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Reply on RC1

Lea Pesjak et al.

Author comment on "Sea ice and productivity changes over the last glacial cycle in the Adélie Land region, East Antarctica, based on diatom assemblage variability" by Lea Pesjak et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1009-AC1, 2022

Referee 1

General Comments:

I really enjoyed reading this study, which presents a paleoenvironmental interpretation of a sediment core recovered from the well north of the Adelie Land continental shelf, within the seasonal sea ice zone. The authors correctly identify a critical gap in our ability to reconstruct paleoceanographic conditions beyond the last deglaciation around the Antarctic margin, due to glacial advances across the shelf that restrict most sediment records to this limited time frame. This means, that to go farther back in time, yet, be relatively proximal to the continent, we must work on cores from the slope and rise, and then to the proximal deep sea. This study does exactly that, working with a core from farther offshore, on the slope, in a water depth of >3000 m. TAN 1302-44, a 3.5 meter, goes back to MIS6, and allows a reconstruction of glacial, deglacial and interglacial progression at this site, using a multi-proxy data set that relies heavily on the diatom assemblage data, and a chronology that is suggested based mostly on matching the Si/Al ratio to the global benthic d18O stack. Radiocarbon dates near the top of the core are also utilized, but they are limited to < the upper 50 cm. While this reduces the robustness of the age model, I also recognize that this is a problem for so many Southern Ocean cores, with an absence of foraminifera that could be used to develop a stable isotope record and hence, a more robust chronology. Overall, the authors do a very good job interpreting the diatom data, along with other proxies, and they provide a strong evaluation of changes in paleo sea ice extent and paleo-productivity over time. Their interpretation of the diatom assemblages is good, and of course, the statistical approach is appropriate, but here, perhaps add in more species-specific commentary – I suggest this below as well, for example when describing the oceanographic conditions suggested by F. obliquecostata and also for Thalassiothrix. Regardless of the principal components, I always go back to the species data! In summary, a strong paper that I recommend for publication; specific comments and questions are listed below; these are intended to add to the depth of their already strong interpretation.

Answer: Thank you for your kind comments and your work in bringing suggestions to this manuscript.

Specific Comments:

- Use of the Eucampia antarctica terminal valve/intercalary valve ratio is appropriate here, as a way to estimate changes in winter sea ice extent – I suggest a more complete explanation of this ratio (Define it once, and then you can call it the Eucampia index, as done by others), and perhaps an interpretation of this in Figure 3, with an arrow indicating more sea ice to the right, and in Table 2 (what about the ratio – higher or lower? Be specific), line 337.

Answer: Index is introduced and defined in text (line 266); Figure 3 caption, and in Table S1 (Supplement). The index is corrected instead of terminal/intercalary ratio in Table 2, Figure 3 and in Table S3. Higher index is pointed out in Fig 3 with arrow showing more sea ice, and better defined in Table 2, as suggested. Index is also written in abstract (line 23).

- And the authors correctly point out that in some of the intervals, the number of Eucampia counted are simply too low to have a statistically reliable number – in general this might be any time you've counted fewer than 100 specimens that could be identified as either terminal or intercalary, as many times that determination is not possible.

Answer: I agree.

- Also, I wondered which variety of Eucampia was present, var. antarctica or var. recta – or a mixture of the two?

Answer: This distinction wasn't made. It is likely that it is a mixture of the two, but that could also depend on the interval.

- Third paragraph of introduction – perhaps slightly re-frame this to compare the utility and challenges of shelf versus slope/rise versus deep-sea records.

Answer: Re-framed paragraph (line 76-84).

- What kind of core? (piston core?)

Answer: Added: gravity corer with a 2-tonne head (line 105).

- Table 1: In the supplement you explain where the "% microfossil" estimates come from – but since I had a question about this as I read, I suggest that the explanation come in the main text as opposed to the supplement. With the diatom estimates, given that you are working with samples that you sieved, and that you made these estimates on the sand fraction (>63 microns), I am not sure how reliable this number is, even as an estimate. Bottom line, this methodological information should be up front, if you decide to retain the estimates in your paper, since this estimate doesn't include so many diatoms, which are mostly silt-sized. I don't have a recommendation either way.

Answer: Diatom estimates are included in main text now (line 150). I have not deleted them as they strengthen biogenic silica, Si/Al and IRD data.

- Diatom counts: I am very comfortable with the diatom assemblage data, and roughly, but less so, the diatom counts. The diatom counts are useful in terms of evaluating if samples are diatom-rich or very diatom-poor and the bSi data provide a quantitative comparison. But absolute abundance data might be helpful here (or in future work). I

am not really sure why the diatom counts per slide are presented in figure 2 – since this is non-quantitative. The authors state this on line 199 – the qualitative nature of the data. In the future I suggest using a different technique to make quantitative slides, for example, perhaps adopting the method described by Scherer (1994) [Scherer, R. (1994). A new method for the determination of absolute abundance of diatoms and other silt-sized sedimentary particles. Journal of Paleolimnology, 12, 171–179. https://doi.org/10.1007/BF00678093] and revised by Warnock and Scherer (2014)[/ [Warnock, J. P. and R. P. Scherer (2014), A revised method for determining the absolute abundance of diatoms, J. Paleolimnol., doi:10.1007/s10933-014-9808-0.]

Answer: I agree, thank you for the suggestions. I have removed diatom counts per slide in Fig. 2 and Fig 3. Instead, I include IRD counts, as per Referee 2 suggestion. Following this, discussion on diatom count results was removed (line 678-682).

- Chronology – as noted in my comments above, the chronology is based on several radiocarbon dates in the upper 50 cm and comparison of the Si/Al data to the LR04 stack. Given the lack of foraminifera and the limits for radiocarbon dating, I think the authors have done what they can. I wondered if they are able to look carefully at the MIS6/5e boundary to see if samples from MIS6 have any Rouxia leventerae – a good biostratigraphic marker. This may not be possible, given the scarcity of diatoms in the MIS6 section. Any evidence for MIS3, which does show up in Sabrina Slope piston cores (Holder et al., 2020)? Perhaps looking carefully and at higher resolution around 140 cm, where there is an increase in bSi might reveal an indication of MIS3? It's a possibility. Perhaps as well, one deeper radiocarbon date? Also, note that the text, line 169 indicates 2 radiocarbon dates, but figure 3 shows 3 dates, and Table S2 has 4 dates listed. I suggest including the radiocarbon data table in the main paper, not in the supplement.

Answer: *Rouxia leventerae* wasn't identified in any of the slides analysed (this is now added to text; line 223) and all of the slides were carefully analysed, even the barren slides. However, additional analysis of the deeper core, older MIS 6, and MIS 7, may provide some answer to this question in the future.

I agree that a more detailed/ higher resolution analysis of diatom assemblages may help in distinguishing age and paleoenvironments, such as perhaps determining MIS 3. At this stage there is too little evidence for MIS 3, biogenic silica would need to be analysed in higher resolution also.

A deeper radiocarbon date isn't possible because in general the radiocarbon dates become unreliable at depth, in this case beyond 25 cm, which was seen in other 2 sediment cores in the area at similar depths (Pesjak 2022, thesis). The ages suggest a sedimentation rate which is too high. This problematic happens during the glacial as the sedimentation processes involve a lot more terrigenous matter influx, relative to biogenic.

I have brought the radiocarbon table into the manuscript now. I added an explanation in the Age Model section (line 214-217) explaining the reason why the two deeper dates were excluded. Figure 2, and Fig S1 show all 4 dates, as they are in original Table S2. Fig. 3 shows no dates.

- Section 3.2 is overly long and detailed. I suggest paring this section down to highlight specifics that are critical to the interpretation The details can be found in the supplementary material data table.

Answer: This section (now named as Section 3.1) is now rewritten to highlight interpretation as suggested by the referee, and it is also simplified (line 315).

- Perhaps spend a little time discussing the significance of F. obliquecostata as a strong sea ice indicator. I double checked with your counts, and yes, this does dominate, by a long shot. This shows up in what you plot as the Fragilariopsis group, but the dominance of F. obliquecostata is strong evidence for extensive sea ice. See Crosta et al., 2022, for a summary.[Crosta, X., Kohfeld, K. E., Bostock, H. C., Chadwick, M., Du Vivier, A., Esper, O., Etourneau, J., Jones, J., Leventer, A., Müller, J., Rhodes, R. H., Allen, C. S., Ghadi, P., Lamping, N., Lange, C., Lawler, K.-A., Lund, D., Marzocchi, A., Meissner, K. J., Menviel, L., Nair, A., Patterson, M., Pike, J., Prebble, J. G., Riesselman, C., Sadatzki, H., Sime, L. C., Shukla, S. K., Thöle, L., Vorrath, M.-E., Xiao, W., Yang, J., 2022, Antarctic sea ice over the past 130,000 years, Part 1: A review of what proxy records tell us, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2022-99.]

Answer: I agree and have highlighted that it dominates the group in the Results, Section 3.1 (line 370). I have also discussed F. obliquecostata as a strong sea ice indicator and presented the reference as suggested (line 369).

- Figure 3 – I usually prefer greater uniformity in selection of the x-axis scaling. Certainly, a single scale would be inadequate, given the extreme differences in the contribution of different species, but in this figure, every species has its own scale. **Answer:** I agree and have made scale amendments in Fig. 3, to have more uniformity where possible.
- Actinocyclus ingens LAD 0.43-0.5 Ma [Cody, R.D., Levy, R.H., Harwood, D.M. and Sadler, P.M. Thinking outside the zone: High-resolution quantitative diatom biochronology for the Antarctic Neogene. Palaeogeogr. Palaeoclimatol. Palaeoecol. 260, 92–121 (2008)]. Plus, I would classify it as fairly robust (line 451).

Answer: This reference (line 381; line 545)) and description is now added (line 549).

- Lines 470-471 – Thalassiothrix is found in sediment cores from the nearby Sabrina Slope (Holder et al., 2020); I suggest deleting reference to Leventer 1992 – true, about surface sediments, but the Sabrina Slope data are from not so far away. In discussing the habitat for Thalassiothrix, perhaps consider referencing: [P.G. Quilty, K.R. Kerry, and H.J. Marchant, A seasonally recurrent patch of Antarctic planktonic diatoms, Search, pp.48-51, 1985.]

Answer: Holder et al. (2020) do not mention *Thalassiothrix* but *Eucampia antarctica* as a proxy for CDW. I added Quilty et al. 1985 as suggested (line 564).

- Lines 574-575: Perhaps back off this statement; the data are not strong, given the resolution.

Answer: Ok (line 672).

- Lines 587-588: What does the sedimentology / x-radiographs suggest? Any evidence for downslope transport?

Answer: The 340-320 cm interval comprises an increase in m silt to clay fraction. And the X-radiographs show laminae. However, these don't necessarily indicate turbidity currents (Rebesco, M, Hernández-Molina, FJ, Van Rooij, D & Wåhlin, A 2014, 'Contourites and associated sediments controlled by deep-water circulation processes: state-of-the-art and future considerations', *Marine geology*), although these are common sediments on the Antarctic margin (Escutia et al. 2003). There could however additionally be a possibility this interval is a turbidite- due to pyrite present (Presti et al. 2011) - which is mentioned in the section (line 687).

Technical Corrections:

Line 23: Eucampia antarctica terminal/intercalary ratio of what? Low? High? Be specific. **Answer**: 'high' added (line 23).

Line 41: Pritchard. **Answer:** Corrected (line 50; 56).

Line 50: lowering temperatures – how does this impact productivity? I think you mean that it influences the species composition, but the way it's written implies it influences whether productivity is high or low? **Answer**: This is now corrected by deleting 'lowering temperatures' (line 51).

Line 55: decrease in production of AABW – a cause or effect? **Answer:** This has been corrected to be both, ice sheet melt causes AABW decrease, but this in turn can affect ice sheet melt (Silvano et al 2018). Line 56;57.

Line 584: diatom abundance interval instead of diatom interval. **Answer:** This sentence has been erased as per Ref. 2. comment 3. (Line 682).

Line 589: pyrite is found instead of pyrites are found. **Answer:** Corrected (line 688).

Diatom data table, several mis-spellings (Supplement Table S1)

Actinocyclus actinochilus. Corrected.

Coscinodiscus oculoides. Corrected.

Fragilariopsis angulata is now F. rhombica. Corrected.

Fragilariopsis Barbieri. Corrected.

Fragilariopsis pseudonana. Corrected.

Rhizosolenia polydactyla. Corrected.

Rhizosolenia inermis is now Proboscia inermis. Corrected.

Thalassiothrix antarctica. This was written as suggested.

Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-1009/egusphere-2022-1009-AC1-supplement.pdf