EGUsphere, referee comment RC1
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Comment on egusphere-2022-1009

Anonymous Referee #1

Referee 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-RC1, 2022

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.

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

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

What kind of core? (piston core?)

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.

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.]

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.

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.

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.]

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.

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).

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.]

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

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

Technical Corrections:

Line 23: Eucampia antarctica terminal/intercalary ratio of what? Low? High? Be specific.

Line 41: Pritchard

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?

Line 55: decrease in production of AABW – a cause or effect?

Line 584: diatom abundance interval instead of diatom interval

Line 589: pyrite is found instead of pyrites are found

Diatom data table, several mis-spellings

Actinocyclus actinochilus

Coscinodiscus oculoides

Fragilariopsis angulata is now F. rhombica

Fragilariopsis barbieri

Fragilariopsis pseudonana

Rhizosolenia polydactyla

Rhizosolenia inermis is now Proboscia inermis

Thalassiothrix antarctica