CP-2021-134 Replies to the Editor and three reviewers

This document combines the published responses we made to our three reviewers and community comment by Tim Heaton and colleagues. As a supplement to this file we also show our edits in a "tracked changes" manuscript. Throughout the document the original comments are in black and our replies are in blue italics.

CP-2021-134 Editor's comments

I have assessed the Reviewers' and community comments as well as your responses to them and invite you to submit a revised manuscript. Your revised manuscript should include the modifications detailed in your responses apart from one comment that I detail below.

We provide a tracked-changes manuscript to accompany this document, which shows the changes we have made in response to the comments below. All graphics incorporating age model information and all text where dates are cited have been edited.

I agree with Reviewer 2 that your results cannot add information on the role of sea ice in impacting the concentration of atmospheric CO2. I therefore agree with Reviewer 2 that your results cannot "challenge the importance of sea ice for CO2 drawdown", but your results do not either suggest that sea ice contributed to the CO2 drawdown. Therefore, I would suggest to remove "including drawdown of CO2" where you added it in the Conclusions:

"These results challenge existing hypotheses add to a growing body of evidence which shows that seasonal changes in sea-ice extent and the presence of polynyas emphasise multi-year sea ice as a key were likely important drivers of sea ice-climate feedbacks including drawdown of CO2 during glacial stages, ..."

We have removed the text about CO2 as suggested by the Editor.

Unrelated to this, to enhance the understanding of Figure 6 on its own, it could be helpful to add arrows with direction of polynya opening.

We agree and have annotated Figure 6 to indicate the inferred opening of shelf polynyas, alongside the dietary interpretation shown for the geochemical indicators.

Finally, in response to Reviewer 1, I would like to point out that numerical simulations also suggest that enhanced SO upwelling should lower surface d13C (e.g. Menviel et al., 2015, GBC).

We thank the Editor for directing us to this manuscript, which we cite in our revised text.

CP-2021-134 Reviewer 1

The reviewer comments are in black text; our replies are in blue italicised text.

This paper describes a detailed investigation of an ancient deposit of snow petrel oil in Antarctica and how it relates to changes in climate and sea ice conditions during the late Pleistocene. It is a unique record and the authors used a number of analytical methods and proxies to infer changes in petrel diet and a paleoclimate record dating from ~22,000 to 28,000 yrs ago. The multiple methods they used served as a cross-check on the results of each analysis, providing strong evidence to back their conclusions, such as the Cu signature for krill in the diet. The paper is also well written and presented, so I do not have many comments for revising this paper for publication.

We thank the reviewer for their positive comments.

The method for calibrating their radiocarbon ages needs more explanation. The papers they refer to for their delta-R value of 880 +/- 150 yrs are based on penguin dates, and to my knowledge similar corrections based on dates from modern, pre-bomb snow petrels have not been completed. Using penguin corrections may be okay since the diet of the petrel is somewhat similar, but another analysis of radiocarbon corrections based on two additional modern, pre-bomb penguin dates can be found in Emslie (2001, Antarctic Science). This study indicates that a delta-R of 700 +/- 50 yr is more accurate for the Antarctic Peninsula and perhaps the Weddell Sea as well. I'm not sure how much this would change their age ranges for the petrel oil, but it should at least be considered. In addition, the diet of penguins from which corrections are based is not that similar to the diet of snow petrels. Penguin prey are larger size—larger krill, larger silverfish, etc., and we know from other studies that isotope values in krill will change with ontogenetic stage, or size of the krill, and oceanographic conditions (see Polito et al. 2019, doi:10.1002/lom3.10314). This is likely true for squid and silverfish as well and, since snow petrels are feeding on much smaller prey than penguins, it could affect the stable isotope values in the petrels by up to 2.4‰ (or more than the change seen in their samples) as well as their delta-R value.

We agree with the reviewer that there are challenges in the calibration of the radiocarbon ages. The reviewer highlights 2 related concerns: (1) the value of delta-R (ΔR) we chose to use; (2) the use of penguin data in generating that ΔR, and the potential for isotopic differences between penguin prey and snow petrel prey.

In response to (1), the comment to our manuscript by Tim Heaton et al. (cp-2021-134-CC1-supplement) also notes a recommendation to use the ΔR given for Hope Bay in the MARINE20 Reservoir Age database (ΔR 670 $\pm$ 50 ^{14}C years), which draws on the penguin bone data from Hope Bay presented in Björck et al. (1991), and which was applied by Sterken et al. (2012) with a minor error in the calculation. We will apply the recommended ΔR value in our revised manuscript, editing Table 1 and the subsequent figures accordingly (see our reply to Heaton et al.). This change does not lead to significant shifts in our chronology given other uncertainties in the calibration process, as detailed in our reply to Heaton et al.

In response to (2), we acknowledge it is likely that additional calibration uncertainties are introduced by using ΔR values obtained from penguins, for the reasons and published works noted by Reviewer 1. Different ^{14}C signals have been reported in modern shells, seaweeds and selected predator tissues (Gordon and Harkness, 1992). A range of ^{14}C ages was also reported in three samples of post-bomb snow petrel stomach oils (550-800 ^{14}C yr)(Hiller et al., 1995). Further research is warranted to better understand and quantify uncertainties introduced by using penguin bone ΔR: until this is resolved we will apply the recommended values from the MARINE20 database (Heaton et al., 2020).

One weakness of this study, as discussed by the authors, is determining if baseline polynya carbon and nitrogen values changed over time (and very likely did), which in turn would influence the stable isotope values independent of dietary change. One way to test this is by using compound-specific stable isotope analysis so that source and trophic amino acids can be analyzed to determine if a true dietary shift occurred, or if changes in baseline productivity occurred, or both. I am not familiar enough with the stomach oils of petrels to know if the proper amino acids can be extracted and analyzed in this manner, but the authors do not mention this either way. Perhaps their use of multiple proxies helps resolve this issue and if so that should be stated. Their measurements of Cu certainly help show the likely change of krill in the petrel diet over time.

We agree with the reviewer that this is a challenge for our interpretation of the stable isotope data. We presented compound-specific $\delta^{13}C$ measurements from fatty acids as one approach to try to disentangle different contributions to the bulk $\delta^{13}C$ signal, but as both we and the reviewer note, we did not find a clear signal of the baseline changes. We agree that targeting amino acids could provide a solution to this problem (e.g. Johnson et al., 2019; McMahon et al., 2015). We plan to undertake further work in this direction, but it is beyond the scope of this paper.

Another set of data that might help would be from ice cores. In Fig. 6 they present some of the WAIS ice core data, but has DMS been analyzed from these cores? DMS can be a proxy for sea ice extent (e.g., Goto-Azuma et al. 2019, Nature Communications). This would be another independent data set that could help strengthen their results for presence of polynyas and open water, or extensive sea ice in the past.

The oxidation products of dimethyl sulphide (DMS) found in ice cores are methanesulphonate (MSA, exclusively from DMS) and sulphate (SO_4^{2-}, multiple origins). We note that not all phytoplankton are strong sources of DMS (Kaufmann et al., 2010).

Reduced marine biogenic sulphate fluxes at EDML (and other ice core sites) during the last glacial were proposed by Goto-Azuma et al. (2019) to reflect reduced DMS emissions overall, since leads and polynyas might be expected to contribute DMS emissions as observed today. We agree with the reviewer that this would be useful information to include in Fig. 6., acknowledging that the age model uncertainties on the stomach-oil deposits and temporal smoothing on the ice core data limit our ability to link our millennial-scale changes between the two records. A preliminary comparison with the data of Goto-Azuma et al. (2019) is shown below, noting that the $nssSO_4^{2-}$ record is relatively low resolution and that Kaufmann et al. (2010) caution against over-interpretation of millennial-scale changes in this record. In broad terms, however, our stomach-oil deposit lies during an interval of broadly low residual $nssSO_4^{2-}$ indicating a time of low DMS emissions when the $ssNa^+$ record also indicates more extensive winter sea ice.

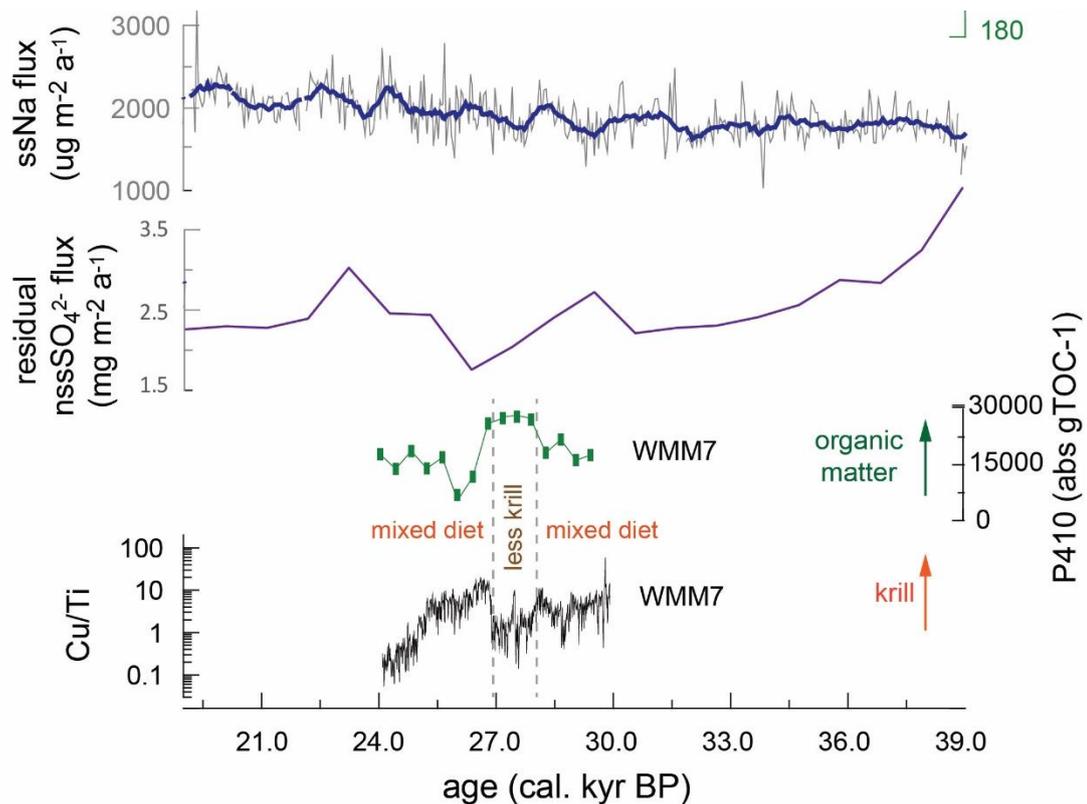



Figure 1. A selection of data from Fig.6 in the original manuscript, adding the residual nssSO$_4^{2-}$ flux calculated by Goto-Azuma et al. (2019) at the EPICA Dronning Maud Land ice core, which records aerosol changes in the Weddell Sea. The age model here is adjusted from our original submission, using the Holocene "no ice" ΔR proposed in the Comment by Heaton et al. (cp-2021-134-CC1-supplement).

Line 545: wouldn't enhanced upwelling increase (enrich) the carbon isotope values?

We are guided here by the observed upwelling of CO$_{2(aq)}$ rich waters from the Circumpolar Deep Water to the surface ocean, which is characterised today (and during the last glacial stage) by lower δ^{13}C than at the surface (Bostock et al., 2004). Our proposal was that a greater influence of low δ^{13}C CDW could have reduced the δ^{13}C of the krill via its prey: this can be clarified in a revised version of the manuscript:

"Alternatively, declining δ^{13}C$_{14:0}$ could reflect enhanced upwelling of CO$_{2(aq)}$ rich but low δ^{13}C circumpolar deep water (e.g. Bostock et al., 2004) during the season of krill production."

Section 4.3 is a bit long, with some repetition from previous sections so I suggest cutting this down a bit.

We will review section 4.3 to identify repetition and delete as needed during the generation of a revised version of the manuscript.

Figures and tables are all necessary for the paper and are well presented.

We thank the reviewer for this positive comment.

References cited in the authors replies:

Björck, S., Hjort, C., Ingólfsson, O., and Skog, G.: Radiocarbon dates from the Antarctic Peninsula – problems and potential, in: Radiocarbon Dating: Recent Applications and Future Potential, edited by: Lowe, J. J., Quaternary Research Association, Cambridge, 55-65, 1991.

Bostock, H. C., Opdyke, B. N., Gagan, M. K., and Fifield, L. K.: Carbon isotope evidence for changes in Antarctic Intermediate Water circulation and ocean ventilation in the southwest Pacific during the last deglaciation, Paleoceanography, 19, 10.1029/2004PA001047, 2004.

Gordon, J. E., and Harkness, D. D.: Magnitude and geographic variation of the radiocarbon content in Antarctic marine life: Implications for reservoir corrections in radiocarbon dating, Quaternary Science Reviews, 11, 697-708, https://doi.org/10.1016/0277-3791(92)90078-M, 1992.

Goto-Azuma, K., Hirabayashi, M., Motoyama, H., Miyake, T., Kuramoto, T., Uemura, R., Igarashi, M., Iizuka, Y., Sakurai, T., Horikawa, S., Suzuki, K., Suzuki, T., Fujita, K., Kondo, Y., Hattori, S., and Fujii, Y.: Reduced marine phytoplankton sulphur emissions in the Southern Ocean during the past seven glacials, Nature Communications, 10, 3247, 10.1038/s41467-019-11128-6, 2019.

Hiller, A., Hermichen, W.-D., and Wand, U.: Radiocarbon-dated subfossil stomach oil deposits from petrel nesting sites: novel paleoenvironmental records from continental Antarctica, Radiocarbon, 37, 171-180, 1995.

Johnson, J. J., Olin, J. A., and Polito, M. J.: A multi-biomarker approach supports the use of compound-specific stable isotope analysis of amino acids to quantify basal carbon source use in a salt marsh consumer, Rapid Commun Mass Spectrom, 33, 1781-1791, 10.1002/rcm.8538, 2019.

Kaufmann, P., Fundel, F., Fischer, H., Bigler, M., Ruth, U., Udisti, R., Hansson, M., de Angelis, M., Barbante, C., Wolff, E. W., Hutterli, M., and Wagenbach, D.: Ammonium and non-sea salt sulfate in the EPICA ice cores as indicator of biological activity in the Southern Ocean, Quaternary Science Reviews, 29, 313-323, https://doi.org/10.1016/j.quascirev.2009.11.009, 2010.

McMahon, K. W., Polito, M. J., Abel, S., McCarthy, M. D., and Thorrold, S. R.: Carbon and nitrogen isotope fractionation of amino acids in an avian marine predator, the gentoo penguin (Pygoscelis papua), Ecol Evol, 5, 1278-1290, 10.1002/ece3.1437, 2015.

Sterken, M., Roberts, S. J., Hodgson, D. A., Vyverman, W., Balbo, A. L., Sabbe, K., Moreton, S. G., and Verleyen, E.: Holocene glacial and climate history of Prince Gustav Channel, northeastern Antarctic Peninsula, Quaternary Science Reviews, 31, 93-111, https://doi.org/10.1016/j.quascirev.2011.10.017, 2012.

CP-2021-134 Reviewer 2

The reviewer comments are in black text; our replies are in blue italicised text.

The manuscript by McClymont and co-authors presents an innovative multi-proxy study in one sequence of stomach oil deposits from Dronning Maud Land, Antarctica, over the ~29-22 ka BP period. Their geochemical and isotopic data suggest changes in the diet of snow petrels, which they relate to changes in summer sea-ice conditions affecting the birds' foraging areas. If the Antartic winter sea-ice edge during the Last Glacial Maximum is relatively well known (Gersonde et al., 2005; Allen et al., 2011; Benz et al., 2016; Lhardy et al., 2021), it is not the case for the summer sea-ice edge. Previous studies (Gersonde et al., 2005; Lhardy et al., 2021) suggested that a tongue of summer sea-ice cover covered the Weddell Sea until 15°E, probably as a result of a stronger transport of sea ice by the Weddell Gyre (Ghadi et al., 2020). However, it is unclear whether this tongue is made of compacted sea ice or not. Here, the sole presence of the stomach-oil deposits argues for spring/summer open waters at foraging distance from the nesting area. This indicates that the tongue did not reach 15°E at very high latitudes (near coastal) or that summer polynyas existed within the sea ice over the 29-22 ka BP period. The present study provides important insight into an almost unknown parameter and is therefore of prime importance.

The manuscript is very well written, well-structured and well-illustrated. The data (XRF core-scanner, FA concentrations and isotopes) are promising and give much more information than the commonly used bulk d15N.

We thank the reviewer for their positive comments.

I however have several concerns with over-interpretation of the data and overall reaching of the manuscript that I would like to be addressed-discussed. I hope that my comments are sensible and will prove useful.

Major concerns

As a non-specialist in fatty acids (FA) I found the proof of concept, summarized in Table 3, a bit weak and vague. A better case for modern FA production and preservation must be done as it is the backbone of this paleo-study. Even though if the main interpretations are drawn from Cu/Ti (XRF data) and chlorins (pigment data) (figure 6). For example, the low C18:1 and C16:1 concentrations in the WMM7 deposits, as compared to modern values, is thought to reflect a "dietary intake" (lines 378-387) different than today. The subsequent paragraph (lines 389-402) try to define the whole spectrum of the FA concentrations in snow petrel preys, but somehow fails to explains the low concentrations of C18:1 and C16:1 in WMM7. Indeed, it is mentioned that krill and fish have high C18:x. Nothing is said about C16:1. There are also other parts where I was a bit lost with FA. Overall, and maybe because there might be little modern data, the reader is left with a lot of uncertainties and with the feeling that the use of FA is stomach oil deposits is very tentative.

The use of FA in stomach-oil deposits is indeed very tentative, and in our manuscript (and in our previous work by Berg et al., 2019) we highlight that there are several potential controls over FA distribution including dietary sources and post-depositional alteration (Section 4.1). Table 3 was intended to highlight our guide for interpreting the stomach-oil deposits, since as the Reviewer also notes for lines 389-402, there is a range of literature examining FA distributions in a range of snow petrel prey but few studies are from our region or assess all snow petrel prey in a systematic way, generating uncertainties as we here (and in Berg et al., 2019) discuss. We were careful not to over-interpret the data given these uncertainties,

and Table 3 was an attempt to tease out the main signals we use for interpretation here, hence the note in the caption "The prey biochemistry information is used as a framework to interpret the chemical signatures".

Our suggestion that a dietary intake drives the FA signals was outlined in lines 378-387 as a result of the similarities of our FA distributions to some modern snow petrel stomach oils and prey, and Holocene stomach-oil deposits. We are actively pursuing a better characterisation of snow petrel stomach oil FA in modern birds and in latest Holocene deposits, to better understand the relative influence of diet and post-depositional alteration. To address the reviewer concerns about the tentative nature of the work, we could end this paragraph with the following addition (underlined) to the existing sentence:

"Acknowledging these uncertainties in the role played by location, prey FA signals, and post-depositional alteration, we here suggest that the fatty acid signatures in WMM7 primarily signal a dietary intake, rather than variable preservation."

The reviewer expresses concern that we do not discuss C16:1 nor C18:1 (lines 389-402). The previous paragraph noted the possible influences over C18:1. We note here that our text in lines 389-402 refers to C16:0 not C16:1. In this paragraph, we explore the range of key FA recorded in snow petrel prey and our stomach-oil deposit (using Table 3 to summarise the complexities of this section). We note (line 397-398) that C16:0 has a mixed signal from multiple sources. As a result, we did not use this FA alone to infer dietary changes since it could reflect different contributions from krill, fish and squid (in the later sections we note where we think e.g. krill could have been contributing, line 419-420). We can clarify the sentence here with the following addition (underlined):

"The C16:0 thus has a mixed origin from krill, fish and squid in contrast to C14:0 (krill) and C18:x (fish), and thus cannot be used in isolation as an indicator of diet."

The WMM7 deposits is structurally composed of three units, which is confirmed by cluster analysis performed on XRF core-scanner data, especially Cu/Ti, Br/Ti and S/Ti. Authors attribute these units to different foraging and diets, which they try to support with organic data (FA and pigments). I however disagree with the description-interpretation of many records. Indeed, when looking at figure 4, it is clear that the cluster analysis conducted on organics is only driven by variations in pigments (P410 and P435 define units O3-O1). All other records show either no temporal differences (FA %) or high variability throughout the sequence with no relation to the units (C/N, FA ratios). The same is true for figure 5 in which all records appear very noisy. The authors nonetheless mention that many of these records bear differences between the three units (lines 415-433; lines 455-457) and their descriptions of the records do not fit what is observable. Probably because they based their descriptions on the cluster analyses, which are driven by specific records (not all of them). However, a simple ANOVA would show that the values in unit II and are not statistically different than the values in units III and I for FA%, FA ratios, FA d13C and C/N.

We guided our discussion of the data by the outputs of the cluster analysis using the broken-stick model, which by its nature identifies clusters of similar samples and the point at which those clusters are no longer statistically significant. We want to reiterate here that we do not exclusively use the clusters (and the lithological units) to interpret our data. Our discussion is structured by cluster, since they are the large-scale features of the deposit, but we continue to describe and discuss the evolution of the data through time including within units e.g. in lines 417-418 when we note that the FA likely show decreasing contributions of krill through time within Unit I.

We have performed a Kruskal-Wallis test on the organic indicators, to test the null hypothesis that samples taken from Units I, II and III were taken from populations with the same median. The Kruskal-Wallis test is advised rather than a one-way ANOVA test because the samples sizes were unequal between Units (Hammer et al., 2001). The

Kruskal-Wallis test gave a p-value of 2.116^{26} and a statement that "there is a significant difference between sample medians", confirming the result of our cluster analysis. Given the reviewers concern that our analysis might be biased by the presence of the pigment data, we also ran the same statistical test on the fatty acid % alone: this showed a smaller but still significant difference between sample medians ($p=1.313^{-15}$). Thus, although the variations in fatty acid composition are harder to visualise on our graphics, they are still recording changes through the deposit which can be differentiated when examined collectively.

For example, author state lines 415-417 "Between 28.8-26.8 ka (~Unit III) elevated Cu/Ti and C14:0 contributions (low C16:0/C14:0 and C18:0/C14:0) identified krill as an important component of snow petrel diet, but likely decreasing through time". However, C16:0/C14:0 and C18:0/C14:0 ratios appear identical, both in term of absolute values and point-to-point variability, in between the three units. Similarly, there is such a high variability in the FA d13C data (Fig 5c) that it is difficult to see any correspondence between records (lines 455-457) and any trend (lines 418-420), defined herein on 2-3 points. Although being a clear improvement over bulk d15N, I think that authors ought to be more cautious in (over)interpreting their FA% and FA d13C data.

The text the reviewer cites here shows our exploration of the data within the Units, but we did not use the FA data to define those units. We agree that the FA and C/N data do not strictly follow the 3 unit structure, and so we describe their trends through time within each unit in the Discussion, or note where there is support between proxies e.g. low Cu/Ti in Unit II has some support from declining C16:0/C14:0 in terms of a reduced krill input (Fig. 4).

We disagree with the reviewer that the C16:0/C14:0 and C18:0/C14:0 ratios are the same in Unit III: both are declining moving upwards through Unit III (supporting our note that krill contributions were decreasing through time), but C16:0/C14:0 is ~1.75 and C18:0/C14:0 is ~0.3. We note that we should expect to see some differences between these two ratios, since we are comparing C14:0 ("krill") to C18:0 ("fish") or C16:0 ("mixed" source as noted above).

We agree with the reviewer that there is high variability in FA $\delta^{13}C$ which makes correspondence with other records difficult to assess. Although we tried to focus on shorter-term oscillations in our text, we can insert the following text (underlined) to the start of our FA results description (line 326 in the submitted manuscript):

"No long-term trends in fatty acid d^{13}C are observed through WMM7: several short-term oscillations are observed instead (Fig. 5c)."

Authors may consider using SIZER software (Chaudhuri and Marron, 1999) to check whether transitions between units in relevant records are significant.

We have noted above that both the broken-stick model used in our cluster analysis (and our subsequent Kruskal-Wallis tests of unit differences) confirm that there are significant differences between Units/clusters. Whilst SiZer may allow an independent assessment of trends through time, we are concerned that for many of the records the low number of data points (≤15) would make this analysis difficult, meaning that only the XRF data are likely to be compatible, but relying on XRF data is expressed as a concern in the next reviewer comment.

In conclusion, only pigments and XRF ratios, including Cu/Ti, appear to vary according to the deposit units. Other records are too noisy to be robustly interpreted. I however do not think that this alters the main interpretations about the snow petrel diets and foraging habits. However, one may question the utility of the FA data in the present study, especially as

additional tests on individual records would be necessary to ascertain that values are significantly different in each unit.

We agree that the strongest signals come from the XRF and the pigments, with supplementary information provided by the other proxies. Indeed, we focus our attention on these records for our climate summary (Fig. 6), because they show that snow petrel diet changed through time, which we seek to understand.

The authors state several times that "Our results challenge hypotheses that the development of extensive, thick, multi-year sea-ice close to the continent was a key driver of positive sea ice-climate feedbacks during glacial stages". If I understood well, the rationale behind this statement is that polynyas within LGM summer sea ice would have allowed strong outgassing of CO2 to the atmosphere. This would have reduced the impact of Antarctic sea ice onto the carbon partitioning between the ocean and the atmosphere. Authors mainly refer to two old publications, Stephens and Keeling (2001) and Morales-Maqueda and Rahmstorf (2002), to support this statement. However, it is worthnoting that there is no sea-ice seasonality in S&K2001 who prescribed a fixed sea-ice cover, probably the LGM winter sea ice defined by CLIMAP (1976, 1981). So obviously, any polynya in such a high sea-ice cover (maximum winter sea-ice extent) would led to CO2 outgassing. There is similarly no seasonality in MM&R2002, but their representation of winter sea-ice cover was closer to geological evidences (Burckle et al., 1982; Crosta et al., 1998). Because of the presence of leads within the winter sea ice, the direct impact of sea ice on atmospheric CO2 (ice capping reducing CO2 outgassing) was reduced compared to S&K2001. Here, the new data from WMM7 deposits suggest the presence of SUMMER polynyas off Droning Maud Land when LGM sea ice has already retreated from its winter mean extent of 35-40 million of km2 to its summer mean extent of 10 million of km2, thus exposing a large surface of open ocean in which CO2 outgassing can take place. For this reason, I doubt that removing few thousands of km2 of sea ice, if polynyas were present, would have changed anything to the CO2 balance. At least, through the ice capping process. More recent hypotheses on the control of Antarctic sea ice on CO2 involve less vertical mixing either by subsurface stratification (Sigman et aL, 2021) and/or deep stratification (Galbraith and Delavergne, 2018; Marzocchi et al., 2019). Polynyas could potentially have enhanced deep stratification if sea-ice formation was sustained during the summer season and that salt were advected to the sea-floor without promoting vertical mixing (brines hypothesis in Bouttes et al., Bouttes et al., 2011). Which is not proved. Additionally, one may question how sea-ice formation in such polynyas compares quantitatively to the ~30 million of km2 of sea ice formed seasonally to reach back the winter extent.

In conclusion, I would tame the term "challenge" and the overall reaching of the manuscript on this aspect. It is far beyond the science presented therein.

We thank the reviewer for the exploration of the complexities of the glacial sea-ice environment and its feedbacks, including nuances of the models which we cited. We propose to update our citations and text accordingly for the final paragraph of the discussion (where we outlined the impact of polynyas on the previous suggestions of the sea-ice cap mechanism, lines 577-587):

"Polynyas may also have affected the strength of the sea ice/climate feedbacks during MIS 2: introducing only 2-8% open waters into the LGM sea-ice pack (compared to 10-20% for winter today) reduces the Southern Ocean contribution to the LGM CO_2 draw-down from ~80% to 15-50% via enhanced ocean-atmosphere CO_2 transfer (Morales Maqueda and Rahmstorf, 2002). In contrast, increasing brine formation during sea-ice formation transfers dense water and carbon to the deep ocean (Bouttes et al., 2011), and could have been enhanced by polynya formation (Paillard and Parennin, 2004). Brine formation over the continental shelves or at the ice-sheet margin, has been

proposed as <s>would have been</s> conducive to formation of dense glacial AABW and the associated deep-ocean storage of CO_2 (Paillard and Parrenin, 2004; Adkins, 2013; Adkins et al., 2002). It is currently difficult to evaluate the relationship between the proposed variability in polynya positions and the millennial-scale oscillations in atmospheric CO_2 (Fig. 6), in part because it is unclear whether the variations in surface ocean productivity observed in the stomach-oil deposits are related to changes in the efficiency of the biological pump and CO_2 drawdown (e.g for Unit II with high chlorin inputs). Furthermore, the relative impact of polynyas compared to other Southern Ocean carbon cycle processes is unclear, given that a combination of brine formation related to sea ice growth, changes in deep ocean stratification, and iron fertilisation of subantarctic waters is invoked to account for the observed CO_2 drawdown (e.g. Bouttes et al., 2011; Marzocchi and Jansen, 2019; Sigman et al., 2021). The relative impact of the polynyas between winter and summer seasons during the last glacial stage is also uncertain, since the large changes in sea-ice extent (Fig. 1) will likely also have affected the air-sea gas exchange on seasonal timescales. Our age model uncertainties also limit confident correlation between WMM7 and the ice core CO_2 record, so that further testing is required to explore whether polynya development along the DML coastline <s>impacted</s> contributed to observed changes in atmospheric CO_2."

In the conclusions, where the reviewer is concerned about the use of the term "challenge", we can alternatively state:

"These results <s>challenge existing hypotheses</s> add to a growing body of evidence which shows that seasonal changes in sea-ice extent and the presence of polynyas <s>emphasise multi-year sea ice as a key</s> were likely important drivers of sea ice-climate feedbacks including drawdown of CO_2 during glacial stages, ..."

Minor comments

Throughout the text: Harmonize sea ice (when a noun) and sea-ice (when an adjective). I found "sea ice" and "sea-ice" along with "sea-ice cover" and "sea ice cover".

We will thoroughly check.

Lines 88-89: I may have misunderstood the sentence, but crustaceans are invertebrate (not vertebrate)

This is a typo; we will correct it.

Line 117: Please give more evidence for the absence of hiatuses.

We can add to this sentence (addition underlined):

"No hiatuses were visible in the stratigraphy, which would be indicated by breaks in the structure or visible sediment which would be deposited and concentrated during an interval when stomach oils were not being deposited. The linear age model (Fig. 2) also indicates continuous accumulation".

Lines 139-141: A greater deltaR during the LGM, as evidenced for the SO open ocean (Siani et al., 2013; Gottschalk et al., 2020), would make the age of the sequence younger by few hundreds of years. But I doubt that this has any implication on the interpretations as it would still be dated from around the LGM.

We have addressed this concern in response to the comment by Tim Heaton and co-authors (cp-2021-134-CC1-supplement). A higher delta-R does make the overall deposit age younger, but does not change the relative sequence of events described here.

Line 227: I think that PAST as a fixed number of degrees of freedom, which might not be sufficient to deal with the autocorrelation of the series (Bretherton et al., 1999). However, this may not be very important here given the high score on PC1.

We have been unable to isolate the details of the PAST3 approach to addressing auto-correlation, but as the reviewer suggests this may not be problematic given the high score on PC1.

Line 253: Does the fact that there is no trend in Fe/Ti and Si/Ti mean that Fe and Si are mainly of minerogenic origin. The very high absolute values in Fe cps and the high score on PC1 argue for that. Are Fe/Ti and Si/Ti useful?

In this deposit the absence of a trend in Fe/Ti and Si/Ti suggests a mainly minerogenic origin since Ti is a minerogenic indicator. To ensure that this message is clear we can add text (underlined) to our original statement

"There were no clear down-core trends in Fe/Ti and Si/Ti (Fig. 3a,b) indicating a dominant minerogenic source for Fe and Si".

We think that it is still useful to show this data, because Fe has been found in krill (Palmer Locarnini and Presley, 1995) and Si could be an indicator of diatom abundance, even though in this particular instance their source can be shown as mainly minerogenic.

Lines 424-434: I do not agree that unit II shows increasing C16:0/C14:0 and C18:0/C14:0 values. Similarly, I do not agree that unit II shows a decrease in d15Nbulk. I did not get what are the "prey with a phytopkankton-dominated diet" if not the krill. But low Cu/Ti values argue for a lower krill preying.

We are describing a trend within Unit II i.e. the increase in the two FA ratios from low values at the base of Unit II to higher values at 26.0 ka (both ratios have a peak within Unit II at this point) (Fig. 4.). The early part of Unit II also sees $\delta^{15}N$ decreasing from ~12 ‰ at the base of Unit II to a minimum at ~26.5 ka. Since the reviewer notes concern in an earlier comment about how the FA data is described, we can clarify this sentence by shifting the FA data to the end:

"Low Cu/Ti ~~and increasing C16:0/C14:0 and C18:0/C14:0~~ indicate a prolonged (~1100 yr) interval where krill was not a major component of snow petrel diet (Figs 3,4) supported by increasing C16:0/C14:0 and C18:0/C14:0 in the early part of Unit II."

With reference to the "phytoplankton-dominated diet" in the prey: this means that the direct prey of the snow petrels are dominated by a phytoplankton diet, which is then reflected in the prey tissues, but this does not need to be krill given other herbivores and omnivores in the Antarctic ecosystem.

Lines 455-457: Not very evident from Fig 5c. Concomitant peaks and lows.

Our text indicated that from 24.2-23.5 ka (the uppermost 3 peaks in the FA d13C data), the FA d13C "fluctuate in parallel". This is visible in Fig. 5c: all FA are low at the first data point, increase to the next data point, then all decrease again for the uppermost point. However, we recognise that the magnitudes of change in each indicator are not the same, and can use the term "concomitant".

Lines 526-529 & 549-550: How could there be polynyas over the shelf when the ice sheet covered it all (figure 12 in Hillenbrand et al., 2014?

This specific point is addressed in lines 563-570 and in our delineation of ice-sheet extent in Fig. 1. Two scenarios are proposed for ice sheet extent during the LGM by Hillenbrand et al. (2014), whereby the ice may have been close to the modern (allowing access to the continental shelf) or at the continental shelf edge. We note that our data may in fact suggest the more restricted scenario of Hillenbrand et al. (2014) is feasible (lines 565-567).

Lines 566: Mackintosh et al., 2014, deals with east Antarctica from 30°E to 140°E, not the Weddell Sea sector.

This citation should be Hillenbrand et al. (2014) and will be corrected in a revised manuscript.

Figures and tables

Fig 3: As the XRF data are presented on a log scale, the variations do not appear very important and it is sometimes difficult to see differences between the three units. And even for Cu/Ti, differences appear very small on a log scale.

We agree that the presentation of element ratios on a log scale sometimes makes it difficult to see differences between the units since it smooths some of the variability (we showed a comparison in our Figure E1). We did this to focus our attention on the longer-term trends, but can present the data without the log scale in the main body of the manuscript in a revised Figure 3:

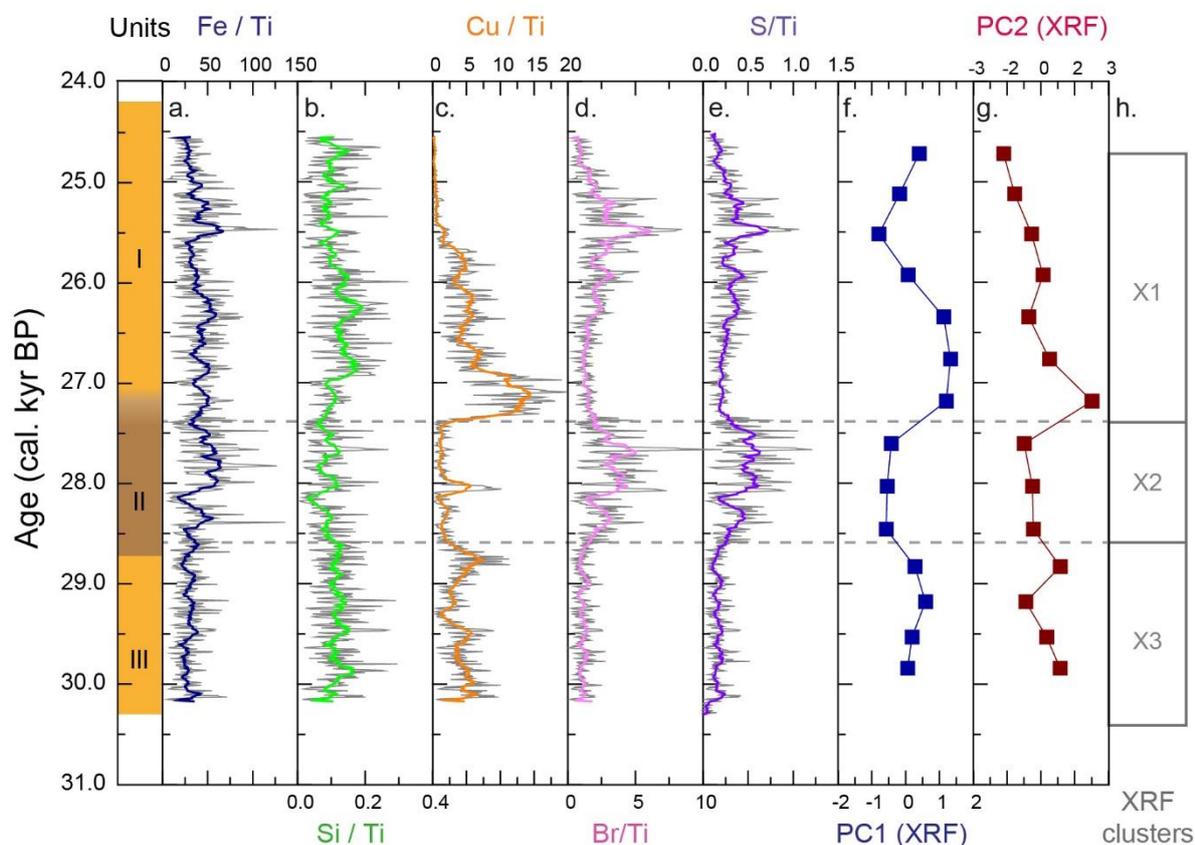



Fig. 4: It is clear that the O clusters are here driven only by the pigment records. The FA and FA ratio records do not follow the deposit units (I, II and III) nor the organic clusters (O1, O2 and O3), and are, as such, not discriminatory for the cluster zones and subsequent interpretations.

We addressed this concern above.

Fig. C1: the orange square at ~26.2 ka BP does not fit with the peak in Cu/Ti observed at that time. This is the only one that is offset from the raw and smoothed curves. It shows a low bin when raw and smoothed values are as high as in units III and I. Weird.

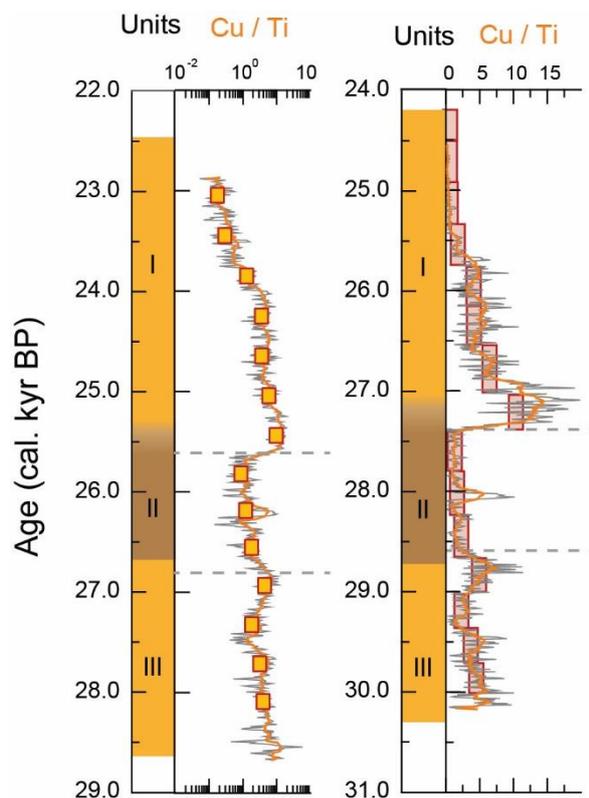
This is a function of the plotting format: the orange square (the re-sampled data) spans more of the data than the size of the symbol implies e.g. the data point of concern here includes both the peak in the un-smoothed Cu/Ti but also the troughs before and afterwards. The squares actually represent continuous sampling through the sequence, without gaps, but our plotting software shows them as separated and centred on the mid-point of the re-sampled part of the sequence. We can replot this graphic with the heights of the square boxes increased to ensure that they form a continuous sequence if this would improve the message in Fig. C1:

Comparison of original Cu/Ti smoothing display (left) shown in Fig. C1 and the result of extending the boxes to span the smoothing window (right). As the re-sampling was undertaken by depth, the height of the bars varies through time. Note that the revised graphic is also plotted on the "Holocene no ice" radiocarbon calibration suggested by Heaton et al. (cp-2021-134-CC1-supplement).

Table 2: PCA is driven by only one element, Fe. This might be because raw data have been used and that Fe cps are much higher than any other element cps. The use of log data or,

even better, normalized data would probably reduce the overwhelming statistical importance of Fe. Other elements may appear significant too.

Our initial exploration of the data by PCA (as shown in the manuscript) sought to identify which components accounted for the overall geochemical variations in the sequence, so we did not normalise them, and used the variance-covariance approach recommended by Hammer et al. (2001).

When we run the same analysis on the normalised XRF data (i.e. all elements expressed as X/Ti) we still get the same dominance in PC1 by Fe, then Cu and then Ca (see Table below). PC2 is also dominated by Cu (negative loading) then Ca (positive loading). PC1 on the normalised data accounts for 84% of the variance and PC2 accounts for 12%.

Table 2 from the main manuscript, showing principal component loadings of the original XRF data (left), and the results when all element data are normalised to Ti (right).

Principal component loadings (no corrections)	PC 1	PC 2	PC loadings of element / Ti data	PC 1	PC 2
Si	0.003	0.001	Si	0.001	0.004
P	0.001	-0.002	P	0.002	-0.004
S	0.004	-0.009	S	0.008	-0.019
Cl	0.013	-0.034	Cl	0.024	-0.010
K	0.028	-0.051	K	0.034	-0.072
Ca	0.162	-0.165	Ca	0.1747	-0.275
Ti	0.026	-0.019	Ti	-	-
Cr	0.004	-0.003	Cr	0.006	-0.010
Mn	0.010	-0.006	Mn	0.010	-0.010
Fe	0.961	-0.181	Fe	0.960	-0.130
Cu	0.217	0.962	Cu	0.207	0.925
Zn	0.008	-0.005	Zn	0.014	-0.021
Br	0.019	-0.008	Br	0.053	-0.134
Rb	0.002	-0.004	Rb	-0.000	-0.000
Sr	0.041	-0.091	Sr	0.053	-0.130
Zr	0.009	-0.005	Zr	0.010	-0.020

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CP-2021-134 Reviewer 3

The reviewer comments are in black text; our replies are in blue italicised text.

This is an interesting, original and well written study.

We thank the reviewer for their positive comments.

My main concern is the distribution of krill in coastal waters during the breeding season of snow petrels. The table 3 and the discussion section (lines 515-530) state that snow petrels feed less on krill near the continental shelf. However authors only consider the main krill species, Euphausia superba : "Post-larval krill are mostly oceanic (Atkinson et al., 2008)" and "adult krill move to deeper waters for egg development (Nicol, 2006) (lines 520 -525). Another krill species, Euphausia crystallorophias, known as ice krill, are closely associated to sea ice, feeding on diatoms under the ice and living in coastal waters, where it replaces the more oceanic E. superba. High densities of E. crystallorophias can be found in coastal polynya during the Antarctic summer (La, et al. 2015) and can be preyed by snow petrels (Ridoux & Offredo 1989). This should be considered to avoid shortcuts in the discussion (lines 525 "the observed shift in fatty acid and element profiles in Unit II suggests that fish became more important to snow petrel diet, suggesting that polynyas had opened up over the continental shelf between 26.8-25.7 ka. We hypothesise that these shifts in foraging habitat reflect changes in sea ice conditions, by either influencing prey distributions or access to surface waters for feeding". The discussion should thus include the distribution and ecology of E. crystallorophias for a more nuanced picture of the link between dietary changes (fish vs krill) and foraging habitat (pelagic vs neritic).

We agree with the reviewer that the presence of Euphausia crystallorophias in coastal waters could contribute to the signal of krill observed in our stomach-oil deposits, given their high densities in coastal polynyas (La et al., 2015). However, our argument for the shift towards a more shelf-dominated diet comes from the evidence that the proportion of fish in snow petrel diet becomes very high in shelf waters, even when E. crystallorophias is present (e.g. Ridoux & Offredo, 1989 state 95% fish and 2% euphausiid by mass in their study, with both E.superba and E. crystallophorophias present). Ridoux & Offredo (1989) describe the snow petrels as being "distinctive in their preference for fish" (p.142), and confirm that this dietary preference has been observed elsewhere (p.143). We also noted as a guide for Table 3, that where krill has been observed in higher proportions of snow petrel diet, this has occurred when feeding beyond the continental shelf (lines 517-519 and references therein).

We can clarify our text to highlight the potential availability of E. crystallorophorias in shelf waters, but also to flag that this species is not known to be a significant contributor to snow petrel diet. In response to the reviewers comment we also explored our data further to see if we could identify a signal of E. crystallophorias. There have been fewer studies of lipids in E. crystallorophorias for us to refer to, however some common characteristics have emerged and are noted below. In response to the reviewers comments we propose to adjust our text as follows (line numbers refer to those in the submitted manuscript, underlined text shows our additions):

- *Table 3: we will ensure that Antarctic krill (E. superba) is stated here. We can add details of E. crystallophorias biochemistry to the line referring to the Continental shelf environment.*
- *(line 520-525): "Post-larval Antarctic krill are mostly oceanic (Atkinson et al., 2008)" and "adult Antarctic krill move to deeper waters for egg development (Nicol, 2006) (lines 520 -525)".*
- *(lines 394-397): "The dominant fatty acids in WMM7 are consistent with the main snow petrel prey (Table 3): Antarctic krill (Euphausia superba, high abundances of $C14:0$, $C16:0$, sometimes $C18:1$ (Cripps et al., 1999; Raclot et al., 1998)), squid (dominated by $C16:0$, plus longer-chain fatty acids $C20:5$ and $C22:6$)(Raclot et 395 al., 1998)), and both notothenoid and myctophid fish (high concentrations of $C18:1(n-9)$, $C16:0$, and several mono- and poly-unsaturated $C20$ and $C22$ fatty acids (Imber, 1976; Raclot et al., 1998; Mayzaud et al., 2011)). Although only recorded as a minor (~2%) contributor to snow petrel diet (Ridoux and Offredo, 1989), in coastal waters ice krill (Euphausia crystallorophorias) is found in very high densities (La et al., 2015), characterised by high abundances of the $C18:1w9$ and $C16:0$ fatty acids as well as $C14:0$ and $C16:0$ alcohols (Ju and Harvey, 2004; Bottino, 1975). The $C16:0$ fatty acid thus has a mixed origin from krill, fish and squid in contrast to $C14:0$ (Antarctic krill) and $C18:x$ (fish, ice krill)...."*
- *lines 415 onwards: we will ensure our text refers to "Antarctic krill" or "ice krill" where it is appropriate to differentiate these two.*
- *Lines 436-441: "The similarity in the trends between d13C16:0 and d13C18:1 (Fig. 5c) confirms an increased importance of prey with C18 fatty acids in their tissues between 26.8-24.7 ka, consistent with incorporation of a phytoplankton signal in predator tissues, through their consumption of copepods, squid or fish, which can occur with minimal alteration (e.g. Lee et al., 1971). Today, elevated C18:1 (and C16:0) fatty acid contents have been recorded in E. crystallophorias (ice krill) (Ju and Harvey, 2004; Bottino, 1975), which inhabit coastal waters (La et al., 2015). However, ice krill remain a very minor component of snow petrel diet even in these settings, where fish dominate (Ridoux and Offredo, 1989). Elevated (~20%) C18:1 through Units I and II is consistent with an increased contribution of fish to the snow petrel diet between 26.8-25.7 ka, but identifying the particular fish species is more challenging."*

References :

La, et al. 2015. High density of ice krill (Euphausia crystallorophias) in the Amundsen sea coastal polynya, Antarctica" Deep sea research Part I: Oceanographic Research Papers, 95,75-84

Ridoux, V., & Offredo, C. (1989). The diets of five summer breeding seabirds in Adélie Land, Antarctica. Polar Biology 9(3), 137-145.

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Comment by Tim Heaton et al. (cp-2021-134-CC1-supplement.pdf)

The reviewer comments are in black text; our replies are in blue italicised text.

We congratulate the authors on a novel approach providing a first step to answering a key question regarding the past extent of sea ice in the polar regions (in this case Antarctica). As authors of the Marine20 curve, this is a topic which is of direct interest to us since, as noted by MyClymont et al. (under review), the extent and location of this ice makes a considerable difference to air-sea gas exchange and hence the concentration of ^{14}C in the surface ocean in polar regions. Currently very little is known about the extent of this ice, making calibration of ^{14}C from marine samples challenging.

We thank the comment authors for their positive comments.

In our comment, we restrict our attention to the calibration of polar ^{14}C samples. In brief our contribution consists of:

a) We would no longer recommend the use of Marine13 or any earlier marine product for any ^{14}C calibration – the statement in the Marine20 paper (Heaton et al., 2020) regarding Marine20 being unsuitable for polar calibration applies equally, if not more so, to the Marine13 curve (Reimer et al., 2013).

b) Calibration of ^{14}C marine samples from polar regions, from pre-Holocene time periods, is complicated since, at high-latitudes, the value of ΔR during glacials is unlikely to remain constant, or similar to the values seen during the recent past and Holocene. This is primarily due to localised sea-ice and regional winds during cold stadials.

c) Calibrating polar ^{14}C marine samples from cold stadials using any Marine calibration curve (Marine20 or any earlier product) and an estimate of ΔR based on samples from the recent past is likely to lead to bias and overconfidence. The true calendar age will likely be more recent than the calibrated age estimate generated using a constant ΔR based on samples from the recent past.

We agree with these three points, and respond to the detailed comments below.

We propose, for those wishing to calibrate polar ^{14}C marine samples, using two different values of ΔR – one representing a low ^{14}C -depletion scenario (corresponding to little sea-ice) similar to the Holocene; and the other a higher ^{14}C -depletion scenario (corresponding to higher levels of sea-ice). The true calendar ages of the samples should hopefully lie between the calibrated age estimates obtained in these two extreme scenarios.

We discuss the impact of this recommendation in the text below.

In this comment we discuss how one might adjust ΔR for the specific location of these snow petrels. We are currently in the process of providing a short note to describe to wider users how this adjustment might be done for any sample. In our comment, we introduce the term ΔR_{20} to refer to the value for use with the Marine20 calibration curve (and ΔR_{13} the value for the Marine13 curve).

Calibration of Marine ^{14}C Samples in Polar Regions

The aim of the marine calibration curves (such as Marine20 and earlier products) is to provide a "best estimate" of the global-scale surface water ^{14}C concentration that has factored out the effect of large-scale carbon cycle changes (e.g., changes in atmospheric

[14]C, CO_2, ocean circulation, ...). The Marine20 curve should do this more accurately than Marine13.

However, if there are significant localised effects in the region from which the sample arose, such as polar sea ice, these will cause extra localised ^{14}C depletion. These effects mean ΔR_{20} will be significantly larger pre-Holocene, compared to post-Holocene, in polar regions.

Location specific estimates of the overall oceanic ^{14}C depletion (i.e. total MRA) are available under fixed carbon cycle and climate scenarios using the LSG ocean general circulation model (Butzin et al., 2020) at https://doi.pangaea.de/10.1594/PANGAEA.914500. These LSG estimates can be used for calibration – by adjusting the IntCal20 curve – however it is important to note these LSG scenarios are not transient, in terms of climate, and so calibrating against any individual scenario is still likely to lead to overconfidence.

We suggest that to calibrate marine ^{14}C samples from polar regions, one uses Marine20 but considers two extreme scenarios: one accounting for minimal further polar ^{14}C depletion for which the ΔR_{20} is small; the other for maximal further polar ^{14}C depletion for which ΔR_{20} is large. Calibrating against Marine20 under these two scenarios should provide bracketing calendar ages for the true age of the sample. We select these two ΔR^{20} values ($\Delta R^{\text{Hol}}{}_{20}$ and $\Delta R^{\text{Icy}}{}_{20} R$) based upon the latitudinal averages of the LSG model under the PD (present day) and GS (glacial) scenarios.

We thank the authors for this suggestion, and discuss the impact of their recommendation in the text below, and make changes to Table 1 to demonstrate the impact of these two calibration scenarios.

1) *Low-depletion ΔR – assuming no regional effect of sea ice cover*

Estimate a $\Delta R^{\text{Hol}}{}_{20}$ (so-called since it is based on Holocene data) based on the Bjorck et al. (1991) pre-nuclear weapons testing samples. Then calibrate using this $\Delta R^{\text{Hol}}{}_{20}$ estimate against Marine20. You will have to update your $\Delta R^{\text{Hol}}{}_{20}$ to match the Marine20 curve. In your case, the correct $\Delta R^{\text{Hol}}{}_{20}$ to use is 670 ± 50 ^{14}C yrs (updated to correspond to Marine20).

Calibrating under this scenario will provide a calendar age estimate assuming there is no regional sea ice and so there is no further localised depletion. Our calculations suggest that, using Marine20 and a $\Delta R^{\text{Hol}}{}_{20}$ of 670 ± 50 ^{14}C yrs, then, e.g., 25980 ± 133 ^{14}C yrs BP will calibrate to 28,680 cal yr BP (median, with a 2σ interval of [28300, 29000] cal yr BP).

We provide an updated Table 1 where this calibration has been applied.

2) *High-depletion ΔR – including an effect for regional polar sea ice causing further*

localised depletion

The calendar ages obtained above in the low depletion scenario (using a Holocene based $\Delta R^{\text{Hol}}{}_{20}$) are probably too old (biased). There is likely further local ^{14}C depletion due to the sea ice, especially around the LGM.

To include the effect of the sea ice in your region, and get an idea of the likely bias, we can compare the regional LSG and global Marine20 estimates. This suggests that, during the last glacial period, there might be up to c.a. 1800 ^{14}C yrs additional ocean ^{14}C depletion at a latitude of 70ºS. This estimate is based upon a latitudinal average of the difference between Marine20 and the LSG GS scenario (having shifted the LSG so that its PD scenario aligns with Marine20 in the Holocene).

To approximately model the effect of this potential level of additional marine ^{14}C depletion then you boost your ΔR_{20} accordingly, i.e., use ΔR^{GS}_{20}= 670 + 1800 = 2470 ^{14}C yrs. Our calculations indicate that, using this value of ΔR^{GS}_{20} and Marine20, 25980 +/- 133 ^{14}C yrs BP will calibrate to 26,920 cal yr BP (median, with a 2σ interval of [26500, 27200] cal yr BP).

We provide an updated Table 1 where this calibration has been applied.

Proposed new Table 1 for the manuscript:

Table 1 Radiocarbon dates and calibrated ages. All ^{14}C analyses were performed on bulk samples at CologneAMS, Germany. COL3022 was previously published (Berg et al., 2019). All calibrations to calendar ages used MARINE20 (Heaton et al., 2020). To explore the likely range of impacts of sea ice on our ^{14}C calibrations, we first apply the nearest Holocene ΔR of 670 ± 50 yr (Björck et al. 1991) which assumes no sea ice at WMM7 ($\Delta R^{no\ ice}$). Calibration assuming enhanced sea ice cover, as suggested for the last glacial stage, is undertaken by adding 1800 yr of additional ocean ^{14}C depletion as suggested by Heaton et al. (2020; 2021).

Depth (mm)	Unit	AMS Lab ID	Median Age (^{14}C yr BP)	+/- (^{14}C yr BP)	Calibrated age (cal. yr BP) MARINE20, $\Delta R^{no\ ice}$ 670 ±50 yr	Calibrated range (2σ)	Calibrated age (cal. yr BP) MARINE20, $\Delta R^{sea\ ice}$ 2470 ±50 yr	Calibrated range (2σ)
0	I	COL3022	21,550	110	23987	23668-24366	22061	21736-22358
0	I	COL4327	21,660	104	24124	23758-24495	22171	21859-22492
40	I	COL4326	23,170	114	25760	25488-26091	23810	23502-24167
79	I/II	COL4328	24,790	115	27350	27093-27614	25603	25263-25868
108	II/III	COL4329	25,980	133	28585	28215-28938	26825	26389-27126
135	III	COL4325	26,920	149	29531	29116-29867	27642	27266-28057
160	III	COL4324	27,730	148	30307	29949-30685	28522	28105-28912

What to plot in terms of calendar ages?

These two (high- and low-) depletion scenarios should provide a bracketing lower- and upper- set of calendar ages for each ^{14}C sample. These are however evidently very wide (the difference in the median calibrated ages under these two scenarios is 1760 cal yrs).

Around the LGM, we might expect the calendar ages obtained under the high-depletion scenario (i.e., option 2) to be more accurate, especially in such a polar location (around 70˚S). However, until we know more about sea ice extent and regional palaeoclimate it will be challenging to be definitive – the correct calendar ages could lie anywhere in between the two scenarios.

We agree that it is informative to see the range of ages generated using these two approaches. However, we also think it is important that we note clearly in the text that these are presented as end-member scenarios, with reality lying somewhere in between. As the snow petrels are foraging in open waters within the sea-ice pack or close to the sea-ice margin, where air-sea gas exchange is taking place, we consider that the high-depletion scenario is likely to be an over-estimate. If snow petrels are feeding in polynyas during our time interval, that situation in turn implies that there is better sea-ice exchange than the LSG scenario predicts.

We would suggest that when plotting the proxy on a timescale (as in Figures 3 – 6) that the high depletion scenario might be shown, but with a clear explanation that this is an extreme scenario (and likely providing the most recent estimates of the calendar ages). Further, we suggest that perhaps in the main text, Table 1 shows the calibrated ages under both extreme (high- and low-) depletion scenarios in separate columns.

As we note in our previous reply, we consider that the high-depletion scenario (which shows younger ages) is likely over-estimating the delta-R where our snow petrels are foraging. We prefer that when we show our data on a timescale (Figures 3-6), we show the data using the oldest likely ages (i.e. with ΔR^{Hol}_{20}) and acknowledge that these are maximum estimates with reference to Table 1. Adopting this approach also means that for those researchers using stomach-oil deposits to trace ice-sheet thinning histories (e.g. Hiller et al. (1988), and as reviewed by Hillenbrand et al., 2014) where the oldest data of snow petrel occupation is important, the palaeo-environmental and palaeo-glaciology data are using the same age model approaches, rather than two age models appearing in the literature for the same deposits.

We could show selected data under the two age estimates in Figure 6 (climate links) or instead as a separate Appendix (for example, as we do for comparing normalised and original XRF data). We think that a revised Figure 6 (shown overleaf) would be the best plot for showing the impact of our alternative age constraints in the context of other climate records, as it highlights the challenge of making millennial-scale links which we refer to in the main text.

We suggest it might be possible in the future for you to use the simultaneous sea-ice proxy information you have (i.e., stomach oil composition) to determine for each individual ^{14}C sample a suitable level of local depletion ΔR_{20} before calibration. This could use the sea-ice proxy as a sliding scale to transfer from the high- and low-depletion scenarios.

Perhaps as your project progresses further, it might also help us to determine sea ice extent in a way we can incorporate that information into future IntCal curves.

This is also our hope, that we can use our stomach-oil deposits to learn more about the sea-ice environment and its changes through time, so that we might contribute to improving chronological constraints.

Again, we would like to thank you for the opportunity to comment on your paper. It is an exciting project that we look forward to learning more about,

Timothy J. Heaton

Edouard Bard

Christopher Bronk Ramsey

Martin Butzin

Peter Kohler

Paula J. Reimer

Proposed revision to Figure 6 to show impact of different calibrations:
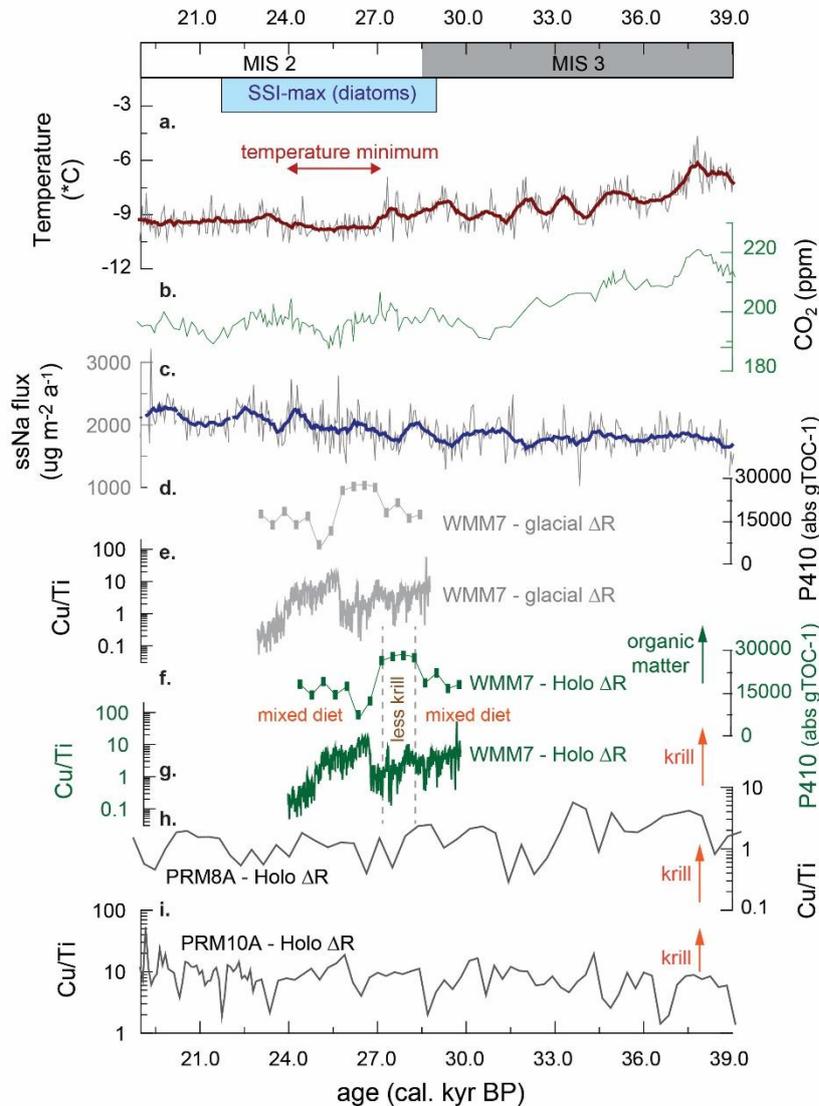
Figure 6: variations in snow petrel diet across the MIS 3-2 transition, including the interval of maximum summer sea-ice extent (SSI-max) in the Scotia Sea from Allen et al. (2011) and plotted in Fig.1. (a.) air temperature recorded by δD in EPICA-DML ice core (Jouzel et al., 2007); (b.) atmospheric CO₂ from West Antartic Ice Sheet (WAIS) ice core (Bauska et al., 2021); (c) sea-salt Na flux from EPICA-DML ice core (Fischer et al., 2007); (d.) WMM7 chlorin pigment abundance, interpreted here as an enhanced phytoplankton/reduced krill signal, plotted using the Holocene (low/absent sea ice) ΔR from Table 1 and Figs. 3-5; (e.) WMM7 Cu/Ti ratio, interpreted here as evidence of enhanced krill inputs plotted using the Holocene ΔR from Table 1 and Figs. 3-5. As discussed in the text, we infer the loss of krill from the snow petrel diet ~25 ka to represent polynyas opening over the continental shelf; (f.) WMM7 chlorin pigment abundance, interpreted here as an enhanced phytoplankton/reduced krill signal, plotted using the Glacial Stage (enhanced sea ice) ΔR from Table 1; (g.) WMM7 Cu/Ti ratio, interpreted here as evidence of enhanced krill inputs plotted using the Glacial Stage (enhanced sea ice) ΔR from Table 1. (h.) and (i.) Cu/Ti signals in other DML stomach-oil deposits, from analysis in Berg et al. (2019) and re-calibrated using the Holocene ΔR from Table 1.

Final Minor Addendum (Sterken et al., 2012):

We wondered why you used a ΔR uncertainty of ± 100 ^{14}C yrs when Sterken et al. (2012) use the Bjorck et al. (1991) uncertainty of ±50. Normally, in the radiocarbon community, when one reports ± in this way, one is referring to the 1σ value. It is not necessary to double that for input into OxCal or CALIB although certainly justifiable given that the ΔR value was based on a 1903 penguin bone sample and when the Marine13 you were calibrating against assumed a constant reservoir offset from the atmosphere during the period of your samples.

Our rationale for this was to address the unknown uncertainties of the calculated delta-R value, but we welcome the recommendation to apply the published uncertainty, especially since Table 1 shows that there is larger calibrated age uncertainty associated with choice of delta-R.

In general, we think the ΔR value reported in Sterken et al. (2012) is slightly wrong for use against Marine13. This ΔR is based upon penguin bones which were collected in 1903 which have a ^{14}C age of 1280 ± 50 ^{14}C yrs BP (Bjorck et al., 1991). To work out ΔR_n, where n represents the Marine curve you are using, you have to look at the offset between that Marine curve and the observation in the specific year of interest:

Marine13 – the mean of Marine13 in 1903 (47 cal yr BP) is 450 ^{14}C yrs BP (not the 400 ^{14}C yrs BP as stated by Sterken et al., 2012). Using the correct Marine13 values this would equate to a ΔR_{13} of 830 ± 50 ^{14}C yrs (not 880). We think Sterken (2012) may have erroneously subtracted the difference between the present-day (i.e., at 0 cal BP) Marine and IntCal curves. This is not the correct way to calculate the depletion since IntCal does not go through 0 ^{14}C yrs BP at 0 cal yrs BP.

Marine20 – the mean of Marine20 in 1903 is 610 ^{14}C yrs BP. This equates to a ΔR_{20} of 670 ± 50 ^{14}C yrs as stated in your suppl. information.

We thank the authors for this clarification, and as noted above we will use the recommended ΔR_{20} of 670 ± 50 ^{14}C yrs.

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