

Clim. Past Discuss., author comment AC1  
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## Reply on RC1

Nele Lamping et al.

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Author 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-AC1>, 2021

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## Rebuttal Letter to Referee #1

Dear Editor, dear Referee #1,

we appreciate your constructive and helpful comments on our original submission, which have significantly contributed to the improvement of the manuscript. Below, please find our responses to your comments. Regarding the new TEX<sub>86</sub><sup>L</sup> calibration and additional modelled and WOA13 data (see supplementary material), we also restructured the subsections in Sect. 4 Results and discussion.

**RC1.1:** I was wandering why authors decided to present (and discuss!) data only for specific (HBI triene Z and Brassicasterol) phytoplankton derived HBI indices in the main text while moving others into the supplementary. Do data in supplementary add anything to the study? Are there any key outcomes? If so which ones etc. I think it would be nice to comment on those additional data. It also implies that those outcomes presented in the main text (based on HBI Triene Z and Brassicasterol) have shown most promise (reflect environmental settings best) in previous calibrations and have been applied most extensively, while I`m not convinced that`s the case in Southern Ocean. While they have been utilised fairly extensively in the Arctic and subjected to several calibration studies, It`s not been the case in the Southern Ocean (as author also point out) and applicability of approach utilising any of these pelagic lipids is vastly unexplored.

**Author`s response:** The dinosterol and HBI E-triene concentrations and thereof derived PIPSO<sub>25</sub> indices show very similar patterns when compared to the brassicasterol and Z-triene data presented in the main text. In order to avoid repetition while describing these results, we prefer to publish the data as supplement. This also allows other researchers to consider the applicability of dinosterol and/or HBI E-triene as phytoplankton markers for own studies. We now add a sentence in Sect. 4.1, commenting the similarity of the datasets.

**RC1.2:** There seems to be weighting towards HBIs, which somewhat detracts from GDGT outcomes. It made me wonder if perhaps "less" could be more. Should authors concentrate either on GDGTs or on HBIs? While they carry out the evaluation between the individual indices and satellite/modelling data I was missing an intercomparison between lipid derived proxies and where outcomes from one support/contradict those derived from other.

**Author's response:** We recognize your point here, which has similarly been mentioned by Referee #2. We have now, based on the suggestion by Referee #2, used a different calibration for subsurface oceans by Kim et al. (2012) and compared those new temperature reconstructions to instrumental and model data. This enabled us to emphasize the GDGT data in the manuscript. In order to prevent repetitions when splitting the data into two papers, we prefer to keep both proxies (HBIs and GDGTs) in the manuscript and publish the data set as one. We note that we do not directly compare IPSO<sub>25</sub> and TEX<sub>86</sub><sup>L</sup> as these proxies relate to different environmental variables (i.e. sea ice and subsurface ocean temperature). However, we now comment on the relation between WOA-derived sea surface temperatures and PIPSO<sub>25</sub> values.

**RC1.3:** Have authors considered including any taxonomy work? It seems like biomarkers are depicting some regional differences (e.g. EAP vs WS or EAP vs WAP or even WS vs AS) and I was wondering to what extent these could be observed via differences in diatom distributions. Could taxonomy/diatom work also provide some indications about productivity or phytoplankton composition differences that authors refer to in text (e.g. lines 247-249, 282 etc.)?

**Author's response:** We agree that taxonomy work would add to a more detailed assessment of the environmental conditions in the different regions and it would be interesting to see, whether diatom distributions follow a similar pattern than the biomarker reconstructions. Taxonomy work, however, is not within the scope of this manuscript but we now address this point in Sect. 7 regarding future work: "Further taxonomy work, the composition of the proxy's source habitat (basal sea ice, platelet ice, brine channels) and its connection to platelet ice formation via in situ or laboratory measurements are required to better constrain the proxy's potential for sea ice reconstructions." An important aspect that should be mentioned here as well concerns the preservation of diatoms. Particularly in coastal (often heavily sea ice covered) areas the application of diatoms as environmental proxies can be affected by opal dissolution. This would certainly impact comparisons and/or correlations with other environmental proxies and we suspect that such a study would benefit from a larger data set that also contains sample material from more distal ocean areas (off the continental shelf).

**RC1.4:** Introduction seems to be rather generous towards HBIs but relatively scarce on GDGTs. I think it would be worth expanding this part and provide overview of current knowledge with respect to use of GDGTs in Southern Ocean.

**Author's response:** We agree that GDGTs have not been sufficiently introduced and now add a paragraph on the state of the art concerning GDGTs in the Southern Ocean and we also consider ocean temperatures in the description of the study area to streamline the further presentation and discussion of GDGT data.

**RC1.5:** Title: perhaps consider rephrasing. "...sea surface.." Authors state that it's not

clear if GDGT based temperatures they've derived represent SST, near-surface or sub-surface (e.g. lines 448-449).

**Author's response:** In regard of the newly calibrated TEX<sub>86</sub><sup>L</sup> data and the consideration of instrumental (World Ocean Atlas) as well as model-derived subsurface ocean temperature data and the respective modifications to the manuscript, we also changed the title to: "Evaluation of lipid biomarkers as proxies for sea ice and ocean temperatures along the West Antarctic continental shelves".

### Line specific comments and amendments:

**Previously line 56:** "...emerged as a robust proxy..." seems to contradict with authors conclusions (c.f. line 532).

**Author's response:** We now express it more carefully by changing the term *robust* to *potential*, which also better agrees with our conclusions.

**Previously line 65:** I think authors might have wanted to say "by analogy" rather than "because of the structurally close relationship of this lipid..." e.g. HBI trienes are also structurally similar to IP<sub>25</sub> and IPSO<sub>25</sub>

**Author's response:** We followed your suggestion and changed the sentence to: "Belt et al. (2016) introduced the term IPSO<sub>25</sub> ("Ice Proxy of the Southern Ocean with 25 carbon atoms") by analogy to the counterpart IP<sub>25</sub> in the Arctic."

**Previously line 68:** change "...reconstructions is..." to "...reconstructions are .."

**Author's response:** We changed the sentence to: "Hitherto, only a relatively small number of studies based on IPSO<sub>25</sub> for recent and Holocene sea-ice reconstructions are available in the Southern Ocean..."

**Previously lines 140-141:** internal standards-provide mass equivalent added

**Author's response:** We now provide the mass equivalents added to the sediments.

**Previously lines 247-249:** "... ,pointing to elevated productivity..." somewhat misleading given follow up sentence. Rephrase to ... pointing either to elevated productivity or reworked terrigenous organic matter

**Author's response:** We merged the two sentences and changed it as suggested to: "...pointing either to elevated productivity or reworked terrigenous organic matter in these areas..."

**Previously lines 267-269:** sentence beginning “Here productivity of the source diatoms..” any studies or diatoms data that could provide some support if that is the case?

**Author’s response:** While Moore and Abbott (2002) mainly focus on satellite estimates of surface chlorophyll concentrations to assess phytoplankton blooms at the polar front, Kemp et al. (2006), for example, report on higher concentrations of certain diatom species (incl. *Rhizosolenia* spp - a producer of HBI trienes; Belt et al., 2017) at oceanic frontal zones. A recent study by Cardenas et al. (2019) using surface sediments from the Drake Passage, however, documents an only minor abundance of *Rhizosolenia* spp. in those samples. We now mention this study and conclude that biosynthesis of HBI trienes by other pelagic diatoms should be considered.

**Previously lines 283:** 2 magnitudes – add *orders of* after 2

**Author’s response:** We changed it accordingly to: “... (more than 2 orders of magnitudes) ...”

**Previously lines 337-341:** “This difference between..” seems like really important point to consider! Where does this leave sterols as a phyto counterpart in PIP index?

**Author’s response:** We note that the use of sterols and HBI trienes to determine PIP<sub>SO<sub>25</sub></sub> indices partly owes to previous observations that the latter may be absent in some environmental settings and using e.g. dinosterol as phytoplankton marker still enables a proper assessment of surface conditions (Lamping et al., 2020). Accordingly, consideration of both HBI trienes and sterols - though belonging to structurally different compound classes - seems reasonable and this has also been documented within Arctic Ocean studies where brassicasterol- and HBI Z-triene-based PIP<sub>25</sub> records exhibit very similar patterns over the last deglacial suggesting an overall reliable response to large-scale climate-driven environmental changes. The herein documented differences in sterol- and HBI triene-based PIP<sub>SO<sub>25</sub></sub> indices accordingly contribute to the evaluation of these still relatively new approaches for Southern Ocean sea-ice reconstructions.

**Previously line 364:** “.., with up to 11C..” – add *temperatures* after with

**Author’s response:** We added *temperatures*: “...with temperatures up to ~11 °C.”

**Previously line 395:** “-0.5 – should .5 precede -0.5 ?

**Author’s response:** This must have been an error in the formatting, but we corrected it from “-0.5 to -1 °C” to “-1 to -0.5 °C.”

**Previously line 402:** “The sea ice biomarker IPSO<sub>25</sub> is hence..” Are there any detailed time series sea ice studies to support this statement? Analogy is based on what is known about IP<sub>25</sub> in the Arctic, but I don’t think this is yet true for IPSO<sub>25</sub> in Southern Ocean.

**Author’s response:** Time-series studies (such as Brown et al. (2011) on IP<sub>25</sub>) focusing

on IPSO<sub>25</sub> are not yet available, but the general consensus, based on the main algae bloom in the Southern Ocean and, more importantly, the main bloom of the source diatom of IPSO<sub>25</sub> *B. adeliensis*, is that IPSO<sub>25</sub> can be interpreted as spring sea-ice indicator (see Belt, 2016; Riaux-Gobin et al., 2013).

**Previously line 458:** I think authors shouldn't use reference which have not been at least accepted for publication.

**Author's response:** The work by Spencer-Jones et al. (2020) is now published in the journal *Biogeosciences* (<https://doi.org/10.5194/bg-2020-333>).

**Previously line 477:** remove `HBI diene`

**Author's response:** We removed the term *HBI diene* as suggested.

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Please also note the supplement to this comment:

<https://cp.copernicus.org/preprints/cp-2021-19/cp-2021-19-AC1-supplement.pdf>