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Comment on egusphere-2022-1009

Matthew Chadwick (Referee)

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-RC2, 2022

This article is an interesting and valuable contribution to our understanding of seasonal sea-ice zone dynamics across a full glacial-interglacial cycle. The palaeoenvironmental conditions are reconstructed from a marine sediment core located further south than previous reconstructions of a full glacial-interglacial cycle and thus represents a valauble new data point. The authors use a combination of sedimentological and diatom species assemblage analyses, alongside statistical analysis, to reconstruct the palaeoenvironment of the continental slope region off Adelie Land. This multi-proxy data set is used to investigate the variations in environmental conditions between glacial and interglacial periods, as well as during the glaciation and deglaciation transitions, back to MIS 6.

Overall, the authors do a good job presenting and interpreting the diatom data, and show a good appreciation of the limitations and challenges. Particularly those associated with transport and dissolution of diatoms, and establishing robust chronologies for Southern Ocean marine sediment cores. Whilst I think this manuscript should be published, there are some areas of concern that I would like to see addressed, and think would help strengthen the manuscripts conclusions.

Specific Comments

- How did the authors determine which age model details were presented in the main manuscript and which were only in supplemental? For example, in section 2.5 biogenic silica, Si/Al, and IRD are listed as some of the primary data used in age model construction but only the first two have detailed methodologies in the main manuscript. I appreciate that the authors probably don't want to spend too much of the manuscript detailing all of the sedimentology, but I think the current separation could benefit from reassessment.
- Robust age models for Southern Ocean marine sediment cores located so far south are often challenging and I largely agree with the logic used by the authors for the

chronology in core Tan_44. However, I think the age model would benefit from additional biomarker evidence (e.g., the last occurence of *Rouxia leventerae* at the MIS 6-5e boundary). The authors themselves mention the problems with Antarctic ice sheet advance removing the deposited sediments, and the addition of biomarkers would help establish that the interglacial identified as MIS 5e isn't actually an older interglacial.

- I have a couple of points on the diatom preparation and counts. Firstly, the authors mention that for species that are highly fragmentary, only the ends were counted, was the same process applied to other pennates? Or were they only counted if >50% of the valve was present? If the latter, how did the authors ascertain they had >50% of the valve for broken valves of species such as *Fragilariopsis cylindrus,* which are linear and isopolar? Secondly, the counts are detailed as >400 valves but it is unclear when the count was stopped, did the entire slide need to be counted, or did the count just continue until the 400 point had been passed? Without details on this it is hard to know how to interpret the diatoms per slide values given in Figure 2. Either way, I would still advise removing this metric from figure 2 and the discussion as it is highly qualitative given the method of slide preparation. Thirdly, I am somewhat confused by the criteria used to include or exclude species/groups from the analyses. Lines 202-3 imply that only species with >2% abundance throughout the core are included in the analysis, but figure 3 and the discussion clearly include species for which this isn't the case (e.g. *Actinocyclus ingens*)? For groups, seemingly the dominant species only needs to have >2% abundance in a single sample, which seems rather inconsistent. I would also caution the authors against grouping by morphology, for example within the *Thalassionema* genus there are substantial difference sin environmental preference despite very similar morphologies.
- For section 3.4 the authors argument would be strengthened by the inclusion of some p values to show the statistical significance of the regressions. Especially as, to me at least, the r^2 values seem rather low for all of the regressions.
- The paragraph in lines 408-24 feels rather contradictory. The authors seem to suggest both that there is significant reworking of the diatom assemblage, and that the assemblage is a faithful reconstruction of the overlying environmental conditions. The justification for why the authors consider this assemblage to be truly autochthonous needs to be made clearer. Otherwise the reader is left questioning whether the PC1 assemblage can really be trusted any more than the PC3 for reconstructing environmental conditions.

Technical Corrections

Line 23 - It isn't specified whether it is a high or low *Eucampia* terminal/intercalary ratio associated with PC2.

Line 29 - Should be *oliveriana* not *oliverana* (mispelt throughout manuscript).

Line 130-1 - Are the anomlaous spikes identified by statistical comparison to surrounding data or just by eye?

Line 150 - Core site Tan_68 is shown in Figure 1 but not reference at all in the manuscript.

Line 157-8 - The lines showing the average position of the monthly sea-ice edge are not explained in the figure caption. I assume the lines are sourced from Fetterer et al. (2017) and the blue shading from Spreen, Kaleschke & Heygster (2008) but this also isn't made clear.

Line 165 - There is no explanation in the main manuscript on what the D and R in the %microfossil row stand for.

Line 169 - Only two radiocarbon dates are mentioned but Figure 2 and Table S2 both contain 4.

Line 374 - The PC3 and biogenic silica regression has an $r^2 > 0.1$.

Line 451 - I would consider *A. ingens* to also be fairly robust so don't think the except is necessary.

Line 589 - Should be "pyrite is".

Line 607 - kyrs as one word.