Climate of the Past Discussions

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Comment on cp-2021-29

Anonymous Referee #1

Referee comment on "Bottom water oxygenation changes in the southwestern Indian Ocean as an indicator for enhanced respired carbon storage since the last glacial inception" by Helen Eri Amsler et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-29-RC1, 2021

Review of the paper « Bottom water oxygenation changes in the Southwestern Indian Ocean as an indicator for enhanced respired carbon storage since the last glacial inception» by Helen Eri Amsler, Lena M. Thöle, Ingrid Stimac, Walter Geibert, Minoru Ikehara, Gerhard Kuhn, Oliver Esper and Samuel L. Jaccard, submitted to Climate of the Past.

The authors present new records of redox-sensitive elements to reconstruct bottom water oxygenation changes from the last glacial inception to the Holocene. These records were obtained on a North South transect of marine sediment cores in the western Indian sector of the Southern Ocean. The authors provide records of exported biogenic silica in the same cores to determine whether the bottom water oxygen changes are linked to increased organic carbon sedimentation or circulation changes. These data therefore provide important information concerning the mechanisms involved in the air-sea partitioning since the last glacial inception.

The paper is thus within the scope of "Climate of the Past" and could be of great interest for the community.

However part of the methods needs to be clearly explained.

The weakest part of the paper concerns the age models. For the sub-Antarctic core DCR-1PC the age model has been established in a previous paper (Crosta et al., 2020).

However, it is necessary for the reader to see figures with

- the depth of the 14C dates and the tie-points on the aU, Mn/Ti and opal records to see where are the chronological constraints (could be added on figure 4 but it would be nice to see also the records that have been tuned)
- a depth/age plot.

In fact, the 14C dates presented in Crosta et al. 2020 for this core probably indicates a hiatus of ~5kyr between 33 and 41cm, that would roughly correspond to isotopic stage 2. This possibility should be discussed when considering this time period.

For the other cores, the dating strategy is not explained. Why correlating the core signals to benthic LR04-stack, while the sub-Antarctic core age model has been established by tuning with EPICA Dome C deuterium record? If there is a scientific reason to link the magnetic susceptibility records and the LR04-stack that have been aligned together, it has not been explained. Comparing the same/similar records of two neighbouring marine cores does not need a long explanation but any other tuning between various records requires at least a short explanation of the underlying assumptions.

The introduction is well written and the lines 59 to 69 clearly present the goal of this study. However it is disappointing to have a very simplified presentation of the role of iron in the Southern Ocean. This study concerns the Indian sector of the Southern Ocean, not the Atlantic sector and dust is probably not the major source of iron at the cores locations (Tagliabue et al., 2017, 2014 and reference therein) at any time of the last glacial cycle.

For bottom water oxygenation proxies, the authors indicate that they considered two different $^{238}U/^{232}Th$ ratio, 0.5 for cores within CDW with a large NADW component and 0.27 for the deeper and southern core PS2603-3 influenced by AABW and thus Antarctic continental crust. Within the discussion, the authors consider changes in the deep Southern Ocean circulation during the last climatic cycle, with shoaling of the NADW influence (Govin et al., 2009 should be cited for the circulation changes within the Indian sector of the Southern Ocean during the glacial inception). The authors should thus consider a possible decrease of the $^{238}U/^{232}Th$ ratio for the shallower cores during the glacial stage. It might not change significantly their results but it would be nice that they indicate the corresponding uncertainty.

Other questions and minor corrections are indicated with the manuscript line numbers in the following part.

Change Sigman et al., 2020 to Sigman et al., 2021

All the figures have a 2 before their number, to be suppressed.

Line 210 to 214, aU do not peaks at peak glacial conditions but at the transition to termination 1

Line 254 to 270: the authors could also consider the possible hiatus in the core with a missing isotopic stage 2.

Line 286: I do not understand the sentence: in the Polar frontal zone the nutrient availability was reduced compared to interglacial period but the nutrient availability is always higher in the Polar frontal zone than closer to the Subantarctic front. Again consider also a possible hiatus, as indicated by 14C data.

Line 306 “alternative” , n missing

Line 315 Is it the sampling resolution or the uncertainty of the age models that precludes to assess the potential time lag between cores?

Line 316 to 325 the increase in aU seems to be at the beginning of the Holocene not during the deglaciation, as well as the opal peak in the PS2603-3. Do the authors consider a possible 5kyr error on the age scale at that time? We really need to see the records that were tuned to benthic LR04 record or EPICA Dome C deuterium and the tie points considered.