

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-19

Anonymous Referee #2

Referee comment on "Evaluation of lipid biomarkers as proxies for sea ice and ocean temperatures along the Antarctic continental margin" by Nele Lamping et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-19-RC2>, 2021

The work proposed by Lamping and co-authors is focusing on the application of several lipid biomarkers to improve reconstructions in sea-ice conditions in West Antarctica. Part of this work corresponds to an extension of the Vorrath et al. (2019) study, but now include a new model-data comparison. In addition, the authors are also presenting a couple of other lipid tracers, including the GDGTs and their use as an ocean temperature proxy. The comparison between the proxy and instrumental data with the modelling simulations is absolutely fundamental to better understand the meaning of these molecular proxies, their locus of production, their transport, etc. and that will definitely serve the scientific community for future investigations on sea-ice conditions and to scrutinize other parameters using marine sediments.

The manuscript is overall well written and worth to be published. However, I am honestly wondering if all the data presented here should not be the focus of two distinct papers. The authors have sometimes real difficulties to connect the HBI and GDGT proxies, which is not surprising given the fact that they both record different physical variables (sea ice vs ocean temperatures). The manuscript title is a good proof of that as the authors are clearly proposing to "elucidate modern West Antarctic sea-surface conditions", opening a perspective for paleoclimate studies, which is not really the link we would immediately make when reading the presentation and interpretation of the two proxies. In addition, I have several comments and concerns regarding the GDGT section which needs further considerations and improvement and should include more recent work on this proxy around Antarctica.

HBIs and other productivity biomarkers - I have no major comments on the interpretation of their data. I have nevertheless one main point that should be clarified further than the authors maybe do. The surface sediment samples selected for this study are probably spanning a "large" period of time than what one would ideally expect when strictly looking at the modern conditions (<40 years). We all know how hard it is to get very recent surface sediments and therefore we can only support this work, despite significant uncertainties surrounding the proxy interpretation as underlined by the authors. As they

mentioned, "Vorrath et al. (2019) conducted radiocarbon dating on selected surface sediment samples from the Bransfield Strait, concluding that their biomarker data reflect the past two centuries ", explaining why the authors justify that "the different time periods covered by the different methods need to be considered and kept in mind when interpreting the results ». I presume that Lamping and colleagues have no or very few radiocarbon or ^{210}Pb dating on their selected samples in the different investigated areas. I would strongly suggest to mention that regional sea ice has been probably quite variable over the last two decades and centuries which means that comparing concentrations of the HBIs and other sterol concentrations and ratios with satellite and model data can significantly differ owing to the difference of ages. In addition, I would also suggest the authors to clearly state that many studies have shown that significant degradation of organic compounds occurs both within the water column and surface sediments as a result of microbial activity and this might change from one area to another. It means that variations in concentrations between two sectors might not strictly reflect a real change in production of these compounds in the surface waters but might also report two different degradation states. Two surface sediment samples with two very different ages, about few decades, may exhibit two different concentrations which do not necessary mean that sea-ice concentration was higher or lower between them during a specific period of time but instead that the organic compounds could have been more degraded in one area, especially where the oldest sediment are found. In conclusion, I would insist more on this point in addition to the others convincingly raised by the authors.

I have more concern regarding the GDGT interpretation. There is now an emerging consensus that GDGT are more reflecting along the Antarctic margin the subsurface ocean temperatures (SOT) (0-200m, 100-200m, 50-400m water depth depending on the studied area) rather than SSTs (Kim et al., 2012; Etourneau et al., 2019; Liu et al., 2020). This is mostly linked to the fact that the GDGTs might be more synthesized by Traumarchaeota living at the intersection between the cold and low saline surface waters with the subsurface warm waters (CDW and WDW) around Antarctica. I would first suggest to consider the calibration of Kim et al. 2012 for converting the GDGT ratios into SOT ($\text{SOT} = 50.8 \times \text{TEX86L} + 36.1$), which may reduce the temperature range and the unrealistic warm values found in the north, and then compare with model and instrumental data at different subsurface water depths. Furthermore, I would also suggest to consider that the GDGT seems to be produced mostly during the late winter and early spring (Murray et al., 1998; Kalanetra et al., 2009), even though we clearly need more data from the water column to confirm such hypotheses. Therefore, mapping the ocean temperatures at different seasons and the most appropriate depths would be worth to try. This might provide further constrains on the use of the GDGTs as paleotemperature proxy.

In conclusion, I believe the authors should split their data in two different papers which would make their interpretation clearer and more focused.

Few minor comments:

- Line 30: more recent references, especially in the context of the modern global warming and West Antarctica sea ice decline (eg. Wang et al., *J. of climate*, 2020)?
- Lines 36-37: I would adequately include references in the right places (ie. In the

40-year satellite record, sea-ice extent in East Antarctica is increasing (Comiso et al., 2017; Parkinson and Cavalieri, 2012), experiencing an abrupt reversal from 2014 to 2018 (even exceeding the drastic decay rates reported in the Arctic; Parkinson, 2019).

- I think it is important that the authors more clearly mention the warm water circulation in the Weddell Sea related to the Warm Deep Waters (the equivalent to the CDW along the WAP) and their origins.
- Western and Eastern AP. Why not to use acronyms WAP and EAP for Western and Eastern Antarctic Peninsula, respectively?
- Line 248: confusing between "these areas" and "this area"
- Line 249; 275-280: or something else? Could be related to local upwelling forcing too
- Missing several references (eg. Vernet et al. 2019, Cao et al 2019...)