13 May 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.” We are happy that the reviewers and the Editor recognised the significance of our work, and we share their enthusiasm for the results of this study. As you will see in our point-by-point response, we have considered all the reviewers’ 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:

General answer: We thank both reviewers for the time they dedicated to the revision of our manuscript and for their comments.

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

Reviewer 1

- *Does the paper address relevant scientific questions within the scope of BG?* Yes
- *Does the paper present novel concepts, ideas, tools, or data?* New foram data acquired with traditional morphological approach, not well suited in these deep sea environments where a big part of the community has soft-shells

We thank the reviewer for this comment and mostly agreed on it. However, on the contrary to deeper environments of the Mid-Atlantic ridge and elsewhere, soft-walled organisms are rare in the Lucky Strike vent field. Indeed, we carefully checked samples collected in the Lucky Strike vent field during a previous cruise, preserved in a solution of buffered formaldehyde 4%. These samples were unfortunately not stained, hence not of use for the present ecological study (table added in supplementary not for publication). In these samples, we did not observe a significant number of soft-walled individuals compared to hard-shelled ones (representing less than 2% of observed foraminifera). A few of them, such as *Vanhoeffenella* spp. were observed in the present study and were indeed correctly preserved probably because the time in ethanol before analysis was very short (< 1 month). Furthermore, the comparison between pelagic sediments and metalliferous sediments showed strong differences in foraminiferal assemblages of taxa identified with the present protocol. Even though some soft-shelled individuals could have been underestimated (which is unlikely considering previous observations), this would not prevent the observations of environmental control on foraminiferal community. In addition, the use of dead communities (in which soft-shelled foraminifera always disappear) allowed to interpret more general trends disconnected to seasonal variability, integrating a longer period of record. Hence, in order to better know the community as potential bio-indicator, we believe that the protocol used in the present study is sufficient.

- *Are substantial conclusions reached?* Mainly based on comparisons with other foram assemblages

Our conclusions are based on multivariate statistical approach, comprehensive environmental dataset, particularly considering the available data on deep-sea hydrothermal vents and on an evaluation of living and dead faunal distributions for the first time in this vent field. Both the multivariate stats and the matrices correlation highlight the environmental control (hydrothermal-derived elements) on communities, the impact of microbial mats on carbonate preservation and the potential use of benthic foraminifera as bio-indicator of vent conditions. Hence, we consider our conclusions not mainly based on comparisons with other assemblages but mainly on environmental interpretations.

- *Are the scientific methods and assumptions valid and clearly outlined?* The protocols used do not allow to view the total foram community. There is a lack of direct evidence for the trophic role of forams.

We agreed on this reviewer's point. A dedicated sampling for trophic network would have been of interest to more elaborate on the different strategies of species in the area. Unfortunately, this study has not been designed from the beginning for it, we will add this in perspective of the paper. However, we believe that the environmental data provided are sufficient to make hypotheses on their feeding strategy.

- *Are the results sufficient to support the interpretations and conclusions?* In many cases, no. For example, the trophic network would need more investigations and the absence of forams in certain environments may be due to the protocol used (more precise examples in the detailed comments).

See the answers above regarding the protocol used for soft-shelled foraminifera and trophic analyses. In addition, even an under consideration of soft-shelled fauna do not prevent the interpretation of the most dominant taxa (calcareous) for their response to the environment. Finally, the absence of foraminifera, fossil, benthic and planktonic is a result by itself suggesting a carbonate dissolution which is not connected to the protocol or the observation of soft-shelled foraminifera. The main conclusions are consequently supported by the results in our opinion.

- *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?* The authors need to say if they picked wet or dry to collect forams.

We agreed that the methodology part was not detailed enough. We modified as following: Each core has been sliced on board every centimeter and split in two equivalent volumes for each horizon, one preserved at -20°C for geochemical analyses and one preserved in a solution of Rose Bengal 2 g L^{-1} in 96% ethanol for living benthic foraminiferal identification, individually wet-picked in petri dishes (Table 1). Since the preservation used was not the best for soft-walled foraminifera as formalin would have been, this study may underestimate this group. However, a previous check of formalin-preserved samples without staining did not show high abundance of soft-walled foraminifera in the area and in order to be consistent with comparable literature in vent studies focuses on benthic foraminifera, ethanol preservation has been used.

- *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?* In the intro, there is a need for more refs from other authors and older than the

work of the first author for forams. More refs are also needed in the discussion on foram trophic strategies, extreme environments in general and with low pH.

References have been added in both introduction and discussion. See in responses to detailed comments.

- *Does the title clearly reflect the contents of the paper?* The paper is more about the communities of forams found in different environments around hydrothermal vents than on their tolerance to ecological parameters

This paper is actually based on the environmental control on foraminiferal communities, demonstrating their tolerance or not to vent conditions in our opinion.

- *Does the abstract provide a concise and complete summary?* Yes
- *Is the overall presentation well structured and clear?* Yes
- *Is the language fluent and precise?* Not always, see detailed comments.
- *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?* The authors need to check the scientific names of forams through the manuscript and supplementary files as there are many typos in addition to some taxonomic discussions raised in detailed comments.

We thank the reviewer for pointing out this. Names have been checked and corrected accordingly.

- *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?* Exposed below in detailed comments.
- *Are the number and quality of references appropriate?* More refs are needed in the intro and discussion.
- *Is the amount and quality of supplementary material appropriate?* Plates 1a and 1b: some scientific names with typos, some sides not ideal for taxonomic identification

This has been all corrected.

General comments:

This is a good point to finally have a study of foraminifera from these environments. The environmental data are well presented and there are some interesting points raised. However, this study does not take in account the complete community of benthic foraminifera as it is only focusing on hard shelled forams. This is problematic as it is known that a substantial part of the foram community is represented by soft-shelled forams in the deep sea (see work of Andy Gooday on morphology and Jan Pawlowski's group on eDNA).

See our response below regarding this particular point. In particular, the deep-sea environments mentioned by the reviewer here with eDNA studies are much deeper and consequently very different in terms of foraminiferal composition. Once again, a previous check of this vent field sediments with an adapted protocol for soft-shelled organisms revealed the almost absence of them (supplementary), but more importantly, the conclusions of the present study do not suffer from a potential underestimation of this group in our opinion.

The vision of the trophic network is a bit simplistic and too theoretical; there is a lack of field proofs directly acquired from the forams (e.g. labeled isotopes, TEM images or microbiome analyses) as only

environmental analyses are presented here and connected to foram assemblages without strong or direct evidence.

We agreed on that. As this study was the first one dealing with foraminiferal assemblages in this environment, the protocol planned for it was not designed to anticipate all food web hypotheses. Unfortunately, we cannot change it now, but we added some limitations in our interpretations to better consider it based on the reviewer's comments. Furthermore, we believe that this study still represents a very important first step to understand foraminiferal ecology in deep hydrothermal vent systems but we have added what has been suggested as perspective work in this area.

Detailed comments:

l. 20-21:"feeding on pre-degraded OM characterised by phytoplankton detrital signal" → based on which data?

We added in the sentence: "as shown by the $\delta^{13}C$ of the TOC".

l. 24-25: only data for hard-shelled forams, what about soft-shelled ones?

This is now better explained in the methods. Again, we did not observe many soft-shelled foraminiferal individuals in other formalin-preserved samples, at least not more than those mentioned in the present study.

l. 28: field instead of filed

Thanks for pointing out, this has been corrected.

l. 34: "benthic organisms" is vague, you mean macro-organisms, metazoa?? Forams can also feed on larger organisms, e.g. nematods (Dupuy et al. 2010) or scavenge on dead animals (Nomaki et al. 2025).

We modified the sentence accordingly.

l. 36-37: at least since the 1960s!

Corrected.

l. 43: there is an important body of references predating this one!

We added older references here: Mackensen et al., 1995; Altenbach et al., 1999; Loubere and Fariduddin, 1999

l. 45: there are very different kings of extreme environments (e.g. anoxic, deep sea, temperature, salinity,…), here you focus only on cold seeps or hydrothermal vents, right?

Yes, indeed in the sentence it was written "where fluids enhance microbial activity", hence this focuses on fluids-rich sediments that directly targets the purpose of the paper in order to not compare with other extreme environments that have no links in terms of environmental context.

l. 47: expand the number of citations if possible

References have been added here: Alongi and Pichon, 1988; Gooday et al., 2008; Panieri, 2006; Bernhard and Panieri, 2018

l. 48: here again, you refer to a certain type of extreme environment, not all of them

The sentence has been modified.

l. 75-78: you need to say that only hard-shelled forams were studied here

A clarification in the methods has been added to explain this.

l. 98: what do you mean with “local biology”, biodiversity? Metabolic activity?

We changed this accordingly.

l. 125: Rose Bengal in English

This has been corrected.

l. 125-126: this protocol is only well suited for hard-shelled forams, ethanol does not preserve well soft-shelled forams. Did you do wet or dry picking? Could you tell it in the manuscript?

We added the information of wet picking in the methods. See the new version of this part:

Each core has been sliced on board every centimeter and split in two equivalent volumes for each horizon, one preserved at -20°C for geochemical analyses and one preserved in a solution of Rose Bengal 2 g L-1 in 96% ethanol during several weeks for living benthic foraminiferal identification, individually wet-picked in petri dishes (Table 1). Then, the samples were wet sieved under filtered (0.2 µm) seawater and all foraminiferal analyses were done on the >63 µm size fraction. Since the preservation used was not the best for soft-walled foraminifera as formalin would have been, this study may underestimate this group. However, a previous check of formalin-preserved samples without staining did not show high abundance of soft-walled foraminifera in the area and in order to be consistent with comparable literature in vent studies focuses on benthic foraminifera, ethanol preservation has been used. All living individuals (>63 µm) were sorted for foraminiferal counts. Dead individuals (>63 µm) were sorted after drying the samples and using an Otto microsplitter to estimate the foraminiferal densities.

l. 128: Loeblich and Tappan 1987 instead of 1988 (a lot of discussions about that, e.g. ww.cambridge.org/core/journals/journal-of-paleontology/article/abs/publication-date-of-foraminiferal-genera-and-their-classification/FEC5AADF8ED5E8160F4674202DB514F5)

Thank to the reviewer for the notice, we corrected this reference accordingly.

l. 159-163: to include dead forams can give long term tendencies, but does not replace the missing data of soft-shelled forams

The sentence has been changed accordingly. Once again, the idea of including dead community is to avoid the impact of seasonal changes including longer term trends. Soft-shelled foraminifera could not be used for this since they are not well preserved after their death, hence general trends integrating several seasons can’t be based on a dataset including soft-shelled foraminifera.

l. 164-165: you need to update your taxonomy here, C. wuellerstorfi and C. pachyderma were moved to another genus a long time ago (Cibicidoides, Schweizer et al. 2009) and there are new discussions on placing wuellerstorif in Lobatula (WoRMS), there are also discussions about Epistominella in WoRMS

All species names have now been corrected according to their status in WORMS.

l. 177: can you really speak about microhabitat characterisation when you do bulk analyses?

We are really 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.

Fig. 3: could you analyse all the samples that you examined for forams? If yes, say it in the caption as it is not clear which samples you show here

The caption has been changed accordingly.

l. 201-211: are these sedimentation rates typical for this kind of environment? The deep sea?

Yes, typical for Atlantic deep sea, we added a reference for it.

l. 230-231: did you collect more than 300 individuals per sample? Or 300 for live and 300 for dead forams? Could you clarify this sentence?

We clarified this both in the methods and in the result part, explaining that we sorted all live individuals and a minimum of 300 dead individuals.

l. 232: are the species evenly distributed or are there some dominant ones?

The relative abundance of species is developed in the following part of the manuscript, this part is dedicated to general abundances and diversities.

l. 236-237: you only used the counts of live forams here?

As already explained in methods, standardized living foraminiferal data were used to perform the PCA.

l. 257-259: this sentence is difficult to understand, a bit out of context

It's actually the interpretation of foraminiferal distribution on the environmental domains defined by the PCA. We added the following sentence to make a clearer link to this point: The living foraminiferal signals plotted in the PCA demonstrate how fauna distributes on these environmental domains.

l. 270: Miliolida order

This is now corrected.

Fig. 5: it could be interesting to distinguish the different orders with specific colour codes

We modified the figure as suggested.

l. 292: 44% is not very high, is it statistically strong enough?

First, yes, the RDA is statistically strong enough, with an ANOVA-like permutation test for redundancy analyses with a p value of 0.007. This information is now included in the manuscript. Also, even though not very high, 44% is quite a good percentage when it comes to ecological studies, especially in the deep sea. Furthermore, we performed a forward selection analysis prior the RDA to avoid co-linearities, selecting two environmental variables. Those two variables alone explained 44% of the variance among the community which is quite a strong result. Even though the explanation of the 2 axes is not perfect, this is already quite informative for deep sea foraminifera, especially from very contrasted environments in our opinion.

Furthermore, the change asked by the other reviewer to put community separately in the RDA also changed its axes significance to 54%.

l. 316: did you check for the presence of benthic squatters in the planktonic foram shells? (e.g. https://www.researchgate.net/publication/315797952_Benthic_foraminiferal_squatters_species_that_inhabit_the_tests_of_other_foraminifera)

There were no living specimens within planktonic shells, we didn't include squatters to reach our 300 individuals of dead foraminifera.

l. 321-323: foraminiferal potential food source as there was no direct measurement from the forams themselves

The word "potential" has been added. In addition to answer this point, the source is clear according to the $\delta^{13}C$ signal, and the ability to feed on microbes for foraminifera has been evidenced many times.

l. 329-330: this sentence is unclear, develop how it is a key

We modified the sentence to better explain why they are key environments in this ecosystem as following: Additionally, microbial biofilms were not included in statistical tests because of the absence of fauna, being nevertheless key for distribution understanding, since they represent a significant surface covering hydrothermal vent sediments but preventing foraminiferal life and carbonate accumulation.

l. 348-350: it was shown in other environments with low pH (Baltic Sea, Charrieau et al. 2018 or mudflat with cable bacteria, Daviray et al. 2024) that rotaliids can survive in low pH environments, even with a dissolved shell; therefore other approaches could be useful here to check that no foram can live on these mats (e.g. formalin fixed samples to preserve soft-shelled, dissolved or naked forams or eDNA/eRNA to get total foram diversity)

We do not talk about soft-shelled foraminifera here since there is no link with dissolution of carbonate and soft-shelled foraminifera. We actually showed here the absence of dead shells, even of planktonic foraminifera, while thousands of them were found all around the mats, which suggests shells dissolution.

l. 368-370: do you have refs for this suggestion? The presence of both foram groups and food source is not enough to suggest

We agreed and we added references showing this hypothesis already proposed on the same basis in the literature. We also believed that thanks to these references and providing the identification of the

food source available in the sediment and showing a correlation with a phytoplanktonic signal of the organic matter is enough to suggest as hypothesis the feeding strategy of these species.

l. 370-376: interesting hypothesis

Thanks.

l. 379-382: I agree with the difference of geochemistry, but how the sampling strategy of both studies Ccould explain this difference?

As already mentioned in this sentence, the sampling strategy of the two studies defined the distance to the vents. The study of Krüger et al. (2025) aimed at studying the dispersion of the plume at large spatial scale (200 m to 41 km) and its impacts on fauna. The present study focuses on hydrothermal vent-derived sediments at a much closer spatial scale (>50 m) distance from active chimneys to sampling sites. Then, the closer community to the vents of Krüger et al. (2025) corresponds to further distance than all stations studied in the present study. We added a sentence in the text in that regard.

l. 382-384: this is not a criterion to distinguish species as C. wuellerstorfi are also attached to their substrate; moreover, you could potentially have other species of cibicidids here

See our response below, following the next comment.

l. 384-386: Lobatula lobatula (formerly C. lobatulus) is a true genetic species found in shallow water and is well separated from these deep sea specimens, even if some look morphologically similar.

We checked the different morphospecies of *Cibicidoides* and *Cibicides* present here. Other species (<5%) have been observed but not discussed in the study as being minor. The morphological differences observed in *L. wuellerstorfi* individuals seem very close to the description given in the reference cited in the text.

l. 391: there are older refs with in situ observations (the cited paper deals with specimens in aquaria)

We have now added more references here of in situ observations: Mackensen et al., 1995; Murgese and De Deckker, 2005; Gooday et al., 2008

l. 397: agglutinated instead of non-calcareous (as only hard-shelled forams were examined here)

Vanhoeffenella sp. and the specimen of allogromid cited in this sentence are actually soft-shelled foraminifera and not agglutinated species. This explains the use of the term non-calcareous foraminifera in the sentence and it would be an error to change it to agglutinated species.

l. 397-398: Vanhoeffenella instead of Vanhoeffela and Martinottiella instead of Martinorinella

Thank you. This has been corrected.

l. 399-400: there is a huge variety of monothalamids, not only sessile species, but also vagile ones, soft-shelled ones, naked ones of all sizes. With the methodology used here, it is impossible to get track of all of them. Therefore you need to specify which kind of monothalamids you mean here.

We mentioned here directly the soft-shelled species observed and cited in the previous sentence (i.e., Vanhoeffenella and an allogromid species), both monothalamids.

l. 401-402: their occurrence there is mainly limited because of the sampling protocol, it is then impossible to say that they are absent in Lucky Strike

Once again, we agree that the protocol used was not the best for soft-shelled foraminifera, but it is very important to keep in mind that samples storage in ethanol did not exceed a month before the analysis, which makes a huge difference (compared to several months) for general soft meiofauna after our experience. However, this protocol has been often used for benthic foraminiferal studies. Even in the most recent study focusing on deep sea hydrothermal vent from the Mid Atlantic Ridge (Krüger et al., 2025) making the comparison easier keeping the same protocol. Furthermore, the reference cited in this sentence also used ethanol preservation and did observe this group. We did observe some individuals of this group as well and we checked at a preliminary step formalin-preserved samples without more individuals than presented here (see table below, could be added as supplementary, not for publication), but without a staining protocol for this previous dataset, we cannot use the results for ecology purpose.

0-1 cm	2005-17_CL7	1996-8_CL7	1999-11_CL7	2004-16_CL6	1996-8_CL6	1998-10_CL6	2004-16_CL7
	MS_REF	Cap2	SC	ET_W2	Cap	MS_N	ET_W
Alabaminella weddellensis	1						
Ammodiscus sp							
Anomalinoides globulosus		4		6	2		
Bolivina alata							
Bolivina spathulata				3			
Cibicides pachyderma	2				6	4	
Lobatula wuellerstorfi	22	6	32	13	6	32	5
Cornuspira sp.					4	1	
Cruciloculina triangularis	27	29	7	26	22	71	4
Epistominella exigua						6	
Eubuliminella exilis				2			
Favulina hexagona							
Fissurina annectens				3		4	
Fissurina foliformis				15		2	
Fissurina laevigata							
Fissurina orbignyana	21	15	1	125	19	21	9
Fissurina var. saturni				4			
Gavelinopsis translucens				54	1	2	1
Globobulimina affinis							
Globocassidulina subglobosa	2			33		9	6
Hoeglundina elegans		5					
Karreriella bradyi						1	
Lagena semistriata							
Lagena striata	1					2	
Lagnea radiata		1		21	1		
Laeticarinina pauperata		2					
Lenticulina orbicularis	1			1			
Lenticulina sp							
Martinottiella cylindrica							
Melonis affinis				2			

Neolenticulina variabilis		1					
Nodosaria sp					1	1	
Nonionella jacksonensis							
Protoglobobulimina pupoides							
Pullenia quinqueloba	1	16	5	17	7	5	8
Pyrgo inormata		53			66	5	2
Pyrgoella irregularis	4	11		6	18	24	4
Pyrgo depressa	25	5			9	3	
Quinqueloculina auberiana	121	110		115	163	130	12
Reophax agglutinatus							
Reussoolina laevis							
Robertinoides sp.	1	1		3	4		
Spiroloculina sp.	1	4		1	7	3	
Stainforthia fusiformis							
Textularia sp.				1			
Trifarina sp.							
Triloculina oblonga	8	14		3	7	15	1
Trochammina sp.		1					
Uvigerina auberiana		2		1			
Uvigerina peregrina				4			
Vaginulina sp.							
Vanhoeffenella sp.							
undetermined soft		1	1		2	2	

l. 402-404: do you have other refs showing narrower ecological preferences in agglutinated forams compared to calcitic ones?

We added references stating the same thing: Heinz et al., 2005; Panieri and Sen Gupta, 2008; Martin et al., 2010.

l. 405: which species? Belonging to which groups? Be more precise here

We modified the sentence citing agglutinated species.

l. 407-408: is it due to lower densities of biocoenoses or to lower preservation conditions after death?

Even though we can't exclude low preservation after death, the individuals found within the thanatocenose were visually very well preserved, while no living fauna was found in this station, hence we believe that this signal is mainly due to low densities of living fauna. We modified the sentence to make it clearer.

l. 412: fresher OM deduced from what? Foram community or direct measurement?

It is indeed deduced from foraminiferal community, and written "potential" in the sentence. Furthermore, these stations are characterized by a microbial signal on its source (-25‰ of $\delta^{13}C_{TOC}$ versus -22‰ for phytomarine signal as in references), which means a local productivity on the contrary

to phytodetrital exports from the euphotic zone of the water column that would involve a longer time and transport of the OM particles.

l. 413: was the higher microbial productivity measured?

Yes. See the previous answer.

l. 416-419: this is true for opportunistic species, but it is not the case for all deep sea species

Ok. We are actually focusing on the opportunistic behavior of species from our study. We modified the sentence accordingly.

l. 423-426: seasonal sampling would be needed to check that assumption

Agreed. But comparing a single sampling period with integrated time of several seasons as represented by the dead community at least allows to compare the difference between the present sampling period and an averaged signal. Since a seasonal deep-sea sampling is unlikely in the middle of the Atlantic Ocean, we believe that this assumption is relevant.

l. 426-428: is it also the case in your samples?

We considered all foraminifera >63 µm in the present study, hence the same observation is valid in the present study for the metalliferous sediments, as now noticed in the text.

l. 432: could you say which living community?

We modified the sentence adding “the foraminiferal living community”.

l. 434-436: could this change observed between live and dead assemblages be due to environmental changes (vent activity, current,…)?

The hypothesis for vent activity change is developed a few lines below in the text.

l. 439-441: live Fissurina?

No, the dead and living individuals are clearly separated in the figure and results, we modified the sentence.

l. 447-448: absolutely, therefore it is difficult to make hypotheses out of dead assemblages here

The hypothesis is based on the fact that for all stations, the activity and currents may change while this observation was only true for this site. The chemical composition of the sediment and pore water here highly suggest a chimney dismantlement, which impacts the chemical composition of the habitat, only observed in Capelinhos cores. Even if we already mentioned the other hypothesis of environmental change through time, the evidence of the chemical context is directly explaining the absence of living fauna. Before this dismantlement, the community of foraminifera was probably able to occupy the habitat as observed in the fossil record with environmental changes or not.

l. 456-457: as explained in the introduction, the bypass is more due to a lack of study of the meiofauna and could not give a full picture of the trophic network in these environments

Agreed, we had this part in the sentence: as well as too limited number of studies on vent meiofauna.

l. 466-468: the demonstration is rather indirect and needs more direct evidence

Once again, the demonstration of the microbial origin of the organic matter measured is evidenced by its isotopic signal $\delta^{13}C_{TOC}$.

l. 469: what is the evidence of this adaptation?

This is based on the community without any opportunistic species in reference cores, which have been on the contrary observed in metalliferous sediments.

l. 472: organic-walled specimens could not be targeted by this study with the protocol used

Although potentially underestimated (very unlikely), we did observe them in other stations and zero in these cores.

l. 482: foraminifera cannot be a trophic chain by themselves, only an element of the chain

We modified the sentence accordingly.

l. 484: hydrothermal chimneys inhabited by microbial communities?

We modified as suggested.

l. 487-488: you need to develop this sentence to link both before and after sentences

We rephrased all this part to make better links.

Supp. Files:

Corrections to make in the figure caption:

1A: 10) Fissurina, 13) Fissurina, 28) other side more interesting for identification, 32) same

1B. 8) is not Pyrgo, 9) Fissurina, 11) Fissurina, 25) other side more interesting for identification, 28) looks more like Planulina ariminensis than C. wuellerstorfi, 31) Vanhoeffenella

We made all changes as suggested, although unfortunately we cannot go back to the image acquisition to change the side or add new specimens to image the other sides of the species observed.