Response to Referee #2

We sincerely thank referee #2 for your insight and constructive comments and recommendations on our original submission. All comments have been carefully reviewed, and integrated into the revision to enhance clarity and refinement of the manuscript. Below, please find our responses (in red; *revisions already made in the manuscript are shown in italics*), to your comments.

RC2.1.1 General comments: One of my biggest concerns is, that Khoo et al. focus in their Introduction on the active role of sea ice in the climate system. Further, they mention to identify potential tipping points in the ice-ocean-atmosphere system by reconstructing past sea-ice changes. In the Discussion, the authors exclusively discuss the reaction of sea ice to meltwater or solar forcing. Instead of discussing the active role of sea ice in the climate system and potential tipping points, they discuss the forcing mechanisms on sea ice. Which is, nevertheless, extremely important to understand.
In the Abstract, sea ice-glacier interactions are put into focus, which is only shortly mentioned in the Introduction. In the Discussion this process is only mentioned in Chapter 5.2. I have the feeling the Authors could be more precise here and try to set the focus of this study more clearly.

Author's response: Thank you for your comment. Our intent of the first paragraph in the Introduction is to emphasize the importance of studying (past) sea ice variability, hence we will retain the impact of sea ice on the climate system and glacier-sea ice interactions. However, we will remove "identifying potential tipping points in the ice-ocean-atmosphere system" to avoid confusion. Regarding the comment on sea ice-glacier interactions, we refer to the response provided for RC2.3.1 in the Specific comments below.

RC2.1.2 General comments: I noticed, that a lot of abbreviations are used, which often are not necessary because terms are not used regularly throughout the manuscript. This makes the manuscript hard to read: e.g. HSSW only used 3 times, SOM only used once, HASO also only used once. I would recommend to only use abbreviation if a term is used more than 3 times.

Author's response: Thank you for your suggestions. We will update the revised manuscript accordingly.

Specific comments

RC2.2 Abstract:

- A lot of detail is given on the used proxies, the model study, however, is not mentioned.
- The general outcome of the study is very short. I would appreciate a bit more detail.

Author's response: We agree with the reviewer, and will add the use of the numerical model as well as expand on the outcome of this study to the revised abstract.

RC2.3.1 Introduction: In the Abstract you put the focus on sea ice-glacier interactions, which is also discussed later in the Discussion. However, in the Introduction, this is only mentioned in one sentence, and the focus is laid on the feedback mechanisms of sea ice on solar radiation and ocean circulation. More information on the glacier-sea ice interaction specifically for Antarctica would be nice.

Author's response: We agree with the reviewer and provided examples of ice shelf-sea ice studies in Antarctica to the revised manuscript. The following has been added to the revised manuscript:

"Sea ice also serves as a crucial buttressing force at the ice front, effectively preventing or delaying the occurrence of potential calving events (Robel, 2017). This phenomenon was evident at locations such as the Mertz Glacier Tongue (Massom et al., 2015) and the Totten Ice Shelf (Greene et al., 2018) in East Antarctica. Furthermore, the presence of a sea-ice buffer in front of the ice terminus acts to diminish ocean swells as they propagate towards land. For instance, Massom et al. (2018) observed a substantial increase (orders of magnitude) in wave energy experienced at the fronts of the Larsen ice shelves and the Wilkins Ice Shelf when the sea-ice buffer was removed."

RC2.3.2 Introduction (L48-54): Jumping between proxy archives here, which is very confusing. Please separate sedimentary and glacial proxies.

Author's response: We acknowledge the recommendation and incorporated the changes into the revised manuscript:

"Presently, numerous methods are used to reconstruct past sea-ice conditions, including biogenic proxies (e.g., biomarkers, diatoms, dinoflagellate cysts, foraminifera and ostracods) and sedimentological proxies (e.g. ice-rafted debris) in marine sediments, as well as chemical compounds archived in ice cores (e.g., methanesulfonic acid and sea-salt (ssNa+); de Vernal et al., 2013 and references therein)."

RC2.3.3 Introduction (L56-64): I agree with your statement, that the number of LIG sea ice reconstructions are limited, However, in your text you mention 184 studies in sea ice in Antarctica summarized in Crosta et al., (2022). This is in strong contrast to the general phasing you use, as reconstructions being "limited" and scares". Hence, I would recommend to change the wording in L56 and L64, to point out that biomarker studies (with their advantages over other proxies) are few in Antarctica.

Author's response: We agree with the reviewer that the number '184' is considered strong. However, the intent of this statement(s) is to highlight the disparity in number of (paleo)records between (a) different regions in the SO (i.e. opal belt (40 – 60ºS) vs. south of 60 ºS), (b) different timescales (Holocene vs. LGM vs. LIG and older), and (c) lack of records from past warmer periods (i.e. interglacials). Rather than the number of diatoms vs. biomarker studies in the SO. Hence, we will maintain the original statement, with some revision for clarity:

"The compilation documents 20 studies on sea-ice variability during the Holocene (0-12 ka before present (BP)), 150 records detailing changes at the Last Glacial Maximum (LGM; ca. 21 ka BP or Marine Isotope Stage (MIS) 2), and a mere 14 sea-ice records dating back to around 130 ka BP. Notably, just two records extend beyond MIS 6 (ca. 191 ka BP; see also Fig. 3 in Crosta et al., 2022). Their work underscores the pronounced dearth of (paleo) sea-ice reconstructions, particularly in regions south of 60°S, notably in the Atlantic sector, and during the Last Interglacial (LIG) and beyond. This scarcity of records, in particular proximal to the continental margin, is attributable to difficulties in recovering marine sediment cores in the polar regions that at present are still subject to heavy year-round ice cover, and a lack of continuous sedimentary records due to erosion and disturbance at the sea floor during past glaciations."

RC2.3.4 Introduction (L117-125): I would recommend to formulate the research question you aim to answer more clearly. Here you mention to close a knowledge gap, which I feel is not sufficient enough and not doing right by the relevance of your study.

Author's response: Thank you for the comment. We agree that the term "fill this gap" may not be the right term to use in this case. We will therefore replace 'fill this gap' with 'aim' to better express our intent of this study.

RC2.4 Results (L320): input instead of inputs

Author's response: Thank you for your comment. We corrected this.

RC2.5.1 Discussion (L525-528): Could you elaborate more on the lack of SSST and OT reduction at your core site in Powell basin. How do you explain this while associating it with increased meltwater inflow from the Antarctic Ice Sheet?

Author's response: Thank you for the comment. We will add the following statement to address the concern:

"In the Powell Basin, however, this cooling event is not reflected in ocean temperature (Fig. 4g) and we propose that the lack of temperature change during this event may be attributed to the discharge of meltwater from expanding sub-ice shelf cavities, which caused a stronger stratification and an effective isolation of the warmer subsurface layer."

RC2.5.2 Discussion (L539-545): How do you explain the strong seasonality in sea ice concentrations in Powell Basin?

Author's response: Thank you for the comment. As presence of WSI is also indicated in marine core PS2305-6 (Bianchi and Gersonde, 2002; Gersonde and Zielinski, 2000), located slightly north of our

core site, we propose that ice-sheet derived meltwater may have acted as a driving mechanism in promoting sea ice formation in Powell Basin during winter. We will incorporate this discussion in the revised manuscript.

RC2.5.3 Discussion (L657-702): I appreciate the acknowledgement of the large age uncertainties for the Holocene, however, the low data availability in your record should also be acknowledged. The interpretation of warm/cold or more/less sea ice phases of the Holocene is based on one data point only. I am not sure if it is wise to interpret these small-scale Holocene changes in your record. I would rather focus on the general glacial-interglacial trends, which is the focus of your study and the strength of your records. At least be more careful in the Holocene section of your Discussion.

Author's response: We agree with the reviewer that the data used in the interpretation of the climate variability of the Holocene is limited. However, we believe that, with the analysis of multiple proxy data, the interpreted climate variability (i.e. warm/cold intervals) remains valid. Nevertheless, we have revised the text to point out the low data availability for the interpretation of the Holocene climate changes.

"We acknowledge that the age constraints and data availability of core PS118_63-1 for the Holocene is limited and exercise caution on the interpretation of the Holocene proxy records. Nevertheless, our data still permit the discrimination of Holocene warming and cooling trends."

RC2.6 Figures:

Fig 1

- The insert map should at least include an overview circulation and regional names, e.g. Scotia Sea, otherwise it is hard to follow Chapter 2. Maybe a map showing the Atlantic Sector with regional names, currents, etc. would be more sufficient.
- The dashed light blue line (summer sea ice extent) is barely visible

Fig 4

- This figure is the key figure of the manuscript, but hard to decipher as it holds a lot of records and data. Please add numbers, letters, etc. to refer to the single plots in the figure captions. I see that the authors try to establish a color-coding distinguishing between different core locations (PS62/219-1 always in orange-brown colors). Maybe this could be done better. Further I am not sure if plotting the diatom species cugr. And F. obli on the same axis. Variations of F. obli are hardly visible.
- OTRI-OH' plot: Please indicate in captions what the light blue and dark blue (running average?) lines represent.

Fig 5

- The brown star (PS62/219-1) is hard to see with dark blue background. The red line (15% sea ice coverage) is hardly visible at all.

Fig 6

- I understand SSSTdiatom for PS118_63-1 is not available for the PGM and LGM, it is irritating to have an 'empty' graph. I would suggest to add an 'n.a.' onto the graph where data is not available
- The SST scale could be adjusted, as the largest change occurs within the SST range of -2 – 14 °C, if you adjust the SST scale the critical changes would stick out more.

Fig 7

- Here the OT scale should also be adjusted, shown OT stop around 15-16°C but the scale goes up to 21°C
- What does the dark blue line indicate? Running average of OT?

Author's response: We thank the reviewer for the detailed review to improve the figures. We will incorporate all suggested changes in the revised manuscript.

RC2.7.1 Supplementary Material (Fig S1):

- Could you please give more detail on the comparability of XRF Ti counts and EDML - d18
- I do not understand how you choose peaks for calibration in both records, and why you excluded two of them.

- Why do you use Ti counts alone? A more sophisticated approach would be to use element ratios? (Hennekam & deLange, 2012)

Hennekam, R., deLange, G. (2012). X-ray fluorescence core scanning of wet marine sediments: methods to improve quality and reproducibility of high-resolution paleoenvironmental records. Limnology and Oceanography: Methods 10

Author's response: Thank you for your comment. We will make adjustments to Supplement S1 to provide additional details on the selection of tie points used in the final age model. Pertaining to the use of Ti counts alone, we recognize that utilizing XRF-element ratios could offer a more nuanced analysis. However, in our case, we believe that the use of Ti counts alone is sufficient to capture the terrigenous signal in our study. Moreover, we integrated other proxy records such as TOC, MS and wet bulk density for a comprehensive age-depth comparison with the EDML δ18O record. This information will be included in the revised manuscript. See also similar comment from reviewer #1 RC1.1.

RC2.7.2 Supplementary Material (Fig S2b):

- I find it hard to see a correlation pattern here. There should be more information on how the peaks where chosen (or not) for calibration).

Author's response: Thank you for your comment. We acknowledge that there appears to be minimal correlation evident in this XRF-Fe pairing. This is why a significant portion of the tie points associated with this pairing were eventually rejected. Nonetheless, we did not completely dismiss this pairing as we aim to establish a robust selection of tie points. During the analysis, we compared multiple records (from the same core) simultaneously to ensure that the age-depth ranges for the tie points do not differ too much between each record. More details on the selection/rejection of the tiepoints will be added to the revised manuscript to provide clarity (refer also to above response for RC2.7.1).

RC2.7.3 Supplementary Material (Table S1):

- What reservoir correction did you use?

Author's response: Thank you for your comment. The reservoir ages for the two radiocarbon dates are approximately 2.2 kyrs. They were derived using the PaleoDataView software and will be included in Supplementary Table S1.