Response to the Editor,

Dear Xavier Crosta,

we appreciate your constructive comments and remarks on our original submission. We now provide an annotated textfile of the manuscript which includes the comments and corrections received from the reviewers as well as a “clean” and “reader-friendly” version of the revised manuscript. As requested, we now also provide more detailed information in the supplementary material. Below, please find our responses to your comments.

XC:
The manuscript presents a wealth of proxies dealing with phytoplankton productivity, ocean temperature and sea ice conditions. The new data are compared to similar records from both side of the Antarctic Peninsula. However, all these data are discussed in a very descriptive way (cold, warm, more or less sea ice) and very little information on the potential drivers are provided: insolation (summer vs spring), ice-sheet retreat, regional oceanography (fronts' migrations, upwelling of CDW), winds, etc…. There is almost no explanation about the differences observed between the different proxies (MAT vs PIPSO25; TEX vs RI-OH) in the same core or between the same proxies in the different cores. So I recommend to better use the distribution of the three cores to present a much better assessment of the external forcings and internal drivers that may explain the regional differences.
Author's response: We agree that the previous version of the manuscript did not provide sufficient details on the potential driving mechanisms of the documented sea ice and ocean temperature changes. In the revised manuscript we address these drivers and also discuss potential causes for the discrepancies observed between the individual proxies and the different core sites.

XC: It is not clear why there are two sets of corrections for each 14C dates. The ventilation age of 1200 years represents the total reservoir age calculated locally on living benthic carbonate organisms. It embeds the 400 years global reservoir age and the local deltaR. This correction is commonly used in many Antarctic coastal regions when dealing with carbonate remains. The additional reservoir age correction derived from Butzin's model represents the same metric in a temporal framework. In conclusion, it seems to me that you corrected twice the raw 14C dates by the same metric, which is obviously a mistake. If so, the whole age model must be revised, and the interpretations too. If I am wrong, you must explain why you correct twice you 14C dates and what represents each corrections. Please justify why you used SH20 that is generally adapted to terrestrial sequences?
Author's response: We agree that the consideration of ventilation ages was not made clear in the earlier version of our manuscript. Details on this are now provided in the supplementary material (section 1). Due to the lack of planktic foraminifera, we base the age model of core PS97/072-1 on benthic AMS ^{14}C dates obtained from shell fragments and benthic foraminifers. This is usually done without problems on shallow shelves (< 400 m) where the benthos lives in a mixed layer - or in areas where one already has benthic-planktic age pairs from which one knows the ventilation age and can transfer it for dating at nearby sites. To account for the relatively deep water depth (ca. 2000 m) at core site PS97/072-1, we added a ventilation age of 1200 years to the reservoir age to estimate the additional age of the deep water masses relative to the surface water. This is needed as the model of Butzin et al. (2017) only accounts for a reservoir age of the upper water masses between 0 - 300 m. No close-by benthic-planktic foraminiferal radiocarbon pairs exist to constrain our estimated ventilation age of 1200 years, to the best of our knowledge. However, earlier comparisons of dated benthic calcareous material with AIOM-derived ^{14}C ages, which had been in turn compared to planktic foraminiferal ages in various locations at the Antarctic Peninsula (Domack et al., 2001; Barcena et al., 2006; Heroy et al., 2008), as well as ^{14}C ages of deep-water corals from nearby Drake Passage, independently dated by U/Th (Burke and Robinson, 2012), indicate radiocarbon ventilation ages between ca. 1100-1400 years as acceptable approach. For the calibration of the (reservoir and ventilation age corrected) 14C dates, we used the IntCal20 curve as this calibration does not automatically subtract “time-varying” marine reservoir ages as the new Marine20 curve. This allowed us to substract the reservoir and ventilation ages individually based on our assumptions.

Minor comments

XC: A list of the diatom species included in each group (lines 261-262) would be welcome in the SOM.
Author's response: We now provide such a list in the supplementary material (Section 3).

XC: I would say that IPSO25 decreases continuously until 5.5 ka BP when looking at the smooth curve (line 293).
Author's response: Yes, we corrected this.

XC: Which species are present in the sea-ice group before12.8 ka BP (line 299). As there are only few diatoms, this high % of sea ice diatoms might be related to preservation issues and might be tricky to interpret.
Author's response: The species of the sea ice group are listed in the taxonomic list (Supplementary Material, Section 3). The two samples older than 12.8 ka BP are dominated by *Fragilariopsis curta* (up to 14%) and *Thalassiosira antarctica* (up to 8 %). These species are heavily silicified, which makes them relatively resistant to silica dissolution.

XC: Summer insolation is referred to only once (line 370). So why presenting it in figure 5? Do you think that TEX and RI-OH can be produced during different seasons? If so, present spring insolation and discuss all this more exhaustively.

and

XC: Forcings and drivers must be discussed in more details as done lines 378-381 and elsewhere. For example, there is no explanation for the observed differences between RI-OH and TEX lines 398-402. Only an observation. Either detail forcings/drivers in each chapter (period) or in a separate chapter as the precessional processes you are dealing with might be rather similar throughout the Holocene, though with the addition of the ice-sheet and bipolar seesaw during the late deglacial.

Author's response: We now link the different trends in RI-OH'- and TEX86-based ocean temperatures to summer and spring insolation trends, respectively, and accordingly included the latter in figure 5. We prefer to discuss the paleoclimate events in chronological order and to include the discussions of contrary proxies in these chapters. Considering other published records from Bransfield Strait and the WAP, we now discuss potential drivers of ocean temperature and sea ice changes recorded in the study area. We hereby decided to not focus on the EAP side (e.g. James Ross Island) any more as this area is affected by different forcings than the WAP.

XC: How do you explain still high BSi, TOC and total diatom abundances during the last 1 ka if sea ice was very thick and pervasive (lines 414-418)? Do you think that the slight temperature drop as recorded in the regional marine and ice cores could conduct to a very heavy sea ice cover?
Author's response: We agree with the editor that the high TOC and opal contents as well as diatom abundances speak against a pervasive sea ice cover and changed our interpretation of minimum HBI concentrations. We now relate this drop in HBI concentrations to a shift in the diatom community rather than to an abrupt readvance of an ice cover.

XC: This reference is largely outdated as some records were produced since 1990 (line 420).
Author's response: We changed our discussion regarding the LIA as the age model is not robust enough to reliably confine such a short time interval (see comment below).

XC: Figure 1: I wonder if the CTD plot is useful. Not used in the discussion.
Author's response: The CTD plot helps to illustrate the dominance of cold Weddell Sea Water below 200 m water depth, while the upper 200 m are dominated by warm Bellingshausen Sea Water. The potential impact of BSW intrusions impacting the sea ice conditions at core site PS97/072-1 are now further discussed in the manuscript.

XC: Figure 3-5: Detail what the black curves represent. Repeat in figure 5 what the grey band represent.

Author's response: The black curves display running averages – this is now mentioned in the figure captions.

XC: Table S1: Detail on which type of calcareous material were performed the 14C dates.
Author's response: We used shell fragments and benthic foraminifers. This information is now provided in table S1.

XC: Section S2: How robust is the tie-point? It could be moved upward the MUC by 5 cm. Would it change the interpretation about the LIA?
Author's response: The TOC and biogenic opal data used for identifying tie-points for the correlation of the MUC and the piston core are not very distinct and we decided to change our discussion regarding the LIA as the age model is not robust enough to reliably confine such a short time interval.

XC: Section S3: How do you explain the very low C/N values in the bottom part of the core where BIT values are highest. BIT values suggest high terrigenous input from land despite what is mentioned lines 21-23. Could this affect GDGTs during the 14-12 ka BP period?
Author's response: We now comment on this in the supplementary section 4. Maximum BIT values of up to 0.05 between 13.5 ka and 12.0 ka BP may evidence an additional input of soil-derived GDGTs in response to glacier activity. However, these values are distinctly lower than the 0.3 threshold signaling enhanced input of terrigenous organic matter and since also the C/N values do not point to higher input of terrestrial material during this time interval, we conclude that the GDGT paleothermometry was not affected by soil-derived GDGTs.

XC: Table S5: Please remove the first line with the repetitive core name. This is very confusing. Add the core name in the table caption.
Author's response: As the table contains a lot of information and is very busy, we now provide the link to the online repository (https://doi.pangaea.de/10.1594/PANGAEA.952279) to access and download the PANGAEA-formatted table.

Rebuttal Letter to Referee #1

Dear Referee #1,

we appreciate your constructive comments and remarks on our original submission, which have helped to clarify and improve the manuscript substantially. We now provide an annotated text of the manuscript which includes the comments and corrections received as well as a "clean" and "reader-friendly" version of the revised manuscript. As requested, we now also provide more detailed information in the supplementary material. Below, please find our responses to your comments.

<u>RC1.1:</u> I commend the authors for the considerable time and effort that has been invested to produce these micropalaeontological and biomarker data. The record has the potential to be a valuable addition to the literature on past ocean and climate conditions in the NAP. Unfortunately, this potential is not realised in the current manuscript. While the general structure and style of the text is appropriate, greater attention to detail and a more nuanced consideration of the palaeoceanographic conditions are required throughout the manuscript, especially with regards to the sea ice environment.
<u>Author´s response:</u> We agree with the reviewer that the palaeoceanographic discussion in the previous version of our manuscript was not very detailed and we note that we, in the revised version, pay more attention to the meaning of our own proxy records and other records published in the study area.

<u>RC.1.2 - Study Area:</u> The paragraph on the oceanographic setting mostly describes the sources and distribution of sub-surface water masses which is difficult to follow and too detailed considering the water masses are not mentioned in any other part of the paper except figure 1. There is no mention of the modern sea ice conditions or seasonal variability. Consider whether the Northern Antarctic Peninsula would be a better description of the regional context of this study and be aware that WAP (West Antarctic Peninsula) is currently used to describe: 1) the ocean/seas over the continental shelf west of the AP landmass; 2) the western coast of the AP landmass; and 3) the whole area west of the AP spine (land and ocean). Please provide an accurate description of WAP and/or NAP and use the term consistently throughout the manuscript.
<u>Author´s response:</u> We thank the reviewer for the comment on the geographical definition of the West Antarctic Peninsula and accordingly changed the title and the text of the manuscript. We also revised the description of the study area and now provide information on the modern sea ice conditions.

<u>RC.1.3 - Sea ice:</u> Descriptions of sea ice variability are too vague throughout. Whilst the relative changes in sea ice could appear to make sense in the time slice sections of the discussion, the sequence of Holocene sea ice changes summarised in the conclusions highlights the poor choice of terms used to describe the reconstruction:
• Post ACR: '*retreat in spring sea ice*' - spring sea ice retreats each year, what do you mean here?
• Early Holocene: '*slightly decreasing spring sea ice and highly variable WSI*'
• Mid-Holocene: followed by '*lower spring sea ice… sea ice seasons were short and sea ice cover was significantly reduced to a minimum around 5.5 ka BP, even though high seasonal amplitudes and short-term, centennial changes in sea ice conditions occurred*'.
• Late Holocene: 'variable WSI'
Recurring phases of 'decreasing/lower spring sea ice', 'short sea ice seasons' and 'variable WSI' are difficult to put into context without some qualification. I suggest you add detail to convey the likely duration of sea ice cover (eg. 3-6 months), timing of break up/melt (eg. early/mid/late spring) or use the values of your IPSO/WSI results. Also need to make it clear whether 'more(less) sea ice' relates to duration/extent etc. or use more precise terms.

Author´s response: In the revised version of our manuscript, we tried to be more specific with the description of the sea ice conditions and to eliminate vague wording. In the Discussion chapter, we now provide information on how 'high' and 'low' PIPSO25 values are interpreted in terms of the duration and concentration of sea ice.

RC.1.4 - Diatom groups: Composition of the various ecological groups is not given in either the main manuscript or supplementary files; frequently refer to 'open ocean' group without explanation as to whether it is the 'warm' or 'cold' ocean group or a total or both groups.
Author´s response: Details on the ecological groups can now be found in the supplement. We also clarified that the "open ocean" group refers to either cold open ocean species or warmer open ocean species. We also added the surface temperature range for each ecological group. The main text and figure 3 refer to the warm open ocean species assemblage.

RC1.5 - Literature: Very sparse reference made to other Bransfield Strait/NAP studies. Since your study is based on single site it would be useful to establish whether the climate signals are 'locally' coherent before comparing them with WAIS/EPICA, Palmer Deep etc.
Author´s response: In the revised version of the manuscript, we paid more attention to other records from the NAP and the WAP revealing similarities as well as differences in the local environmental development during the Holocene. Specifically, we now refer to records published by Barcena et al. (1998), Heroy et al. (2008), Milliken et al. (2009), Kyrmanidou et al. (2018) and Roseby et al. (2022).

RC1.6 - Comparison of proxy records: Would be useful for the results or discussion to have a dedicated section comparing the different PS97/72-1 proxy records and providing potential explanations to reconcile results. Especially with regards to:
- different signals/trends between proxies
- reliability and accuracy of proxies at the site
- how amplitude of signals relates to changes in sea-ice/temperature (broadly linear relationships or not? unknown?)

Author´s response: We revised the text and now discuss discrepancies or contrasting trends of the different proxies. Hereby we preferred to discuss the paleoclimate events in chronological order and to include the discussions of contrary proxies in these chapters. Regarding the divergent trends observed in RI-OH' and TEX86L ocean temperatures we argue that RI-OH' seems to be mainly driven by summer insolation, while TEX86 records (Bransfield Strait and Palmer Deep) rather depict the spring insolation trend. On account of the different trends in the sea ice diatom assemblage and WSI, we note that the discrepancy may be due to silica dissolution. Many of the species that we use as sea ice indicators, such as F. curta, can dominate the marginal ice zone adjacent to a retreating ice edge. They seem to do equally well in the sea ice and then are successful in seeding the water column, as contrasted with sea ice obligate taxa.

RC1.7 - COMMENTS & SUGGESTIONS:
See annotated PDFs for specific comments and suggestions on the text, figures and supplementary information.
Author´s response: We wish to thank the reviewer for the very detailed and thorough comments provided in the annotated PDFs. They have been considered in the revised version of the manuscript.

RC1.8 - MINOR COMMENTS: Disordered use and introduction of abbreviations; Inconsistent use of sea-ice/sea ice; Inconsistent use of spacing between signs/units and numbers eg. < 3% and <3 %; Check whether citations should be listed in date order
Author´s response: We corrected all editing inconsistencies/errors.

Rebuttal Letter to Referee #2

Dear Referee #2,

we appreciate your constructive comments and remarks on our original submission, which have helped to clarify and improve the manuscript. We now provide an annotated text of the manuscript which includes the comments and corrections received as well as a "clean" and "reader-friendly" version of the revised manuscript. As requested, we now also provide more detailed information in the supplementary material. Below, please find our responses to your comments.

RC2.1: 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 a characterised by stable environmental conditions…), and would benefit from more elaborate discussions which includes cases when e.g. proxies do not agree (see below).
Author´s response: We revised the text and the wording to describe the environmental conditions in more detail. We now also discuss discrepancies between the applied proxies. Regarding the divergent trends observed in RI-OH' and TEX86L ocean temperatures we argue that RI-OH' seems to be mainly driven by summer insolation, while TEX86 records (Bransfield Strait and Palmer Deep) rather depict the spring insolation trend.

Major comments
RC2.2 - Age model: I was wandering 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 wandering 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.
Author´s response: We agree with the reviewer and point out in the text "that (as) the lack of age constraints between 12 ka and 6 ka BP may introduce chronological uncertainties, we only focus on overall trends reflected in our data and refrain from detailed allocations of known climatic events in this time period." The overall climatic trends depicted in core PS97/072, however, are in general agreement with other records from the study area.

RC2.3 - 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?
Author´s response: We revised the text and now discuss discrepancies and contrasting trends of the different proxies.

RC2.4 - 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.
Author´s response: We revised also this part of the manuscript and provide a more detailed assessment of the sea ice and ocean temperature conditions.

RC2.5 - 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.
Author´s response: We thank the reviewer for spotting this. We now conclude that the low HBI concentrations seem to refer to a reduction in spring sea ice – also since elevated TOC, biogenic opal and warm open ocean diatoms suggest higher primary productivity. In general, in the revised version of our manuscript, we tried to be more specific with the description of the sea ice conditions. In the Methods chapter, we now also provide information on how 'high' and 'low' PIPSO25 values are interpreted in terms of the duration and concentration of sea ice.

RC2.6: 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 quantifies 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..
Author´s response: We considered and addressed your comments in the revised manuscript.

RC2.8 - 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)?
Author´s response: We agree with the referee and accordingly changed the plot now showing PIPSO25 values for JPC10.