Author's Response for "**Community Structure and Taphonomic controls on benthic foraminiferal communities from an Antarctic Fjord (Edisto Inlet, Victoria Land)**"

Galli G., Caridi F., Giordano P., Morigi C., Sabbatini A., and Langone L.

Dear Editor and Reviewer,

We would like to thank you again for acknowledging the novelty and impact that our work could have. All the comments and suggestions were interesting, and we feel that the concerns and doubts highlighted by both reviewers were thoughtful.

We accepted every grammatical and rephrasing suggestion proposed by both reviewers, and, for the sake of simplicity, we will not discuss them in here.

To not repeat ourselves with similar reasoning concerning both for the water mass discourse, the sediment accumulation rates and the Organic Carbon, in this response we are going to respond to the more in-depth questions risen in the previous interaction that were not covered by the public discussion response. Following the latter, we added:

- A statement with the time that passed from the collection of the core and the sampling of the organic carbon was added in the Section 2.1
- A brief explanation on the possible effects of the different sedimentation regimes was added at the end of Section 4.2
- The relationship between *M. arenacea* and HSSW was stated multiple times across section 4.3.3 and 4.3, as well as in the conclusion section

Following, a detailed point-by-point response for every concern raised by the reviewer. We hope to have been exhaustive.

Reviewer #1 point-by-point response:

- Lines 65-66: There have been numerous other studies of the distribution of modern benthic forams across the Ross Sea continental shelf, not just in the troughs, including McKnight 1962; Kennett 1966, 1968; Pflum 1966; Fillon, 1974; Osterman and Kellogg, 1979; Melis and Salvi 2009. Please see Seidenstein et al. 2024 J. Micropalaeontology for citations.

Accepted. Added the reference suggested by Rev#1 and changed the text accordingly. We do not cite Seidenstein et al., (2024), because the study focusses on Pliocene-Pleistocene warm-water incursion on the Ross Sea using foraminiferal communities while our focuses on modern fauna.

- Line 94: reduced vertical mixing due to the stratification caused by summer meltwater?

Accepted and sentence added accordingly.

- Line 110, Table 1 caption: Maybe add something in the caption about the location of these 5 cores in the fjord.

In the caption, we integrated with the relative locations of the cores over the studied transect.

- Line 149-150: Can you provide a couple of example citations for "commonly employed method for that analysis and more suited for the scope of the study"

Accepted. Added two references: Majewski et al., (2020), and Peck et al., (2015)

- Line 150: also, studies by Melis et al., and Ishman et al., routinely uses the 50 or 63 μm fractions

Accepted. Added two references: Melis and Salvi, (2009) and Ishman and Sperling, (2002).

- Line 180: Is the sediment core also from the inner fjord? If so, maybe mention it here.

In this line, we talk about the location of the sediment core analysed in Galli et al., (2025), which is shown in Figure 1, as indicated in the text. We do not feel that explicitly writing the relative location of core TR17-08 add any new information respect to what is already shown in Figure 1.

- Line 194: referring to the planktic foraminifera as indicator of primary productivity, "or open water conditions? OR open water, primary productivity regimes?"

This can be the case; however, it is important to notice that in our setting, the difference between season is quite remarkable. Open water condition prevails over the austral summer, while the sea-ice cover persist throughout the winter months. We added this information.

- Line 195-196: "the almost complete absence of the latter [planktic test]" likely due to corrosion due to the presence of HSSW?

As stated by the Reviewer#1, the presence of enhanced dissolution conditions on the outer part of the fjord could be attributed also to presence of HSSW, as shown in Caridi et al., (2026). While we agree with the statement, we prefer to incorporate this line of reasoning on the Discussion part rather than the Result one.

- Figure 6: Please define what axes MDS1 and MDS2 means?

In an nMDS analysis, the MDS can be regarded as homologous of the Principal Component from the PC analysis. The point of the nMDS used in this study was not to define the environmental spectrum in which our community sits, but rather showing the effect of multiple environmental gradients (as indicated by the geochemical parameters) on the benthic foraminiferal communities. This is why we used polygon to highlight singular site. We do not feel that discussing the MDS meaning could add any new information, since the communities' structures are highlighted in the Figure 4 and Figure 5, as well as in Figure 9.

- Line 291: Are the sediments of the inner fjord rich in diatoms too? Typically, biosiliceous sediemnts are very corrosive to foram carbonate; we've documented this in the Pliocene (Seidenstein et al., 2024) and Miocene (Bombard et al., 2024) of the Ross Sea continental shelf.

As stated in the response to the reviewer on the public discussion, the sediments on the inner fjord are mostly - if not only, composed by biosiliceous material, a very common type of sediments within the Antarctic Continental Shelf due to the very seasonal peaks of productivity that this region experience. We agree that this could be yet another cause of taphonomic control on the assemblages. We added a statement accordingly, keeping in mind that the two paper cited by the Reviewer #1 concerns very different timescales than ours.

- Line 315: Very high dissolution conditions likely due to the high concentration of OM?

Accepted and added accordingly.

- Line 358-359: The discussion of sea-ice is not clear to me, i.e., how this might be affecting the assemblages.

We apologize for the confusion and for the poor style when explaining the effect of variability of the environmental parameter at the entrance site. Our line of reasoning was that in enclosed basin most environmental variability is at the entrance, where water masses exchange and where the landfast sea-ice cover is less robust because far from the anchored point (e.g., the coast). Hence, these locations might experience the most different conditions throughout the years, giving birth to the differences between the living and dead assemblages. We changed the phrase accordingly.

- Line 371: *M. arenacea* is […] and/or dissolution conditions, including due to the influence of HSSW?

In Caridi et al., (2026) the presence of HSSW has been noted. Since the site is located at the entrance, the hydrographical regime could bring HSSW at these sites. We added a phrase accordingly.

- Line 433: *M. arenacea* has been shown to be associated with HSSW

Accepted and changed accordingly.

- Line 451: Caused by sea-ice break up, high productivity, high flux of organic matter and dissolution of some calcareous species of benthic forams?

Accepted and changed accordingly to emphasize the indirect role of sea-ice cover variability on the benthic foraminiferal communities.

Reviewer #2 point-by-point response:

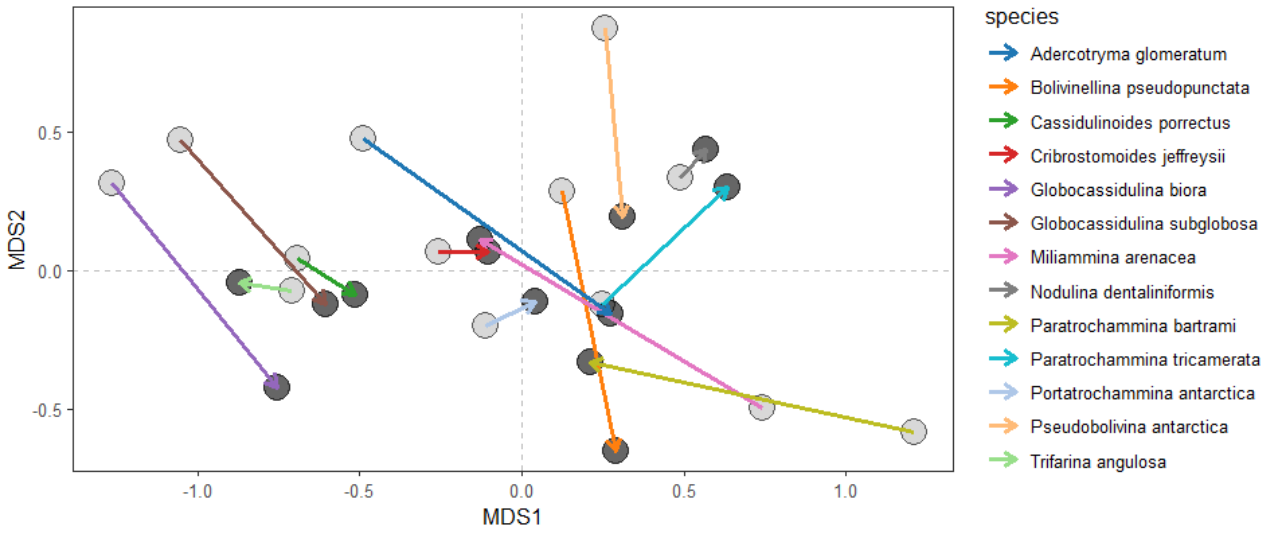
- For what it concerns the figure: 1) to add ocean circulation to fig.1; 2) to use the same color for a same species in figure 3 and 4; 3) If possible, to add a figure where nMDS for a same station is given for living and dead faunas (to visually see the discussed differences); 4) add to figure 8 the number of points available for each period represented by a colored bar (n=XXX).

We thank the reviewer for all the suggestions regarding the figures.

For point 1, since we added a short description of the oceanographic settings in Section 1.1-Line 100 and since it is not the scope of our study to frame the oceanographical context we do not feel to add the oceanographic circulation. Important information about the results from the CTD casts can be found in Caridi et al., (2026), which is cited along Battaglia et al., (2024).

For point 2, we acknowledge that the colour coding across the two figures should be the same. However, it is very difficult – if not impossible - to find color-blind friendly palette with that number of categories. We decided to use this kind of color-coding between the two assemblages because it better emphasize the communities' changes throughout our transect, rather than the showing what it is in common, which is described and visualize in figure 9.

For point 3, overlapping the nMDS between the living and dead assemblage is wrong, since the species space (e.g., relationship with the species and MDS) changes between the two models. So, if we would overlap the dead nMDS on the living nMDS model, for example, this would be wrong because the two axes are not *strictly* the same. For this reason, we decided to not add another nMDS plot. To prove our point, in the figure below, only the common species between the two assemblages are shown and they are compared by using a color-coded arrow connecting the living (grey dots) and the dead (black dots) assemblages. As shown in the figure, just by looking at the common species between the two assemblages, the species-space changes quite a lot, proving the incomparability between the two nMDS coordinate, if plotted together.



For point 4, instead of adding the number of points to the already crowded figure 8, we added the number of points in the results from the analysis (Section 3.4) in brackets.

- Line 50: you should cite the TROX model from Jorissen et al., (1995)

Accepted and added accordingly.

- Line 82: “tidal”glaciers? This information can be very important, for interpretation of continental supply from glaciers.

Knowing the nature of the 4 glacier that directly affect the sediment supply might be a key information to properly disentangle the complexity of the Edisto Inlet. However, to our knowledge, no glaciological study has been carried out on any of the glacier. Of notice, is the fact that one of those (the one next to Luther Peak, Fig. 1) is still missing a proper name.

- Lines 99-100: Can you please add more details about ocean circulation in the fjord? Even just reporting the data from Battaglia et al., (2024)

We added few lines where we explain the oceanographical context using the study of Battaglia et al., (2024) and Caridi et al., (2026).

- Line 10?: How much high sedimentation rates? Can you give a range?

Following the previous response, we added few lines after the oceanographical context to better frame the differences in the sedimentation regimes. We also showed that, during the last 3600 yrs BP, the sedimentation rates were higher until 700 yrs BP with values of about 0.7-0.5 cm yr^{-1}. Then, the sedimentation rates suddenly decrease to values of about 0.07 - 0.04 cm yr^{-1}.

- Table 1: I think the date for station 34 should be 2023, not 2024

We apologize with the reviewer for the typo.

- Line 116; Line 127 and Line 140 – related comment of Rev#2

While we remind to the response published in the public discussion on the Organic Carbon data, we do not think that adding the information requested by the author adds new information or complements the already explicitly written methods showed in Section 2. We added a brief sentence where we write that one year passed from the collection of the sediment cores and the geochemical analysis.

- Line 151-135: The branched forams are always excluded from counting because of this problem, but they are normally included in biodiversity data as qualitative information. This should be the case here, also. They can be used as indicators of high hydrodynamics.

We agree with the Reviewer about the use of branched forams as indicator of high hydrodynamics. However, counting fragment could be misleading and they are known to be difficult to identify at the species level. Despite this, in the Supplementary Material we added the downcore concentrations of tubular fragments.

In the figure, tubular fragments across all the stations have been found decreasing with depth, and with a maximum concentration of it at site 178, aligning with our interpretation that, over the entrance high hydrodynamics develop because of the water mass exchange between the inner and outer part of the fjord.

- Figure 5 and text in general: Please be clear and consistent with the terminology in order to avoid confusion. You should have living (RB stained), dead (instead of fossil?) and paleo (or fossil?) faunas.

We thank the reviewer for raising this point. We agree that consistency was lacking. Throughout the manuscript we indicate with *living* the Rose Bengal-stained specimens, with *dead* the samples analysed in this study and with *fossil* the one from core TR17-08.

- Line 285-286: it seems that the sentence lacks a word (a very harsh what?)

We apologize with the reviewer for this. We added “environment”.

- Lines 334-336: I would rather say that the results is a combination of mortality and taphonomy.

Accepted and changed accordingly.

- Line 362: I would add that more studies need to be done to fully understand the variability of deep and intermediate water masses (possibly linked to benthic ecology) in this area

Accepted and changed accordingly.

- Line 372: is that possible that OM is more efficiently remineralised compared to the inner fjord and less OC is therefore buried?

We agree with the reviewer. However, this interpretation is written a few lines above the Line 372.

- Line 464: is that possible that agglutinated species are also indicative of less OM availability?

We agree with the reviewer and this is why, from Line 458-460 we wrote “At the sill, the dominant faunal component of the first centimeter are miliolids, while, going deeper into the sediments, specimens deep infaunal species such as *Nodulina dentaliniformis* appears, indicating a higher hydrographic regime, in conjunction with a less oxygen-depleted environment with less OM content”.