Climate of the Past Discussions

Clim. Past Discuss., referee comment RC2
https://doi.org/10.5194/cp-2021-134-RC2, 2021

Comment on cp-2021-134

Anonymous Referee #2

Referee comment on "Summer sea-ice variability on the Antarctic margin during the last glacial period reconstructed from snow petrel (*Pagodroma nivea*) stomach-oil deposits" by Erin L. McClymont et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-134-RC2, 2021

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.

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 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.

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.

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

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.

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.

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”.

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

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

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.

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.

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?

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.

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

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?

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

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.

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.

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.

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.

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