16th October 2025

Dear Alessio Rovere, Editor,

We kindly attach major revisions to the manuscript egusphere-2025-513. Here we wish to clarify some additional changes made to the manuscript which became evident when working on the revisions. These comments are in addition to replies to reviewers #1 & #2 which are also contained within.

- We no longer emphasise the weak but positive relationship between $C_{14:0}$ fatty acid and cholesterol, but instead mention the potential links in a more qualitative manner, while continuing to note the literature. We now put less emphasis on the krill argument for cholesterol as there may be multiple contributory factors to these trends. The revised discussion puts less emphasis on cholesterol and more focus on dietary fatty acids.
- We have added significant commentary on preservation in the new section 4.1 (Proxy interpretations). This relates to both fatty acids and the numerous potential controls on $\delta^{15}N$, including degradation and the role of guano. In responding to reviewers comments we felt a change in structure was necessary and that bringing information on proxy interpretations earlier in the manuscript would help set the scene for later palaeoclimate discussion.
- We have opted, in line with recommendations from reviewer #1 to fully remove section 4.7. These commentaries have been combined throughout the manuscript, but in particular in section 4.3 which is now renamed (Coherence with other records of Holocene environmental change).
- Altering the structure and integrating a zone-centric approach to section 4.2 (Interpreting changes in sea ice in mid-Holocene stomach-oil deposits) has meant that the previous section 4.3 (Reconstructing Ocean productivity using snow-petrel stomach oil markers) is redundant and so has been deleted.
- The abstract and conclusion now better reflects the modified interpretations suggested by reviewer #1. In particular, where possible we try to first state what we see from the data (e.g. more or less productivity), followed by the mechanisms/processes controlling this.
- We decided, based on the new interpretations suggested by reviewer #1 to remove the previous conceptual diagram. This is because our new focus more on feeding at the MIZ makes these conceptual diagrams less appropriate. We think the updated text better communicate the new scenarios (and uncertainties) in sea ice and environmental change over the records timeframe.

We hope that these changes are clearly addressed in our response. If you have any further queries, please feel free to contact us for clarification.

Yours sincerely,

Mark Stevenson and coauthors

Reviewer #1

https://doi.org/10.5194/egusphere-2025-513-RC1

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.

We 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.

We find this general comment helpful and by addressing the specific comments below together with streamlining text in the manuscript, we believe we have improved the clarity of the manuscript. We believe some of this impression was a result of the complexity in combining such a large amount of multi-proxy data, which we feel we have improved in the revision. Specifically, restructuring the discussion has helped reduce the complexity of the manuscript which has included developing a new section (4.1) on proxy interpretations and removing the previous section (4.7) on wider palaeoclimate synthesis, but reintegrating this information into other sections. With much condensing and streamlining the discussion now just has three major sections.

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 previous lines 342-343 we have now added in a paraphrased version of the sentences above proposed by reviewer

#1, plus comments on ENSO and transport of subpolar surface waters as a response to southern Westerlies reinforcement. This can now be found at lines 389-393. Additionally, we have checked the manuscript for other instances where sea-ice expansion may have been confused with sea ice conditions over the continental shelf and have made 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 is helpful and have done this in the revision (see new section "2.1 Regional context" (lines 79-87)). In terms of snow petrel foraging behaviour and whether the Maud Rise could have been a foraging zone, based on wider research from the ANTSIE project, we believe it can be, but that tracking the MIZ might be more preferential. 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 (Wakefield et al., [in review] *Movement Ecology*). We have therefore added a paragraph to highlight the nuances of snow petrels tracking the MIZ while also, under certain circumstances being able to head out to polynyas such as the Maud Rise. This is now referred to in section 2.1 (lines 88-98) which helps set the context and we believe is 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 was the calibrated ^{14}C age ranges, while the grey stippled lines were the 95% confidence interval of the overall Bayesian model. The thin red line in previous figure 2 was 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.

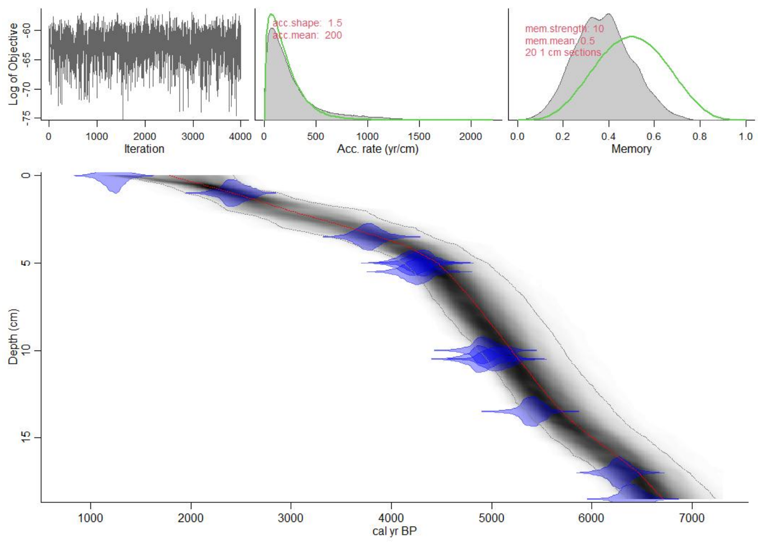


Fig.1 Rejected alternative model from R Bacon [code: Bacon('3012MUM2', d.by = 0.05, thick = 10, acc.mean = 200, acc.shape = 1.5, d.min = 0, d.max = 18.5, rotate.axes = TRUE, title="")]. *Reasoning: uppermost date is rejected from the model (close to 95% age range).*

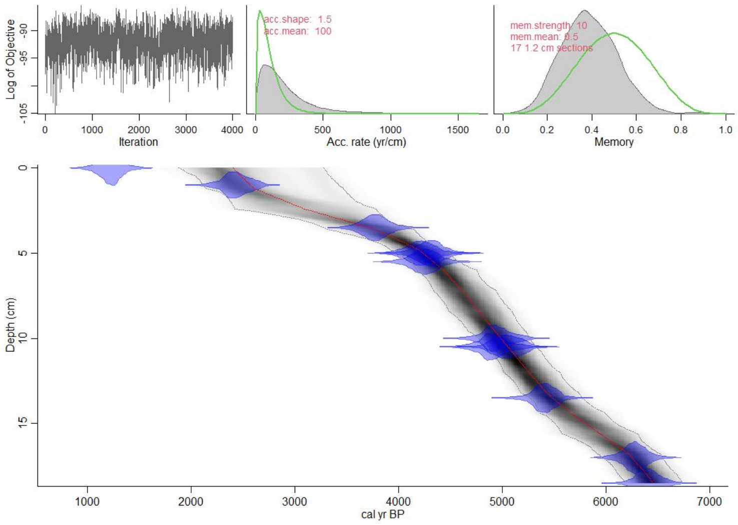


Fig. 2 Rejected alternative model from R Bacon [code: Bacon('3012MUM2', d.by = 0.05, thick = 1.2, acc.mean = 100, d.min = 0, d.max = 18.5, rotate.axes = TRUE, title="")]. *Reasoning: uppermost date is significantly rejected from the model (close to 95% age range).*

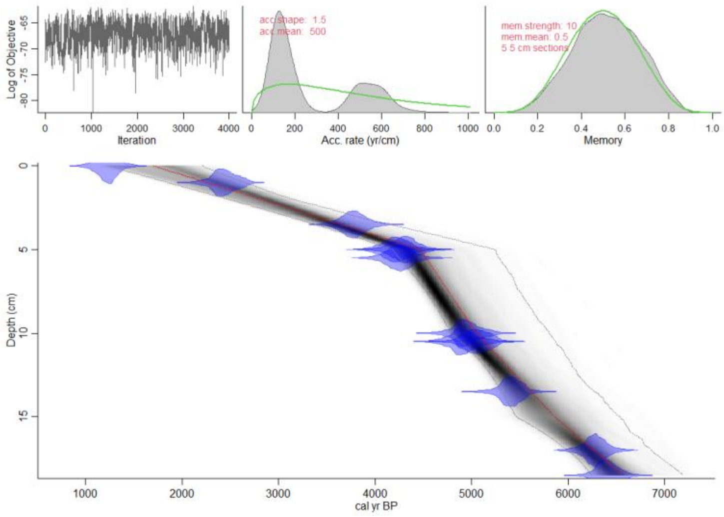


Fig. 3 Rejected alternative model from R Bacon. [code: Bacon('3012MUM2', d.by = 0.05, thick = 1.2, acc.mean = 100, d.min = 0, d.max = 18.5, rotate.axes = TRUE, title="")]. *Reasoning: uppermost date is significantly rejected from the model (close to 95% age range) and model features a pronounced shift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

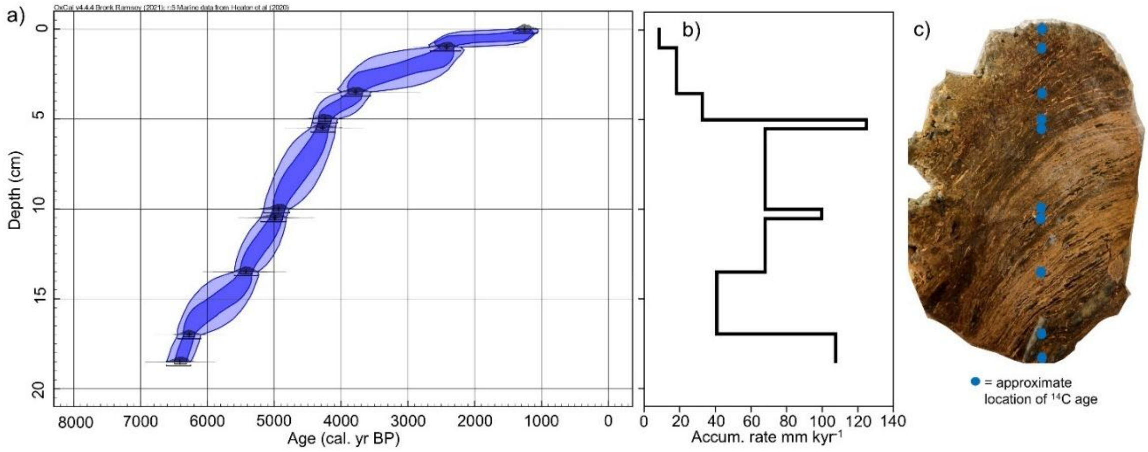


Fig. 4. a) Revised age-depth model developed in OxCal used replace the previous Fig.2 in the main text of the manuscript. b) accumulation rate between age control points calculated from median ages in the Bayesian model in (a). c) photograph of deposit 3012MUM2, with positions of ^{14}C ages in blue. 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 have used this new revised model in the manuscript and have updated stratigraphic diagrams accordingly. The model slightly decreases the oldest ages, meaning that our reconstruction now spans 1830-6390 cal. yr BP, rather than the previous 1983-6724 cal. yr BP. This is due to OxCal passing the lowermost age more closely through the median age (tighter model), rather than the accumulation rate changing the trajectory too significantly. The impact on our stratigraphic diagrams has been relatively minor and has been updated in the revision (e.g Fig.5).

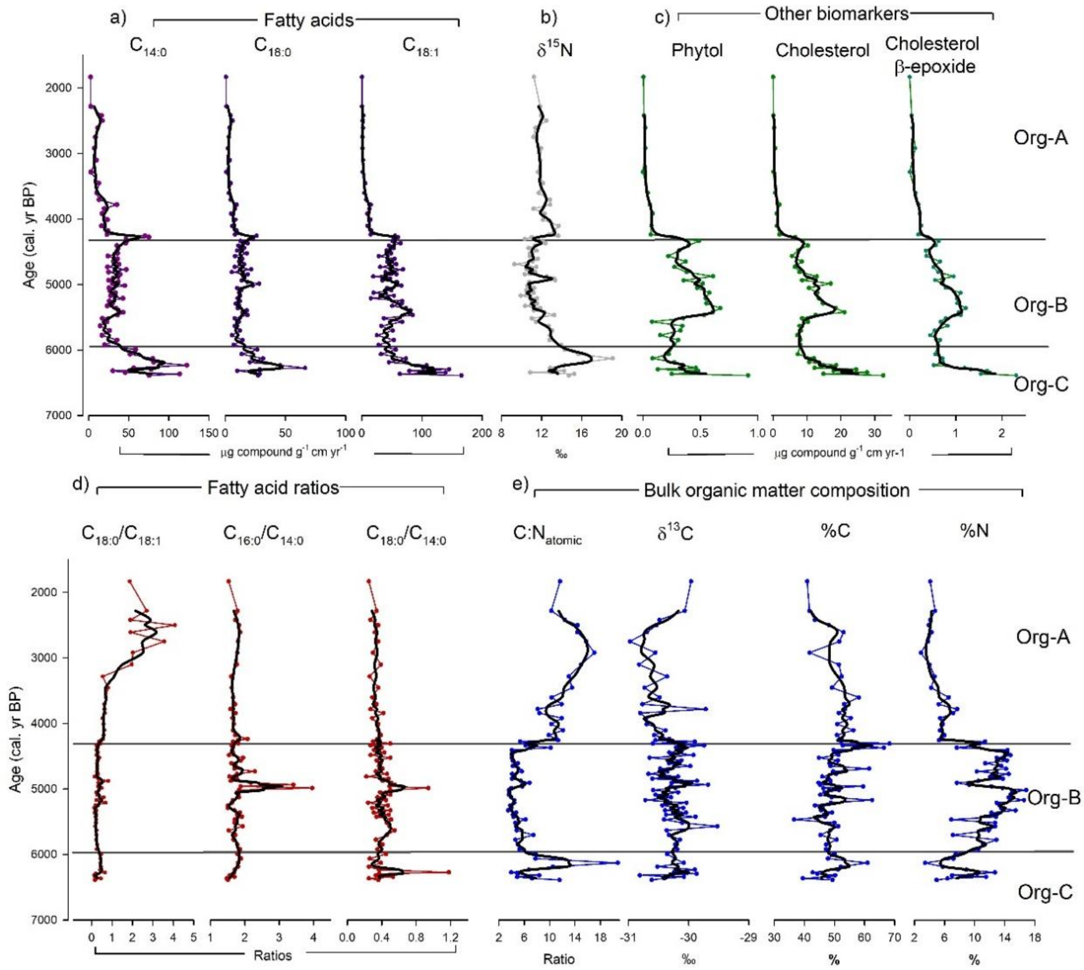


Fig. 5. Revised Fig.3 with new age-depth model applied to the stratigraphic diagrams and flux calculations from the revised accumulation rates. Trends and pattern remain broadly similar, despite slight changes to the model. The nature of the new model is that there is a slight pulse in many biomarker 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 was previously detailed in the supplements (SI Table 1 & 2) and has now been added to section 2.8 (Previous section 2.7):

"Input data for the organic PCA included bulk organics and biomarker concentrations, rather than fluxes (which included TOC normalized data and ratios). Input data for the inorganic PCA included XRF units in counts per second, rather than fluxes."

(lines 221-224).

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 & 7).

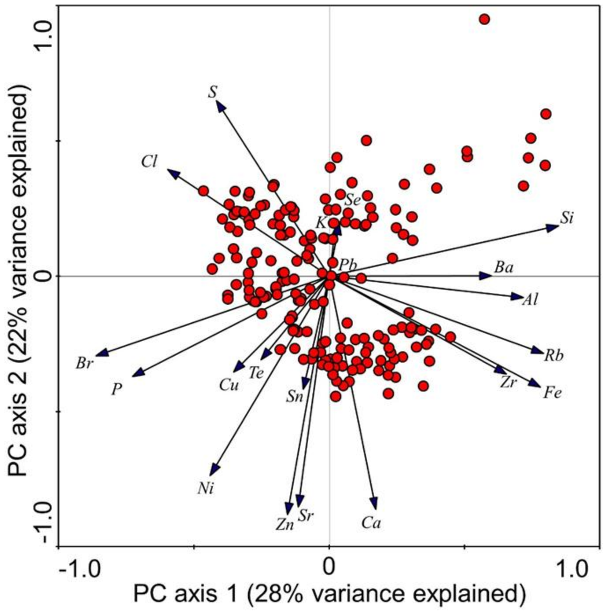


Fig. 6. Principal components analysis (PC) biplot of inorganic parameters (Se, Pb, Si, Ba, Al, Rb, Zr, Fe, Ca, Sr, Zn, Sn, Ni, Te, Cu, P, Br) based on log^{10} transformed and centred datasets (Existing SI Fig. 10).

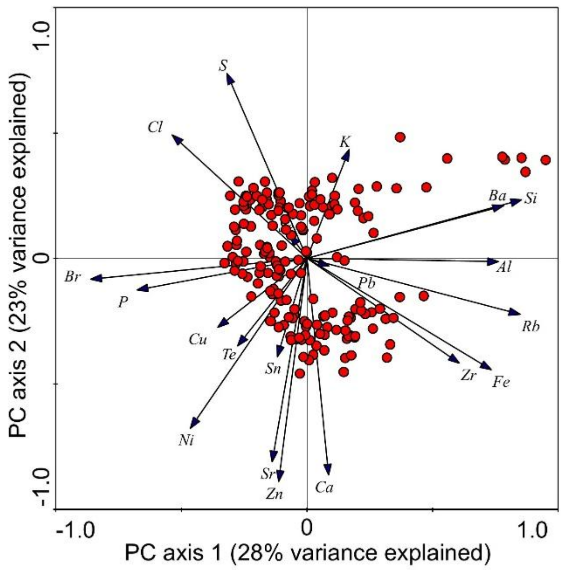


Fig. 7. Principal components analysis (PC) biplot of inorganic parameters (Se, Pb, Si, Ba, Al, Rb, Zr, Fe, Ca, Sr, Zn, Sn, Ni, Te, Cu, P, Br) based centred datasets, without any log10 transformation.

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 have therefore removed 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.

It is now evident that the PCs have a relatively low explanation of the variations as noted in the results section:

Lines 269-271: "Broadly, PC axis 1 (30% variance explained) for the organic geochemistry indicators had high positive loadings in C:N ratio and $C_{18:0}/C_{18:1(n-9)}$ (FA), while PC axis 2 (24% variance explained) had strong positive loadings in $C_{16:0}$ (FA), C18:0 (FA), $\delta^{15}N$ and C:N ratio (SI Table 2 & SI Fig. 9)."

Lines 327-330: "Based on the PCA of counts per second data, PC1 reflected 28% of the variance (SI Table 1; SI Fig. 10) and included Si, Ba Al and Fe, which tend to peak around ~6300 cal. yr BP in zone XRF-C (SI Fig. 5). PC2 reflected 22% of variance (SI Table 1; SI Fig. 10), and included S, Cl, Br, P and Cu, which shift towards lower values and lower variability in XRF-A (Fig. 4)."

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 (previous 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 went 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, we now agree with the reviewer and to increase clarity we have added in a new section at the start of the discussion on proxy interpretations:

"4.1 Proxy interpretations" (lines 341-381). In this section we group information and interpretations previously sprinkled throughout the results into a dedicated section. This includes comparing isotopic and biomarker compositions with previous studies and interpretations of the Antarctic krill marker and effect of guano.

The typos have been very kindly identified by reviewer #1 in the edited PDF and have been corrected. We attempted to use the hyphenated 'sea-ice' and non-hyphenated 'sea ice' depending on circumstances according standard English. We have updated 'Sea ice' to be 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). We have updated 'sea ice' to be 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.

Based on this suggestion we have modified the manuscript interpretations to focus more on the central Weddell Sea and the importance of snow petrels tracking the MIZ. This includes:

- Section on "suitability for snow petrel habitat" in the methods (lines 88-98), which highlights the importance of tracking the MIZ.
- Mentioning of the importance of the MIZ at the end of section 4.1 (Proxy interpretations) in the discussion to set the scene for subsequent interpretation (lines 380-385).
- Increasing the importance of the MIZ in section 4.3 "interpreting changes in sea ice in mid-Holocene stomach oil deposits" (lines 383-456).
- Adding reference to the MIZ in both the abstract and conclusion.

(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 (just 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 have adjusted the interpretation of high levels of $C_{18:0}$ and $C_{14:0}$ (previous lines 353-355), inferring productivity to take into account open water conditions in the Weddell Sea throughout much of the Holocene. As the structure has been significantly revised you are referred to section 4.2 (Interpreting changes in sea ice in mid-Holocene stomach-oil deposits) which has been substantially revised (lines 386-460).

We have also increased the importance of the MIZ in our discussion which also assisted with (1, above), this includes:

- Section on "suitability for snow petrel habitat" in the methods (lines 88-98), which highlights the importance of tracking the MIZ.
- Mentioning of the importance of the MIZ at the end of section 4.1 (Proxy interpretations) in the discussion to set the scene for subsequent interpretation (lines 376-381).
- Increasing the importance of the MIZ in section 4.3 "interpreting changes in sea ice in mid-Holocene stomach oil deposits" (lines 382-456).
- Adding reference to the MIZ in both the abstract and conclusion.

(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 have reduced data over-interpretations in the revision. Thank you for identifying the mistake in previous 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 have adjusted the text in this part of the manuscript (now section 4.3) to account more for the position of the retreating ice edge, 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.

We have therefore adjusted the first part of the discussion on zone Org-C to be as follows (lines 401-408):

"Between 6390–5960 cal. yr BP we infer that the snow petrels at Heimefronfjella had a mixed diet which reflected access to areas of high productivity similar to today, with a MIZ situated in pelagic waters but with the potential for foraging over the continental shelf when the MIZ was situated closer to the coast. This interpretation is based on geochemical evidence of a diet sporadically high in either Antarctic krill (Euphausia superba) (evidenced by FA $C_{14:0}$, Cu: pelagic waters) or fish (evidenced by $C_{18:0}$ (FA): continental shelf waters). While we are unable to constrain sea-ice extent precisely, we interpret the high but variable snow petrel stomach oil accumulation rates to reflect repeated nest occupation from an accessible foraging habitat. The MIZ must therefore have been located within the modern snow petrel foraging range i.e. within ~1200 km from Heimefrontfjella (Fig. 1; 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 has helped guide our use of terminology. In this instance we have focused more on sea ice 'conditions' or 'presence' rather than 'extent' in the revision (except where required when referring to other cited studies, which may occasionally use 'extent').

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 the deposit ended. We are mindful that our note about the specific nest site is likely to cause confusion and so have removed reference to any nest site abandonment in the revision.

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

We have deleted the previous section 4.6 and distributed this information throughout previous sections, mainly in section 4.3 (Coherence with other records of Holocene environmental change). Much of this information has been summarised and heavily condensed and is now presented between lines 457-488.

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 have certainly helped improve the manuscript. In the revised manuscript we have also taken into account the typographical comments in the provided PDF.

Xavier Crosta

Reviewer #2

General comments

The manuscript is about the use of a fossil stomach oil deposit of snow petrels as an archive for paleoenvironmental conditions in the Weddell Sea region during the Holocene. This is a relatively novel approach as a limited number of studies have been published on this topic so far, making this study a valuable contribution to a better understanding of paleoenvironmental conditions in this sector of the Southern Ocean. The authors analysed bulk isotopic (^{13}C and ^{15}N) parameters, lipid biomarker compounds (n-fatty acids, n-alcohols, Phytol and sterols) and elemental analysis (XRF-Scanning) to infer past changes in the composition of snow petrel diet. These changes are linked to the prevailing sea ice conditions in the foraging range of the birds.

We thank reviewer #2 for positive comments on the manuscript.

Although the approach is innovative, the discussion presented in the manuscript is vague and gives the impression that the interpretation is not necessarily supported by the data presented.

Noting comments from both reviewer #1 and #2 we have made revisions to strengthen and clarify the interpretations. In particular, we have further highlighted the importance of the MIZ (marginal ice zone) now that we have knowledge from modern snow petrel tracking that the birds tend to feed at this zone when sufficient openings in the sea ice enable feeding (Wakefield et al., [in review - preprint] *Movement Ecology*). Based on snow petrel tracking data we also know that especially during the pre-laying exodus and incubation phase snow petrels can routinely travel >700km and so the Maud Rise and other parts of the Southern Ocean can be potentially in-scope for feeding. However, a key point that is emerging is that snow petrels spend the majority of their time within the MIZ, foraging ~ 2° S of the ice edge. In fact, there is a high correlation between foraging latitude and ice-edge latitude ($R^2 = 0.98$; $p<0.001$) (Wakefield et al., [in review - preprint] *Movement Ecology*). Additionally, we know from recent tracking studies that later in the season as the sea ice retreats the birds track that ice edge retreat (Honan et al., 2025).

Much of this is now referred to in section 2.1 (lines 88-98) which helps set the context and we believe is a great improvement. Changes to the discussion also include:

- A revised discussion context statement at the end of section 4.1 (Proxy interpretations) which includes reference to ice configuration (lines 376-381).
- Increasing the importance of the MIZ in section 4.2 “interpreting changes in sea ice in mid-Holocene stomach oil deposits” (lines 383-456).
- Adding reference to the MIZ in both the abstract and conclusion.

Clarifying this nuance between where birds may spend the majority of time feeding and other locations where they may potentially reach at larger distances has added clarity to the manuscript.

In the revision have endeavoured to keep the edits succinct, recognising comments from both reviewers on the complexity of the manuscript at present.

The use of individual lipid compounds to reconstruct snow petrel diet is a significant simplification, given the complex patterns in the prey organisms and potential post-depositional alteration, but a necessary one to derive qualitative paleo-proxies. Therefore, the derivation of dietary composition from the lipid data should be dealt with in greater detail.

We thank reviewer #2 for highlighting the challenges faced with linking individual lipids from snow petrel deposits to initial source origin. In the initial manuscript we chose not to concentrate on this in the discussion section but instead signpost the existing work that has been done on this (e.g. Cripps et al., 1999; Lewis, 1966; Ainley et al., 2006; McClymont et al. 2022), some of which is in the introduction. Rather than place the derivation of dietary composition from the lipid data in the discussion, which we were concerned might distracted from the higher impact findings, we chose to have some minor interpretations in the results section as qualifiers, to make the discussion focus on the deposit interpretations. Nethertheless, following on from comments also from reviewer #1 we have added a short section at the start of the discussion, in which we outline the derivation of dietary composition in our proxy interpretation, given that snow petrel stomach oils are fairly new and novel in their application. This section is entitled "Proxy interpretations" (section 4.1, lines 342-349). Within the introduction we also refer the reviewer to lines 64-71 for more context on the applicability of lipid distributions.

Similarly, the link between diet and foraging region, sea ice and marine productivity needs to be outlined in a more concise way.

We believe that modifications to the manuscript, particularly emphasising the importance of the MIZ as a key zone where snow petrels forage has helped address this comment, as per the recommendations of Reviewer #1. Specifically, changes to the discussion section include:

- A revised discussion context statement at the end of section 4.1 (Proxy interpretations) which includes reference to ice configuration (lines 376-381).
- Increasing the importance of the MIZ in section 4.3 "interpreting changes in sea ice in mid-Holocene stomach oil deposits" (lines 457-488).
- Adding reference to the MIZ in both the abstract and conclusion.

I therefore suggest that the manuscript should be revised in order to better justify and elaborate on the interpretation of the data. Below you will find specific comments on individual paragraphs in the text. I have made only a few comments on the discussion chapters, as these should be streamlined overall.

We thank you for your general comments and believe that streamlining the discussion has helped improve the manuscript.

Specific comments

Line 102 – 108 Description of ^{14}C sample preparation procedures

The description for the two procedures (bulk sample and acid treatment) differ in the detail given. Please add how the samples were graphitized at BETA in order to provide the same level of detail. Also, please rephrase the sentences in line 106-107, as it is not clear, whether the CO_2 that was released by the acid treatment was graphitized or whether the residue was further processed for ^{14}C analysis. If the latter is the case- how was the sample transferred into CO_2?

We have added additional information regarding how the samples were graphitized at BETA. Samples were untreated and oxidised to CO_2 by combustion in a pure isolated oxygen environment. They were then converted to graphite with Co powder as a catalyst. To clarify lines previous 106-107, the acid treatment and water wash was conducted prior to graphitization, meaning it was the residue that was processed for ^{14}C analysis. For the samples pre-treated at SUERC, the pre-treated sample was recovered as CO_2 by heating with CuO in a sealed quartz tube. These clarifications have been updated in the manuscript and can be found at lines 127-134.

Line 110 ff: Age-depth model

The age-depth model shown in figure 2 does not fit to the calibrated ages of individual ^{14}C dates. The authors state that this is an effect of the Bayesian approach that does not necessarily lead to an age-depth model that passes through the median of each date.

However, the age-depth model they present seems to systematically overestimate the ages of the sample in units Org B and C. Given that the interpretation of the results is mostly based on the resulting accumulation rates, the author should re-calculate the age-depth model by modifying some of the priors to achieve a better fit.

A similar concern was reported by reviewer #1, and we have responded to explain our modelling approach in more detail in our reply to reviewer #1. Briefly, we explored different model priors in Bacon but were unable to improve on the model in the initial submission. We therefore adopted a new model in OxCal which ensures that the reconstruction passes through the median ages of each calibrated ^{14}C measurement. This has adjusted the age limits of the reconstruction slightly, but has only slightly altered accumulation rates, without changing overall interpretations. The new model can be found in Fig. 2 of the manuscript with the description of the method at lines 135-141.

Line 120ff: Reporting of ^{14}C ages in Table 1

Mixing the measured ^{14}C values with the results from the age-depth model is confusing. Please clearly separate these. You could for example add the modelled ages in the data table provided via Pangea, remove the two depths (0.5 and 18.25 cm) without ^{14}C ages from Table 1 and add calibrated values for each ^{14}C age. To complete the documentation of ^{14}C analysis, please add $F^{14}C$ values and round ^{14}C ages.

We have made this change. The new table 1 includes depth (cm), AMS lab ID, Age (^{14}C yr BP), Error ± (^{14}C yr BP), $F^{14}C$ and Bayesian modelled calibrated age using P_sequence in OxCal (cal. yr BP) (median, max, min). We have removed the two depths (0.5 and 18.25 cm) without ^{14}C ages from table 1 as we agree with the reviewer that this information is more than is needed. In table 1 ^{14}C ages have been rounded, with exact values retained in the Pangea table.

Line 192ff: Statistical analyses

Did you use original values (counts, concentrations) of accumulation rates to conduct the cluster analysis and PCA? Please state in the text.

Please see our reply in response to the similar point raised by Reviewer #1. The update can be found at lines 221-224.

Which elements/compounds did you include in the statistical analyses? Please name them here. XRF data: Is it really useful to include "all" parameters in the analysis? I'd recommend to use those that are relevant for answering the research question, as e.g. Te, Se, Rb and Pb are not further discussed in the manuscript.

The elements/compounds included in the statistical analysis are presently listed in SI Table 1 (XRF) and SI table 2 (organics). Rather than list all the compounds in the text as a list we suggest referring to the supplementary information (e.g. line 225). Our aim with the PCA was to identify whether there were any similarities between elements which could explain the sources of the elements recorded in the deposit (e.g. did elements of likely biogenic origin align together).

Here, we compare the previous PCA analysis (Fig. 1) with a revised PCA which removes Te, Se, Rb and Pb (Fig. 2).

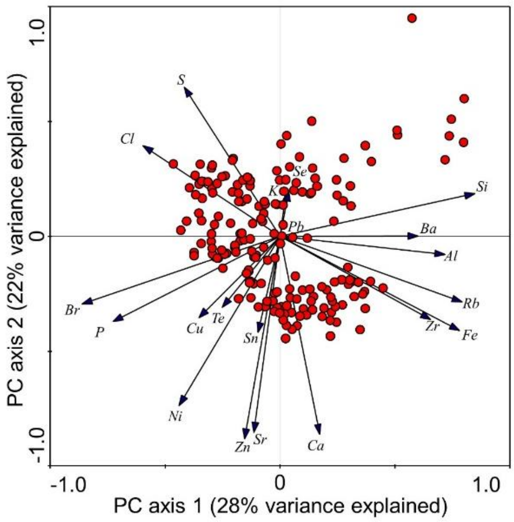


Fig.1 Principal components analysis (PC) biplot of inorganic parameters (Se, Pb, Si, Ba, Al, Rb, Zr, Fe, Ca, Sr, Zn, Sn, Ni, Te, Cu, P, Br) based on log[10] transformed and centred datasets (SI Fig. 10).

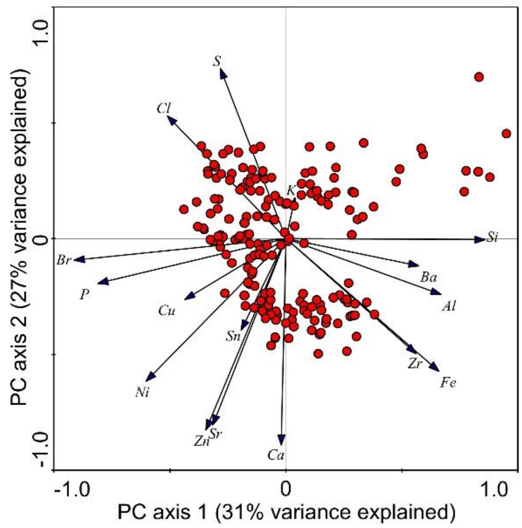


Fig.2 Principal components analysis (PC) biplot of inorganic parameters (Si, Ba, Al, Zr, Fe, Ca, Sr, Zn, Sn, Ni, Cu, P, Br) based on log[10] transformed and centred datasets (reduced number of variables).

Reducing the number of variables has slightly increased the amount of variance explained, increasing from a total of ~50% to ~58%. It does not change the overall pattern of the PCA significantly. Based on this and the fact that that Te, Se, Rb and Pb have a relatively unclear distribution (see SI Fig. 6) we have removed these variables from the inorganic PCA and updated the figure in the supplements.

Line 210 ff: Sampling and validity of accumulation rates

The interpretation of environmental changes is strongly relying on the accumulation rate of specific lipids. However, I see some issues with this procedure and don't think that accumulation rates can be used here.

The first concern is a geometrical effect of sampling: In figure 2 the age-depth model and the stomach oil deposit with the profile and sample positions for ^{14}C ages (and biomarkers) are shown. The sampled profile is not perpendicular to the layering in the deposit, and hence the "depth" distance between two samples in the profile does not correspond to the thickness of material that was deposited between two points in time. Therefore, the interpolation of ages between ^{14}C-dated samples may be valid procedure to estimate the age of a sample, but the depth scale is not reflecting the actual built-up of the deposit through time (see also figure uploaded in separate file).

The second concern is that the depth-scale is only valid along the one sampled profile and not representative for the whole deposit. In SI figure 1 the complex depositional history/internal structure of the deposit is shown. The lithological units can be correlated through the deposits but they vary in thickness and the same unit can have different thickness depending on where in the deposit you measure. Assuming that the boundaries of a unit correspond to specific time horizons, this means that different thicknesses were deposited during the same period, depending on where you look in the deposit and hence accumulation rates are variable within one lithological unit.

The sampling approach for 3012MUM2 was conducted as shown in Fig. 2 and SI Fig. 1 to enable a high-resolution record without avoiding hiatuses, which might not be possible to track laterally. We suggest alternatively that there are less likely to be large differences in relative accumulation rate evident between units, bar the truncation of Unit 5 to the left side of the deposit (and the emergence of Unit 4b) (SI Fig.1). However, we also agree that given the more complex accumulation history of individual spits, compared to a marine sediment for example, we have caveated our interpretations carefully and highlighted that we selected a sampling line which represents the maximum accumulation rate (which we have added to the methods). This sentence has been added to the methods (lines 107-109):

"We selected a sampling line which represented the maximum accumulation rate and a continuous sequence through the stratigraphy; noting some heterogeneity either side of this line (SI Fig. 1c)"

We believe that the improvements to the age-depth model outlined in response to Reviewer 1 will help improve the accuracy of the accumulation rates and flux models and will help address some of these concerns (e.g. new model presented in Fig. 2). Additionally, in the supplementary information we present organic parameters as concentrations relative to TOC (SI Fig 2, 3 & 4) and XRF parameters as CPS. The patterns are similar to those presented in the main text figures which give us confidence that the flux calculations are valid. We have added to the main text the caveats of sampling heterogeneity (e.g. lines 107-109) and the potential for variable deposit buildup, whilst noting that our interpretations are robust based on the data which is not scaled to accumulation rate. This is highlighted at lines 247-248 in the results:

"Given similar patterns with biomarker concentrations (SI. Fig. 2, 3 & 4) we consider the trends we have identified to be robust."

Line 224: Origin of fatty acids in stomach oil deposits

The authors suggest that "samples likely comprised predominantly wax esters" – I don't think that this conclusion is supported by the data presented by the authors. The concentrations of fatty acids are > 10 times higher than those of n-alcohols (line 232 and line 233). Given the 1:1 ratio of fatty acids and n-alcohols in wax esters, this suggests additional sources of fatty acids. Please discuss further, what other sources of fatty acids may occur in the deposit and what the implications are for the interpretation of fatty acids with regard to snow petrel diet.

The early literature suggested that wax esters were a key part of snow petrel stomach oil deposits but reviewer #2 is correct to point out that the fatty acids in these deposits are substantially higher than *n*-alcohols, and so suggest a higher free fatty acid contribution. Have altered our text in section 3.3 accordingly to recognise this difference. It now reads (lines 248-250):

"The 3012MUM2 deposit samples likely comprised wax esters consistent with existing stomach oil studies (Imber, 1976; Lewis, 1966, 1969; Warham et al., 1976; Watts and Warham, 1976), and contributions from free lipids."

More generally, both wax esters and free fatty acids are sourced from the partially or fully digested prey remains, whose fatty acid distributions can be diagnostic of the prey (Hiller et al. 1988; Horgan and Barret, 1985; McClymont et al. 2021; Warham, 1977). This approach for snow petrel stomach oils was confirmed by Berg et al. (2023).

Line 230: Identification of C18:1 homologue

In avian dietary studies and also in lipid studies of krill, fish and other marine organisms it has been shown that more than one C18:1 compound is abundant and the position of the double bond is diagnostic for specific sources (e.g., Connan et al. 2008, Yang et al. 2016). Which C18:1 compound are you referring to here or did you sum up all C18:1 compounds? Please clarify.

The 3012MUM2 chromatograms are strongly dominant in one $C_{18:1}$ compound so we only integrated the dominant peak. This is in comparison to other snow petrel stomach oil deposits which sometimes have a secondary $C_{18:1}$ peak which is immediately after the main peak (e.g. Berg et al. 2023). Through comparison to a synthetic mixed fatty acid standard (Supelco 37 Component FAME Mix; CRM47885 (LOT: LRAC9768)) our dominant $C_{18:1}$ fatty acid is $C_{18:1(n-9)}$, since it has the correct spectra and comes out at the same time as this peak in the standard. We have added this detail to the text and highlighted the sources of the interpretation (lines 184-186):

"Compounds were identified from their respective mass spectra and retention times, with quantities calculated relative to the peak area of the internal standard heptadecanoic acid and an assumption of a 1:1 response (validated by comparison with Supelco 37 component FAME mix (CRM47885, Merck))."

Line 235: Cholesterol as marker for krill

Here it is stated that cholesterol is a marker for krill, because it accounts "for more than 76% of total sterols in krill (Ju and Harvey, 2004) and is typically lower in concentration in fish." This conclusion should be better substantiated, as cholesterol is probably the most common sterol in both groups, so it cannot be said that an increase in cholesterol is associated with more krill. Eating more fish also leads to more cholesterol. My suggestion would be to refer to compound-ratios when discussing relative changes in the composition of snow petrel paleo diet (Also for fatty acids). Please provide refences for the concentration and abundance of cholesterol in fish.

Proportionally cholesterol is a higher component of krill than fish, this is well known in dietary studies. However, despite searching, there are not many detailed studies of different studies available in the literature. One example is the, cholesterol in *Dissostichus mawsoni* ranges from 4.73 – 14.29% of total lipids in Clarke et al. (1984). Birds would have to eat by weight more fish to absorb the same amount of cholesterol than krill. The comment *"Cholesterol is a ubiquitous marker but in this context could be considered a krill marker (both Antarctic and ice krill), since it can account for more than 76% of total sterols in krill (Ju and Harvey, 2004) and is typically lower in concentration in fish,"* takes into account this uncertainty. However, we recognise that to be more clear we have provided the following reference for fish (Clarke et al., 1984), where the authors showed that the relative importance of cholesterol compared to other sterols and fatty acids was minor. Please see lines 259-264:

"Cholesterol is a ubiquitous marker but in this context could be considered a krill marker (both Antarctic and ice krill), since it can account for more than 76% of total sterols in krill (Ju and Harvey, 2004) and is typically lower in concentration in fish (e.g. cholesterol in Dissostichus mawsoni ranges 4.7-14.3% of total lipids (Clarke, 1984))."

There is unlikely to be a specific sterol marker of fish versus krill in the environment, as the reality is that the main differences between sterol markers are between the animal and plant sterols. Future work should look at the stable isotope composition of individual sterols and the end-member distribution of sterols in fish versus krill to better apportion these contributions.

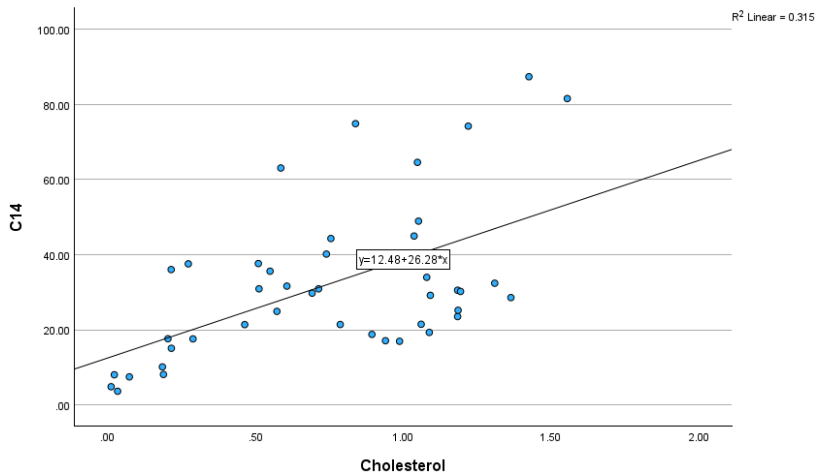
More generally, we now place less emphasis in the interpretation on cholesterol as a marker for krill as we acknowledge it does have other sources. This means in the revision we apply caution when interpreting cholesterol results in the discussion.

Line 246 ff: PCA results

To what extent can the assumptions about the sources of biogenic components be confirmed by PCA? Are there any indications of a correlation between cholesterol content and C14 fatty acids? Please add this information to this chapter or insert a paragraph on this topic elsewhere in the discussion.

The organic PCA biplot (SI Fig. 9) shows that cholesterol plots along PC1 (negative), separate from the saturated fatty acids which include the proposed Antarctic krill marker $C_{14:0}$ (positive PC2), although they also plot with negative PC1 values. The organic PCA biplot shows cholesterol is more closely associated with the monounsaturated FA $C_{16:1}$ and $C_{18:1}$ ($C_{18:1}$ source mainly fish, references in McClymont et al. 2022), complicating our previous interpretations. However, when we plot FA $C_{14:0}$ vs cholesterol (as µg compound g^{-1} cm yr^{-1}) there is a relatively weak linear relationship, although we note that this is positive i.e. that more cholesterol broadly aligns with more $C_{14:0}$ FA, supporting a likely Antarctic krill source. Although the relationship is relatively weak (R^2 linear = 0.315). We can posit that there are varied dietary controls on cholesterol compared with fatty acids. There is probably not enough evidence to state the link between cholesterol content and $C_{14:0}$ FAs are the strongest.

We refer to the new section 4.1 on 'Proxy interpretations' (lines 341-381), which discusses biogenic sources more generally, setting the scene for subsequent interpretations.



Line 255 ff: Biomarker zones in the deposit

How do the units defined by the cluster analysis of organic compounds correlate to the visible lithology and to the units defined by XRF?

The deposit shows some changes in colour and was divided into lithological units (supplement). How do these relate to the cluster units identified by the cluster analysis? Is the change in colour/texture related to changes in inorganic and organic composition? And in the cluster analysis? Please point that out in the results or discussion sections.

We did not record texture during our analysis as we did not observe any distinct changes in texture through the deposit. We did note changes in colour, which is visible in the stratigraphy as the Reviewer points out in Supplement Fig. 1c. However, our statistical analysis revealed only 3 clusters based on either the organic or XRF analyses, whereas 8 zones of colour are visible. However, organic clusters did not align with inorganic XRF clusters, with the organic clusters most useful for interpreting changes in colour. In terms of lithological units in supplement Fig. 1c, zone Org-C closely matched with zone 8 (darkest layer, black/brown), with zones 4-7 matching most closely with zone Org-B (lighter, yellowy/orange) and zones 1-3 matching with Org-A (medium-dark, grey brown to black brown). We have highlighted this section 3.4 'Changes in biomarkers through time' (lines 280-283):

"Although clusters were identified from organic analyses, there were 8 zones of colour visible in 3012MUM2 (SI Fig. 1c). In terms of lithological units, zone Org-C closely matched with zone 8 (darkest layer, black/brown), with zones 4-7 matching most closely with zone Org-B (lighter, yellowy/orange) and zones 1-3 matching with Org-A (medium-dark, grey brown to black brown).

Line 294ff: Interpretation of inorganic composition

The discussion on the sources of inorganic elements in the deposits needs to be more concise.

The sources of the elements in the deposit are not "local erosion" or "wind-blown" as this ascribes processes and not distinct sources. E.g., some of the local erosional products are likely also windblown to the snow petrel nest. And if Cl, S, Br and P are "windblown" they still have to come from somewhere. I'd suggest to distinguish between minerogenic material derived from bedrock, reflected by the elements Fe, Al, Mg and Ca and other elements such as Si, Ti and Zr. In the lithological description (SI Fig. 1) the rock fragments were assigned as "granite". What rock type was the sample analysed for elemental composition?

For the second group of elements, please discuss, where the windblown particles come from.

Reviewer #2 carefully observes that the interpretation of inorganic composition in a primarily organic deposit was challenging. By "wind blown" we were referring to elements likely derived from sites external to the local area i.e. to separate them from local weathering (and aligning more with the wind-blown inputs of dust to the ice cores). We appreciate that this distinction was not clear and have edited the text accordingly, removing reference to 'windblown particles' as we appreciate this is unlikely to be conclusive.

For general context, windblown particles in Antarctica can come from a variety of sources including erosion of local/regional mountains, long distance transport from other areas of Antarctica (e.g. McMurdo dry valleys) (Diaz et al., 2020) and long-distance dust transport from South America (e.g. glaciated parts of Tierra del Fuego) (Li et al., 2010; Shi et al., 2023). However, in this instance, due to the overall trends similarities between Br, P (guano), S (guano) and Cu (krill), on reflection we agree with the reviewer that Br is unlikely to be windblown. We have decided to group inorganic compounds now by PCA associations (e.g. Fig. 4) rather than strictly define any individual compounds as coming from definitive sources as there may be mixed sources.

Within the text we now discuss inorganic interpretations mainly in the new 'Proxy interpretations' section (4.1).

The rock fragment analysed was a gneissic granite, which is typical of the local rock which is variable as it is known to be composed of parageneisses which have undergone complex retrograde metamorphism (Juckes et al. 1969). This is noted in line 206 & 354.

Line 304: Is it possible that Ca is derived from carbonate in that specific layer (e.g. incorporated fragments of egg shell)?

It is possible that the peak is related to an egg shell fragment, although we have not detected egg shell remains in our deposits and XRF zone B represents ~2.3 cm of accumulation so we would expect to see fragments of shell (unless they have dissolved). We also do not see any corresponding increases in our organic proxies for a shift in diet at this time.

Line 335ff: Role of ice shelf retreat on accessibility of foraging areas

This section is not linked to the findings of this study. Only in line 340-341 is stated that "Maintenance of the ice fronts in a retreat scenario from the start of the record is consistent with our evidence of increased availability of productive foraging habitat". This statement needs to be clarified: Why is the retreat of Brunt Ice shelf consistent with productive foraging habitats, and how do you infer the productivity of the foraging habitat from your data?

Our aim was to set the context of the deposition of the 3012MUM2 deposit in a period of relative stability in terms of the ice shelf extent. We have deleted the previous section 4.1 (line 335ff) and started the next section (now 4.2 *"Interpreting changes in sea ice in mid-Holocene stomach-oil deposits"*), with a line that confirms the ice sheet had reached its modern position before the start of the record, citing regional studies which confirm this (Nichols et al., 2019; Johnson et al., 2019; Hillenbrand et al., 2017; Grieman et al., 2024). The information on the link between ice fronts and productive habitats plus cavity expansion has been deleted, as the previous sentence was an oversimplification. This has been replaced by the edited contribution from reviewer #1 (see lines 387-392).

Line 429: Nitrogen isotopes

Please clarify what the potential end-members for your isotopic composition are. The range of 9 to 19 permill in ^{15}N values is quite large. Discuss if a shift in the order you find is reasonably explained by nutrients in the ocean (glacial/interglacial shift are c. 4 permill only, Horn et al. 2011). What could be other effects? E.g., Alteration by weathering and degradation, different fractionation depending on the sample composition (in the nitrogen-containing compounds of the sample, what are these?).

Since snow petrels primarily feed on fish and Antarctic krill, these are the main direct modern endmembers at the present day. The reviewer is correct that the range of ^{15}N values we record is large (modern Southern Ocean analysis of likely snow petrel prey give ranges of 5-11.2 ‰ (Rau et al., 1992)). Paleo-endmembers which would be most informative are regrettably not available. In the manuscript, we are presently interpreting the high $\delta^{15}N$ values to reflect a baseline shift (i.e. in $\delta^{15}N$ values in phytoplankton) which is propagated up through the food chain. In this instance past variations in the $\delta^{15}N$ of circumpolar deep water (CDW) could likely have played a role (Kemeny et al., 2018). High productivity at the base of the food chain (e.g. in algae/POM) can lead to high levels of $\delta^{15}N$ at the base of the food chain which can propagate up into primary (Antarctic krill/fish) and secondary consumers (snow petrels). There is also the possibility that some N is sourced from guano, then there would be a secondary consumer (snow petrel) effect i.e. an impact of the digestion (potentially also involving microbes) by the snow petrel and subsequent preferential excretion of higher $\delta^{15}N$. It is known that seabird colonies can lead to the preferential enrichment of $\delta^{15}N$ in the surrounding organic matter and that this can be a marked increase (Wainright et al. 1998).

We can also consider the sea ice circumstances that may lead to higher $\delta^{15}N$. For example, in McClymont et al. (2022) between MIS 2 and 3 $\delta^{15}N$ was also relatively high ranging between 11.1 ‰ and 12.6 ‰. In this instance during the glacial stage there was more sea ice and less CDW upwelling, leading to high $\delta^{15}N$ because of high nutrient utilisation. Additionally, $\delta^{15}N$ values in modern and paleo Antarctic stomach oil residues range 6.1 – 11.7 ‰ (Berg et al. 2023), which is broadly in line with most 3012MUM2 measurements, except for the peak in zone Org-C. In the case of our deposit 3012MUM2 although there would not have been as much sea ice as during the glacial stage, certainly enhanced productivity brought about by water mass upwelling could partly explain elevated $\delta^{15}N$.

Other explanations for large shifts in $\delta^{15}N$, such as alteration by weathering and degradation are possible. Weathering would lead to release of ^{14}N in preference to ^{15}N, resulting in elevated $\delta^{15}N$ values due to the biosynthetic pathways within microbes (Macko and Estep, 1984). However, in chromatograms from 3012MUM2 microbial fatty acids were not a major component (chromatograms were dominated by saturated FAs), but it is acknowledged that microbial FAs are likely to be more susceptible to diagenesis.

We were unable to fully confirm the source(s) of the nitrogen-containing compounds in this study to fully understand the values we have determined here, but this can be a direction for future work. We have integrated these comments to help improve this study.

Our revised $\delta^{15}N$ interpretation is now split between two sections:

Section 4.1 "Proxy interpretations" between lines 359-371.

Integrated within Section 4.2 *"Interpreting changes in sea ice in mid-Holocene stomach-oil deposits"* where relevant such as lines 416-422.

Line 516: Interpretation of abandonment of the nesting site at 2000 due to sea ice: I don't think that the ^{14}C age from the top of the deposit can be inferred to mark the timing of abandonment because 1) the deposit may have been degraded/eroded from the top when the nest was no longer occupied and therefore the age can only indicate a maximum age of abandonment. 2) The abandonment of the nest could as well have other reasons, such as physical properties of the nest cavity (See Einoder et al. 2014).

Specifically, we were focused on nest abandonment, rather than colony abandonment. However, a similar comment was made by reviewer #2 and therefore we have removed the comments on abandonment from this manuscript.

Technical corrections

Line 21 and 516: 2000 cal yr BP instead of 6700?

Corrected to 1830 (lines 23 and 509)

Line 68: You state here, that the deposit is well preserved - please explain how you come to this assessment.

Percent C composition is relatively high (~35-70%), the deposit remains laminated when cut and organic extracts are extremely rich in extractable lipids, especially with respect to fatty acids (which in many environments would be more susceptible to degradation).

Line 70: “radiocarbon-based age-depth model” instead of “radiocarbon dated age-depth model”

Thank you, this has been corrected (see line 74).

Line 150ff: Biomarker analysis- Which fractions did you separate/in which fractions did you recover n-alcohols, phytol and sterol? Please add.

We carried out a four stage fractionation of the neutral lipids. F1 = hexane; F2 = DCM; F3 = DCM:MeOH (1:1); F4 = MeOH. These were recovered in F3 (see 172).

Line 193-196: The sentence seems to be incomplete, please revise

We have checked the sentence and it is rather long. We suggest to break it into two to clarify:

“For each dataset (organic (Org A-C) and inorganic (XRF A-C), separately) a constrained hierarchical cluster analysis based on sample order was performed using the rioja package in R (Juggins, 2020). The cluster analysis was compared with the broken stick model of random zones (Bennett, 1996) to identify the maximum number of statistically significant clusters.” (lines 215-218).

Line 193-194: What is Org 1-3 and XRF 1-3? Is it the “Units” that are defined by the cluster analysis? In Figure 2, 3 and 4 and in the subsequent text, the authors refer to Org A-C and XRF A-C. Please clarify

Yes Org 1-3 and XRF 1-3 are the units defined by cluster analysis and should be Org A-C and XRF A-C, this has been corrected.

Line 199: Please list the elements (XRF) and compounds (organic) that were included into the PCA.

We would prefer that the list of elements and compounds remain in the Supplementary Information, since this will otherwise add several long sentences to the methods section.

Lines 204-207: Incomplete sentence, please revise.

We have revised the sentence to improve clarity and adjusted the ages to take account of the new model produced in OxCal (lines 229-232):

"When taking biomarker and isotope samples we avoided the margins of the deposit, which were easily deformable. As a result, the oldest and youngest biomarker and isotope samples lie at 0.5 cm (1830 (1170–2530) cal. yr BP) and 18.25 cm (6390 (6210–6590) cal. yr BP) respectively in the Bayesian model, or ~1800 to 6400 cal. yr BP when rounded to closest 100 years."

Line 220-221: Please check the values given for ^{15}N: lowest value is equal to mean value

Thank you, the mean $\delta^{15}N$ should be 12.2 ‰ (line 245).

Line 238: Please add reference for the cholesterol concentration in fish to support that cholesterol is indicative for krill in stomach oil deposit. As stated in line 236 cholesterol is ubiquitous.

An example is *Dissostichus mawsoni* where cholesterol ranges from 4.73 – 14.29% of total lipids in Clarke et al. (1984) (see line 261). Further text has been added in response to the comment by Reviewer #2 above.

Line 233: What are "key n-alcohols"? Better name the compounds. Up to here the results of n-alcohol analysis have to been mentioned in the text.

Key *n*-alkanols are defined in SI Fig.3 and are $C_{14:0}$, $C_{16:0}$, $C_{18:0}$, $C_{20:0}$ and $C_{22:0}$. We have added this to the text at line 264-265 and refer to SI Fig.3.

Line 320: "Bedrock contamination is unlikely given local gneiss bedrock" This sentence is out of context, please revise.

Have deleted this sentence and revised. We have replaced with a sentence to describe rather than interpret the elemental composition of the gneissic granite bedrock. Please see line 354.

Line 322: typo "lotted"

Apologies for the typo. We have changed to 'plotted'.

Rounding of ^{14}C ages: Please check through the manuscript and use the general rounding convention for ^{14}C ages (see table below)

Age	**Nearest**	**Error**	**Nearest**
<1000	5	<100	5

1000-9999	10	100-1000	10
10000-20000	50	>1000	100
>20000	100		

We have placed rounded ^{14}C dates in the manuscript text and table 1. The exact dates (calibrated and uncalibrated) remain in the Pangaea upload.

Figure S4: Which parameters are shown here? What are blue data points and what are the black data points?

Parameters are listed in the figure above each stratigraphic diagram and so we initially chose not to rewrite them in the figure caption as there are many (x11). However, we have been able to add this text as it was not immediately clear to the reviewer and does fit on the same page. Blue data points are the original measures, with black points the three point moving average. We have made this amendment in the figure caption.

References:

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EINODER, D., et al. 2014. Cavity characteristics and ice accumulation affect nest selection and breeding in snow petrels Pagodroma nivea. *Marine Ornithology* 42, 175-182.

HORN, M.., et al. 2011. Southern ocean nitrogen and silicon dynamics during the last deglaciation. *Earth and Planetary Science Letters* 310, 334-339.

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Citation: https://doi.org/10.5194/egusphere-2025-513-RC2

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