Point-to-point response of: "Edisto Inlet as a sentinel for the Late Holocene environmental changes over the Ross Sea: insights from foraminifera turnover events"

Dear reviewers and editor,

We would like to thank all of you for appointing us to the major shortcomings that this manuscript had, and we apologize for the ambiguity that arise between previously published paper and data regarding this study. We also think that these reviewers' comments and suggestions were highly beneficial for our study, and we would like to thank the reviewers for taking their time to correct and suggesting these improvements. We acknowledge that in the suited section.

Since "major revisions" were suggested to the first version of the manuscript we tried to satisfy all the reviewers concerns and the editor suggestions. However, a more in depth response (point-to-point) was done only for the comment of the rev#1 (https://doi.org/10.5194/egusphere-2025-309-RC1) and these as well as the grammatical corrections risen by rev#1 are indicated in the **Detailed response section** following this one. It is important to note that the results and discussion part was completely reorganize in accordance with the rev#1 and rev#3 concerns. However, we still feel that the results and discussion should not be divided because they give a cohesive and organic structure to the text.

As to what concerns the **major changing on the text**:

1) The **abstract** was extended to provide the major information that are captured by this study. In this part of the text, we explicitly added the information about the RoC and the Turnover Event associated, the changes in the major species assemblages, as well as stating more directly the inferences derived from the RoC curve. In addition to this, we briefly extended the regional implications and stated the importance that this study has because it integrates community-level proprieties into paleoenvironmental and paleoclimatology interpretations.
2) In the **introduction** we changed the order of the text, with the description of the previously published study happening earlier in the text to strength our findings and the importance of the study. In accordance with rev#1 suggestions, we also stated the hypothesis that the RoC should be higher in periods of unstable environmental condition and lower in stable periods. We believe that this strengthens the message delivered.
3) In the **method** section, we removed the first subchapter, and we described the method for the dating and constructing the age-depth model in the supplementary material (as well as the material used to construct it with a comparison between the Marine13 and Marine20). More discussion on this matter is reported later in this response.
4) The **results and discussion** section were highly condensed. While there were no significant changes about the RoC and the assemblage composition part, a complete reorganization of the environmental conditions section was done. First, we removed all the subparagraph and changed the figure regarding the environmental evolution of Edisto Inlet to a more specific one, highlighting the transition between the multi-year landfast ice scenario (3.6-2.7 kyrs BP) to the seasonal sea-ice scenario (2.5-1.5) while also indicating the changes in the benthic foraminiferal assemblages. We think that this figure is better suited for the scope of this study and shows with a greater detail the main conclusion of this manuscript. We also changed the Table 1 to be aligned with this new outline, highlighting both the proxy and the microfossil assemblage. We removed all the overlapping part of the discussion with the previously published paper as well as rearranging the text to always discuss the RoC and the microfossils assemblage interpretation first, and the XRF results later. Previously published data are always discussed considering the RoC and the XRF. Following the suggestion of rev#1 and rev#3 we reduced the number of interpretations, agreeing with their statement about this overly complicated and difficult to follow part. We think that this new way of discussing the environmental evolution of the Inlet is easier and more intuitive than the previous version, while delivering the important implications that the transition has. We thank the reviewers for suggesting that. For what it concerns the regional implications, no significant changes were made because we think that, after refining the previous discussion part, this delivers the message of the regional implications in a strong and cohesive way.

For instance, we added a brief description of the work of Mezgec et al., (2017) after the SAM implications because it strengthens the view of this atmospheric mode acting on our study site and, in the Ross Sea.

5) The conclusion was extended to be aligned with the changes done on the text.
6) In the supplementary material we added the discussion about the age-depth model as well as removing the species list of the core TR17-08 because it was previously published, following rev#1.

We think that this changes on the manuscript strengthen our initial text and hypothesis, and we hope that this view will be shared by the editor and the reviewer. While doing this, we realize that the study in the initial part was lacking the delivering of this important message, and we thank the reviewers for having highlighted that.

Concerning the points risen by the reviewers and the editor, we will respond them here in more detailed matter in addition to the already published response to the public reviewer report.

1) The use of new data and the importance of the RoC

The new data concerns the use of the XRF, while using the micropaleontological data collected and published from Galli et al., (2023) and Galli et al., (2024), as stated by both rev#1 and rev#2. To remove this ambiguity, we added a full description of the first published results at the of end introduction sections, on the context of the previously published article of this region. In addition to this, in the first part of the method section, it is explicitly written that the data of the micropaleontological assemblages, as well as the BFAR, PFAR and IRD were derived from Galli et al., (2024). We think that this removes all the ambiguity regarding this matter. We also removed the interpretations from Galli et al., (2024) and Galli et al., (2023) as stated in the point 3 on the major changing list.

Also, as highlighted several times by the rev#1, we focus on the RoC analysis because it is an important component of our study and can provide valuable information, not only for polar regions, but for the whole micropaleontological community since it is sensible to changes in the compositional assemblages that could be missed. This is now discussed better at the end of section 3.1 by showing the alignment with the results with the previously published interpretation of the last 2 kyrs BP as in Galli et al., (2023). The importance of the RoC is also emphasized in the Section 3.2 regarding the environmental transition, where it is highlighted the relationship between the macrofaunal response and the RoC of the benthic foraminifera (now this can be easily seen in Fig. 5a).

Reviewer#2 highlighted that our Supplementary Material 2 were not aligned with FAIR. However, we disagree with this statement because our first worksheet of the SM2 reports the depth, the age and the raw counting of the benthic and planktic foraminifera species. The SM2 is also reporting the Age-depth model, and the RoC values to reproduce our result, while also implementing the data of the BFAR, PFAR and IRD from Galli et al., (2024).

2) The age-depth model used in this study

One of the major shortcomings, and concerning, about this study was the age-depth model. This was highlighted by rev#3 because, as he\she noted, this regional implication relies on an accurate and robust timeline. We agree with this statement. Another point risen by the same reviewer was the presence of an ambiguous values of ΔR of 791 ± 121 in the first version of the manuscript, while it was specified a value of 790 ± 125 on where the first age-depth model was published (Di Roberto et al., 2023). However, the statement of the rev#3 about the value of 791 ± 121 not being reported in Hall et al., (2010) is not true, because the values is recommended for the whole Ross Sea region, as can be red in the Section 4 of the cited article: "*[...] The results presented here indicate that, for Holocene Southern Ocean samples, a delta-R value of 791 ± 121 should be applied*."

Moreover, the value of 791 ± 121 yrs BP have been used quite extensively, even in recent publication over the same region and over similar time scale (see Wood *et al.* Sedimentary DNA insights into Holocene Adélie penguin (*Pygoscelis adeliae*) populations and ecology in the Ross Sea, Antarctica. *Nat Commun* **16**, 1798 (2025). https://doi.org/10.1038/s41467-025-56925-4).

While we are confident that the age-depth model of Di Roberto et al., (2023) is robust, we still wanted to test if the calibrated ages using the Marine20 were highly different by using the methodology suggested by rev#3. The values of ΔR 790 ± 125 years proposed by Di Roberto was calculated following the methodology of the paper of Hall et al., (2010), by using the coral samples reported in that study and following the personal recommendation of P. Reimer (see the Supplementary Material of Hall et al., 2010).

Following Heaton et al., (2023) we used the Calib database to calculate the regional ΔR of the Victoria Land Region by using the most proximal points from our sites (a total number of points of 12 were used). The ΔR calculated this way is 622 ± 177 years (Table S1 of the values used to calculate this ΔR are reported in the new Supplementary Material file). However, when using the new Marine20 with this new ΔR, the calibrated ages do not different significantly form the age-depth model of Di Roberto et al., (2023). In this sense, while little-to-no discrepancies are present in the median values (except sample VI 12-13), the error where much larger for the Marine20 calibrated dates. This are reported in the table S2 of the supplementary material. Thus, our proposed age-depth model is robust and accurate and can be used to derive meaningful timeline for the environmental evolution of Edisto Inlet. To further show that the age-depth model used in Di Roberto et al., (2023) was robust, we computed another one using Bacon (Blaauw and Christen, 2011). The results show an almost perfect agreement between the average linear sedimentation rate of 0.5 cm/yr and a bottom core dated with a difference of only 10 years (Marine13: 3646 yrs BP vs Marine20: 3636 yrs BP). This implies that the age-depth model constructed by Di Roberto et al., (2023) is robust and reliable in terms of timing of the event and environmental interpretations. Moreover, it is important to notice that our discussion and results are not interpreted at the decadal scale, which is where differences could arise and disrupt the interpretations, but rather at centennial and millennial scale. These changes in the calibrated ages do not affect either our interpretation or the significance of the regional comparison discussed in this study. Hence, for comparing the results of Galli et al., (2023) and Galli et al., (2024) we decided to use the already published age-depth model. However, we acknowledge that transparency is important. In the new supplementary materials, we show the values of the ΔR calculated as suggested by rev#3 (Table S1), while also reporting the uncalibrated ages used to construct the age-depth model (Table S2), compared with the newly derived one. We also showed the Bacon results in the Figure S2. We hope that this reasoning is shared among the reviewers and the editor.

Another minor adjustment involves the figure 1 and acknowledgment section. The first figure of the paper reported the presence of Antarctic Circumpolar Current (ACC) over the Ross Sea shelf break. However, this is not true because is the Antarctic Slope Current (ASC) that flows along the shelf break. We changed the figure accordingly as well as in the text. In the acknowledgment section, on the first version of this manuscript we indicated the *R/V* Laura Bassi has the ship that retrieved the data. However, the ships for that expedition were the *Italica*. We changed the text accordingly and apologize for both the overlooking.

Detailed response to the rev#1 comments
Abstract

-Line 13: What are "key" environmental changes? Please be more specific what kind of environmental changes are meant here.

Since the works focus on marine sediment cores and benthic foraminifera, the word "key" refers to palaeoceanographic changes. The word "key" was changed accordingly.

-Line 14: I would suggest deleting "all" before "interacting components". Nobody can do this. "Proxies" are used two times in one sentence. "Micropaleontological proxies" could be replaced by "Microfossil assemblages".

We accepted the suggestions of the reviewer #1 for Line 14.

-Line 15: "abundances of species"? Better would be "changes in microfossil assemblages".

We accepted the suggestions of the reviewer #1 for Line 15.

-Line 20-22: The authors use a ROC analysis, addressing the whole community and highlight this (compare with lines 16-17) but here they say that the most important species are investigated?

This apparent contradiction is because the RoC was used on the whole benthic foraminifera community to detect the TE, while the interpretation of the succession of the environmental changes was based on the most common species (> 10%). The phrase from line 21-22 was changed to clarify this point.

-Line 23: Which proxies come from nearby cores? Please be more specific here.

Accepted.

-Line 25: Write out "mCDW" when using the first time.

Accepted.

-Line 26-27: This sounds like this will be a future study? I would suggest removing this sentence.

Accepted.

Introduction:

Generally: It would be better to first characterize the studied region and observed climate changes and then to come to the foraminifera as tracers/proxies. I assume there are more findings in the studied region than mentioned here?

The study area is relatively new in respect of its investigation. Most, if not all, of the study that were carried out on Edisto Inlet are cited in the text: Finocchiaro et al., (2005), Tesi et al., (2020), Di Roberto et al., (2023), Galli et al., (2023), Battaglia et al., (2024) and Galli et al., (2024). In addition, the major findings of these studies are already reported in the text.

However, to have a more comprehensive view of the environmental evolution we explicitly added how the MCA and LIA were recognised to highlight the response of this area to regional changes on the end of the Introduction as well as clearly stating the previous results from the study of *Galli et al., (2023)* and *Galli et al., (2024)*.

Line 33: Here the authors state that foraminifera can be used as tracers. In the abstract they speak about "proxies" (line14). Both is possible. If you can quantify something, you can say "proxy", if you only see changes without a quantification then you have to say "tracer".

In Line 14, Changed "proxy" in "paleoenvironmental tracers" as suggested by the reviewer #1.

Line 34: Please remove "the" before "benthic and planktic foraminifera". Not all foraminifera have a test, please add "most".

Accepted.

Line 34-35: Please add "in marine settings" at the beginning of the sentence. Foraminifera can also be found in salt marshes and terrestrial salt meadows.

Accepted

Line 36: "Most of the studies" and then only one specific reference? Please add some more.

Accepted.

Line 39-41: Is the late Holocene a good analogue for future climate? I would not say so. Further, I would suggest removing the sentences line 39 to 41. These sentences have no connection to the sentences before and after. Perhaps, these sentences could be integrated at the beginning of the introduction?

Accepted and removed.

Line 43: Please replace "phases" by "changes".

Accepted.

Line 47-50: A lot of "used" here. Replace some of them by other verbs.

Accepted.

Line 53: "significant" are used two times here. Significant should only be used in context with a significance test etc.

Accepted and removed.

Line 63: Which kind of "changes" - environmental?

Accepted.

Line 68: "Corethron pennatum" is another diatom species?

Yes. It is clarified now.

Line 70: "diatoms" and not "diatom's".

Accepted.

Line 74: The last sentence could be removed.

Accepted.

Line 78-79: Here it would be better to formulate some hypotheses.

For this section we stated the following hypothesis under the advice of rev#1:

"If this interpretation of unstable-to-stable transition happens, then the rate at which the benthic community is shifting should be higher before the transition, and lower after, reflecting a period in which a stable community cannot form"

Study area

Line 80-95: This chapter could be merged with chapter 1.2 "Study area".

Accepted

Line 82: Please remove the "." And replace "both flows" by "both flow".

Accepted.

Line 89: "form" instead of "forms".

Accepted.

Line 94 "Drygalski area" – where is this area? Could be marked in Figure 1 or shortly mentioned in the text?

We specify in the text that the Drygalski area is located on the western part of the Ross Sea.

Line 103: Please add "cores" in the figure caption.

We do not understand what the reviewer is referring here since the caption of the panel c of Figure 1 clearly state that the points presented in the map are marine sediment cores.

Line 111: "from" instead of "on".

Accepted.

Line 114: "saltier" and "colder" compared to what?

This is compared to the outer part of the fjord. To better elucidate this point, we added: "[…] in the inner part of the fjord in comparison to the outer ones"

Line 115: No new paragraph here.

Accepted.

Line 117-118: This sentence could be removed, I would say. The study is mentioned in the above text.

Accepted.

Methods:

General: What data is new and must be described here? Be careful with data published before.

Line 1232/123: "PNRA"?

The acronym stands for the National Italian Program for Antarctic Research. We indicated that in the text.

Line 126: "ten" instead of "10".

Accepted

Line 152: Please add "," before "we computed".

Accepted.

Line 166: Referring to Fig. 2 is wrong here. It should be Fig. 3.

Accepted.

Line 167/173: The packages and R must be cited. Please provide references and versions.

Both the version of R and the package used are cited here, more than once.

Line 174-176: If there are only a few foraminifera from 0.7 kyrs BP until today, then the authors should not calculate ROC for this time interval. I have no experience with ROC analyses, but I am not sure whether the ROC analysis helps for the study. The interpretations can solely be based on the assemblages.

As stated in the online response of the reviewer #2, The RoC is a statistical procedure that can be used to detected significant changes in the ecological community. Thus, while the marginal helping that this analysis seems to aid is not on the interpretation, but rather on the definition on where this shift of the ecological community is happening. We disagree that this can be also done by using "solely the assemblage" because the choice of the species to look at can be subjective and the same goes true for where to put the transition, since this is mostly done manually by looking at curves. Also, as stated in the manuscript, the changes are in minor components while the major fjord-like indicators species remaining almost dominant throughout (Fig. 5). Thus, this analysis can let us discriminate between different succession of the community because of the Turnover Event detected by the RoC instead of looking at specific species response.

The first point raised by the rev#1 is a good point: since very few foraminifera were present above the 0.7 kyrs BP, the RoC might not be informative. This is way the RoC where computed two times. The first one, using all the record, while the second one is computed only using the 3.6-0.7 kyrs BP (Fig. 4).

It might be pointed out that another way in which assemblage analysis is undertaken in micropaleontological analysis is the use of multivariate techniques, mostly Principal Component Analysis (PCA). However, PCA assumes that the variables (species in this case) have a high degree of linear relationship between each other, which is often not the case for ecological entities. In this sense species are used as "elemental ratio" rather than an ecological community, and, by using PCA it is also impossible to not loose information because of the way it is constructed. This can be misleading and might miss important ecological information.

This can be seen in the paper of Galli et al., (2023) where almost all the signal using a PCA was dominated by the *G.biora, P. antarctica* and *P. bartrami* (Table 3, Fig. 5 of cited article).

Thus, the RoC is an important technique because 1) removes the uncertainty associated on where to put the transition limits of ecological variables, in this case the benthic foraminiferal species, and 2) because it does no assume any relationship between the species but analysis the stratigraphical dissimilarities of all the ecological entities along a temporal framework while reteaning all of the information of the dataset.

Line 182-198: To many paragraphs in the subchapter.

Accepted.

Line 185: Be more specific here regarding the Ca/Ti. It is a proxy for exactly what?

The Ca/Ti has been used extensively across different regions and sites to detect changes in the biogenic sedimentation vs lithogenic sedimentation because of Ca being mostly derived from calcifying organisms and Ti being derived mostly from continental sources. This has been specified in text, following the suggestion of rev #1.

Line 191: A specific R-package was used? If so, please mention here.

Not a package was used but rather a function (*lm*) from the base version of R. We changed the text accordingly.

Line 200: A total of 51 foraminifera or foraminiferal species?

A total of 51 foraminiferal species. However, we removed this sentence because it overlaps with previously published study.

Line 202-203: Can go to methods section.

We attached that to the method section in the first part of the section when describing the core expeditions and general characteristic of the marine sediment core.

Line 204-208: Somewhat repetitive to the description in the methods section; sentences could be removed or added to the methods section.

We agree with the rev#1 and decide to remove the sentence.

Line 210: "large" instead of "big".

Accepted.

Line 238: I would suggest replacing "we compare" by "is compared".

Accepted.

Line 243: Globocassidulina subglobosa and not G. subglobosa (first time usage).

We agree that this is the first time of this species being mentioned. However, it shares the same genus as the previously mentioned one. Hence, it is not mandatory to write the same genus twice when dealing with different species belonging to the same genus.

Line 244: At least Globocassidulina subglobosa is a cosmopolitan species and not restricted to Antarctica.

In the sentence it is not specify the endemicity of the species being restricted to Antarctica, but rather its occurrence in this geographical region. We changed the text accordingly.

Line 254: Please remove "the" before G. biora.

Accepted.

Line 256: Please provide a reference for the opportunistic behavior of Nonionella iridea.

The reference is stated in the sentence following the point. However, we decided to remove this definition and merge the two sentence into one so the reference is at the end of the statement.

Line 259-261: I would agree with this interpretation since this species becomes not dominant. But what is about the time interval between around ~3000 yrs BP – there is a large shift (increase in agglutinated species, decrease in calcareous)?

This is a good observation, and we thank rev#1 for asking this question. The presence of an increase in the agglutinated form could be driven by an increase in the dissolution condition for the carbonate. However, this is also closed to the 3 kyrs BP change in the as highlighted by the *N. iridea* and thus this could be the results of increasing organic matter content that enhances the already high dissolution condition (Fig. 4 and 5).

Line 268: conditions instead of "condition"; "for the carbonaceous fauna" could be removed.

Accepted.

Line 269-271: Can the authors be sure that temperature plays a role for this species? Could salinity changes also be possible?

Disentangling those effects is difficult, and we agree that salinity could also drive the change. Indeed, many factors could drive this change. For example, another factor that could drive this change is the amount of dissolved oxygen.

These three parameters (Temperature, salinity and dissolved oxygen) strengthen our interpretation on the mCDW entering the fjord. In a recent article of Lehrman et al., (2025) on the Twaites glacier, the presence of *E. exigua* has been found to be related to the increase in CDW content and shallowing of the CCD. Conversely, *M. arenacea* has been found to be related to the presence of oxygen.

We agree with the rev#1 and remove the hypothesis of a warmer water masses on this sentence because it is already described the subsequent section of this paragraph.

Line 272-274: The authors want to state that under the influence of warmer water masses, phytodetritivourous input is higher? What means "from the top" – in surface water? Line 278: Does ice free conditions resulted in a higher surface production?

Accepted and replace "top" with "surface water to the bottom". An underlying warm water mass has the effect of increasing the ice-free season, thus promoting primary productivity and an increase in the phytodetritous input from the surface layer. The record from Edisto Inlet is almost completely dominated by the summer signal, therefore, an increase in the primary productivity could be related to the prominence of an ice-free season. This because more light can be available to the surface making the phytoplankton community being able to thrive for longer period. This has the net results of increasing the organic matter content at the bottom.

Line 280-283: I am not sure whether this interpretation can be made. This species (T. angulosa) has a rather low abundance in the core. Line 283: A major environmental shift should be visible in the foraminiferal record. I cannot see it.

The use of the RoC helped us to identify the major compositional change that could not be visible otherwise. Thus, is the procedure that is telling us that there are significant changes happening at the time, and we can connect them to specific changes that are happening to the less represented species. This is why it is possible to state that Edisto Inlet was still having fjord-like environmental features (represented by *Globocassidulina biora*, *G. subglobosa, Paratrochammina antarctica and Portatrochammina bartrami*) while the major changes can be described by focusing on the accessory species. This strengthen the use of this statistical analysis to detect and disentangle complex environmental relationship, while giving us an object way to define where these changes are happening.

Line 286: The authors refer to Fig. 5 here – why?

This is referring to the figure 5 because in that figure both the TE and the associate statement refers to change in the assemblage composition which can be seen in the figure 5 but not in the figure 4.

Line 288: strong instead of "significant"; are visible instead of "are present".

Accepted.

Line 291: “as testified”.

Accepted

Line 293-294: “high dissolution conditions” due to the absence of calcareous species?

The higher dissolution conditions can be inferred by the increase in the arenaceous species and the loss of calcareous test that has no analogue over the entirety of the record.

Line 295-297: There is a clear shift in the abundance of agglutinated (decrease) versus calcareous species (increase)? If dissolution is responsible for the dominance of agglutinated species between 1.2 and 0.7, then conditions could have changed to a less dissolution environment.

We agree that this could be the case, following the major period of dissolution conditions. However, the low number of tests found over this period makes it difficult to give a reliable interpretation on the assemblage.

Line 298: Environmental conditions are also interpreted in section 3.1. Another header here?

Accepted and removed.

Line 299-319: This subchapter could strongly be condensed. The other proxies/tracers used for comparison can be introduced when they are discussed in the subchapters 3.2.1 to 3.2.6.

Condensed and reduced.

Line 303-308: This paragraph seems a bit displaced here. Could be added to section 3.1?

Removed.

Line 309-313: Partly repetitive. The information for what the BFAR and BFAR can be used, could be provided in the methods section.

Accepted.

Conclusions:

Line 523: “Local level” is correct but then the statement in lines 502-505?

We changed the text to highlight that the local environmental transitions in Edisto is linked to the regional atmospheric and oceanographical changes.

Line 528: “exigua” in italics.

Accepted.

Line 537-538: “could offer key insights” in the future? A next paper about the same core?

The statement is very well supported. While no other studies are scheduled for this sediment cores, other researchers might be interested in high-resolution records and can look at this site as an important location to study interactions between the cryosphere and the biosphere, as highlighted by the RoC paper as well as other studied marine sediment cores on the region.

Figures/ tables:

Figure 2: x-axis: Modelled "age" instead of "date".

The figure 2 was moved to the supplementary material as Figure S2 with the description of the horizon used to date.

Figure 5: Some of the plots have no x-axis numbers. If the scaling is the same for all plots (and it seems so), then this might by okay, but a similar scaling should be mentioned in the figure caption. When looking at Fig. 5, I cannot see larger changes in the assemblages for the 2.5-2.7 kyrs interval? Are less abundant species make the difference?

Added and changed accordingly. The difference is present in the minor components of the fauna. However, this component are still relevant, reflecting the transitions stated in the manuscript.

Figure 6 caption: species names in italics and please add "core" to line 327; "Paleoenvironmental reconstruction" (line 320) – "Proxy records" or something similar would be better? What is the blue interval in the figure – not mentioned in the caption?

The figure 6 was changed and the new figure 6 (Figure 5) is clearer with less climatic phases in accordance with the new discussion sections.