

30 June 2026,
Dr. Mark Lever
Associate Editor, EGUSPHERE

Dear Dr. Mark Lever,

Please find attached the revised version of our manuscript ID egusphere-2026-133 entitled “Benthic foraminiferal species tolerance for hydrothermal activity: a case of study from the Lucky Strike vent field.” Once again, we are happy that the reviewer recognised the significance of our work, and we share his/her enthusiasm for the results of this study. As you will see in our point-by-point response, we have considered the new comments and revised the manuscript accordingly.

We hope that this version will be satisfactory and we thank you for your time and interest in this matter.

Sincerely,

Pierre-Antoine Dessandier, on behalf of all co-authors.

Response to reviewers' comments:

Note: Our responses to the reviewers' comments are in blue.

Submitted on 22 Jun 2026
Anonymous referee #1

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

This article is worth publishing as there is a lack of meiofauna and forams studies in hydrothermal vents. However, there were some methodological weaknesses which were pointed out in my first review such as the methodology used to study forams (favouring hard shelled ones), the geochemical studies (bulk analyses cannot give information at the micrometer scale, which is needed to talk about microhabitat differences) and the lack of direct methods to elucidate the trophic role of forams.

Authors have added explanations for reasons why they used this specific morphological methodology. It is good to have added the precedent test with formaline l. 130-139. The best argument for using the RB-alcohol method is comparison with previous studies. I am OK with that, particularly because now the lack of monothalamids (artificial or natural) is added in the results and discussion. I just have a last question, which does not need to be answered here but is for further studies by the authors: are you sure that you were able to recognise all soft-walled forams in the formaline samples? Some of them can be very tiny or peculiar. eDNA studies show a high presence of monothalamids in all kinds of environments, not only the deep sea, but also bathyal, neritic and intertidal sites.

We thank the reviewer for this remark, we will definitely take into account the soft-shelled foraminifera in our future plans in the area and other deep-sea systems, which is already

planned by developing other methodological approaches (flow cytometer) and by using molecular biology for the future. Regarding the question on identification of previous (formalin-preserved) samples, as said in the methods, we could have missed some organisms, but comparing with our other samples from different environments (e.g., Pacific CCZ abyssal plain) where there were a lot of soft-walled foraminifera observed, we can be confident that they were far less abundant in the Lucky Strike vent field.

About the bulk geochemical analyses, one of my comments was not understood by the authors: l. 177: can you really speak about microhabitat characterisation when you do bulk analyses? Answer: We are sorry to admit that we do not clearly understand the comment of the reviewer here. We characterized by geological and chemical analyses the habitat of microfauna. To avoid some confusion, we changed the title to Habitat Characterization. To explain my comment, their analyses are never fine enough to be at the scale of the microhabitat as they do bulk analyses of 1cm slices of sediment instead of finer slicing or in situ analyses like 2D gels for example. Now the authors have made corrections, but there are still some parts of the text where they should change microhabitat to habitat: l. 17, l. 199, l. 287, l. 501, l. 552.

This is clearer now; we thank the reviewer for explanations. Suggested modifications have been made accordingly.

As there was no dedicated sampling for trophic network on site, authors have now added this part in perspective of the paper, which is OK for me.

The number of live individuals never reaches 300 in the samples, therefore it would be important to clearly say it at the beginning of the results, as the statistical analyses may be less strong with these low numbers, and keep it in mind for the discussion. The authors disagree with my statement that their conclusions are mainly based on comparisons with other foram assemblages, but with low live forams numbers, statistical analyses are weaker than usual.

We understand the point made by the reviewer here, our response mentioned that the interpretation is based on a robust environmental characterization and not only foraminiferal communities, but we agreed that living individuals are less than 300 and we added in the methods that this may affect the statistical approach for the living community.

Talking about the absence of forams, it is always more difficult to prove it than their presence. The authors apparently did not understand my comment on l. 348-350, where I do not speak about soft-shelled forams, but the organic lining of dissolved calcareous forams. In some environments like the Baltic Sea or estuaries with cable bacteria, dead foram shells (PF and BF) disappear by dissolution, but some calcareous forams can survive without their shells and are identified by their organic lining. Did you look at the presence of organic lining as some calcareous forams can survive low pH without their calcitic shell (see the papers of Charrieau et al. 2017 in the Baltic or Daviray et al. 2024 with cable bacteria)? In this situation, eDNA could also help investigate the presence of forams.

We are sorry for the misunderstanding, since it was mentioned by the reviewer in his questions “formaline preserved soft-walled foraminifera” as well, we believed that his point was mainly on that. We didn’t observe any morphological feature or particle that would correspond to a dissolved foraminifer. Furthermore, except in microbial mats, as explained in the text, there was no evidence for low pH in the sediment (i.e., a lot of calcareous planktonic foraminifera, ostracods, gastropods, classical pore water profiles and sediment

color and smell, etc.). These mats may have an impact on carbon budget because of the dissolution of carbonated shells, whether the organic lining remains or not do not change this impact. But once again, we fully agree that a future careful check of all soft-shelled/remaining organic lining of foraminifera using eDNA in these environments would be of a great interest, as mentioned in the text. In order to be more clear in the text, we added this sentence: “There was no visual evidence for remaining organic lining of foraminifera after tests dissolution either, however a future check using molecular biology in these biofilms would better confirm this.”

In the results, it is important to separate well analyses made with live forams from the ones made with dead individuals (as stated by the other reviewer). This is still not very clear from l. 293 including the RDA analysis (were they done with live, dead or both?).

I guess the reviewer here do not mention a reference to lines of the corrected version of the manuscript? Does it correspond to the first version sent? If so: In the corrected version, it was actually already written in material and methods as well as in the figure’s caption of the RDA that this corresponds to the dead community, only (see our response to reviewer 2 on this point). We added this in the results as well, that should correspond to where reviewer 1 is talking about.

l. 17-18 : microhabitat environmental descriptors : the methods used (bulk geochemistry of 1cm slices instead of in situ like 2D gels for example) do not allow a description at the micrometer scale, it would be more honest to speak about habitat environmental descriptors or just environmental descriptors.

This has been modified to habitat.

l. 48: here you speak about a specific kind of extreme environment, please specify which or give other references for examples of extreme environments

Ok, we added the notice that it corresponds to cold seeps.

l. 99: red circles instead of grey ones

Thanks, this has been modified.

l. 124, 397, 411, 506: Zetaproteobacteria instead of zetaproteobacteria

This has been modified.

l. 127: Montsegur-Ref or Reef?

We modified this to better correspond to the rest of the text: Montsegur Ref (i.e., for reference).

l. 130-139: good to speak about the precedent test with formaline

Ok, thanks.

l. 139-141: and WoRMS?

We added this sentence to make it clearer: “A full list of species accepted in WoRMS, with taxonomic references is provided as supplementary.”

l. 176-179: as commented by the other reviewer, you need to analyse live and dead assemblages separately

Thanks to point this out, all changes have been made in the stats, plot and discussion but this small part remained unchanged by error, we modified the sentence as following: “Dead

organisms were considered here in order to restrain seasonal effects and to ensure robust interpretations as rarefaction curves tend to show underestimation through living fauna (supplementary 2).”

l. 181-182: Pooled Fissurina and pooled non calcareous species are artificial groupings of different species with potentially different ecologies, it would be wiser to remove them from the analyses.

Please see our developed response to reviewer 2 on this point.

l. 185 we cannot instead of we can't

Ok, we modified this.

l. 199 Sediment habitats instead, again your geochemical analyses do not allow a fine (i.e. micro) scale description

This has been modified.

l. 162: explain MS Ref first time it appears in the text

This Montsegur Ref appears in the map, in the text describing the map and in the table of station names (Table 1), where all acronyms of stations are explained (e.g., ET for Eiffel Tower, MS for Montsegur, etc.) earlier in the text. We also added here: “south to Montsegur”.

l. 221: Sediments ... show only (instead of shows)

Ok. We modified this.

l. 245: Add the total number of live individuals together with the number of species.

This has been added in the text.

l. 246: highest number of individuals = 101, does it mean that you never reached 300 in any of the samples? In that case, you need to say it in the M&M or results sections.

This has been added in methods.

l. 298: monothalamids instead of allogromids (allogromiids if written correctly) as you use the former later in the text

We modified this accordingly.

l. 323-324: Lobatula instead of Cibicides

l. 333: same comment

According to WoRMS, the current accepted name is *Cibicides pachyderma*.

l. 337: RDA performed on the main species, dead or alive?

Only dead fauna, please see our responses above on this point.

L 429: other refs?

l.454-460: *C. pachyderma* lives in shallower environments (usually bathyal) than *L. wuellerstorfi* (preferentially abyssal)

I think that for this point and others mentioned by the reviewer (i.e., the proportion of soft-walled foraminifera), there is a misunderstanding that is leading to some confusion. Our study area is not abyssal, but actually bathyal (less than 2000 m water depth, as described for classical environment for *C. pachyderma*) but where we did find *L. wuellerstorfi* as well.

l. 503: has instead of have

[We modified this.](#)

l. 504: fossil foraminifera instead of fossils

[We modified this accordingly.](#)

l. 518-519: we cannot exclude that the observed fossil Fissurina species come... instead

[We have modified as suggested.](#)

l. 562: by removing instead of by removal

[This has been changed as suggested.](#)