



EGUsphere, referee comment RC2
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Comment on egusphere-2022-804

Anonymous Referee #2

Referee comment on "Deglacial and Holocene sea-ice and climate dynamics in the Bransfield Strait, northern Antarctic Peninsula" by Maria-Elena Vorrath et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-804-RC2>, 2022

Study by Vorrath and colleagues presents a new sedimentary record (PS97/072-1), utilising multiproxy approach to provide sea ice and temperature reconstructions in Bransfield Strait (Antarctic Peninsula) over the past ca14ka. Antarctic Peninsula has experienced some profound climatic changes over the last decades and thus paleoclimatic studies in this important region of the Southern Ocean are of real significance.

While manuscript is presented well and supported with good quality figures, I feel that it requires more clarity, particularly in relation to descriptions of climate variability, throughout the text (e.g. late-Holocene is characterised by stable environmental conditions...), and would benefit from more elaborate discussions which includes cases when e.g. proxies do not agree (see below).

Major comments

Age model – I was wondering if authors could perhaps elaborate on the strength of the age model presented? There are quite large changes in the sedimentation rates, but with no further age points, I was wondering how sure authors can be of a linear nature of the age model between ca 5-12ka and beyond. There is a 10-fold difference between the highest and lowest sedimentation, which surely if present between 5-12ka, would have an impact on the reconstructed ages and overall climatic reconstructions

Climatic reconstructions - There were times where proxies disagreed or more, data did not support authors climatic reconstructions which I think provides a room for further

elaboration. For example,

4.1 – L338-338 – seemingly a difference between sea ice diatoms vs WSI.

4.2 – L361-362 – diverging trends in SOTs vs SSST

4.2 – L357-358 – I don't think this is really supported by the data, particularly with WSI. Sea ice associated diatoms show an increase, at least during the early parts of an Early Holocene. Room for further discussion?

4.4. – L410 – Again, I think there is a room for further discussion. Looking at PIPSO, WSI and temperature records, I don't think data quite support the statement of the stable environmental conditions.

L413 – 414 – "Minimum PIPSO25 values..." – Could authors elaborate on this please. There are low HBI concentrations during ACR yet PIPSO was considered as an indication of the continuous sea ice cover.

Minor edits

L95-96 – "...as reliable proxies..." – this is not such a clear cut as authors state!

L96 – diunsaturated; change to di-unsaturated please

L104 – "...robustly reconstruct" – again, I think this statement needs rephrasing.

L186 - "...slightly overestimated..." – please elaborate by how much

L189 – “..identifications of HBIs..” – and GDGTs?

L190 – internal standards – please state quantities added

L197 – Please, could you state the retention indices for HBIs. Also I might be wrong, but I think only mass spectra of HBI triene are presented in Belt et al 2000.

L212 – please provide details of the n-alkanes standards.

L237 – m/z 1296

L295 – “..and shows high..” – to use elevated instead of high would be more appropriate I think

L304 – “Sea ice concentration estimates..” ?

L324 – significant – is it supported by statistical treatment of data. Perhaps consider using an alternative wording.

L333 – “..a very thick or permanent..” – what does very thick mean? Continuous sea ice cover might be more appropriate?

L433 – we note..

Figures – I think a schematic would be a fitting addition as well.

Figure 4 – I didn't quite get a point of diene/triene plots (d and e). Why not convert the data to either PIPSO (authors have both IPSO25 and triene data)?