Reviewer #1

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The manuscript by Stevenson and co-authors uses XRF and geochemical data in a snow petrel stomach oil deposit to reconstruct summer sea-ice dynamics in the eastern Weddell Sea over the Holocene. The investigation of this novel type of archive provides complementary information to ice and marine cores and, here, new information from an under-sampled area. The present study is, therefore, timely and of great interest to the paleo-community and beyond.

Thank you for your positive comments, especially relating to the research topic and the data we present.

The manuscript is written in a complicated way, with much diluting information, and the structure is not always sensible. I started listing the comments below, but it was taking too long. I, therefore, present the most important ones only and direct the authors to the annotated manuscript for additional ones.

This general comment helpful and by addressing the specific comments below together with streamlining text in the manuscript, we plan to improve the clarity of the manuscript. We believe some of this impression can be a result of the complexity in combining such a large amount of multi-proxy data, which we hope to improve in the revision.

Sea-ice extent vs sea-ice duration: There is constant confusion between sea ice expansion (winter sea ice edge farther to the north), which you can infer from ice core data or a transect of marine cores, and sea ice conditions over the continental shelf. For example, Lines 342-343 the cooling of surface and freshening of shelf surface waters since the middle-Holocene (Ashley et al., 2021) conducted to increase sea-ice concentration and sea-ice duration on the continental shelf (Crosta et al., 2008; Mezgec et al., 2017; Johson et al., 2021), but did not result in greater sea-ice expansion that conversely recessed in the open ocean (Nielsen et al., 2004; Xiao et al., 2016). This dichotomic behaviour is possibly related to the latitudinal insolation and thermal gradients (Denis et al., 2010) as well as the multi-centennial expression of climate modes (Crosta et al., 2020).

We thank the reviewer for clarifying the difference between sea ice expansion and sea ice conditions over the continental shelf. In this instance, at lines 342-343 we propose to add in the sentences above proposed by reviewer #1. Additionally, we will check the manuscript for other instances where sea-ice expansion may have been confused with sea ice conditions over the continental shelf and make corrections accordingly.

Regional settings: Section 2.1 does not contain a regional context despite the title. I would recommend to detail the regional oceanographic and cryospheric system that will help understand the interpretations thereafter. In this optic, the seasonal sea-ice cycle must be described to know the mean location of the modern sea-ice edge each month as a basis of past changes inferred from the stomach oil deposit analyses. Visualising the seasonal cycle in the northeastern Weddell Sea would also allow to better understanding of the spatio-temporal foraging behaviour of the snow petrel and clarify whether the Maud Rise polynya could have been a foraging zone (section 4.5) despite being very remote (Figure 1).

The regional context part of the subtitle (2.1) referred to the map figure and caption. However, we do agree that adding detail on the regional oceanographic and cryospheric system and snow petrel foraging behaviour would be helpful and plan to do this in the revision. In terms of snow petrel foraging behaviour and whether the Maud Rise could have been a foraging zone, there is evidence that it can lie within foraging ranges of snow petrels in this region today. Modern GPS tracking of snow petrels from their nest site and out into the open ocean show that these birds can routinely travel >700 km depending on which part of the breeding cycle is being recorded (Wakefield et al., [in review - preprint] *Movement Ecology*). As a result of that analysis, we inferred that much stomach oil is deposited early in the breeding season when birds are defending their nests from other snow petrels. Based on travelling distances, during the pre-laying exodus and incubation phase snow petrels could certainly reach the Maud Rise (Wakefield et al., [in review - preprint] Movement Ecology) although we have not observed a preference for foraging in that area. Specifically, the Maud Rise is used, but not prioritised by birds from Svarthamaren and Utsteinen (the locations from which monitoring was carried out).

A stronger signal has been observed of the birds tracking the MIZ (marginal ice zone) where access to broken sea ice and rich feeding grounds is most likely. From Heimefrontfjella we posit that there are three options for the birds: (1) snow petrels head north to the MIZ associated with the outer ice edge; (2) snow petrels head north east to the Maud Rise where melt/intermediate sea ice is earlier; (3) snow petrels forage in the MIZ near the coast/around coastal polynyas. The most likely scenarios in the early breeding season are that birds forage around coastal polynyas (scenario 3) but also travel to the outer ice edge or Maud Rise (scenarios 1 and 2) where necessary (e.g. due to extensive ice cover at the coast, or richer feeding grounds further offshore). Adding this detail to section 2.1 will help set the context and will be a great improvement.

Age Model: All published Bayesian age models have the mean age (red curve in figure 2) that follows more or less the 14C dates and their envelopes (blue ellipses). Here, it is out of the ellipses by a few hundred years and seemingly follows very thin darkish "lines" scattered throughout the record. Why and what are these thin lines? It is not even clear what represents the blue ellipses. For example, the three at ~5cm are centered at 4200 years BP, while the raw ages are ~5000 years BP, and the calibrated median is ~4500 years BP (Table 1). Finally, the caption of Figure 2 must be detailed for the red lines and its envelope, the blue ellipses, etc.

The current age-depth model was developed in Bacon in R using gamma Bayesian techniques. The aim of this approach was to ensure that the accumulation rate and its evolution, as well as the general curve trajectory of the model, was taken into account in addition to the mean calibrated age uncertainty. This resulted in the 'best' model for the Bayesian model lying towards the older ages within the calibrated age estimate, especially for the dates older than 4 ka BP. The theoretical advantage of this approach is that the best fit line is not forced through the mean of the calibrated age range, as this can be inappropriate due to calibrated ages having a probability distribution which does not necessarily lie on the mean value (Blaauw and Christen, 2011; Lacourse and Gajewski, 2020; Trachsel and Telford, 2017). This Bacon model can be theoretically improved thorough comparison with neighbouring ages and the overall model trajectory. However, the disadvantage is that changes in accumulation rate can have a disproportionate impact on the age model which may be the case here.

In the original age-depth model the transparent blue ellipses are the calibrated ^{14}C age ranges, while the grey stippled lines are the 95% confidence interval of the overall Bayesian model. The thin red line in figure 2 is the mean age of the model.

We have now investigated changing model priors in R Bacon (e.g. 'd.by', 'thick', 'acc.mean' and 'acc.shape') to try to develop a tighter model, reducing the effect of the change in sedimentation rate. However, unfortunately each attempt resulted in either the identification of the uppermost or lowermost date as an outlier, or the median line continued to be outside of the majority of the age distribution closer to the 95% likelihood, rather than the 68% distribution represented by the wider parts of the blue elipses (not the tails). Examples are provided in Fig. 1 – 3.

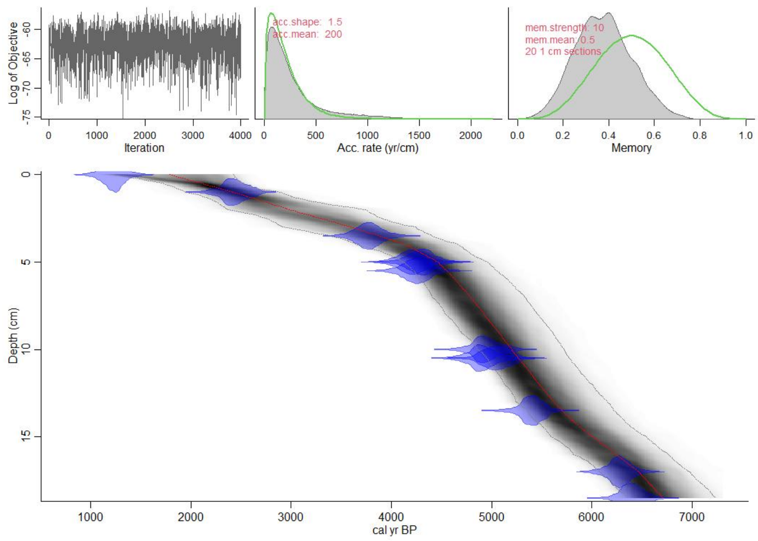

Fig.1 Rejected alternative model from R Bacon [code: Bacon('3012MUM2', d.by = 0.05, thick = 10, acc.mean = 200, acc.shape = 1.5, d.min = 0, d.max = 18.5, rotate.axes = TRUE, title="")]. *Reasoning: uppermost date is rejected from the model (close to 95% age range).*

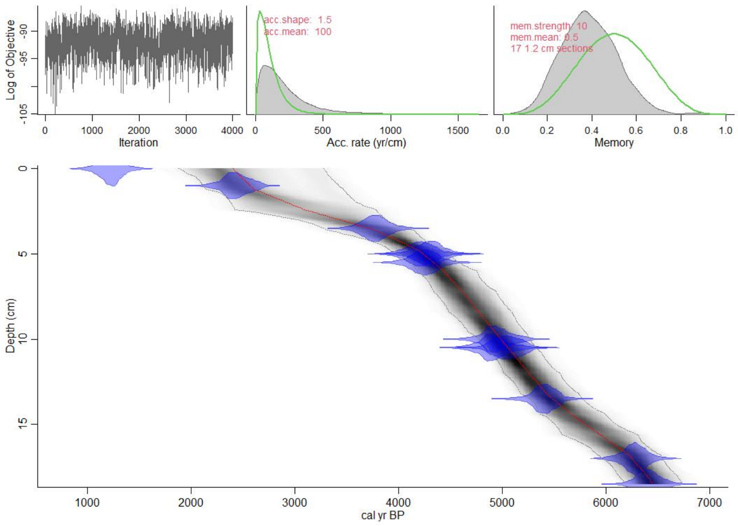

Fig. 2 Rejected alternative model from R Bacon [code: Bacon('3012MUM2', d.by = 0.05, thick = 1.2, acc.mean = 100, d.min = 0, d.max = 18.5, rotate.axes = TRUE, title="")]. *Reasoning: uppermost date is significantly rejected from the model (close to 95% age range).*

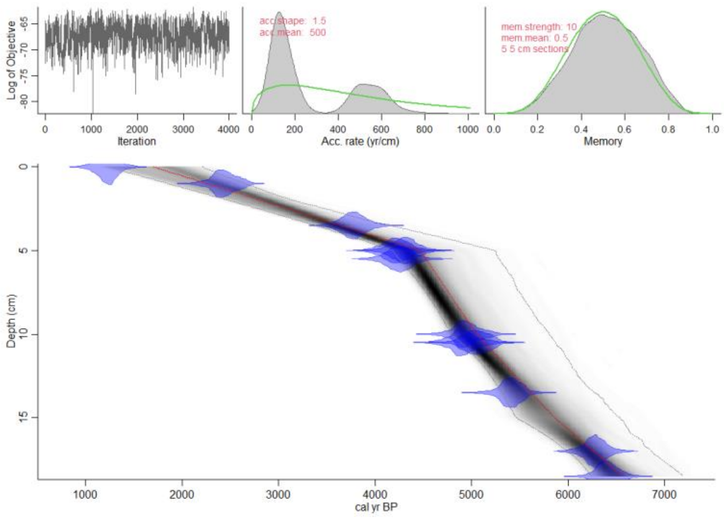

Fig. 3 Rejected alternative model from R Bacon. [code: Bacon('3012MUM2', d.by = 0.05, thick = 1.2, acc.mean = 100, d.min = 0, d.max = 18.5, rotate.axes = TRUE, title="")]. *Reasoning: uppermost date is significantly rejected from the model (close to 95% age range) and model features a pronounced shift in accumulation rate trajectory at ~5 cm.*

We have subsequently explored different Bayesian modelling techniques and have found that when we use a P_sequence (Poisson distribution) model in OxCal using the default settings (applied with a general outlier model, except for paired dates where we used SSimple outlier model) we can encompass all dates within the main age distributions (Fig. 4). This is likely because the age model is tied to pass through the median age for each age distribution, reducing the effect of changes in sedimentation rate, while continuing to 'smooth' the reconstruction between ^{14}C ages.

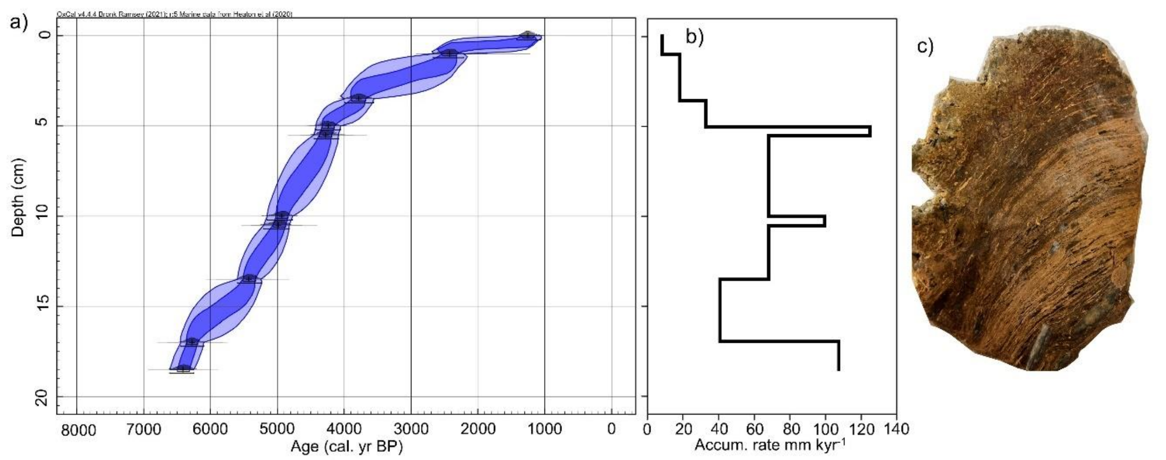

Fig. 4. a) Revised age-depth model developed in OxCal proposed to replace the existing Fig.2 in the main text of the manuscript. b) accumulation rate between age control points calculated from median ages in the Bayesian model in (a). c) photograph of deposit 3012MUM2. Note the increases in accumulation rate ~5 and 10 cm depth are associated with close sampling of radiocarbon ages at these points and coincident short-term shift in the rate.

We propose to use this new revised model in the manuscript and update stratigraphic diagrams accordingly. The model slightly decreases the oldest ages, meaning that our reconstruction now spans 1833- 6389 cal. yr BP, rather than the previous 1983-6724 cal. yr BP. This is due to OxCal passing the lowermost age squarely through the median age, rather than the accumulation rate changing the trajectory. The impact on our stratigraphic diagrams will be relatively minor and will be updated in the revision (Fig.5).

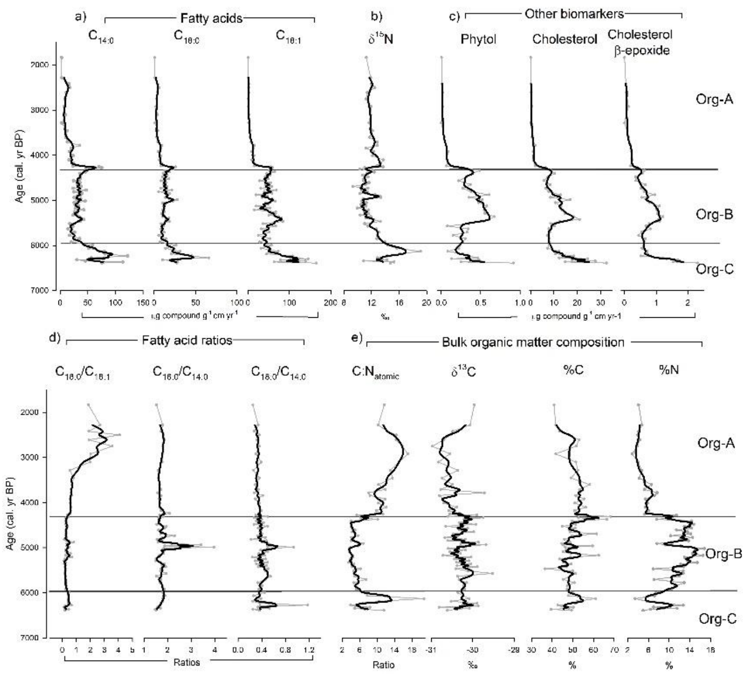

Fig. 5. Revised Fig.3 with new age-depth model applied to the stratigraphic diagrams and flux calculations from the revised accumulation rates. Trends and pattern remain broadly similar, despite slight changes to the model. The nature of the new model is that there is a slight pulse in many biomarker

fluxes ~4350 cal. yr BP in line with sedimentation rate changes at that point (see Fig. 4). Overall, broad trends are not affected by the model change.

Statistical analyses: The input data for the PCA must be detailed in section 2.7. From Supplementary Figures 9-10, I understood that the input data are log10 transformed and centred datasets (not said), but the data presented in Figures 3-4 are raw data (cps for XRF data) normalised to accumulation rates. This explains why there is little resemblance between the PCs and the XRF data that make them (PC2 and Cl and S, for example). Overall, this is very confusing and must be better explained. Overall, I question the utility of the PCA as downcore PCs are hardly interpreted.

Thank you for the suggestion, we do agree it is a good idea to add this. The input data for the PCA is presently detailed in the supplements (SI Table 1 & 2) but could certainly be added to section 2.7. Reviewer #1 is correct that the input data for the PCA is log10 transformed and centred. We have experimented with and without the log10 transformation and found both PCAs to be broadly similar (Fig. 6).

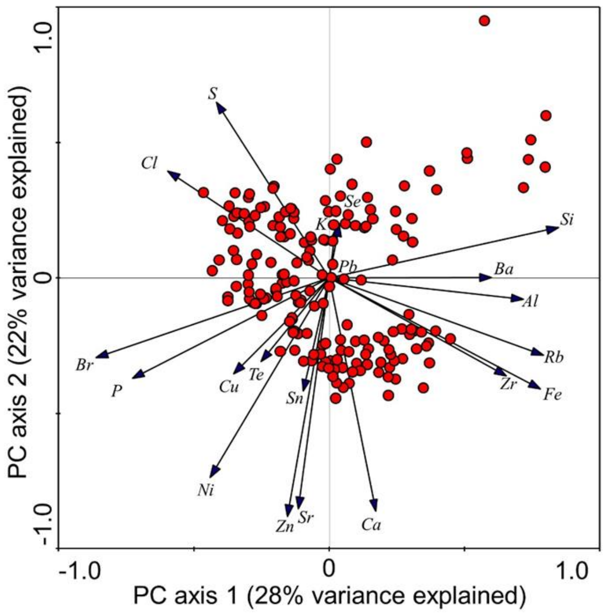

Fig. 6. Principal components analysis (PC) biplot of inorganic parameters (Se, Pb, Si, Ba, Al, Rb, Zr, Fe, Ca, Sr, Zn, Sn, Ni, Te, Cu, P, Br) based on log^{10} transformed and centred datasets (Existing SI Fig. 10).

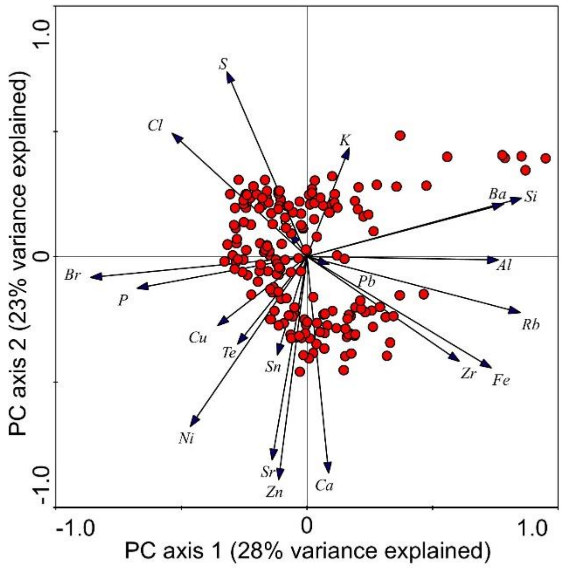

Fig. 7. Principal components analysis (PC) biplot of inorganic parameters (Se, Pb, Si, Ba, Al, Rb, Zr, Fe, Ca, Sr, Zn, Sn, Ni, Te, Cu, P, Br) based centred datasets, without any log10 transformation.

In figure 3 biomarker data is normalised to accumulation rates and in figure 4 XRF data is also normalised to accumulation rates. However, PCs are not normalised to accumulation rates.

Indeed, the downcore PCs in Fig. 4 are hard to interpret as they integrate changes in many elements. We suggest to remove downcore PCs from Figure 4 because they are only commented on slightly, but retain as noted in SI Fig. 2 and SI Fig 5 which is more appropriate as they are adjacent to concentration data.

We plan to make a comment that the PCs have a relatively low explanation of the variations. For example, the organic PC biplot confirms only ~54% of the variance is explained and the inorganic PC biplot has only ~50% of variation explained (SI Fig. 10).

Structure: The Results present many interpretations, which complicate the reading. For example, lines 217-218 and 220-221 refer to previous publications; lines 227-229 and 310-320 identify the meaning of the proxies. The former type of interpretation can be sprinkled throughout the Discussion, while the former can be gathered in a section dedicated to the use of the proxies.

There are also many typos and non-consistent use of sea ice vs sea-ice.

The information provided in the results section (lines 217-218; 220-221; 227-229 and 310-320) was intended to provide context of the wider studies, uniqueness of the deposits and highlight the source of the elements. Although we acknowledge this information goes beyond listing results initially, we did not want to put this information in the discussion to distract from the higher impact findings and at the time felt that a section in the discussion dedicated to the use of the proxies might distract from the flow. However, despite these concerns, to increase clarity we suggest we could add in a new section at the start of the discussion on proxy interpretations.

The typos have been very kindly identified by reviewer #1 in the edited PDF and will be corrected if the manuscript is selected for revision. We attempted to use the hyphenated 'sea-ice' and non-hyphenated 'sea ice' depending on circumstances according standard English. 'Sea ice' is hyphenated when used as a compound modifier before a noun (e.g. sea-ice extent, sea-ice configurations, sea-ice concentration, sea-ice scenario, sea-ice edge, sea-ice record, sea-ice cover, sea-ice dwelling and sea-ice dominated). 'Sea ice' is not hyphenated for all other instances (e.g. the extent of the sea ice).

Interpretations: (1) It seems to me that most of the interpretations are indicative of environmental conditions in the central Weddell Sea, where summer sea ice is present today but represents a fifth of the foraging area. I am less convinced it is valid for the northeastern Weddell Sea and Lazarev Sea, where the continental shelf is very narrow.

Recent findings from the wider ANTSIE team who have tracked the movement of modern snow petrels suggest that these birds will tend to track the MIZ where access to broken sea ice and rich feeding grounds is most likely (Wakefield et al., [in review - preprint] *Movement Ecology*). Although no birds were tracked from Heimefrontfjella, the nearest tracking location at Svarthamaren ((71°53.4'S, 005°09.6'E) identified snow petrels tracking the MIZ into the central Weddell Sea. Coastal foraging was also observed (Wakefield et al., [in review - preprint] Movement Ecology) in areas which also have a narrow continental shelf (Honan et al. 2025), and seems more dependent on the presence of sea ice. We suggest to add to the discussion section the importance of the snow petrels tracking the modern position of the MIZ to better foreground the discussion, adjusting interpretations accordingly.

(2) I sometimes did not get the reasoning by which foraging in coastal polynya is inferred (lines 353-355), which is not supported by the data (lines 360-363). Overall, it seems to me that authors infer either an open ocean or coastal polynya foraging, while snow petrels may have foraged at the marginal ice zone as it recessed from offshore to the coast over the feeding season. Marine cores show open ocean conditions during summer off northern Ross Sea, Wilkes Land, Prydz Bay, Western Antarctic Peninsula, etc. I do not understand why the authors here suggest the presence of summer sea ice off the northeastern Weddell Sea (Figure 5).

This comment is very helpful and after considering Antarctic Holocene scale climate change through marine proxies referred to in Crosta *et al.* (2022) in more detail, we do agree that foraging at the marginal ice zone is on balance a more likely scenario. The previous interpretation (polynyas) did not fully take into account that there were open conditions elsewhere in Antarctica and this relied on much more extensive sea ice to be present in the Weddell Sea which is unlikely. Our tracking data shows that snow petrels are very closely linked to the position of the MIZ and this is probably the main source of feeding, with ecologically the birds able to reach the Maud Rise intermittently during the pre-breeding and pre-laying exodus (Wakefield et al., [in review - preprint] *Movement Ecology*). We therefore plan to adjust the interpretation of high levels of $C_{18:0}$ and $C_{14:0}$ (lines 353-355), inferring productivity to take into account open water conditions in the Weddell Sea throughout much of the Holocene and increase the importance of the MIZ in our discussion.

(3) Some data are over-interpreted. For example, it is said that the cholesterol is higher in Org-C zone, which is not true in Figure 3 and 6. Authors infer feeding in coastal polynya (lines 425-426), which is contradictory with krill feeding as these organisms need a deep water column for their biological cycle.

We will reduce data over-interpretations in the revision. Thank you for identifying the mistake in line 421. Cholesterol is high in Org-C zone but not as high as the peak in zone B. In response to the comments above we plan to adjust the text in this part of the manuscript (section 4.4) to account more for the position of the MIZ, with the role of high productivity polynyas being a potential minor secondary contributor to the high productivity observed, or depending on the magnitude not at all. From our discussions we also note that it is possible for there to be high productivity early in the season as the sea ice melts, in this part of Antarctica, something that we plan to mention in the revision as it can highlight the position of the MIZ (Wakefield et al., 2025).

(4) By comparison to other coastal sites (marine cores), open summer sea-ice conditions may have prevailed off the nesting sites during the late Holocene. Additionally, nesting was possible during the last glacial at the nearby Untersee Oasis when sea-ice conditions were much harsher (McClymont et al., 2021). Do you really think that “increased sea ice extent restricted access to foraging grounds and by ~1700 cal. yr BP resulted in abandonment of the nest site”?

The wider evidence that open summer sea-ice conditions were present throughout much of the Holocene will help guide our use of terminology. In this instance to focus more on sea ice ‘conditions’ rather than ‘extent’ in the revision.

We do not think that the entire colony has been abandoned, only that the specific nest site was abandoned. On reflection, a specific local nest issue (e.g. boulder fell into the nest) would not have any relationship with climate and so is less relevant. The reviewer is correct that snow petrels persevered in other parts of Antarctica during more harsh climates, and in fact, we have evidence for occupation in deposits neighbouring 3012MUM2 which continued after ~1700 cal. yr BP. We are mindful that our note about the specific nest site is likely to cause confusion and so propose to remove reference to any nest site abandonment in the revision.

(5) Eventually, I do not see the added-value of section 4.6.

We do think that section 4.6 provides valuable insight into wider paleoclimate processes across the Weddell Sea, into the Southern Ocean and beyond to the opposite side of Antarctica, off Adélie Land, through similarities in proxy timing. These foreground how snow petrel deposits are excellent archives of change and are sensitive to climate change at multiple scales. These key changes are summarised in Fig. 6 which help put the manuscript into context. In particular, we found synchrony with: 1) South Atlantic cores; 2) Coastal sediments the opposite side of Antarctica, off Adélie Land; 3) Similarities and differences with Antarctic Peninsula and ice sheet records. Our concern is that to remove this information and distribute it throughout the preceding discussion will complicate and remove these impactful discoveries. Instead, we propose to consolidate and shorten this section to be less descriptive and focus more on the key synchronous timing, rather than the detail of proxy interpretation or uncertainty. Once we change the interpretations, in line with reviewer #1's recommendations above we believe the value of section 4.6 will become more evident.

I hope these comments and the ones listed in the annotated manuscript will help improve an important study.

Thank you for your helpful comments, they certainly will help improve the manuscript. In the revised manuscript we will also take into account the typographical comments in the provided PDF.

Xavier Crosta

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