors aim to offer direct evidence for a mechanism BIF deposition by aerobic iron-oxidizing bacteria, particularly in light of the organic carbon-poor iron

oxide/carbonate-rich bands found in these formations today. Mat samples were collected in situ from various sites in the region. Data related to the texture and mineralogy, bulk geochemistry, spatially-resolved chemistry (electron microprobe), and C- and N-isotope analysis of the mats is presented and discussed. This data is subsequently related to a microbial model of mineral formation and transformation in BIF based on marine Fe(II)-oxidizing Zetaproteobacteria. The idea that the authors present is intriguing and not hard to get behind (a microbial mat model for mineral formation and transformation that also places some limitations on the preservation of organic carbon). However, at the moment, this manuscript requires much reworking to more convincingly lead the reader through the data available and the intricacies of the authors´ arguments. The mechanisms of mineral formation and transformation in the mats must be more in focus. I recommend major revision here, and make general and details comments below that I hope will highlight weaknesses and recommend some remedies.
Response: We thank the reviewer for the very helpful and insightful review. We have provided explanations and addressed the concerns raised in the manuscript, taking into consideration the comments raised by the other three reviewers. By considering them, we believe the content of our paper has been improved significantly and better focused. We hope the revisions have improved the paper to your expectations.

General comments: 1. The authors are presenting a microbial mat model for BIFs here, but only superficially discuss the organisms forming these mats. It is clear that a large portion of the work on the microbes on site is presented in another manuscript to which the authors refer. Nonetheless, this manuscript, and the arguments brought forth and detailed in Fig 8, do not stand strongly without more information provided to the reader about the microbial players at the center of the story. Without this, how can the connection between the microbes and the data presented here be made? I highly recommend that the authors bring in more information about the microorganisms they are building a model on. It would be necessary to describe them, their distribution (at hydrothermal fields, but also in coastal regimes), and the type of minerals found in their association at other hydrothermal vent locations. I do not see why this has not been done, particularly as it seems that relevant data has been published – why not do this and cite the relevant work? But, in the end, the reader must be able to follow fully.
Response: We have introduced figure 8 in the discussion. Given the restrictive format of Communications Earth & Environment, we have created new sections in the supplementary introduction, entitled “Environments of microaerophilic Fe-oxidizing microorganisms and biomineralization” and “benthic microbial diversity at LSHF” to satisfy the comments raised by the reviewer. In addition, Table 1 now includes information on the Zetaproteobacterial abundance and their associated minerals within each mat.
We politely disagree with the reviewer who is demanding a description of the distribution of microorganisms in coastal regimes as we think it is beyond the scope of this study. Zetaproteobacteria thrive in many hydrothermal systems (Emerson & Moyer, 2010; McAllister et al., 2019) and it is the core of this paper, as an example of a well-described marine iron-oxidizer whose physiology, ecology, and mechanisms of oxidizing ferrous iron to ferric iron has been well-studied and widely reported. We emphasize that the Zetaproteobacteria grow wherever Fe(II)/O2 interfaces exist in modern environments (Emerson et al., 2010; Chan et al., 2016a). Zetaproteobacteria have only recently been discovered in coastal sediment, where they mainly grow on Fe(II)-bearing mild and carbon steel, which results in contrasting Zetaproteobacterial populations relative to those found in hydrothermal habits (McAllister et al., 2019). Even if Fe(II) ions come from mineral weathering and/or Fe(III) reduction, and are subsequently transported in anoxic groundwater today, then there is no link with the Precambrian environmental conditions during which dissolved Fe(II) ions were dominantly sourced by hydrothermal activity (Konhauser et al., 2017).

Chan CS, Emerson D, Luther III GW (2016a) The role of microaerophilic Fe-oxidizing micro-organisms in producing banded iron formations. Geobiology 14, 509–528.

Emerson D, Fleming EJ, McBeth JM (2010) Iron-Oxidizing Bacteria: An Environmental and Genomic Perspective. Annual Review of Microbiology 64, 561–583.
Emerson D, Moyer CL (2010) MICROBIOLOGY OF SEAMOUNTS: Common Patterns Observed in Community Structure. Oceanography 23, 148–163.
McAllister SM, Moore RM, Gartman A, Luther GW III, Emerson D, Chan CS (2019) The Fe(II)-oxidizing Zetaproteobacteria: historical, ecological and genomic perspectives. FEMS Microbiology Ecology 95, fiz015.
Konhauser KO, Planavsky NJ, Hardisty DS, Robbins LJ, Warchola TJ, Haugaard R, Lalonde SV, Partin CA, Oonk PBH, Tsikos H, Lyons TW, Bekker A, Johnson CM (2017) Iron formations: A global record of Neoarchaean to Palaeoproterozoic environmental history. Earth-Science Reviews 172, 140–177.

Reviewer 4 response: Thank you. Fig 8, Panel (a), and the associated description in the caption and text, was what was missing in the first submission. A necessary sketch that brings all readers along with the authors in their thinking and interpretation. In panel (b), are the (II, III) markings to show successive step of microband generation at Y3 (lowest panel in the figure) meant to be lighter grey or black? (they appear grey to me - and perhaps too light in the lowest panel).
Regarding the coastal zetaproteobacteria, the line of argumentation provided here by the authors is well taken: (We emphasize that the Zetaproteobacteria grow wherever Fe(II)/O2 interfaces exist in modern environments (Emerson et al., 2010; Chan et al., 2016a). Zetaproteobacteria have only recently been discovered in coastal sediment, where they mainly grow on Fe(II)-bearing mild and carbon steel, which results in contrasting Zetaproteobacterial populations relative to those found in hydrothermal habits (McAllister et al., 2019). Even if Fe(II) ions come from mineral weathering and/or Fe(III) reduction, and are subsequently transported in anoxic groundwater today, then there is no link with the Precambrian environmental conditions during which dissolved Fe(II) ions were dominantly sourced by hydrothermal activity (Konhauser et al., 2017).
I recommend that at shortened version of this argument be included in the SI. This goes to the extent and limitations of how such microorganisms in the modern can be understood and utilized in past models.

2. The vent sites are heterogeneous on many levels, as is seen throughout the results. This is, of course, part of working with environmental samples. But, it does not make the task of using environmental samples for the purposes of building a model for events in the far past any easier. Also, the authors wish to discuss mechanisms of mineral transformation across many sites. For this reason, the description of the field, the location of the sites, their relation to one another, all this must be as clearly communicated as possible. And, it must be consistent to the figures.
Response: Correct, the LSHF is indeed heterogeneous in terms of fluid composition (Chavagnac et al., 2018), substratum (Rouxel et al., 2004; Escartin et al., 2015), local geology, and tidal forcing effect (Wheeler et al., 2024). However, Zetaproteobacterial mat-hosted ferrihydrite is a ubiquitous feature at hydrothermal sites from the mid-Atlantic ridge (this study and Scott et al., 2015), Arctic mid-ocean ridges (Vander Roost et al., 2017), and Pacific Seamounts (e.g., Singer et al., 2013; Rouxel et al., 2018). We agree that modern environments may be complex to build models for understanding past mechanisms, but modern-day observations are essential for testing hypotheses, and although heterogenous, the data show that some of these environments better-fit conditions that operated in the Precambrian Oceans. We avoid speculative explanations as much as possible. We have described the sampling sites in the supplementary geological setting section in more detail, but note that these sites have been extensively studied and given the word count limitation, we have to restrict ourselves to the most relevant and expect the reader who is interesting a better understanding of this environment to consult the cited literature.

Chavagnac V, Leleu T, Fontaine F, Cannat M, Ceuleneer G, Castillo A (2018) Spatial Variations in Vent Chemistry at the Lucky Strike Hydrothermal Field, Mid-Atlantic Ridge (37°N): Updates for Subseafloor Flow Geometry From the Newly Discovered Capelinhos Vent. Geochemistry, Geophysics, Geosystems 19, 4444–4458.
Escartin J, Barreyre T, Cannat M, Garcia R, Gracias N, Deschamps A, Salocchi A, Sarradin P-M, Ballu V (2015) Hydrothermal activity along the slow-spreading Lucky Strike ridge segment (Mid-Atlantic Ridge): Distribution, heatflux, and geological controls. Earth and Planetary Science Letters 431, 173–185.
Rouxel O, Fouquet Y, Ludden JN (2004) Subsurface processes at the lucky strike hydrothermal field, Mid-Atlantic ridge: evidence from sulfur, selenium, and iron isotopes. Geochimica et Cosmochimica Acta 68, 2295–2311.
Rouxel O, Toner B, Germain Y, Glazer B (2018) Geochemical and iron isotopic insights into hydrothermal iron oxyhydroxide deposit formation at Loihi Seamount. Geochimica et Cosmochimica Acta 220, 449–482.
Scott JJ, Breier JA, Iii GWL, Emerson D (2015) Microbial Iron Mats at the Mid-Atlantic Ridge and Evidence that Zetaproteobacteria May Be Restricted to Iron-Oxidizing Marine Systems. PLOS ONE 10, e0119284.
Singer E, Heidelberg JF, Dhillon A, Edwards KJ (2013) Metagenomic insights into the dominant Fe(II) oxidizing Zetaproteobacteria from an iron mat at Lō´ihi, Hawai´I. Frontiers in Microbiology 4.
Wheeler B, Cannat M, Chavagnac V, Fontaine F (2024) Diffuse Venting and Near Seafloor Hydrothermal Circulation at the Lucky Strike Vent Field, Mid-Atlantic Ridge. Geochemistry, Geophysics, Geosystems 25, e2023GC011099.
Vander Roost J, Thorseth IH, Dahle H (2017) Microbial analysis of Zetaproteobacteria and co-colonizers of iron mats in the Troll Wall Vent Field, Arctic Mid-Ocean Ridge. PLOS ONE 12, e0185008.

Reviewer 4 response: Well understood that you must keep things succinct here in the manuscript, but the manuscript needed some bridging for the reader, without speculation, to appeal to a broader audience within the bio-geosciences. My comment was a reaction to a lack of a mechanistic model (now incorporated in Fig 8), along with a lack of some more information to the sites (now in SI) without requiring the reader to ~8 other manuscripts to follow your arguments. This concern is adequately covered now in the revised manuscript.

3. The authors describe Fe-rich crumbly mats and Fe-rich flocculent mats, but a more in depth discussion of what this means is lacking.
Response: In the supplementary introduction, we provided reasonable suggestions without making speculative comments to explain the origin of the flocculent and crumbly mats.
First, our mineralogical results indicate that the presence/absence of crystalline FeOx is not correlated with the mat type. The six studied mats share more than ten bacterial phyla (Astorch-Cardona et al., 2024). Real-time quantitative PCR revealed that the Zetaproteobacteria class accounts for 11.7% to 45.0% of the bacteria communities, with the highest abundance in the flocculent mats and the lowest ones in the crumbly mats (Astorch-Cardona et al., 2024). Two of the three flocculent mats, presenting high relative abundances of Zetaproteobacteria (at the WS and SI sites), show the less diversified Zetaproteobacterial communities. This could suggest a putative link between the Zetaproteobacterial communities and the type of Fe-rich mats. We hypothesize that flocculent mats are younger than those of crumbly mats.

Astorch-Cardona A, Odin GP, Chavagnac V, Dolla A, Gaussier H, Rommevaux C (2024) Linking Zetaproteobacterial diversity and substratum type in iron-rich microbial mats from the Lucky Strike hydrothermal field (EMSO-Azores observatory). Applied and Environmental Microbiology 0, e02041-23.

Reviewer 4 response: Thank you

4. Why is Fig 8 only discussed in the conclusion? Is this not the central figure that should tie everything together and main message? If so, then this requires the authors to guide the reader through more thoroughly. By the conclusion, the reader should feel a sense of déjà vu, or a restatement, but rather it is the first time the reader sees the main point of the manuscript. The authors must dedicate more of the discussion linking their evidence to this figure. It is currently lacking.
Response: We followed the review's suggestion by introducing figure 8 in the discussion. We have also provided further details in the discussion to guide the reader.
Reviewer 4 response: Thank you

5. There are 3 points outlined in Lines 388-403 that are all conceivable in my opinion. However, I am not convinced that the

manuscript, as currently written, clearly evidences these three points. I recommend that the authors revisit this writing to evidence these points systematically for publication.
Response: This section has been revised per all reviewer suggestions (see lines 388-427).
Reviewer 4 response: Thank you

6. Do the authors envision this mechanism they describe here as being important throughout the entire period of BIF deposition? If not, then what are the limitations? Can they speak to this in the manuscript?
Response: This point was raised by reviewers #1 and #2. We have contextualized our study by relating our study to the genesis of Algoma-type BIFs (see lines 62-72, 147-152, and 464-495). Our analysis shows a microbanding pattern in modern mat-forming Zetaproteobacteria from submarine hydrothermal environments, which could provide valuable insight into the genesis of similar features in some Precambrian Algoma-type BIFs.
Reviewer 4 response: Thank you
Detailed comments: Abstract, line 28: What is meant by "profuse production" … Is this more than usual for this microbe? This sounds this way. And if so, that what triggers this profuse production?
Response: We meant that the Zetaproteobacterial stalks are abundant in our studied samples and this attributed has been found in all Fe-rich hydrothermal vent fields harboring these organisms, with their stalks often being the most dominant feature. This production is triggered by the large hydrothermal flux of dissolved Fe(II) (186-2789 µM, compared to the <10-3 µM Fe in seawater; Chavagnac et al., 2018) and other nutrients. In addition, the mat age is not constrained, but 226Ra/Ba dating of metal-rich deposits indicates that there has been hydrothermal activity at Lucky Strike for at least 6,600 years (Sánchez-Mora et al., 2022). Considering that stalks, excreted by Mariprofundus ferroxydans cells, may reach 2.2 µm length/hour (Chan et al., 2011), this corresponds with the activity of the Zetaproteobacteria.

Chan CS, Fakra SC, Emerson D, Fleming EJ, Edwards KJ (2011) Lithotrophic iron-oxidizing bacteria produce organic stalks to control mineral growth: implications for biosignature formation. The ISME Journal 5, 717–727.
Sánchez-Mora D, Jamieson J, Cannat M, Escartín J, Barreyre T (2022) Age and Rate of Accumulation of Metal-Rich Hydrothermal Deposits on the Seafloor: The Lucky Strike Vent Field, Mid-Atlantic Ridge. Journal of Geophysical Research: Solid Earth 127, e2022JB024031.
Reviewer 4 response: Thank you. Please change from "profuse production" to "abundant production" in the text to align with the meaning described here.

Line 42: banded BIFs…banded Banded iron formations, remove one. Same line "chemical rocks" what is meant? Chemically formed? Abiogenic?
Response: Corrected as requested and as highlighted by reviewer's 1 and 2.
Reviewer 4 response: Thank you.

Line 56: I do not agree that all previous models suggested only planktonic microorganisms. Mats have been discussed before.
Response: We respectfully disagree with the reviewer. To our knowledge, the benthic microbial activity has never been evidenced as a potential contributor to BIF deposition. We will consider integrating references into the text if the reviewer provides references to support this fact. We have revised the paragraph (see lines 73-82).
Reviewer 4 response: The authors have now made extensive changes to this section, so that the text now includes a more careful introduction of previous models. However, my comment was to correct that sentiment that all previously proposed models are strictly planktonic. One example: Planavsky et al., Iron-oxidizing microbial ecosystems thrived in late Paleoproterozoic redox-stratified oceans, EPSL, 2009, for example, in Figure 8, indicate oxygenic phototrophs at the light-penetrated, water-sediment interface. This is not planktonic, but indicates a sediment-bound mat, with microbial activity influencing iron oxidization. This means that the distinction of a "benthic model" is not truly possible as such. I recommend that the authors temper their framing to acknowledge this. The microbial pathways of BIF formation presented in this work are still intriguing and explained in much more detail, novel in its own right, but just not as the first and only suggestion of a non-planktonic microbial mechanism.

Line 59: in oceanic, but also coastal photic zones.
Response: "coastal" has been added.
Reviewer 4 response: Thank you.

Line 62: "ferrihydrite rain" – is a citation required?
Response: Reference added. See for example, the review by Bekker et al. (2014)

Bekker A, Planavsky NJ, Krapež B, Rasmussen B, Hofmann A, Slack JF, Rouxel OJ, Konhauser KO (2014) Iron Formations: Their Origins and Implications for Ancient Seawater Chemistry. In: Treatise on Geochemistry. Elsevier, pp. 561–628.
Reviewer 4 response: Thank you.

Line 77: starting with "One reason…" I do not disagree, but I also argue that maybe there are multiple precursors and mechanisms – and that this is also the reason for the disagreements.
Response: We have rephrased the sentence.
Reviewer 4 response: Thank you.

Lines 103-108, starting with "To assess…" What are the limitations of studying modern hydrothermal vents to understand the

past? Can the authors please add some lines to address the potential limitations (along with the strengths) of the site as a proxy? In other words, how strong is this link?
Response: For clarity, the text has been revised (see lines 114-131).
Reviewer 4 response: Thank you.

Lines 122-123: Which are the dominant organisms in these mats? Please add as it should only strengthen the argument for the proxy.
Response: As indicated above, we have described the microbial diversity in the LSFH mats (see supplementary introduction). For clarity, the text has been revised.
Reviewer 4 response: Thank you.

Lines 125-130: Can the authors be more specific as to which analysis were used here to build this model of microbial mat development using this proxy?
Response: Our conclusions are based on the convergence of our analysis and tools used, not based on a single method. Asking for a specific method, in a way, seems to invalidate the multi-proxy and technique approach we have used to arrive at our conclusions, which would, in effect water down the robustness of our study and conclusions.
Reviewer 4 response: The authors do use specific tools here in this study. What I meant here was to state which ones were relied on. This would not invalidate a multi-tool approach, but highlight it while avoiding over-generalization.

Line 163-165: Can the authors cite similar interpretations?
Response: Reference added. See Chan et al. (2016b).

Chan CS, McAllister SM, Leavitt AH, Glazer BT, Krepski ST, Emerson D (2016b) The Architecture of Iron Microbial Mats Reflects the Adaptation of Chemolithotrophic Iron Oxidation in Freshwater and Marine Environments. Frontiers in Microbiology 7.
Reviewer 4 response: Thank you.

Line 174: Do the authors mean the FeOx at the top and the bottom of the mat? Are the FeOx minerals in Y3 silica poor? Unclear here.
Response: Clarified. We observe low Si concentrations in each laminated structure at the Y3 site.
Reviewer 4 response: Thank you.

Line 190: ending with "not identified" Anywhere? Or at which mat sites? Unclear.
Response: The paragraph has been revised (see lines 205-210).
Reviewer 4 response: Thank you.

Line 194-206: How can we interpret this heterogeneity in a useful way across time and space here, where SiO_2 is discussed as well as later on where C and N systematics are discussed? What does the variation in the sites mean for the model they are describing? The authors must provide some more guidance in the discussion to strengthen the efficacy of their model.
Response: We are unsure what is being requested because we have shown that conditions existing at the Y3 site permit the formation of microbands and therefore reminiscent of like conditions that formed similar lithologies in the Precambrian Ocean, by so doing excluding the other sites. The word count limit of this journal does not allow us to diverge too much into facts not at the core of our manuscript. Nonetheless, we have described our samples and provided references on previous works on these sites that the interested reader can refer too for additional information. In this paragraph, we mention that the substantial variations of major elements at LSHF are related to the incorporation of lithogenic (anorthite, amphibole) and hydrothermally-derived (barite) minerals within the mats, which did not prevent the microbial activity. We have discussed the role of SiO_2 concentrations adsorbed onto the ferrihydrite and possible mechanisms resulting in the absence of Si enrichment at the Y3 site. In addition, we have also discussed the relationship between P and TOC, as P is a macronutrient and then to bind iron oxides strongly. We have provided additional links between the mat elemental composition and our model (e.g., lines 320-336).
Reviewer 4 response: My question was really in search of a more clear presentation of mechanisms. I think that the manuscript edits now address this concern

Line 210: "the above results"…which results are meant, specifically?
Response: We meant the mineralogical and petrographic results obtained using XRD and SEM-EDS analyses. We have clarified this context in the text.
Reviewer 4 response: Thank you.

Line 253: "consistent with" but not indicative of. Can the authors speak to this?
Response: Numerous factors influence the magnitude of C isotope fractionation during primary productivity. They include the different C dioxide fixation mechanisms, pCO_2 of fluids, and volume-to-surface area ratios of autotrophs (Hayes, 1993). Most of these factors are dependent on environmental conditions. This is why we use "consistent with" since all aspects are not fully constrained. In addition, the LSHF mats are a consortium of autotrophic and heterotrophic microorganisms (see supplementary introduction), with the likelihood that several pathways of autotrophic CO_2 fixation coexist.

Hayes JM (1993) Factors controlling 13C contents of sedimentary organic compounds: Principles and evidence. Marine Geology, Marine Sediments, Burial, Pore Water Chemistry, Microbiology and Diagenesis 113, 111–125.

Reviewer 4 response: Thank you.

Line 258-259: This is way too late and way to subtle to be the first mention that Zetaproteobacteria are foundational to the mats in these samples. The manuscript needs a massive reframing for it to stand alone. And there needs to be more images, perhaps of these organisms or a description of what their mineral forming mechanisms are – here to build this bridge to the past as the authors wish.
Response: As indicated in the comment above, we have clarified the role of Zetaproteobacteria in the introduction. Besides, our article does not aim to provide a review of the (i) morphology of Zetaproteobacterial cells and iron biominerals and their related ecological functions, and (ii) filament crystallization. These aspects have been extensively studied and reported and their physiology and mechanisms of oxidizing ferrous Fe to ferric Fe widely document as show in the key references. Our works simply builds on these well-found facts with requiring an extensive review. For examples the references by Chan et al. (2016b) and McAllister et al. (2019) provide the reader an excellent overview of the Zetaproteobacterial biominerals and diversity. Given the specific format of Communications Earth & Environment, this background information has now been added in the supplementary introduction section.

Chan CS, McAllister SM, Leavitt AH, Glazer BT, Krepski ST, Emerson D (2016b) The Architecture of Iron Microbial Mats Reflects the Adaptation of Chemolithotrophic Iron Oxidation in Freshwater and Marine Environments. Frontiers in Microbiology 7.
McAllister SM, Moore RM, Gartman A, Luther GW III, Emerson D, Chan CS (2019) The Fe(II)-oxidizing Zetaproteobacteria: historical, ecological and genomic perspectives. FEMS Microbiology Ecology 95, fiz015.
Reviewer 4 response: Thank you.

Lines 258-275: The interpretation of C- and N- isotopic signatures here is challenging. At best, this is due to the large disparity in temporal and spatial scales between the processes under study. Unless the authors really develop the discussion to lead the reader deftly through, I fear the reader is left unconvinced of the author's conclusions.
Response: Paragraph revised (see lines 262-286).
Reviewer 4 response: Thank you.

Line 273-275: Starting with "Instead, …" this sentence is very difficult to follow.
Response: As indicated above, the paragraph has been clarified.
Reviewer 4 response: Thank you.

Line 277-283: Perhaps a conceptual drawing here would help to bring home the point.
Response: We have modified figure 8 to incorporate a conceptual model illustrating the mat structure and mechanisms that led to the low $^{13}C_{org}$ and $^{13}C_{carb}$ values.
Reviewer 4 response: Thank you.

Line 287: after "nucleation sites" – figure citation?
Response: We referenced this sentence (Johannessen et al., 2017, Environmental controls on biomineralization and Fe-mound formation in a low-temperature hydrothermal system at the Jan Mayen Vent Fields. http://dx.doi.org/10.1016/j.gca.2016.12.016).
Reviewer 4 response: Thank you.

Line 299: Figure? Reference?
Response: References are already provided, see Posth et al., 2014, Biogenic Fe(III) minerals: From formation to diagenesis and preservation in the rock record. http://dx.doi.org/10.1016/j.earscirev.2014.03.012; Boland et al., 2014, Effect of Solution and Solid-Phase Conditions on the Fe(II)-Accelerated Transformation of Ferrihydrite to Lepidocrocite and Goethite. dx.doi.org/10.1021/es4043275).
Reviewer 4 response: Thank you.

Line 306: "in agreement with our observations in Y3" Only at that site? Why? And why not the others? There is a need to improve the ability to understand differences and similarity between the sites for the reader.
Response: "in agreement with our observations in Y3 mats" has been deleted to avoid confusion. The second half of this paragraph and the next section explain the differences in Fe mineralogy controlled by environmental conditions.
Reviewer 4 response: Thank you.

Lines 314-316: Starting with "Further, our mineralogical…" This is interesting! Can the authors provide evidence for this here and discuss? There is more focus needed in general in evidencing the mechanisms proposed. How does this organism drive precipitation and mineral character/transformation in the mats, etc.
Response: We refer the reviewer to lines 195-199 in the result section. Combined XRD, FTIR, SEM-EDS, and electron microbe analyses (Figs. 2i, 3g, 5, 7; Figs. S1-3) show that Si is tightly bound to ferrihydrite and that amorphous silica is lacking. We are unsure what the reviewer means as we do not indicate that microorganisms drive precipitation in this sentence. In addition, following this sentence, we argue that temperature and pH are the controlling parameters of the binding capacity of Fh for Si.
Reviewer 4 response: Thank you.

Lines 322-326: Again, there is a lack of conceptual drawing to distinguish the dominant controls at the sites that drive the variation seen within the model proposed.

Response: We have introduced figure 8 here to support the text.
Reviewer 4 response: Thank you.

Lines 337-342: Starting with “Goethite...” What drives or suppresses this transformation in these mat environments?
Response: The second half of the paragraph has been reworded (see lines 344-353).
Reviewer 4 response: Thank you.

Line 348: What is “all evidence”? Can the authors be specific?
Response: “all evidence” changed to “petrographic observations and all isotopic evidence”.
Reviewer 4 response: Thank you.

Line 354: “indicates that the $\delta 13$C-carb values are controlled by alternative mechanisms” How? Please explain, as rather central to the argument the authors bring.
Response: In this paragraph, we have demonstrated that the 13Ccarb values could not have been controlled by the hydrothermal fluid composition. We explain these alternative mechanisms linked to biological processes in the following paragraph.
Reviewer 4 response: Thank you.

Lines 360-365: This is a long and confusing sentence. Please reword.
Response: Sentences changed (see lines 371-377).
Reviewer 4 response: I believe the sentence has been changed, but now can be found in lines 457-464 (?).

Line 366: “FeOx-rich layer compared to the Fh-rich layer” The terms are not making sense. Is Fh not ferrihydrite? Is that not an FeOx?...
Response: Fh refers to ferrihydrite, which is a poorly crystalline Fe(III) oxyhydroxide, while FeOx refers to crystalline iron oxides.
Reviewer 4 response: Yes, but it should be clarified in the text. For example, lines 390-396, the authors could make this more clear by changing “poorly crystalline nanometrenm-size Fh particles are highly reactive compared to most other FeOx minerals” to “poorly crystalline nm-size Fe(III) oxyhydroxide (Fh) particles are highly reactive compared to most other crystalline iron oxides (FeOx) minerals.” Please change in one spot, which would then be clear for the entire manuscript.

Line 368-372: I am happy to read this interpretation. I agree.
Response: Thank you for this positive comment.

Line 373-379: This information again is coming so late in the manuscript for a paper on the role if microbial mats in the deposition of BIF…And again, I would like to see a conceptual image showing this idea, rather than just a description.
Response: These sentences have been moved to the next section as we have reorganized the discussion following all reviewer’s recommendations. Figure 8b provides the conceptual model illustrating the idea.
Reviewer 4 response: Thank you.

Line 405, Section Formation of Si-rich bands: this is an intriguing concept, but not sufficiently deconstructed here yet.
Response: This section has been revised (see lines 429-462) following the recommendations of reviewer #3.
Reviewer 4 response: Thank you.

Line 443-444: “the striking absence of Si-rich bands in BIF”…What do the authors mean by this? I don´t agree.
Response: This is a mistake. We have changed “in BIFs” to “at LSHF”.
Reviewer 4 response: Ok.

Figure 1: The font in the labels for the sample sites are very hard to read due to the red color and the small size. These labels could also include the acronyms uses throughout the manuscript here as in the caption. The consistency will help keep the reader on track. Please change.
Response: The figure has been modified. Acronyms are only used for the studied sites where the mats thrive. To avoid confusion, we prefer not to use acronyms for describing the main vent site (for example, “Tour Eiffel” is the name of the high-temperature hydrothermal vent, while “North Tour Eiffel” is the site where low-temperature diffuse fluids and microbial mats are described.
Reviewer 4 response: I agree with the decision to avoid acronyms, but the authors have now also added a white background to site labels, which greatly improves the previous red-on-blue/green lettering.

Dear reviewers,

Thank you for your thorough and constructive review. We appreciate the valuable feedback and have made significant changes to the manuscript and supplementary information to address your comments. Below, we summarize the main revisions made, with all line references corresponding to the corrected manuscript (without track changes).

Before responding to specific comments, we would like to emphasize the importance of our study in advancing the understanding of how microbial activity may have contributed to the formation of microbands in Precambrian BIFs. This is supported by *in situ* observations using multiple geochemical proxies and high-resolution analytical techniques. Key improvements have been made to strengthen the context of our study and refine our data interpretations in relation to the existing literature on modern hydrothermal mats and Archean BIFs.

Key changes and highlights:

1. **Mechanistic model for microband formation:**
    - We would like to highlight that the mechanism of production of bands in BIFs is an intensely debated subject for which there is currently no consensus, which goes beyond the simple mechanisms by which iron oxides are enriched in BIFs.
    - Most contemporary studies associating modern marine microorganisms with BIFs, have not shown how the metabolisms of modern Fe(II)-oxidizing bacteria could lead to the production of the unique bands present in BIFs, the main characteristics used to validate and distinguish BIFs from all forms of iron formations lacking bands commonly found in modern Phanerozoic environments harboring the Fe(II)-oxidizing bacteria.
    - This notion is captured in the choice of the title of our paper.
    - At a more holistic level, if we can understand how these bands form, the mechanisms can help us better interpret how the ancient oceans were functioning, the nature of the environment in which BIFs were deposited, including the physical and chemical processes operating in the oceans and the atmosphere at that time.
    - Thus, our study is not a simple reference to the fact microorganisms can form BIFs, it is a study that presents a systematic mechanistic model describing how natural hydrothermal microbial mats over at least the past 6600 years (Sánchez-Mora et al., 2022), can contribute to our understanding of the formation of Fe-rich microbands in BIFs. It highlights a combination of biogeochemical, mineralogical and geological processes that likely operated in the Precambrian oceans.
    - We explore the biotic and abiotic processes responsible for the deposition of these microbands and connect these findings to the formation of BIF microbands billions of years ago.
    - To our knowledge, this is the first study to systematically demonstrate how such microbial mats can transform into organic carbon-depleted, crystalline iron oxide-rich microbands resembling those in BIFs.
    - By combining multiple biogeochemical techniques, we provide rare environmental evidence to support our hypothesis and bridge modern microbial processes with ancient BIF deposition.
2. **Relevance to Algoma-type BIFs:**
    - We relate our findings specifically to Algoma-type BIF microbands and provide detailed analyses of the processes involved. No study had previously linked microaerophilic microorganisms to the deposition of Algoma-type BIFs.
    - While Fe(II)-oxidizing bacteria and their twisted stalks have been widely studied in hydrothermal systems, their specific role in how they are involved in forming microbands relevant to BIFs has remained unclear.
    - For example, most studies have focused on computing rates of iron oxidation under different redox gradients to show that the activity of these organisms can indeed deposit profuse quantities of primary iron oxide minerals to account for BIF deposition (e.g., Dubinina and Sorokina, 2014; Chan et al. 2016).
    - Moreover, it has not been shown how the vast amounts of ferrihydrite precipitated by these organisms can be systematically transformed *in situ* to form the secondary

iron oxide mineral phases prevalent in BIFs, and emphasis is yet to be put on explaining the low organic content of BIFs.
    - Our study addresses this gap, demonstrating how ferrihydrite precipitated by these microaerophilic microorganisms can transform into secondary iron oxide phases while explaining the low organic carbon content in BIFs.

3. **Expanded introduction:**
    - We have introduced the different types of BIFs, clarified the novelty of our study, and articulated its aims.
    - We address the origin of microbanding in BIFs, which likely formed below the storm wave base, as described by Klein (2005).
    - Additionally, we caution against the assumption that fine laminations and a lack of detrital particles in Algoma-type BIFs always indicate deep-water deposition. Instead, we highlight the role of restricted basin conditions as well as limited continental runoff and oxidizing conditions in the Archean (Ernst, 2009; Reynaud *et al.* 2018; Lyons *et al.*, 2024; Rey *et al.*, 2024).
4. **New supplementary introduction:**
    - We added a detailed introduction in the supplementary information, covering:
        - Geological setting and vent sites.
        - Environments of microaerophilic Fe(II)-oxidizing microorganisms.
        - Mechanisms of microbial biomineralization.
        - Observed microbial communities at the Lucky Strike Hydrothermal Field.
    - This synthesis provides a comprehensive explanation of how these entities interact to produce microbands.
5. **Revised discussion on carbonates and iron oxides:**
    - We address the high $\delta^{13}C$ values of carbonates in the Fe-rich mats without unnecessary speculation.
    - We explain why magnetite, rather than hematite, would have been preserved in early Archean BIFs.
6. **Conceptual model (Figure 8):**
    - Figure 8 has been revised to include a conceptual model illustrating the trophic structure of microbial mats and the interplay of Fe and C cycling.
    - This figure has been integrated into the discussion to improve clarity and guide the reader through our interpretations.
7. **Improved discussion of Algoma-type BIFs:**
    - We have revised the discussion to contextualize our findings within the framework of Algoma-type BIF formation.
8. **Evaluation of benthic mats' contribution to BIFs:**
    - We assess the role of benthic mats in contributing to Algoma-type BIFs, combining our observations with existing data.

<u>References</u>:

Chan CS, Emerson D, Luther III GW (2016). The role of microaerophilic Fe-oxidizing micro-organisms in producing banded iron formations**. Geobiology 14,** 509-528**.**

Dubinina GA, Sorokina AY. (2014). Neutrophilic lithotrophic iron-oxidizing prokaryotes and their role in the biogeochemical processes of the iron cycle. Microbiology 83, 1-14.

Ernst WG (2009) Archean plate tectonics, rise of Proterozoic supercontinentality and onset of regional, episodic stagnant-lid behavior. Gondwana Research, Special Issue: Supercontinent Dynamics 15, 243–253.

Klein C (2005) Some Precambrian banded iron-formations (BIFs) from around the world: Their age, geologic setting, mineralogy, metamorphism, geochemistry, and origins. American Mineralogist 90, 1473–1499.

Lyons TW, Tino CJ, Fournier GP, Anderson RE, Leavitt WD, Konhauser KO, Stüeken EE (2024) Co- evolution of early Earth environments and microbial life. Nature Reviews Microbiology 1–15.

Rey PF, Coltice N, Flament N (2024) Archean Geodynamics Underneath Weak, Flat, and Flooded Continents. Elements 20, 180–186.

Reynaud J-Y, Trentesaux A, El Albani A, Aubineau J, Ngombi-Pemba L, Guiyeligou G, Bouton P, Gauthier-Lafaye F, Weber F (2018) Depositional setting of the 2.1 Ga Francevillian macrobiota (Gabon): Rapid mud settling in a shallow basin swept by high-density sand flows. Sedimentology 65, 670–701.

Sánchez-Mora D, Jamieson J, Cannat M, Escartín J, Barreyre T (2022) Age and Rate of Accumulation of Metal-Rich Hydrothermal Deposits on the Seafloor: The Lucky Strike Vent Field, Mid-Atlantic Ridge. Journal of Geophysical Research: Solid Earth 127, e2022JB024031.

Reviewer #1 (Remarks to the Author):

This manuscript systematically describes the petrography, mineralogy, and geochemical signatures of some Fe-rich microbands from the Lucky Strike hydrothermal vent system. From these evidences, authors depict a comprehensive process of the formation of these bands, from microbial Fe(II) oxidation by zetaproteobacteria, to biotic and abiotic precipitation of ferrihydrite/Fe(III) minerals, and eventually to the early diagenesis of primary BIF sediments via dissimilatory Fe(III) reduction to form siderite and magnetite. These microbands share very similar characteristics with some microbands that we commonly see in Banded Iron Formations. Lastly, authors offer a depositional model of these microbands. They conclude that this study helps to delineate both biotic and abiotic processes that are potentially involved in BIFs formation; and explain some of the key but still disputable characteristics of BIFs, such as low organic carbon content.

While that this study will be of interests of deep-time geochemistry and geobiology communities, I believe this manuscript does not fully meet the publication criteria of the Communications Earth and Environment. Therefore, my recommendation is a rejection. My concerns are described and explained below.

My biggest concern is the lack of novelty of this study. The examination of the sediments from this particular study location is both new and well done. The mineralogical, petrographical, and geochemical analysis are indeed detailed and comprehensive. However, the discussions and subsequent conclusions from this study really lack novelty and thus provide only limited new insights. Some of the major conclusions from this study include: microbial Fe(II) oxidation is important for BIFs formation; dissimilatory Fe(III) reduction is coupled with anaerobic Corg oxidation and thus the microbands are organic lean, siderite and magnetite are secondary diagenetic minerals via DIR; and formation/deposition of Si-rich bands is contributed to both a restricted basin morphology and episodic hydrothermal venting. Most of these findings, or at least significant parts of them, are already well known and have been extensively studied from a plethora of previous studies. The following are some what I consider as key references regarding each of findings described above. Authors should go through some of them, incorporate them into the texts, and also discuss/emphasize the novelty of their findings compared to the following studies, which I think should be a vital part of this work.

Response: We sincerely thank the reviewer for acknowledging the effort and novelty of our approach in this study. By employing multiple proxies and geochemical techniques, we systematically demonstrate how deep hydrothermal microbial mats form and transform into microbands. This high-resolution analysis represents a significant advancement, as no prior study has achieved this level of detail. We are particularly gratified by this recognition, as unravelling this systematic process was central to our work.

Our primary objective was to reconstruct these processes with precision, enabling us to argue convincingly for the biotic and abiotic mechanisms driving microbial band formation. Unlike previous studies that broadly assume these are microbial laminations, we provide clear, in situ evidence to support these claims. This approach is especially valuable given the challenges of tracing such processes in ancient BIFs, where biological signatures have long been erased, leaving only distinct mineral patterns as evidence.

Our study is the first to document these processes at such a scale, offering robust evidence for how aerobic and anaerobic microorganisms might collectively contribute to the systematic formation of microbands in Precambrian BIFs. While previous studies have demonstrated that the *Zetaproteobacteria* thrive in hydrothermal systems and drive substantial oxidation of ferrous iron to form Fe oxyhydroxide minerals, their focus has not been on the systematic mechanism of microband formation. This particular contribution is reflected in the title of our paper. Further, previous studies stop short of convincing showing how microbial activity transforms organic matter, sequentially driving the crystallization of iron oxide minerals into bands as they age.

For geochemists and researchers exploring BIF formation, band development, and mineral transformation, this level of detail is critical for unravelling not only microbial processes but also for reconstructing and distinguishing various geochemical, biogeochemical and physical processes operating in the ancient oceans. Our work provides a high-resolution biogeochemical perspective that extends beyond the microbiological and ecological context of these systems. By doing so, we offer a detailed framework for understanding not only microbial iron oxidation but also how microorganisms operate in synergy to contribute to the deposition of iron oxides, their transformation to more crystalline phases and their contribution to microband formation and mineral evolution as sediments age.

Importantly, our study highlights how modern biotic and abiotic processes in Fe-rich systems can establish a foundation for robust models to test hypotheses about microbial involvement in BIF deposition. Using a modern analogue system, we employ high-resolution biogeochemical tools to illustrate how benthic microbial communities, along with their associated members, systematically create Fe-rich microbanding patterns. This evidence leads to a valuable insight: the processes observed in modern submarine hydrothermal environments likely mirror mechanisms that contributed to the genesis of similar features in some Precambrian BIFs

Refs regard microaerophilic Fe(II) oxidizing bacteria in the context of BIF:
Chan, C.S., Emerson, D. and Luther III, G.W. (2016) The role of microaerophilic Fe- oxidizing micro- organisms in producing banded iron formations. Geobiology 14, 509-528
Emerson, D., Fleming, E.J. and McBeth, J.M. (2010) Iron-oxidizing bacteria: an environmental and genomic perspective. Annu. Rev. Microbiol. 64, 561-583
Kappler, A. and Straub, K.L., 2005. Geomicrobiological cycling of iron. Reviews in Mineralogy and Geochemistry, 59(1), pp.85-108.
Refs regard DIR in the context of BIFs
Konhauser, K.O., Newman, D.K. and Kappler, A., 2005. The potential significance of microbial Fe (III) reduction during deposition of Precambrian banded iron formations. Geobiology, 3(3), pp.167-177.
Posth, N.R., Konhauser, K.O. and Kappler, A., 2013. Microbiological processes in banded iron formation deposition. Sedimentology, 60(7), pp.1733-1754.
Refs regard Si bands and Si precipitation in the context of BIFs
Klein, C., 2005. Some Precambrian banded iron-formations (BIFs) from around the world: Their age, geologic setting, mineralogy, metamorphism, geochemistry, and origins. American Mineralogist, 90(10), pp.1473-1499.
Maliva, R.G., Knoll, A.H. and Simonson, B.M., 2005. Secular change in the Precambrian silica cycle: insights from chert petrology. Geological Society of America Bulletin, 117(7-8), pp.835-845.Schad, M., Halama, M., Bishop, B., Konhauser, K.O. and Kappler, A., 2019. Temperature fluctuations in the Archean ocean as trigger for varve-like deposition of iron and silica minerals in banded iron formations. Geochimica et Cosmochimica Acta, 265, pp.386-412.
Response: We appreciate the suggested references and acknowledge the remarkable contributions of many researchers to the role of microbiology in iron oxide formation. For instance, Chan and Emerson have pioneered studies on marine Fe(II)-oxidizing *Zetaproteobacteria* and their potential to deposit BIF through extensive iron oxidation. Similarly, Kappler and colleagues have significantly advanced our understanding of anoxygenic photosynthetic bacteria, emphasizing their physiology, environmental conditions, and their ability to oxidize vast amounts of ferrous iron in early iron-rich oceans.

While these studies have laid important groundwork, our work takes a distinct approach. Building on the established role of *Zetaproteobacteria* in iron oxidation, as highlighted by Chan and Emerson, we focused on biogeochemically characterizing how their extensive iron oxidation progresses through sediment aging. This includes exploring the systematic transformation of primary minerals into secondary minerals, the role of organic carbon in these processes, nitrogen cycling, and the eventual formation of carbon-depleted, iron oxide-rich microbands.

Critically, none of the suggested references provide a systematic study of this nature. Our work bridges the gap between the observed minerals and bands in BIFs and the biological processes that likely contributed to their formation over time. This perspective is essential because BIFs today consist only of preserved mineralogical evidence, requiring detailed studies to infer the biological mechanisms behind their systematic development.

Our study goes beyond understanding the ecology and physiology of Fe(II)-oxidizing bacteria to reveal how their activity contributes to BIF formation in a systematic and time-resolved manner. We have included some of the references suggested by the reviewer in our paper and supplementary information, prioritizing those most directly relevant to our study.

My second issue is the ambiguity of this study. After reading it a few times, I still have no idea what type of BIFs this study is applied to, and also what age(s) of BIFs authors attempt to relate this study to. I think authors should make it loud and clear at the beginning of this manuscript with these questions. Also, what makes this study unique? Hydrothermal systems have been extensively studied, including the Loihi Seamount.

Response: Microbands are a hallmark of all BIFs, found across different types and across Earth's history (Bekker *et al*., 2010; Konhauser *et al*. 2017):

- **Algoma-type BIFs**: Predominantly formed in deep-sea volcanic and hydrothermal settings in Archean cratons, with limited presence in the Proterozoic. In Modern hydrothermal went fields typical banded iron type rocks have been reported (Chi Fru *et al.* 2013, 2018).
- **Superior-type BIFs**: Developed on passive continental margins during the Paleoproterozoic (ca. 2500–1800 million years ago), where upwelling of anoxic, hydrothermal deep-sea fluids mixed with oxygenated shallower continental margins.
- **Rapitan-type BIFs**: Associated with the Neoproterozoic Snowball Earth glaciations, when global ice cover created anoxic seawater conditions, allowing ferrous iron to accumulate in seawater and eventually deposit as BIFs.

The microbands in our study are distinct because they are associated with an ocean containing at least a layer of oxygenated seawater. This environment supported microaerophilic Fe(II)-oxidizing bacteria, such as *Zetaproteobacteria*, suggesting that these BIFs formed during or after the Great Oxidation Event (GOE). The GOE marked the permanent oxygenation of Earth's atmosphere and the surface ocean, ramping up the activity of microaerophilic chemolithoautotrophic iron oxidizers.

This contrasts with the predominantly oxygen-deficient Archean oceans, where *Zetaproteobacteria* or other oxygen-respiring Fe(II)-oxidizing bacteria were unlikely to thrive abundantly. Instead, our model applies to BIFs deposited between ~2500 million years ago and the Neoproterozoic Snowball Earth glaciations.

While anoxic iron-oxidizing processes near hydrothermal vent fields may have driven BIF formation in the Archean Ocean where Algoma-type BIFs formed predominantly, our model suggests that dependent on the range of reach oxygenated waters, Algoma-type BIFs formed across the Proterozoic could have involved microaerophilic microorganisms thriving near hydrothermal systems that delivered anoxic ferrous iron-rich fluids to an overlying oxic water column. The mechanism proposed is one of several mechanisms that could have contributed to BIF formation. It is believed that band formation in BIFs can incorporate a variety of different mechanisms operating at one time as described in our next response below. These aspects have been further included in the paper and supplementary materials to emphasize the goals and outcomes of our study.

Finally, we respectfully emphasize that our study does not exist in isolation or claim to be the first to explore hydrothermal systems or their associated microbial communities. In fact, our work builds upon decades of accumulated knowledge on the activity of microorganisms in hydrothermal systems, microbial diversity, physiology and ecology to draw important conclusions, particularly with respect to the geomicrobiology of these systems. What sets our study apart is its integration of detailed biogeochemical analysis using advanced geochemical tools. These include high-resolution sediment analysis, mineral characterization, and isotope geochemistry, which together offer a systematic perspective often beyond the scope of traditional microbiological approaches. This integrated methodology allows us to address key geochemical processes and insights that are not typically captured by microbiological studies alone.

Bekker A, Slack JF, Planavsky N, Krapež B, Hofmann A, Konhauser KO, Rouxel OJ (2010) Iron Formation: The Sedimentary Product of a Complex Interplay among Mantle, Tectonic, Oceanic, and Biospheric Processes. Economic Geology 105, 467–508.

Chi Fru E, Ivarsson M, Kilias SP, Bengtson S, Belivanova V, Marone F, Fortin D, Broman C, Stampanoni M (2013) Fossilized iron bacteria reveal a pathway to the biological origin of banded iron formation. Nature Communications 4, 2050.

Chi Fru E, Kilias S, Ivarsson M, Rattray JE, Gkika K, McDonald I, He Q, Broman C (2018) Sedimentary mechanisms of a modern banded iron formation on Milos Island, Greece. Solid Earth 9, 573–598.

Konhauser KO, Planavsky NJ, Hardisty DS, Robbins LJ, Warchola TJ, Haugaard R, Lalonde SV, Partin CA, Oonk PBH, Tsikos H, Lyons TW, Bekker A, Johnson CM (2017) Iron formations: A global record of Neoarchaean to Palaeoproterozoic environmental history. Earth-Science Reviews 172, 140–177.

My third issue is authors' attempts to use a MOR as an analogue depositional environment for BIFs in general. I can understand that some reactions authors mention here may be applied to BIFs.

However, I am not fully convinced that MOR is a suitable analogue for BIFs deposition because the depositional environments of BIFs are not the same as the location where the Fe is sourced and no BIFs has been interpreted as being a MOR deposit.

Response: We politely disagree with the proposition that MORs are not typical depositional areas of BIFs (see review by Bekker *et al.* 2010). As we have indicated above, there are three types of BIFs (Bekker *et al.*, 2010; Konhauser *et al.*, 2017). **Finally would like to emphasize that the source of ferrous iron to the Precambrian ocean doesn't always determine where BIFs form**. The most important factors guiding the deposition of BIFs are a high dissolved seawater ferrous iron and silica content, redox conditions, the type of oxidant present, predominant biological and abiological processes, changes in hydrothermal venting pulse, seasonal variation in iron oxidation, etc. Some of these processes are summarized below.

1) Seasonal or periodic changes in biogeochemical activity:

- Banding in BIFs is hypothesized to result from seasonal or periodic variations in the deposition of iron and silica.
- During periods of higher biological activity (e.g., enhanced microbial oxidation of Fe^{2+} by photoferrotrophs or other processes), iron oxides precipitate more abundantly, forming iron-rich layers.
- In contrast, during lower activity periods, silica precipitation dominates, forming the alternating silica-rich bands.

2) Hydrothermal pulses:

- Episodic hydrothermal activity at MORs or submarine volcanoes caused pulses of iron-rich fluids to enter the oceans.
- These pulses periodically enriched seawater with dissolved Fe^{2+} , resulting in iron mineral precipitation during specific episodes, interspersed with silica deposition during quiescent periods.

3) Redox fluctuations in ancient oceans:

- Changes in oceanic redox conditions (e.g., between ferruginous and euxinic/anoxic conditions) could explain alternating deposition of iron oxides and silicates.
- Short-term oxygen "whiffs" in localized oxygen oases may have led to episodic oxidation of Fe^{2+} , forming iron oxide-rich bands, followed by silicate precipitation during anoxic phases.

4) Climatic or astronomical cycles:

- Banding may reflect long-term climate or astronomical cycles, such as Milankovitch cycles, influencing nutrient availability, ocean circulation, and biological activity.
- These cycles could have driven periodic deposition of iron-rich and silica-rich layers.

5) Sedimentary rhythms:

- Sedimentary processes, such as cyclic deposition controlled by ocean stratification or upwelling of iron-rich deep waters, could have contributed to the banding.
- This model emphasizes physical rather than biological or chemical processes in creating the patterns.

6) Microbial mat influence:

- Ancient microbial mats may have played a role in trapping and binding iron and silica particles, creating distinct laminae.
- Variations in microbial activity, influenced by environmental changes, could account for the alternating bands.

7) Chemical self-organization:

- Some researchers propose that the banding may arise from chemical self-organization during mineral precipitation.
- As iron and silica precipitate from seawater, local feedback mechanisms could naturally create rhythmic, alternating bands.

Thus, regardless of the process involved in the formation of the bands in BIFs, oxidation of Fe^{2+} to iron oxides is a unifying factor. Thus, critical to our paper is the Fe oxidation process and subsequent processes involved in the transformation of primary iron oxides and oxidation of organic carbon. The formation pattern depended on factors such as the rate of metal-rich fluid upwelling, ocean depth, and microbial activity.

Bekker A, Slack JF, Planavsky N, Krapež B, Hofmann A, Konhauser KO, Rouxel OJ (2010) Iron Formation: The Sedimentary Product of a Complex Interplay among Mantle, Tectonic, Oceanic, and Biospheric Processes. Economic Geology 105, 467–508.

Konhauser KO, Planavsky NJ, Hardisty DS, Robbins LJ, Warchola TJ, Haugaard R, Lalonde SV, Partin CA, Oonk PBH, Tsikos H, Lyons TW, Bekker A, Johnson CM (2017) Iron formations: A global record of Neoarchaean to Palaeoproterozoic environmental history. Earth-Science Reviews 172, 140–177.

The following are my detailed comments:

Line 34: There should be no fundamental mechanistic difference between the deposition of microbands and other parts (i.e. meso- and macro-bands) of the same iron formations at the same location.

Response: Our work is focused on showing how these microbial communities may contribute to the formation of microbands in BIFs. As we show above, there are various alternative models describing the formation of various bands in BIFs, and the belief is that a variety of different mechanisms contributed to the formation of various bands. Our data specifically highlights the case of microorganisms in the formation of microbands. For example, as introduced in the introduction, *in situ* analyses of Archean BIFs and Quaternary BIF-like deposits led some authors to the conclusion that microbands might represent a specific attribute of BIFs, which is different from those that formed macrobands. Our work provides empirical evidence for these deductions, further highlighting the novelty of our work. We note specifically that understanding how bands formed in BIFs is an ongoing challenging subject of research, which, as we show above, if understood, can lead to the understanding of physical and chemical processes that drove the functioning of the ancient world beyond the mere processes that enriched iron oxides in BIFs.

Line 50-51: I would specify which type of BIFs, Superior-type or Algoma-type? Based on the tectonic setting of the study location, Algoma-type BIFs should be the obvious answer. These two types of BIFs are drastically different and authors should make it clear at the very beginning.

Response: We have introduced the two types of BIFs as recommended (see lines 36-56). The main difference between these two types of BIFs is simply that Algoma-type BIFs formed in deep-sea volcanic centers and Superior-type BIFs in passive near-continental margin settings. The third type of BIFs, Rapitan BIFs, are associated with Snowball Earth glaciations.

Line 56-61: This is implying that there was dissolved oxygen in the deep-water realm in the Archean, which has never been shown.

Response: This is a misinterpretation of our statement. We have not suggested that Archean oceans were oxygenated. The paragraph refers to the metabolic activity of planktonic microorganisms. Planktonic organisms are those floating in the open sea rather than living on the seafloor, for example, the photoferrotrophic, anoxygenic Fe(II) oxidizers, believed to have been widespread in the Archean ocean surface waters.

Line 93-116. Refer to my third point above.

Response: The paragraph has been revised (see lines 62-72 and 143-152). See our response to your third point and the suggested seminal reviews by notable BIF researchers like Bekker and Konhauser. Hydrothermal settings are intimately linked to the deposition of BIFs both close to deep sea vent settings and on passive continental margins. To not emphasize this point goes against the fundamental understanding of the link between hydrothermal delivery of iron, changes in the pulse of hydrothermal activity and BIF deposition.

Line 168: I would delete “for the sake of simplicity”.

Response: Deleted as suggested.

Line 174-175 Is the EDS analysis quantitative? Or is this a simple relative ratio? If it is the latter, then there is no enough evidence to state that there is only little Si in this scenario since Fe concentration is not quantified either.

Response: EDS analysis is a semi-quantitative method, resulting in the use of relative ratios and comparisons of EDS spectra. For example, the CAP mats are characterized by a strong Si peak with an intensity higher than that of the Fe peak (Fig. 2i). In contrast, the Y3 mats are characterized by an absence of Si peak (Fig. 4i) or a Si peak with a lower intensity relative to the Fe peak (Fig.

S4). Besides, we have combined multiple analyses to show that Si is less adsorbed onto the FeOx surface in Y3 mats compared to other mats. We have clarified the sentence (see lines 195-197).

Line 284-287 need refs here.
Response: Done (e.g., Johannessen et al., 2017, *Environmental controls on biomineralization and Fe-mound formation in a low-temperature hydrothermal system at the Jan Mayen Vent Fields*. http://dx.doi.org/10.1016/j.gca.2016.12.016).

Line 292 the widespread association…..has been….
Response: Corrected.

Line 318 How about the pH of the hydrothermal fluids? It also controls the surface reactivity of Fe minerals. How does pH change with temperature for such complex fluids like hydrothermal fluids?
Response: The reviewer is correct. The reactive surface area of ferrihydrite strongly changes with time, temperature, and pH (Hiemstra *et al.*, 2019). We refer the reviewer to the publication of Chavagnac *et al.* (2023), which includes several chemical features of the LSHF hydrothermal fluids performed aboard the research vessel. To clarify the paragraph, we have added information on the pH of the diffuse hydrothermal fluids (see lines 322-336).

Chavagnac V, Destrigneville C, Castillo A, Hodel F (2023) Geochemistry of hydrothermal fluids at the Lucky Strike Hydrothermal Field data from the EMSO-Azores observatory. SEANOE.
Hiemstra T, C. Mendez J, Li J (2019) Evolution of the reactive surface area of ferrihydrite: time, pH, and temperature dependency of growth by Ostwald ripening. Environmental Science: Nano 6, 820–833.

Line 302 I cannot see the implication and relevance of this section in this manuscript. The take-home message from this section is unclear and how it is related to BIFs is not explained.
Response: Our work goes beyond just describing and show the distribution of iron oxide phases in the samples. It seeks to describe and understand the process that would promote the aging transformation of primary ferrihydrite to more crystalline iron oxide phases. This understanding is critical because for example, there is no ferrihydrite, which raises the question of the processes that enabled the crystallization of the original primary ferrihydrite and what the environmental conditions were potentially like, including biotic and abiotic reactions. This is a specific novelty of our work that has not been tackled in previous papers describing these hydrothermal deposits. Thus, this section aims to explain the unique ferrihydrite transformation at the Y3 site to more crystalline iron oxides and why this is not found at the other sites. We show how the hydrothermal conditions control the Fe mineralogy in the mats. This is a prerequisite to provide additional insights into the mineral paragenetic sequence, which is crucial for interpreting similar mineralogical data observed in BIFs. Thus, in our approach we reason that it is rather important to provide a detailed modern understanding of interactions between environmental conditions and Fe-rich minerals before any attempt to link such features to deep geological records. This approach contributes to the novelty and depth of our work and the reliability of using such data in making accurate interpretations of the complex ancient BIF bands and the pathways that led to their mineralogical assemblage, composition and enrichment.

Line 366 I would have to question if this 6 percent of P concentration would make a huge difference in primary productivity. What is the equivalent P concentration in ppm or μM?
Response: We apologize for this mistake. The P concentrations associated with poorly crystalline Fh range from 0.15 to 0.72% (1500 to 7200 ppm), whereas those associated with crystalline FeOx phases are lower than 0.04% (400 ppm). This corresponds to a 3- to 18-fold decrease from poorly crystalline Fh to crystalline FeOx. We have corrected the text (see lines 221 and 377). Bulk geochemical data, together with the lack of age constraints of the mat formation, result in difficulties in estimating the P concentrations released to pore waters that could have sustained local primary productivity.

The decrease in phosphorus (P) during the transformation of amorphous ferrihydrite to crystalline iron oxide minerals, as observed in our study, is not directly relevant to predicting primary production. Instead, it highlights that amorphous ferrihydrite typically has a higher adsorption capacity, which diminishes as it transitions to more crystalline iron oxide phases. This finding reinforces the robustness of our mineralogical analysis and the systematic transformation of primary ferrihydrite into crystalline iron oxides. Secondly, it suggests that microbial reductive transformation of ferrihydrite using organic carbon as an electron acceptor likely contributes to the release of P bound to iron oxides, further leading to the production of organic carbon-poor secondary iron oxide-rich layers. If such a process occurs at a large scale, considering the remarkable size of BIFs, it would certainly have resulted in the re-introduction of substantial amounts of dissolved P into pore waters and migration across the pore water interface to support marine primary production.

Furthermore, it provides valuable insights and caution on the use of different iron oxide minerals in BIFs to reconstruct Precambrian seawater P concentrations. Traditional reconstructions rely on directly quantifying P bound to iron oxides in BIF and using the information to infer seawater P concentrations. Our approach adds a nuanced understanding of these processes, highlighting the fact that the transformation of primary ferrihydrite might interfere with this process. These insights add depth to our study, stemming from our systematic, high-resolution geochemical analysis.

Line 427 please explain what do you mean by “hydrothermal plumbing circulation system”. This is ambiguous and unclear. Also, does this mechanism work on continental shelves? If not, then this statement is overly generalized and can only be applied to a very specific tectonic setting.
Response: We meant the underground architecture of conduits in which hydrothermal water is brought to the surface from deeper reservoirs (see lines 457-460). This is a common way of describing the system of hydrothermal networks that supply hydrothermal fluids to seawater by geochemists and geologists working with hydrothermal, tectonic and volcanic systems. This mechanism has nothing to do with the continental shelf, as we are referring to the architecture of the system that makes and releases hydrothermal fluids into seawater. These could be deep sea or on the continental shelf in the case of shallow submarine hydrothermal systems. For simplicity, we have deleted this statement.

Line 829 Figure 2. Both g and h, the micrographs of the stalks are blurry and difficult to read.
Response: Corrected.

Reviewer #2 (Remarks to the Author):

Summary: The authors present an interesting study in which they performed an in-depth mineralogical and (isotope) geochemical characterization of the Lucky Strike hydrothermal vent site. They combined mineralogical observations (e.g. XRD, FTIR, 57Fe Moessbauer spectroscopy and electron microscopy) with geochemical whole rock analysis (e.g. digestions, XRF and electron microprobe measurements) and stable isotope analysis (C and N isotope systems) to link mineral deposition and transformation with previously established microbial activity. The authors suggest that the mineralogy observed at their sampling sites is the result of the metabolic activity of microaerophilic Fe(II)-oxidizing bacteria (zetaproteobacteria) coupled with the early diagenetic activity of dissimilatory Fe(III)-reducing bacteria. Finally, the authors put their work in the context of the deposition of Precambrian Banded Iron Formations (BIF) and conclude that the processes observed at the field site could potentially explain the presence of organic carbon-depleted Fe-rich microbands in BIFs. The manuscript is certainly timely as it seems to move the interpretation of the BIF rock record forward by connecting (microbial) processes which so far have only been studied separately. Overall, the manuscript is very well written, interesting and relatively easy to understand. However, occasionally the language tends to be a little bit convoluted as I will later point out in my line-by-line comments. In general, the study seems to be conducted in a careful and diligent way and the methods employed are appropriate. Further, I did not find any major flaws in the interpretation of the resulting data. However, I do not have any specialist knowledge on stable isotope geochemistry and can, therefore, only offer limited judgement in this regard. My major problem with this manuscript is the ambiguity when it comes to its contextualization. Major BIFs (Superior-type BIF) are generally believed to have been deposited over the continental shelf and are the result of a combination of (microbial) processes occurring in the water column, the soft sediment and later during low-grade metamorphism. By contrast, minor (and older) BIF (Algoma-type BIF) are generally associated with hydrothermal and volcanic processes. It is not quite clear which type of BIF the authors are envisioning their model to fit and there seems to be some disconnect between the interpretation of the study outcome and the consensus on BIF deposition (see further discussion below).

My feeling is that this should be clarified before the manuscript can be published.

In conclusion, I think that this is a great paper. However, it needs some more polishing and re-contextualization before publication. I have added further comments below and hope that the authors may find them useful. If necessary, I would be glad to review this article again.

Response. We want to extend our sincere appreciation to the reviewer for recognizing the depth of our work and its relevance in contributing to the topic of BIF formation in deep time using modern analogues. We are also grateful for the helpful and encouraging suggestions. As an overview, the concept of microband formation during the conception of our study was not directed to any particular type of BIF as microbands are a common feature of all BIFs, and as indicated in our introduction, inferences were made to previous authors studying Archean BIFs and to modern Quaternary BIF-like analogues on the involvement of microbial activity in the genesis of microbands in BIFs (Katsuta *et al.*, 2012; Chi Fru *et al.*, 2013, 2018). Our study, thus, in a holistic way, provides a proof of concept in a modern *in situ* analogue setting on this possibility. Nonetheless, Algoma-type BIFs, though widespread during the Archean, also occur in Proterozoic Cratons, but less frequently (Bekker *et al.*, 2014). Considering that the mechanism presented here involves microaerophilic Fe(II)-oxidizing bacteria activity and the associated community that developed because of their growth, our model most specifically represents such marine communities that became more widespread after the Great Oxidation Event when the atmosphere and the surface ocean became permanently oxygenated. Nonetheless, even in the anoxic Archean Ocean, communities of microorganisms capable of forming Fe(II)-oxidizing biofilms anaerobically, for example, via nitrate reduction or even by photoferrotrophy, could effectively form such microbands. For example, Chi Fru *et al.* (2013, 2018), suggest that a bloom of Fe(II)-oxidizing microorganisms, not exclusive to benthic iron-oxidizers, followed by encrustation of biomass by iron oxides can effectively form a thin dense band of iron oxides. We have undertaken the corrections as suggested, and below, we have responded to the specific comments.

Bekker A, Planavsky NJ, Krapež B, Rasmussen B, Hofmann A, Slack JF, Rouxel OJ, Konhauser KO (2014) Iron Formations: Their Origins and Implications for Ancient Seawater Chemistry. In: Treatise on Geochemistry. Elsevier, pp. 561–628.

Chi Fru E, Ivarsson M, Kilias SP, Bengtson S, Belivanova V, Marone F, Fortin D, Broman C, Stampanoni M (2013) Fossilized iron bacteria reveal a pathway to the biological origin of banded iron formation. Nature Communications 4, 2050.
Chi Fru E, Kilias S, Ivarsson M, Rattray JE, Gkika K, McDonald I, He Q, Broman C (2018) Sedimentary mechanisms of a modern banded iron formation on Milos Island, Greece. Solid Earth 9, 573–598.
Katsuta N, Shimizu I, Helmstaedt H, Takano M, Kawakami S, Kumazawa M (2012) Major element distribution in Archean banded iron formation (BIF): influence of metamorphic differentiation. Journal of Metamorphic Geology 30, 457–472.

Major comments:
As already pointed out above, while everything seems to be in order regarding the data acquisition and interpretation, my major concern is its contextualization.
There are several reasons for this (see e.g. Klein, 2005; Bekker et al., 2014 or Konhauser et al., 2017, for references):

1) This work describes microbial mats that are closely related to a abyssal hydrothermal system and hydrothermal effluent. It is correct that the Fe source for BIF deposition is believed to have been hydrothermally sourced Fe2+, the primary minerals during BIF deposition are generally agree upon to have been deposited through water column processes (either in the form of Fe(II) or Fe(III) minerals). However, BIFs are not considered to have been formed through microbial mats (as implied in lines 392-395). Instead, as the authors correctly point out in line 62, one major theory for BIF deposition envisions a “water column-derived ferrihydrite rain” to create BIF. While it is conceivable that microaerophilic Fe(II)-oxidizing bacteria would have contributed to the deposition of BIFs, it is unlikely that they did so as part of microbial mats. At least when considering the deposition of major BIF. Instead, they would have needed to be planktonic (see for example the work of Dr. C. Chan, University of Delaware) to be able to migrate along the redox gradient over the shelf area and not being dragged to the seafloor by the stalks and sheaths and associated minerals (see e.g. your line 28 or 291). If the authors want to keep their work within the framework of the deposition of major (Superior-type) BIF, they may want to add a clarification to their discussion. Alternatively, the authors could instead put their model within the framework of the genesis of smaller (Algoma-type) BIF which are generally associated with hydrothermal systems and (submarine) volcanism. The authors seem to already partially have done that in that the BIF described in their reference 4 seems to fall under the Algoma-subtype. The authors could put their work within the framework of Algoma-type BIF and then speculate that under certain circumstances the same processes could be responsible for the genesis of Superior-type BIF.
Response: Reviewer #1 also raised a similar point. Perhaps our hypothesis was not clear enough, as it is generally considered that BIFs and their bands likely formed via a combination of various mechanisms with microorganisms, constituting one potential pathway contributing to this process. We do not assume that BIFs formed entirely by microbial processes, although allusions have been made to this possibility by Konhauser *et al.* (2005). Instead, our study demonstrates a model that might specifically explain the formation of microbands in BIFs, as highlighted in the choice of our title, related to the activity of mat-forming microaerophilic marine Fe(II)-oxidizing bacteria. The Zetaproteobacteria use their stalks as a mechanism to trap and escape entombment by depositing oxidized Fe^{3+} on their stalks and then escaping towards the surface of the mat to continue growth (e.g., Chan *et al*., 2011). This process could then effectively build up iron-rich thin layers in sedimentary environments. Upon the vacation of these, the process of iron oxide deposition becomes self-regulating by halting Fe^{2+} oxidation and precipitation of iron oxides, leaving behind thin iron-rich bands that are further transformed during early diagenesis. We agree that our model best fits Algoma-type BIFs, which we emphasize in our study. Therefore, we have argued that the Fe-rich microbands formed by a consortium of benthic microbial metabolisms could explain the microbanding pattern in Algoma-type BIFs, especially in GOE and post-GOE BIFs. The introduction, discussion, and conclusion now clarify these possibilities, better putting our study in context.

Chan CS, Fakra SC, Emerson D, Fleming EJ, Edwards KJ. 2011. Lithotrophic iron-oxidizing bacteria produce organic stalks to control mineral growth: implications for biosignature formation. ISME J 5:717–727.
Konhauser KO, Newman DK, Kappler A (2005) The potential significance of microbial Fe(III) reduction during deposition of Precambrian banded iron formations. Geobiology 3, 167–177.

2) Redoxcline and oxygen penetration depth in relation to depth of deposition. If assumed that abyssal microbial mats formed by microaerophilic Fe(II) oxidizers indeed contributed to BIF genesis, we face another challenge: The depth of the redoxcline within the Precambrian ocean. While there is still some ambiguity regarding the exact oxygen concentration leading up to and following the Great Oxidation Event (see e.g. Lyons et al., 2024), the way it is currently contextualized in the manuscript, it would require full-depth oxygenation of the early ocean. However, this is at odds with the requirements of other rock formations/ geochemical evidence of that time which generally require/ suggest that at least the deep oceans remained anoxic until some time after the GOE. It is not unlikely that microbial mats consisting of microaerophilic Fe(II)-oxidizing bacteria contributed to BIF genesis in some fashion. But I suggest that the authors carefully think about the way they contextualize their work. There are certain discrepancies between the proposed model and the sedimentological and geochemical framework of BIF deposition which require some clarification and reworking. The obvious caveat is that there are no optimal modern analogues that perfectly resemble the Precambrian water column and sedimentary systems. In this regard the field site utilized in this study already represents a great fit. However, care should be taken to point obvious deviations out and make a good effort to explain/ discuss them.
Response: We agree with and appreciate this observation. We would like to clarify that we do not presume that the Proterozoic marine realm was fully oxygenated and although our model represents an Algoma-type BIF depositional environment in the deep-sea, similar processes could also be occurring in near shallow marine oxygenated hydrothermal environments. For example, such diffuse hydrothermal venting environments prevalent in the Aegean Sea are associated with shallow marine BIF-like deposits (e.g., Chi Fru et al., 2013, 2015, 2018) and in Nagahama Bay of Satsuma Iwo-Jima, Japan (Hoshino et al., 2016). In these environments, their works have also shown the prevalence of *Zetaproteobacteria* (e.g., Callac *et al.*, 2017), previously linked to the deposition of BIFs (Chan et al., 2016). Thus, shallow submarine hydrothermal vent systems on near the continental shelf could have promoted efficient diffusion of hydrothermal plumes (Isley & Abbott, 1999), introducing sufficient dissolved Fe(II) directly to the shallow oceans whenever environmental conditions and depositional settings that could have allowed the formation of Algoma-type BIFs (Chi Fru *et al.*, 2013, 2015, 2018; Dong *et al.*, 2022).These observations indicate that Algoma-type BIFs were likely not restricted to the deep ocean (<200 m as a minimum depth; Klein, 2005) of oceanic basins, but rather by where hydrothermal activity occurred. Indeed, Algoma-type BIFs encompass an extensive range of depositional environments as long as they include (mafic-felsic) volcanic rocks (Isley & Abbott, 1999). We have modified the text accordingly.

Callac N, Posth NR, Rattray JE, Yamoah KKY, Wiech A, Ivarsson M, Hemmingsson C, Kilias SP, Argyraki A, Broman C, Skogby H, Smittenberg RH, Fru EC (2017) Modes of carbon fixation in an arsenic and CO2-rich shallow hydrothermal ecosystem. Scientific Reports 7, 14708.
Chan CS, Fakra SC, Emerson D, Fleming EJ, Edwards KJ. 2011. Lithotrophic iron-oxidizing bacteria produce organic stalks to control mineral growth: implications for biosignature formation. ISME J 5:717–727.
Chan CS, Emerson D, Luther III GW (2016) The role of microaerophilic Fe-oxidizing micro-organisms in producing banded iron formations. Geobiology 14, 509–528.
Chi Fru E, Ivarsson M, Kilias SP, Bengtson S, Belivanova V, Marone F, Fortin D, Broman C, Stampanoni M (2013) Fossilized iron bacteria reveal a pathway to the biological origin of banded iron formation. Nature Communications 4, 2050.
Chi Fru E, Ivarsson M, Kilias SP, Frings PJ, Hemmingsson C, Broman C, Bengtson S, Chatzitheodoridis E (2015) Biogenicity of an Early Quaternary iron formation, Milos Island, Greece. Geobiology 13, 225–244.
Chi Fru E et al. 2018. Sedimentary mechanisms of a modern banded iron formation on Milos Island, Greece. Solid Earth 9, 573–598.
Dong A, Sun Z, Kendall B, Izon G, Cao H, Li Z, Ma X, Yin X, Qiu Z, Zhu X, Bekker A, Poulton SW (2022) Insights from modern diffuse-flow hydrothermal systems into the origin of post-GOE deep-water Fe-Si precipitates. Geochimica et Cosmochimica Acta 317, 1–17.
Hoshino T, Kuratomi T, Morono Y, Hori T, Oiwane H, Kiyokawa S, Inagaki F. 2016. Ecophysiology of *Zetaproteobacteria* associated with shallow hydrothermal iron-oxyhydroxide deposits in Nagahama Bay of Satsuma Iwo-Jima, Japan. Front Microbiol 6:1–11.
Isley AE, Abbott DH (1999) Plume-related mafic volcanism and the deposition of banded iron formation. Journal of Geophysical Research: Solid Earth 104, 15461–15477.
Klein C (2005) Some Precambrian banded iron-formations (BIFs) from around the world: Their age, geologic setting, mineralogy, metamorphism, geochemistry, and origins. American Mineralogist 90, 1473–1499.

Line-by-line comments:

Line 29-32: This sentence seems to be quite long and complicated. I suggest to either rephrase it or shorten it.
Response: Rephrased.

Line 38: What do you mean by “below wave action depth”? BIF are generally considered to have been deposited below storm wave base.
Response: Rephrased. We have added information on the depositional environments of BIFs that range from deep to shallow waters (lines 36-56).

Line 42: “banded BIFs”. That is doubling down on the name. Please remove the “banded”.
Response: Corrected.

Line 42/ 43: This should be “at THE time of deposition”.
Response: Corrected.

Line 48: Please define “deep sea BIF”. Major BIFs have been suggested to have been deposited on continental shelves. Is that what you envision here?
Response: This is a mistake. Corrected.

Line 50: As I pointed out in my major comment, I am a bit uncertain about the connection you make between your “modern abyssal hydrothermal” field site. So again, what kind of BIF do you envision to be formed via this mechanism?
Response: We have clarified this paragraph (see lines 62-72 and 143-152).

Line 69-72: This sentence is quite long and convoluted. I suggest simplifying it for clarity.
Response: Corrected for clarity (see lines 88-92).

Line 77-79: You infer a lack of modern analogues for the Precambrian Ocean here. But there are a variety of lakes throughout Europe, East Africa and Southeast Asian which have been intensely studied and that are frequently used as modern analogues for the early ocean (e.g. Crowe et al., 2008; Llirós et al., 2015; Koeksoy et al., 2016; Friese et al., 2021; Roland et al., 2021; Vuillemin et al., 2018, 2023, etc.). I, therefore, don’t necessarily agree with this statement. Please revise.
Response: Revised. As explained above, we focus on analogues that can inform us on the models involving the formation of BIFs, not just Fe^{2+}-rich anoxic, sulfate-poor waters as described in most of those analogues given above, where hydrothermal processes that are intrinsically linked to BIF deposition are absent, including both abundant dissolved Fe^{2+} and silica. We have clarified this statement in the text. (see lines 96-99).

Line 80: Replace “poor” with “low”.
Response: Corrected.

Line 84: Regarding the organic matter content of BIFs please carefully double-check throughout the manuscript. You cite an organic carbon content of less than 0.5 wt.% here. However, at a later point you cite 0.8 wt.% (line 384). Please check for consistency.
Response: Corrected. We note that there is no specific established average organic carbon concentration in Precambrian IFs. Measured values from the 2.5 Ga Transvaal IFs indicate that the organic carbon content is below 0.8 wt.% (Konhauser *et al.*, 2017), which is similar to concentrations recorded in Quaternary BIF-like deposits (Chi Fru *et al.*, 2013, 2015, 2018).

Chi Fru E, Ivarsson M, Kilias SP, Bengtson S, Belivanova V, Marone F, Fortin D, Broman C, Stampanoni M (2013) Fossilized iron bacteria reveal a pathway to the biological origin of banded iron formation. Nature Communications 4, 2050.
Chi Fru E, Ivarsson M, Kilias SP, Frings PJ, Hemmingsson C, Broman C, Bengtson S, Chatzitheodoridis E (2015) Biogenicity of an Early Quaternary iron formation, Milos Island, Greece. Geobiology 13, 225–244.
Chi Fru E et al. 2018. Sedimentary mechanisms of a modern banded iron formation on Milos Island, Greece. Solid Earth 9, 573–598.
Konhauser KO, Planavsky NJ, Hardisty DS, Robbins LJ, Warchola TJ, Haugaard R, Lalonde SV, Partin CA, Oonk PBH, Tsikos H, Lyons TW, Bekker A, Johnson CM (2017) Iron formations: A global record of Neoarchaean to Palaeoproterozoic environmental history. Earth-Science Reviews 172, 140–177.

Line 84-88: Again, this sentence is quite convoluted. Please simplify.
Response: Corrected (see lines 104-107).

Line 89: I suggest replacing "paramount" with "ongoing".
Response: Replaced.

Line 101-116: I am just wondering if this paragraph shouldn't go to the Materials and Methods as sth. like "description of field site"? You can still provide the most important information here but the way it is currently presented it seems to be too excessive for the introduction.
Response: We have shortened this paragraph, as suggested. We have also extended the description of the field in supplementary information, as recommended by reviewer #4.

Line 144-148: This sentence is too long and convoluted. Please rephrase or break it up into shorter sentences.
Response: Corrected.

Line 156/157: "at THE NTE site".
Response: Corrected.

Line 179: "corals-like" should be singular.
Response: Corrected.

Line 202: "vary considerably" instead of "considerably vary".
Response: Corrected.

Line 203: Replace "whereby" with "where" and put "<0.4%" into parentheses.
Response: Corrected.

Line 204: Again, put the P values in parentheses.
Response: Corrected.

Line 235: Replace "highly differ" with "differ considerably".
Response: Replaced.

Line 266: "subsequently". Remove.
Response: Removed.

Line 267: "Fe(II)-reducing bacteria". Shouldn't this be "Fe(III)-reducing bacteria?
Response: Corrected.

Line 268-275: I don't understand the logic of these two sentences. Please rephrase for clarity. Something seems amiss here.
Response: These sentences have been moved to the supplementary discussion.

Line 305: "… transformation to lepidocrocite …". Please replace "to" with "into".
Response: Replaced.

Line 306: Please replace "… our observations in Y3." with "… our observations at the Y3 site."
Response: "in agreement with our observations in Y3 mats" has been deleted to avoid confusion, as reported by reviewer #4.

Line 309: Either "show ABUNDANT organic molecules" or "show AN abundance of organic molecules".
Response: Change to "show AN abundance of organic molecules".

Line 330/ 331: Sentence following the "whereby". I would rephrase the second half of the sentence into "where the stalk-forming Zetaproteobacteria and organic molecules facilitate the nucleation of biogenic Fh."
Response: Rephrased, as suggested.

Line 334: "texture" should be plural.
Response: Corrected.

Line 347: Wording. Replace "copious" with either "abundant" or "high".
Response: Corrected.

Line 395 and 396: Recent work by Rico et al. (2023) actually showed that at least the Fe(III)-reducing bacterium they utilized in their study was not able to oxidize complex organic matter and couple this to Fe(III)-reduction. Further, to my knowledge, Fe(III)-reducing bacteria generally prefer low molecular weight organic compounds and are usually dependent on fermenting bacteria to make complex organics available to them. I don't know if such bacteria are known to occur on your field site?
Response: This is a very interesting and important comment. As suggested by reviewer #4, we have provided relevant information on the LSHF microbial players in the supplementary introduction. In the LSHF mats, organotrophic microorganisms (i.e., fermenters) are diversified, although their abundance is relatively low compared to others. Therefore, we have revised the paragraph (see lines 410-413), which is now accompanied by a conceptual model illustrating the mat structure at the Y3 site.

Line 396: Replace "small" with "little".
Response: Replaced.

Line 413: "previously discussed" instead of "discussed previously".
Response: Corrected.

Line 422: "aqueous Si" instead of "Si aqueous".
Response: Corrected.

Line 423: "mineral" should be plural.
Response: Corrected.

Line 424: It should be "THE deposition of BIF-like …".
Response: Corrected.

Line 427: What do you mean by "hydrothermal PLUMBING circulation system"? "plumbing" seems to be the wrong word here. Please clarify.
Response: We have rephrased the sentence.

Line 434-436: I am a bit confused here. How do you define biomass? Are you referring to the presence of actively metabolizing cells? Because in my mind "microbial filaments" should generally be part of the biomass. Please clarify.
Response: Correct, we have clarified. We consider microbial filament to be the waste product of the microbial Fe(II)-oxidation reaction that remains behind after the cells are gone (Emerson *et al.*, 2010; Emerson & Moyer, 2010).

Emerson D, Fleming EJ, McBeth JM (2010) Iron-Oxidizing Bacteria: An Environmental and Genomic Perspective. Annual Review of Microbiology 64, 561–583.
Emerson D, Moyer CL (2010) MICROBIOLOGY OF SEAMOUNTS: Common Patterns Observed in Community Structure. Oceanography 23, 148–163.

Line 442: "It is suggested THAT this process points …".
Response: Corrected.

Line 443: “proposed” should be “propose”.
Response: Corrected.

Line 444: Why are Si-rich bands absent from BIFs? They are one of the core components of BIFs and in fact quite abundant on any scale from micro- to macrobands. You mean that this explains the absence of Si-rich bands at your field site? Please revise and/ or clarify.
Response: This is a mistake. We have changed “in BIFs” to “at LSHF”.

Line 522: “Prior TO analysis …”.
Response: Corrected.

Line 523/ 524: Wouldn’t this high pressure and temperature result in the transformation of some of the Fe minerals? Please clarify.
Response: We carried out the preparation of KBr pellets for samples from five of the six studied sites, using the same protocol. It is highly unlikely that the experimental conditions would have modified the mineralogy for the Y3 samples, but not for the other samples. In addition, the FTIR results are consistent with our XRD data where goethite, lepidocrocite, and magnetite have been identified using both methods. We changed “Prior analysis” to “Prior to FTIR analyses”. Importantly, if *in situ* pressure was a contributing factor to primary ferrihydrite transformation, it should be observed across the entire field site. Furthermore, our carbon isotopes suggest the involvement of microbial metabolism.

Line 536/537: Why was specifically this velocity range chosen? Also, for identification of Fe(III) minerals 77 K would have been better. I assume the measurements were conducted at these temperatures due to the absence of a cryostat? Also, please define "room temperature". Assumed room temperature is 20°C, this should be ~293 K. Do these 7 K really make a difference for the measurements?
Response: The velocity range of -12 to 12 mm/s is often used to show sextets (magnetic component) in a sample, whereas that of -4 and 4 mm/s promotes a better resolution of Fe^{III} and Fe^{II} doublets (Schwertmann & Cornell, 1991). The mention of data acquisition over the velocity range of -12 to 12 mm/s at 300 K in the manuscript was removed to avoid confusion, as these data were acquired but not treated and presented in the paper. In addition, at high temperatures, a difference of 7 K does not affect the measurements.

We have performed Mössbauer spectroscopy to identify ferrihydrite where the latter mineral was not observed using XRD. The detection of crystalline FeOx at Y3 for example, was not needed because XRD patterns have already shown the presence of magnetite, goethite, and lepidocrocite.

Finally, the choice to work only on the dataset acquired over the velocity range of -4 to 4 mm/s at room temperature (295 K, controlled) was motivated by: i) the existence of numerous studies defining the spectral parameters for each mineral (i.e., ferrihydrite, edenite, siderite) observed in the investigated samples (Murad & Schwertmann, 1980; Murad, 1988; Murad *et al.*, 1988; Dyar *et al.*, 2006; Andreozzi *et al.*, 2009), while lacking for siderite and edenite at temperatures of 77 K and 4.2 K. This choice allowed a clear identification of peaks after spectral deconvolution, with reliable comparison examples, also in regard of other studies using Mössbauer spectroscopy to characterize ferrihydrite (Sun *et al.*, 2011; Dong *et al.*, 2022, in their supplementary information). Considering our scientific aim, measurements conducted at 77 K or 4 K were therefore not critical.

Andreozzi GB, Ballirano P, Gianfagna A, Mazziotti-Tagliani S, Pacella A (2009) Structural and spectroscopic characterization of a suite of fibrous amphiboles with high environmental and health relevance from Biancavilla (Sicily, Italy). American Mineralogist 94, 1333–1340.
Dong A, Sun Z, Kendall B, Izon G, Cao H, Li Z, Ma X, Yin X, Qiu Z, Zhu X, Bekker A, Poulton SW (2022) Insights from modern diffuse-flow hydrothermal systems into the origin of post-GOE deep-water Fe-Si precipitates. Geochimica et Cosmochimica Acta 317, 1–17.
Dyar MD, Agresti DG, Schaefer MW, Grant CA, Sklute EC (2006) MÖSSBAUER SPECTROSCOPY OF EARTH AND PLANETARY MATERIALS. Annual Review of Earth and Planetary Sciences 34, 83–125.
Murad E (1988) The Mössbauer spectrum of “well”-crystallized ferrihydrite. Journal of Magnetism and Magnetic Materials 74, 153–157.
Murad E, Bowen LH, Long GJ, Quin TG (1988) The influence of crystallinity on magnetic ordering in natural ferrihydrites. Clay Minerals 23, 161–173.

Murad E, Schwertmann U (1980) The Möessbauer spectrum of ferrihydrite and its relations to those of other iron oxides. American Mineralogist 65, 1044–1049.
Schwertmann U, Cornell RM (1991) Iron Oxides in the Laboratory: Preparation and Characterization. John Wiley & Sons.
Sun Z, Zhou H, Yang Q, Sun Z, Bao S, Yao H (2011) Hydrothermal Fe–Si–Mn oxide deposits from the Central and South Valu Fa Ridge, Lau Basin. Applied Geochemistry 26, 1192–1204.

Line 560/ 561: Please rephrase. "… solutions diluted TO DIFFERENT CONCENTRATIONS."
Response: Corrected.

Figures: The figures are very nice. However, I would personally prefer if the pictures/ photomicrographs in Fig. 1-4, 6 and 7 could be lined out with a black line to better distinguish them from the background. I am also wondering if the font size in Fig. 5 and Fig. 8 could be increased a bit? Some of the legends are quite small.
Response: We have modified the figures, as suggested.

Reviewer #3 (Remarks to the Author):

The abstract of the manuscript is very well written and concise to a level not often seen in geoscience literature, and the authors should be complimented on that. The rest of the manuscript is also well-written and can be followed. The structure is appropriate and most of the arguments are backed by the observations and data presented. I agree with most of what the authors are proposing, and the work could be an example of where the principal of uniformitarianism could be applied, to some extent, to Precambrian BIF, where many previous works have concluded that this cannot be done. Where the presented work do not compare as well to Precambrian BIF, the authors mostly highlight this and provide reasonable explanations for the observed differences.

Response: We thank Dr. Bertus Smith for accepting to comment on our manuscript and for his positive review. The comments have helped to significantly improve the content of the manuscript, contributing to better clarity.

I do believe, however, that some facets of the manuscript can be improved. This is not so much related to the observations, data, and ideas proposed by the authors, but rather where they could clarify certain aspects and consider some other aspects. Below are my major comments on this manuscript that need to be addressed by the authors:

1. In lines 80 to 92 the authors provide some good literature background on why there is a lack of direct evidence for biological activity associated with BIF deposition, which I agree with. However, it is worth noting that some examples exist where more direct potential evidence for biological activity associated with iron formation deposition has been documented. The examples include potential fossilized bacterial filaments in IF-associated jasper (Dodd et al., 2017), oncolites (i.e., spherical stromatolites) in shallower water GIFs (Smith et al., 2017; 2020) and stromatolites interbedded with IF (Smith, 2018).

Response: The reviewer is correct. We have included his suggestions (see lines 107-109). We would like to draw attention to the oldest putative microfossils described by Dodd et al. (2017). In the case of the ancient IF-associated jaspers, the Eoarchean filament-like morphologies are debated to be similar to modern structures formed abiotically (Johannessen et al., 2020; Lepot, 2020).

Johannessen KC, McLoughlin N, Vullum PE, Thorseth IH (2020) On the biogenicity of Fe-oxyhydroxide filaments in silicified low-temperature hydrothermal deposits: Implications for the identification of Fe-oxidizing bacteria in the rock record. Geobiology 18, 31–53.

Lepot K (2020) Signatures of early microbial life from the Archean (4 to 2.5 Ga) eon. Earth-Science Reviews 209, 103296.

2. Magnetite is by far the most common Fe-ox mineral in pristine (i.e., not outcrop) Precambrian BIF samples (e.g., Beukes, 1980; Smith et al., 2013; Li et al., 2017) but appears to be much less abundant in the samples studied. The authors take good care to try and explain the origin of the magnetite, but it would be good if they could mention and comment as to why magnetite over hematite preservation is favoured in Precambrian BIFs.

Response: We thank the reviewer for the positive comment. We did not quantify the amount of magnetite in our samples, but it is clear that magnetite formation predates hematite formation. We have created a section in the supplementary discussion to explain the reason why magnetite over hematite is preserved in the early Archean.

3. The authors make good comparisons between the C isotopes in carbonates where there is high TOC, and where there is low TOC, showing the latter isotopes to be more depleted in 13C, implying carbonate formation through organic C oxidation. However, overall, many of the 13C(carb) values are much higher than what has been observed in the geological record. Why are the values so much higher than for Precambrian BIFs (d13C(VPDB) of ca. -20 to -6 per mil) and Precambrian carbonate platforms (d13C(VPDB) of ca. 0 per mil)? Can the authors comment? I think their comparisons and conclusions are valid, but why do they show such high values even though they show the expected relative trend?

Response: This is a very interesting and important comment, which we have had to consider carefully. To our knowledge, there is no comparable data in Fe-rich mats from modern hydrothermal systems. We have found that one $\delta^{13}C$ value (relative to the PDB standard) of authigenic siderite is +6.5‰ in modern metalliferous hydrothermal sediments (e.g., Holm, 1989). It is proposed that this high positive value would have been due to the preferential partitioning of ^{12}C into methane at low oxygen fugacity and ^{13}C into siderite due to the low pH of the hydrothermal solution. In lacustrine environments where microbialites may form, high positive $\delta^{13}C$ values of carbonates (as high as 12‰) are more frequently observed (Valero-Garcés *et al.*, 1999; Beeler *et al.*, 2020; Bougeault *et al.*, 2020). Seafloor hydrothermal fluids are known to be CO_2-rich (Shanks *et al.*, 1995). Therefore, a common process that could affect both lacustrine and hydrothermal environments is CO_2 degassing due to the elevated CO_2 concentration of the fluids. Degassing of CO_2-rich fluids, resulting in the preferential loss of ^{12}C, was proposed to trigger ^{13}C enrichment in the dissolved inorganic carbon (DIC) pool and carbonates. Unfortunately, we cannot confirm the latter hypothesis in our samples as the concentration and isotope composition of DIC pool have not been measured in the LSHF diffuse hydrothermal fluids. Therefore, we hypothesize that one or more of the above processes during diffuse fluid discharge could have led to the high $\delta^{13}C_{carbonates}$ values in the LSHF microbial mats. We only made suggestions because such a discussion would thus essentially be speculative as there is no such data generated in our manuscript to warrant a detailed undertaking. Considering the limited word count of the main text (~5000 words; see the instruction to authors submitting to Communication Earth and Environment), this discussion has been added in a new section in the supplementary information document.

Beeler SR, Gomez FJ, Bradley AS (2020) Controls of extreme isotopic enrichment in modern microbialites and associated abiogenic carbonates. Geochimica et Cosmochimica Acta 269, 136–149.
Bougeault C, Durlet C, Vennin E, Muller E, Ader M, Ghaleb B, Gérard E, Virgone A, Gaucher EC (2020) Variability of Carbonate Isotope Signatures in a Hydrothermally Influenced System: Insights from the Pastos Grandes Caldera (Bolivia). Minerals 10, 989.
Holm NG (1989) The 13C12Cratios of siderite and organic matter of a modern metalliferous hydrothermal sediment and their implications for banded iron formations. Chemical Geology 77, 41–45.
Shanks WC, Böhlke JK, Seal RR (1995) Stable Isotopes in Mid-Ocean Ridge Hydrothermal Systems: Interactions Between Fluids, Minerals, and Organisms. In: Geophysical Monograph Series (eds. Humphris SE, Zierenberg RA, Mullineaux LS, Thomson RE). American Geophysical Union, Washington, D. C., pp. 194–221.
Valero-Garcés BL, Delgado-Huertas A, Ratto N, Navas A (1999) Large 13C enrichment in primary carbonates from Andean Altiplano lakes, northwest Argentina. Earth and Planetary Science Letters 171, 253–266.

4. Lines 373-379: If I read this correctly, the authors want to imply that what they interpret as Si dislodgement during transformation of filamentous to more crystalline structures and an associated increase in Fe/Si ratio could explain the alternating Fe-rich and Fe-poor banding in BIFs. Is it therefore implied that the banding could be diagenetic? Is this happening at a scale in the studied material that is comparable to band widths in BIF? How would this fit with recent work that has been done (Bau et al., 2022; Mundl-Petermeier et al., 2022, Wainwright et al., 2024) that seems support that BIF banding is a depositional/primary feature?
Response: We meant that the FeOx/Si-poor layers likely occurred after deposition, even if our samples are still on the seafloor surrounded by mixed seawater-hydrothermal fluids. Hence, this is an eodiagenetic mechanism corresponding to the initial stage of diagenesis that occurs at or near the sediment surface (Worden & Burley, 2003), where the depositional environment mainly controls the geochemistry of the interstitial waters. We do not intend to exclude models proposing that layering in BIFs is a syn-depositional, but rather, we propose a new pattern to explain the mineralogical transformation of iron minerals. There is no reason to believe that only one mechanism prevailed. We clarified this point in the manuscript (see lines 422-462). In addition, we provided an extensive explanation for contextualizing our work, as reviewers #1 and #2 recommended.

Worden RH, Burley SD (2003) Sandstone Diagenesis: The Evolution of Sand to Stone. In: Sandstone Diagenesis (eds. Burley SD, Worden RH). Wiley, pp. 1–44.

However, later in the manuscript in lines 405-428 it appears as if a primary, hydrothermal venting and Si oversaturation (i.e., a primary depositional process) plays a role in forming Si-rich bands. Are the authors implying that the Fe and Si banding could be both primary and diagenetic? I think this should be cleared up by explaining and delineating these processes better and how such banding could be distinguished when studying samples.
Response: We revised this paragraph to clarify the timing of Si-rich bands in BIFs. We have also amended the text to provide a clear conceptual model of forming Si-rich bands associated with

modern Fe-rich microbial mats. We have also suggested in which context this model could work within the framework of Precambrian BIFs. All these modifications are in lines 429-462.

Below are more detailed comments and recommended corrections:
• Please note that minor improvements can be made with regards to grammar in some places, and this can be easily addressed with some editorial input prior to publication
• Line 42: "banded BIFs" means "banded banded iron formations", which makes little sense. I would recommend deleting "banded" or changing it to "well-laminated" or something similar.
Response: "banded" has been deleted.

• Line 42: Change "chemical rocks" to "chemical sedimentary rocks".
Response: Done.

• Lines 42-44: References need to be added for this statement.
Response: References added (see for example Bekker *et al.*, 2014; Konhauser *et al.*, 2017).

Bekker A, Planavsky NJ, Krapež B, Rasmussen B, Hofmann A, Slack JF, Rouxel OJ, Konhauser KO (2014) Iron Formations: Their Origins and Implications for Ancient Seawater Chemistry. In: Treatise on Geochemistry. Elsevier, pp. 561–628.
Konhauser KO, Planavsky NJ, Hardisty DS, Robbins LJ, Warchola TJ, Haugaard R, Lalonde SV, Partin CA, Oonk PBH, Tsikos H, Lyons TW, Bekker A, Johnson CM (2017) Iron formations: A global record of Neoarchaean to Palaeoproterozoic environmental history. Earth-Science Reviews 172, 140–177.

• Lines 64-65: Magnetite is the much more common Fe-oxide phase in pristine BIFs overall (Beukes, 1980; Li et al., 2017). These lines should be adjusted to convey this more accurately.
Response: Correct. We have clarified the sentence.

• Line 71: There is great debate about the exact timing of the GOE (ca. 2.50-2.19 Ga; e.g., Bekker et al., 2004; Gumsley et al., 2017; Warke et al., 2020; Poulton et al., 2021; Hodgskiss and Sperling, 2022), and the information in this line should be updated to provide a range, which would be better considered and more accurate.
Response: We thank the reviewer for the suggestion. We have rephrased the paragraph (see lines 73-82).

• Line 105: Change "generation" to "deposition".
Response: Corrected.

• Line 128: Add "potentially" in front of "involved".
Response: Done.

• Line 267: Should this not be "Fe(III)-reducing bacteria"?
Response: Yes, it should. Corrected.

• Lines 322-324: Specify where the slightly larger Fh particles nucleate by adding "at the former" at the end of the sentence.
Response: Done.

• Line 334: Change "texture" to "textures".
Response: Corrected.

• Line 339: Change "On the other hand" to "However".
Response: Corrected.

• Line 351: Change "since" to "because".
Response: Changed.

• Line 353: Add Dymek and Klein (1988) as a reference along with Bau and Dulski (1996).
Response: Reference added, as suggested.

• Line 371: Add “was” in front of “lost”.
Response: Done.

• Line 443: Change “proposed” to “propose”.
Response: Corrected.

Reviewer #4 (Remarks to the Author):

Summary: As presented in this communication, the authors studied microbial mats at the Lucky Strike hydrothermal vent field to develop a model of BIF deposition and mineral transformation. The authors aim to offer direct evidence for a mechanism BIF deposition by aerobic iron-oxidizing bacteria, particularly in light of the organic carbon-poor iron oxide/carbonate-rich bands found in these formations today. Mat samples were collected in situ from various sites in the region. Data related to the texture and mineralogy, bulk geochemistry, spatially-resolved chemistry (electron microprobe), and C- and N-isotope analysis of the mats is presented and discussed. This data is subsequently related to a microbial model of mineral formation and transformation in BIF based on marine Fe(II)-oxidizing Zetaproteobacteria. The idea that the authors present is intriguing and not hard to get behind (a microbial mat model for mineral formation and transformation that also places some limitations on the preservation of organic carbon). However, at the moment, this manuscript requires much reworking to more convincingly lead the reader through the data available and the intricacies of the authors´ arguments. The mechanisms of mineral formation and transformation in the mats must be more in focus. I recommend major revision here, and make general and details comments below that I hope will highlight weaknesses and recommend some remedies.

Response: We thank the reviewer for the very helpful and insightful review. We have provided explanations and addressed the concerns raised in the manuscript, taking into consideration the comments raised by the other three reviewers. By considering them, we believe the content of our paper has been improved significantly and better focused. We hope the revisions have improved the paper to your expectations.

General comments: 1. The authors are presenting a microbial mat model for BIFs here, but only superficially discuss the organisms forming these mats. It is clear that a large portion of the work on the microbes on site is presented in another manuscript to which the authors refer. Nonetheless, this manuscript, and the arguments brought forth and detailed in Fig 8, do not stand strongly without more information provided to the reader about the microbial players at the center of the story. Without this, how can the connection between the microbes and the data presented here be made? I highly recommend that the authors bring in more information about the microorganisms they are building a model on. It would be necessary to describe them, their distribution (at hydrothermal fields, but also in coastal regimes), and the type of minerals found in their association at other hydrothermal vent locations. I do not see why this has not been done, particularly as it seems that relevant data has been published – why not do this and cite the relevant work? But, in the end, the reader must be able to follow fully.

Response: We have introduced figure 8 in the discussion. Given the restrictive format of Communications Earth & Environment, we have created new sections in the supplementary introduction, entitled “Environments of microaerophilic Fe-oxidizing microorganisms and biomineralization” and “benthic microbial diversity at LSHF” to satisfy the comments raised by the reviewer. In addition, Table 1 now includes information on the *Zetaproteobacterial* abundance and their associated minerals within each mat.

We politely disagree with the reviewer who is demanding a description of the distribution of microorganisms in coastal regimes as we think it is beyond the scope of this study. *Zetaproteobacteria* thrive in many hydrothermal systems (Emerson & Moyer, 2010; McAllister *et al.*, 2019) and it is the core of this paper, as an example of a well-described marine iron-oxidizer whose physiology, ecology, and mechanisms of oxidizing ferrous iron to ferric iron has been well-studied and widely reported. We emphasize that the *Zetaproteobacteria* grow wherever Fe(II)/O_2 interfaces exist in modern environments (Emerson *et al.*, 2010; Chan *et al.*, 2016a). *Zetaproteobacteria* have only recently been discovered in coastal sediment, where they mainly grow on Fe(II)-bearing mild and carbon steel, which results in contrasting Zetaproteobacterial populations relative to those found in hydrothermal habits (McAllister *et al.*, 2019). Even if Fe(II) ions come from mineral weathering and/or Fe(III) reduction, and are subsequently transported in anoxic groundwater today, then there is no link with the Precambrian environmental conditions during which dissolved Fe(II) ions were dominantly sourced by hydrothermal activity (Konhauser *et al.*, 2017).

Chan CS, Emerson D, Luther III GW (2016a) The role of microaerophilic Fe-oxidizing micro-organisms in producing banded iron formations. Geobiology 14, 509–528.

Emerson D, Fleming EJ, McBeth JM (2010) Iron-Oxidizing Bacteria: An Environmental and Genomic Perspective. Annual Review of Microbiology 64, 561–583.
Emerson D, Moyer CL (2010) MICROBIOLOGY OF SEAMOUNTS: Common Patterns Observed in Community Structure. Oceanography 23, 148–163.
McAllister SM, Moore RM, Gartman A, Luther GW III, Emerson D, Chan CS (2019) The Fe(II)-oxidizing Zetaproteobacteria: historical, ecological and genomic perspectives. FEMS Microbiology Ecology 95, fiz015.
Konhauser KO, Planavsky NJ, Hardisty DS, Robbins LJ, Warchola TJ, Haugaard R, Lalonde SV, Partin CA, Oonk PBH, Tsikos H, Lyons TW, Bekker A, Johnson CM (2017) Iron formations: A global record of Neoarchaean to Palaeoproterozoic environmental history. Earth-Science Reviews 172, 140–177.

2. The vent sites are heterogeneous on many levels, as is seen throughout the results. This is, of course, part of working with environmental samples. But, it does not make the task of using environmental samples for the purposes of building a model for events in the far past any easier. Also, the authors wish to discuss mechanisms of mineral transformation across many sites. For this reason, the description of the field, the location of the sites, their relation to one another, all this must be as clearly communicated as possible. And, it must be consistent to the figures.
Response: Correct, the LSHF is indeed heterogeneous in terms of fluid composition (Chavagnac *et al.*, 2018), substratum (Rouxel *et al.*, 2004; Escartin *et al.*, 2015), local geology, and tidal forcing effect (Wheeler *et al.*, 2024). However, Zetaproteobacterial mat-hosted ferrihydrite is a ubiquitous feature at hydrothermal sites from the mid-Atlantic ridge (this study and Scott *et al.*, 2015), Arctic mid-ocean ridges (Vander Roost *et al.*, 2017), and Pacific Seamounts (e.g., Singer *et al.*, 2013; Rouxel *et al.*, 2018). We agree that modern environments may be complex to build models for understanding past mechanisms, but modern-day observations are essential for testing hypotheses, and although heterogenous, the data show that some of these environments better-fit conditions that operated in the Precambrian Oceans. We avoid speculative explanations as much as possible. We have described the sampling sites in the supplementary geological setting section in more detail, but note that these sites have been extensively studied and given the word count limitation, we have to restrict ourselves to the most relevant and expect the reader who is interesting a better understanding of this environment to consult the cited literature.

Chavagnac V, Leleu T, Fontaine F, Cannat M, Ceuleneer G, Castillo A (2018) Spatial Variations in Vent Chemistry at the Lucky Strike Hydrothermal Field, Mid-Atlantic Ridge (37°N): Updates for Subseafloor Flow Geometry From the Newly Discovered Capelinhos Vent. Geochemistry, Geophysics, Geosystems 19, 4444–4458.
Escartin J, Barreyre T, Cannat M, Garcia R, Gracias N, Deschamps A, Salocchi A, Sarradin P-M, Ballu V (2015) Hydrothermal activity along the slow-spreading Lucky Strike ridge segment (Mid-Atlantic Ridge): Distribution, heatflux, and geological controls. Earth and Planetary Science Letters 431, 173–185.
Rouxel O, Fouquet Y, Ludden JN (2004) Subsurface processes at the lucky strike hydrothermal field, Mid-Atlantic ridge: evidence from sulfur, selenium, and iron isotopes. Geochimica et Cosmochimica Acta 68, 2295–2311.
Rouxel O, Toner B, Germain Y, Glazer B (2018) Geochemical and iron isotopic insights into hydrothermal iron oxyhydroxide deposit formation at Loihi Seamount. Geochimica et Cosmochimica Acta 220, 449–482.
Scott JJ, Breier JA, Iii GWL, Emerson D (2015) Microbial Iron Mats at the Mid-Atlantic Ridge and Evidence that Zetaproteobacteria May Be Restricted to Iron-Oxidizing Marine Systems. PLOS ONE 10, e0119284.
Singer E, Heidelberg JF, Dhillon A, Edwards KJ (2013) Metagenomic insights into the dominant Fe(II) oxidizing Zetaproteobacteria from an iron mat at Lō´ihi, Hawai´I. Frontiers in Microbiology 4.
Wheeler B, Cannat M, Chavagnac V, Fontaine F (2024) Diffuse Venting and Near Seafloor Hydrothermal Circulation at the Lucky Strike Vent Field, Mid-Atlantic Ridge. Geochemistry, Geophysics, Geosystems 25, e2023GC011099.
Vander Roost J, Thorseth IH, Dahle H (2017) Microbial analysis of Zetaproteobacteria and co-colonizers of iron mats in the Troll Wall Vent Field, Arctic Mid-Ocean Ridge. PLOS ONE 12, e0185008.

3. The authors describe Fe-rich crumbly mats and Fe-rich flocculent mats, but a more in depth discussion of what this means is lacking.
Response: In the supplementary introduction, we provided reasonable suggestions without making speculative comments to explain the origin of the flocculent and crumbly mats.

First, our mineralogical results indicate that the presence/absence of crystalline FeOx is not correlated with the mat type. The six studied mats share more than ten bacterial phyla (Astorch-Cardona *et al.*, 2024). Real-time quantitative PCR revealed that the *Zetaproteobacteria* class accounts for 11.7% to 45.0% of the bacteria communities, with the highest abundance in the flocculent mats and the lowest ones in the crumbly mats (Astorch-Cardona *et al.*, 2024). Two of the three flocculent mats, presenting high relative abundances of *Zetaproteobacteria* (at the WS and SI sites), show the less diversified Zetaproteobacterial communities. This could suggest a putative link between the Zetaproteobacterial communities and the type of Fe-rich mats. We hypothesize that flocculent mats are younger than those of crumbly mats.

Astorch-Cardona A, Odin GP, Chavagnac V, Dolla A, Gaussier H, Rommevaux C (2024) Linking Zetaproteobacterial diversity and substratum type in iron-rich microbial mats from the Lucky Strike hydrothermal field (EMSO-Azores observatory). Applied and Environmental Microbiology 0, e02041-23.

4. Why is Fig 8 only discussed in the conclusion? Is this not the central figure that should tie everything together and main message? If so, then this requires the authors to guide the reader through more thoroughly. By the conclusion, the reader should feel a sense of déjà vu, or a restatement, but rather it is the first time the reader sees the main point of the manuscript. The authors must dedicate more of the discussion linking their evidence to this figure. It is currently lacking.
Response: We followed the review's suggestion by introducing figure 8 in the discussion. We have also provided further details in the discussion to guide the reader.

5. There are 3 points outlined in Lines 388-403 that are all conceivable in my opinion. However, I am not convinced that the manuscript, as currently written, clearly evidences these three points. I recommend that the authors revisit this writing to evidence these points systematically for publication.
Response: This section has been revised per all reviewer suggestions (see lines 388-427).

6. Do the authors envision this mechanism they describe here as being important throughout the entire period of BIF deposition? If not, then what are the limitations? Can they speak to this in the manuscript?
Response: This point was raised by reviewers #1 and #2. We have contextualized our study by relating our study to the genesis of Algoma-type BIFs (see lines 62-72, 147-152, and 464-495). Our analysis shows a microbanding pattern in modern mat-forming *Zetaproteobacteria* from submarine hydrothermal environments, which could provide valuable insight into the genesis of similar features in some Precambrian Algoma-type BIFs.

Detailed comments: Abstract, line 28: What is meant by "profuse production" … Is this more than usual for this microbe? This sounds this way. And if so, that what triggers this profuse production?
Response: We meant that the Zetaproteobacterial stalks are abundant in our studied samples and this attributed has been found in all Fe-rich hydrothermal vent fields harboring these organisms, with their stalks often being the most dominant feature. This production is triggered by the large hydrothermal flux of dissolved Fe(II) (186-2789 µM, compared to the <10^{-3} µM Fe in seawater; Chavagnac *et al.*, 2018) and other nutrients. In addition, the mat age is not constrained, but ^{226}Ra/Ba dating of metal-rich deposits indicates that there has been hydrothermal activity at Lucky Strike for at least 6,600 years (Sánchez-Mora *et al.*, 2022). Considering that stalks, excreted by *Mariprofundus ferroxydans* cells, may reach 2.2 µm length/hour (Chan et al., 2011), this corresponds with the activity of the *Zetaproteobacteria.*

Chan CS, Fakra SC, Emerson D, Fleming EJ, Edwards KJ (2011) Lithotrophic iron-oxidizing bacteria produce organic stalks to control mineral growth: implications for biosignature formation. The ISME Journal 5, 717–727.
Sánchez-Mora D, Jamieson J, Cannat M, Escartín J, Barreyre T (2022) Age and Rate of Accumulation of Metal-Rich Hydrothermal Deposits on the Seafloor: The Lucky Strike Vent Field, Mid-Atlantic Ridge. Journal of Geophysical Research: Solid Earth 127, e2022JB024031.

Line 42: banded BIFs…banded Banded iron formations, remove one. Same line "chemical rocks" what is meant? Chemically formed? Abiogenic?
Response: Corrected as requested and as highlighted by reviewer's 1 and 2.

Line 56: I do not agree that all previous models suggested only planktonic microorganisms. Mats have been discussed before.
Response: We respectfully disagree with the reviewer. To our knowledge, the benthic microbial activity has never been evidenced as a potential contributor to BIF deposition. We will consider integrating references into the text if the reviewer provides references to support this fact. We have revised the paragraph (see lines 73-82).

Line 59: in oceanic, but also coastal photic zones.
Response: “coastal” has been added.

Line 62: “ferrihydrite rain” – is a citation required?
Response: Reference added. See for example, the review by Bekker *et al.* (2014)

Bekker A, Planavsky NJ, Krapež B, Rasmussen B, Hofmann A, Slack JF, Rouxel OJ, Konhauser KO (2014) Iron Formations: Their Origins and Implications for Ancient Seawater Chemistry. In: Treatise on Geochemistry. Elsevier, pp. 561–628.

Line 77: starting with “One reason…” I do not disagree, but I also argue that maybe there are multiple precursors and mechanisms – and that this is also the reason for the disagreements.
Response: We have rephrased the sentence.

Lines 103-108, starting with “To assess…” What are the limitations of studying modern hydrothermal vents to understand the past? Can the authors please add some lines to address the potential limitations (along with the strengths) of the site as a proxy? In other words, how strong is this link?
Response: For clarity, the text has been revised (see lines 114-131).

Lines 122-123: Which are the dominant organisms in these mats? Please add as it should only strengthen the argument for the proxy.
Response: As indicated above, we have described the microbial diversity in the LSFH mats (see supplementary introduction). For clarity, the text has been revised.

Lines 125-130: Can the authors be more specific as to which analysis were used here to build this model of microbial mat development using this proxy?
Response: Our conclusions are based on the convergence of our analysis and tools used, not based on a single method. Asking for a specific method, in a way, seems to invalidate the multi-proxy and technique approach we have used to arrive at our conclusions, which would, in effect water down the robustness of our study and conclusions.

Line 163-165: Can the authors cite similar interpretations?
Response: Reference added. See Chan *et al.* (2016b).

Chan CS, McAllister SM, Leavitt AH, Glazer BT, Krepski ST, Emerson D (2016b) The Architecture of Iron Microbial Mats Reflects the Adaptation of Chemolithotrophic Iron Oxidation in Freshwater and Marine Environments. Frontiers in Microbiology 7.

Line 174: Do the authors mean the FeOx at the top and the bottom of the mat? Are the FeOx minerals in Y3 silica poor? Unclear here.
Response: Clarified. We observe low Si concentrations in each laminated structure at the Y3 site.

Line 190: ending with “not identified” Anywhere? Or at which mat sites? Unclear.
Response: The paragraph has been revised (see lines 205-210).

Line 194-206: How can we interpret this heterogeneity in a useful way across time and space here, where SiO2 is discussed as well as later on where C and N systematics are discussed? What does the variation in the sites mean for the model they are describing? The authors must provide some more guidance in the discussion to strengthen the efficacy of their model.
Response: We are unsure what is being requested because we have shown that conditions existing at the Y3 site permit the formation of microbands and therefore reminiscent of like conditions that formed similar lithologies in the Precambrian Ocean, by so doing excluding the other sites. The word count limit of this journal does not allow us to diverge too much into facts not at the core of our manuscript. Nonetheless, we have described our samples and provided references on previous works on these sites that the interested reader can refer too for additional information. In this paragraph, we mention that the substantial variations of major elements at LSHF are related to the

incorporation of lithogenic (anorthite, amphibole) and hydrothermally-derived (barite) minerals within the mats, which did not prevent the microbial activity. We have discussed the role of SiO_2 concentrations adsorbed onto the ferrihydrite and possible mechanisms resulting in the absence of Si enrichment at the Y3 site. In addition, we have also discussed the relationship between P and TOC, as P is a macronutrient and then to bind iron oxides strongly. We have provided additional links between the mat elemental composition and our model (e.g., lines 320-336).

Line 210: “the above results”…which results are meant, specifically?
Response: We meant the mineralogical and petrographic results obtained using XRD and SEM-EDS analyses. We have clarified this context in the text.

Line 253: “consistent with” but not indicative of. Can the authors speak to this?
Response: Numerous factors influence the magnitude of C isotope fractionation during primary productivity. They include the different C dioxide fixation mechanisms, pCO_2 of fluids, and volume-to-surface area ratios of autotrophs (Hayes, 1993). Most of these factors are dependent on environmental conditions. This is why we use “consistent with” since all aspects are not fully constrained. In addition, the LSHF mats are a consortium of autotrophic and heterotrophic microorganisms (see supplementary introduction), with the likelihood that several pathways of autotrophic CO_2 fixation coexist.

Hayes JM (1993) Factors controlling 13C contents of sedimentary organic compounds: Principles and evidence. Marine Geology, Marine Sediments, Burial, Pore Water Chemistry, Microbiology and Diagenesis 113, 111–125.

Line 258-259: This is way too late and way to subtle to be the first mention that Zetaproteobacteria are foundational to the mats in these samples. The manuscript needs a massive reframing for it to stand alone. And there needs to be more images, perhaps of these organisms or a description of what their mineral forming mechanisms are – here to build this bridge to the past as the authors wish.
Response: As indicated in the comment above, we have clarified the role of *Zetaproteobacteria* in the introduction. Besides, our article does not aim to provide a review of the (i) morphology of Zetaproteobacterial cells and iron biominerals and their related ecological functions, and (ii) filament crystallization. These aspects have been extensively studied and reported and their physiology and mechanisms of oxidizing ferrous Fe to ferric Fe widely document as show in the key references. Our works simply builds on these well-found facts with requiring an extensive review. For examples the references by Chan *et al.* (2016b) and McAllister *et al.* (2019) provide the reader an excellent overview of the Zetaproteobacterial biominerals and diversity. Given the specific format of Communications Earth & Environment, this background information has now been added in the supplementary introduction section.

Chan CS, McAllister SM, Leavitt AH, Glazer BT, Krepski ST, Emerson D (2016b) The Architecture of Iron Microbial Mats Reflects the Adaptation of Chemolithotrophic Iron Oxidation in Freshwater and Marine Environments. Frontiers in Microbiology 7.
McAllister SM, Moore RM, Gartman A, Luther GW III, Emerson D, Chan CS (2019) The Fe(II)-oxidizing Zetaproteobacteria: historical, ecological and genomic perspectives. FEMS Microbiology Ecology 95, fiz015.

Lines 258-275: The interpretation of C- and N- isotopic signatures here is challenging. At best, this is due to the large disparity in temporal and spatial scales between the processes under study. Unless the authors really develop the discussion to lead the reader deftly through, I fear the reader is left unconvinced of the author’s conclusions.
Response: Paragraph revised (see lines 262-286).

Line 273-275: Starting with “Instead, …” this sentence is very difficult to follow.
Response: As indicated above, the paragraph has been clarified.

Line 277-283: Perhaps a conceptual drawing here would help to bring home the point.
Response: We have modified figure 8 to incorporate a conceptual model illustrating the mat structure and mechanisms that led to the low $\delta^{13}C_{org}$ and $\delta^{13}C_{carb}$ values.

Line 287: after "nucleation sites" – figure citation?
Response: We referenced this sentence (Johannessen et al., 2017, *Environmental controls on biomineralization and Fe-mound formation in a low-temperature hydrothermal system at the Jan Mayen Vent Fields*. http://dx.doi.org/10.1016/j.gca.2016.12.016).

Line 299: Figure? Reference?
Response: References are already provided, see Posth et al., 2014, *Biogenic Fe(III) minerals: From formation to diagenesis and preservation in the rock record*. http://dx.doi.org/10.1016/j.earscirev.2014.03.012; Boland et al., 2014, *Effect of Solution and Solid-Phase Conditions on the Fe(II)-Accelerated Transformation of Ferrihydrite to Lepidocrocite and Goethite*. dx.doi.org/10.1021/es4043275).

Line 306: "in agreement with our observations in Y3" Only at that site? Why? And why not the others? There is a need to improve the ability to understand differences and similarity between the sites for the reader.
Response: "in agreement with our observations in Y3 mats" has been deleted to avoid confusion. The second half of this paragraph and the next section explain the differences in Fe mineralogy controlled by environmental conditions.

Lines 314-316: Starting with "Further, our mineralogical…" This is interesting! Can the authors provide evidence for this here and discuss? There is more focus needed in general in evidencing the mechanisms proposed. How does this organism drive precipitation and mineral character/transformation in the mats, etc.
Response: We refer the reviewer to lines 195-199 in the result section. Combined XRD, FTIR, SEM-EDS, and electron microbe analyses (Figs. 2i, 3g, 5, 7; Figs. S1-3) show that Si is tightly bound to ferrihydrite and that amorphous silica is lacking. We are unsure what the reviewer means as we do not indicate that microorganisms drive precipitation in this sentence. In addition, following this sentence, we argue that temperature and pH are the controlling parameters of the binding capacity of Fh for Si.

Lines 322-326: Again, there is a lack of conceptual drawing to distinguish the dominant controls at the sites that drive the variation seen within the model proposed.
Response: We have introduced figure 8 here to support the text.

Lines 337-342: Starting with "Goethite..." What drives or suppresses this transformation in these mat environments?
Response: The second half of the paragraph has been reworded (see lines 344-353).

Line 348: What is "all evidence"? Can the authors be specific?
Response: "all evidence" changed to "petrographic observations and all isotopic evidence".

Line 354: "indicates that the δ13C-carb values are controlled by alternative mechanisms" How? Please explain, as rather central to the argument the authors bring.
Response: In this paragraph, we have demonstrated that the $\delta^{13}C_{carb}$ values could not have been controlled by the hydrothermal fluid composition. We explain these alternative mechanisms linked to biological processes in the following paragraph.

Lines 360-365: This is a long and confusing sentence. Please reword.
Response: Sentences changed (see lines 371-377).

Line 366: "FeOx-rich layer compared to the Fh-rich layer" The terms are not making sense. Is Fh not ferrihydrite? Is that not an FeOx?...
Response: Fh refers to ferrihydrite, which is a poorly crystalline Fe(III) oxyhydroxide, while FeOx refers to crystalline iron oxides.

Line 368-372: I am happy to read this interpretation. I agree.
Response: Thank you for this positive comment.

Line 373-379: This information again is coming so late in the manuscript for a paper on the role if microbial mats in the deposition of BIF…And again, I would like to see a conceptual image showing this idea, rather than just a description.
Response: These sentences have been moved to the next section as we have reorganized the discussion following all reviewer's recommendations. Figure 8b provides the conceptual model illustrating the idea.

Line 405, Section Formation of Si-rich bands: this is an intriguing concept, but not sufficiently deconstructed here yet.
Response: This section has been revised (see lines 429-462) following the recommendations of reviewer #3.

Line 443-444: "the striking absence of Si-rich bands in BIF"…What do the authors mean by this? I don´t agree.
Response: This is a mistake. We have changed "in BIFs" to "at LSHF".

Figure 1: The font in the labels for the sample sites are very hard to read due to the red color and the small size. These labels could also include the acronyms uses throughout the manuscript here as in the caption. The consistency will help keep the reader on track. Please change.
Response: The figure has been modified. Acronyms are only used for the studied sites where the mats thrive. To avoid confusion, we prefer not to use acronyms for describing the main vent site (for example, "Tour Eiffel" is the name of the high-temperature hydrothermal vent, while "North Tour Eiffel" is the site where low-temperature diffuse fluids and microbial mats are described.