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 believe that we can make revisions to strengthen and clarify the interpretations. In particular, we plan to further highlight 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).

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

We note that a large number of comments from reviewer #2 request additional detailed text, which could increase the length of the manuscript. We will endeavour to keep the edits succinct in a final draft, 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). 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 from reviewer #1 we are willing to add a short section at the start of the discussion, in which we can outline the assumptions and caveats to our proxy interpretation, given that snow petrel stomach oils are fairly new and novel in their application.

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 will help address this comment, as per the recommendations of Reviewer #1.

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 will work on streamlining the discussion.

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 will add 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 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 will be updated in the manuscript.

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.

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 are happy to make this change. The measured ^{14}C values were initially combined with the age-depth model to highlight the age span in the relevant model but we can understand how that could be confusing as they are the age ranges for the model, rather than the specific age range for the date. The two depths without the ^{14}C ages were added in Table 1 to highlight the top and the base of the biomarker sampling which is

slightly above and below the span of analyses. However, this information is also within the text in line 101-102 so can be removed from the table. Modelled ages can certainly be added to the Pangea table. We can add $F^{14}C$ values and round calibrated ^{14}C ages, retaining original dates in the Pangea table. We will also note this in the table so that if the dates are recalibrated in the future the exact values can be used.

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.

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 and SI table 2. Rather than list all the compounds in the text as a list we suggest referring to the supplementary information. 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 existing PCA analysis (Fig. 1) with a revised PCA which removes Te, Se, Rb and Pb (Fig. 2).

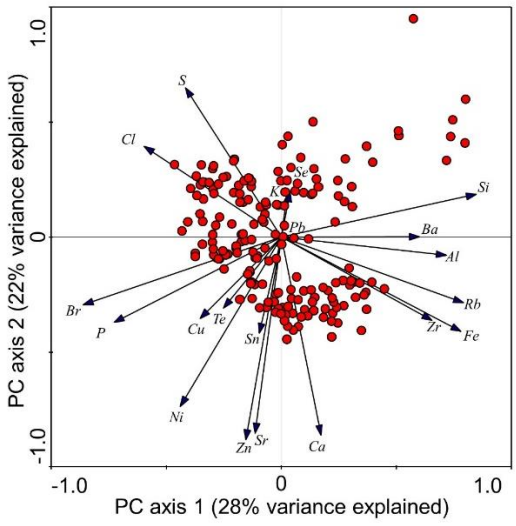


Fig.1 Principal components analysis (PC) biplot of inorganic parameters (Se, Pb, Si, Ba, Al, Rb, Zr, Fe, Ca, Sr, Zn, Sn, Ni, Te, Cu, P, Br) based on log^{10} transformed and centred datasets (SI Fig. 10).

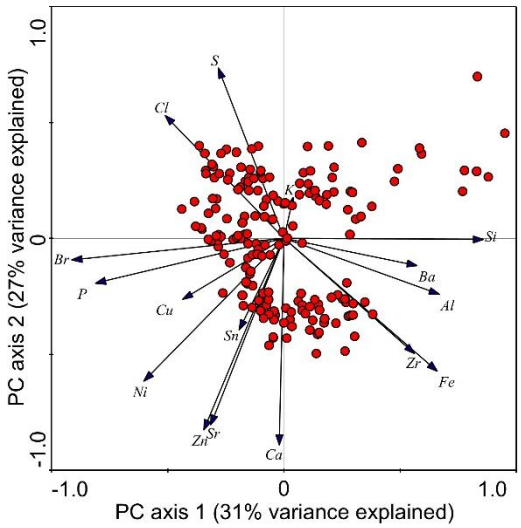


Fig.2 Principal components analysis (PC) biplot of inorganic parameters (Si, Ba, Al, Zr, Fe, Ca, Sr, Zn, Sn, Ni, Cu, P, Br) based on $\log^{10}$ transformed and centred datasets (reduced number of variables).

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 propose to remove these variables from the inorganic PCA.

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/SI figure 1 to enable a high-resolution record without avoiding hiatuses, which might not be possible to track laterally. We disagree that there are large differences in relative accumulation rate 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 should caveat our interpretations carefully and highlight that we selected a sampling line which represents the maximum accumulation rate (which we will add to the methods). 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. 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 will add to the main text the caveats of sampling heterogeneity and the potential for variable deposit buildup, whilst noting that our interpretations are robust based on the data which is not scale to accumulation rate.

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. We will alter our text in section 3.3 accordingly to recognise this difference. 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 probably $C_{18:1(n-9)}$, since it has the correct spectra and comes out at the same time as this peak in the standard. We will add this detail to the text and highlight the sources.

Line 235: Cholesterol as marker for krill

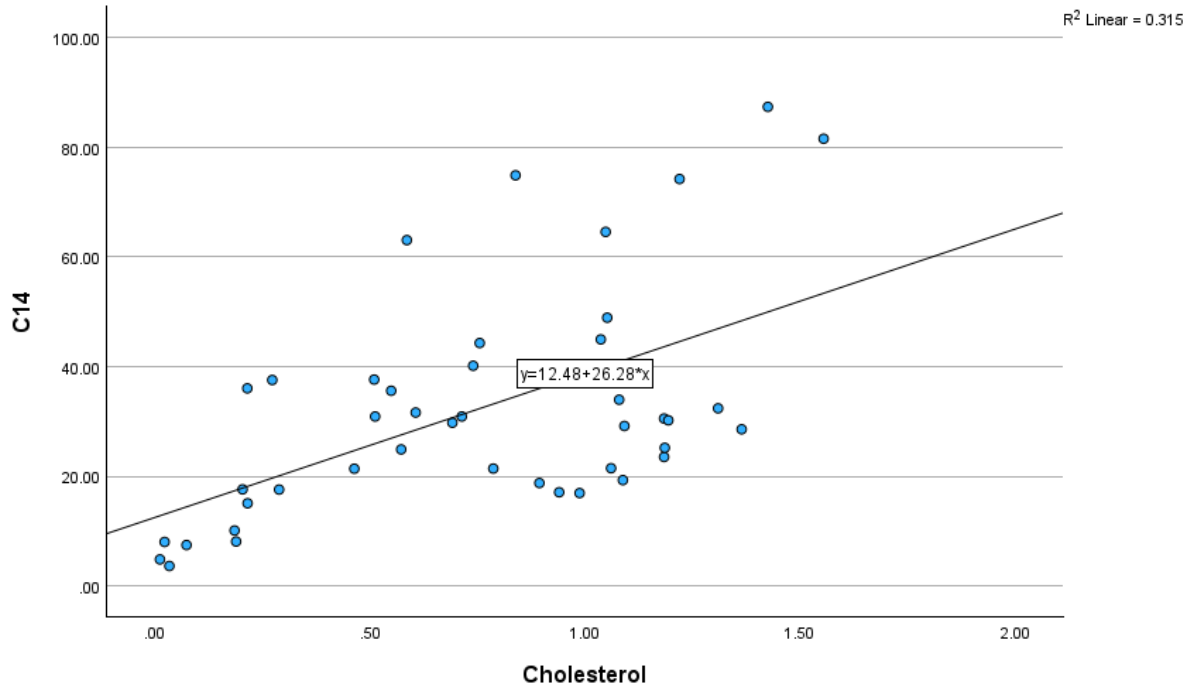
Here it is stated that cholesterol is a marker for krill, because it accounts ”for more than 76% of total sterols in krill (Ju and Harvey, 2004) and is typically lower in concentration in fish.” This conclusion should be better substantiated, as cholesterol is probably the most common sterol in both 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 will provide 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. 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.

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 C14:0 (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, and will adjust our text accordingly.



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 can highlight this in the manuscript.

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 will edit the text accordingly, removing reference to ‘windblown particles’ as we appreciate this is unlikely to be conclusive.

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 will therefore regroup the key inorganic elements in Fig. 4 following the suggestions.

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).

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 propose to delete section 4.1 (line 335ff) and start the next section 4.2 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 will be deleted, as the previous sentence was an oversimplification.

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. Despite this, we can integrate the comments in this reply into the discussion on $\delta^{15}N$ to help improve this study.

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 propose to remove comment on abandonment from this manuscript.

Technical corrections

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

Thank you, this was in error and will be edited in the redraft.

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 will be corrected.

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. We will ensure that this is detailed in the text.

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 1-3) and inorganic (XRF 1-3), 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, 195 1996) to identify the maximum number of statistically significant clusters."

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. We will more clearly and explicitly state that these units were used within down-deposit plots.

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:

When taking biomarker and isotope samples we avoided the margins of the deposit, which were easily deformable. As a result, the oldest and youngest samples lie at 0.5 cm (1833 (1168–2531) cal. yr BP) and 18.25 cm (6389 (6587–6214) cal. yr BP) respectively in the Bayesian model. We here summarise the time interval represented by the biomarker and isotope samples as ~1830 to 6390 cal. yr BP.

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 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). 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 will add this to the text and refer to SI Fig.3.

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

We can delete this sentence and revise. We suggest to replace with a sentence to describe rather than interpret the elemental composition of the gneissic granite bedrock.

Line 322: typo “lotted”

Apologies for the typo. We can change 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 can place rounded ^{14}C dates, rather than actual provided dates in table 1 for both the uncalibrated and calibrated ages. However, we will need to note this in the table caption, to ensure that future remodelling (e.g. with a different/updated calibration curve) will not be hindered. The exact dates (calibrated and uncalibrated) will 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 chose not to rewrite them in the figure caption as there are many (x11), however we will add this text as it was not immediately clear to the reviewer and will fit on the same page. Blue data points are the original measures, with black points the three point moving average. We can make this amendment in the figure caption.

References:

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