Climate of the Past Discussions

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-29

Anonymous Referee #2

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-RC2, 2021

This manuscript presents authigenic uranium (aU) concentrations, biogenic silica (bSi) concentrations and Mn/Ti elemental ratios in bulk sediments obtained with various analytical methods for five cores between 46ºS and 59ºS in the Southwest Indian Ocean. Mn/Ti profile was not obtained for the southernmost core. Three of the five cores provide 120 ka records, one core extends only 40 ka, and another one covers the past 180 ka.

The major conclusion is the important role of physical processes to oceanic carbon storage during cold periods due to reduced ventilation. The biological productivity is considered as a second factor. The link is proposed between Southern Ocean carbon storage and atmospheric CO2 concentration changes on glacial/interglacial timescales.

The strong points of the present study are i) the reconstruction of both oxygenation state and biological production inferred from sedimentary opal content and ii) latitudinal transect covering different frontal zones in the Indian sector of the Southern Ocean. The authors discussed various possibility affecting the aU, bSi and Mn/Ti records. They are careful but they did not explicitly provide their specific objectives and working hypothesis. Consequently, the present manuscript gives impression “just confirming the previous studies”. I will develop my major concerns below.

- Too general objective and poor description of original finding

The major role of ventilation changes to oceanic carbon storage on glacial/interglacial timescales has been already reported by number of studies. What is the focus of the present study? Why are the authors interested in changes since the last glacial inception? Why the transect in the Southwest Indian? If the role of the Indian sector of the Southern Ocean is the primary motivation of the present work, introduction should be focused on state-of-art of the study region.

The discussion is qualitative and only confirms the observation of the previous studies. The authors are careful to interpret the obtained records considering different possibilities (ex. diagenetic burn-down that could modify aU records). But it is difficult to identify robust reconstruction and new insight supported by their own results. Also, there is no figure comparing the new results with previously obtained records except dD and pCO2 to discuss the processes that the authors proposed.

Reorganization of the manuscript with addition of discussion figures will be useful to identify targeted objective and working hypothesis to emphasize original aspect of the present study.

- Lack of demonstration about age model quality

The authors described the age model in section 2.2 and Table 1 but the provided information is insufficient. Since sedimentation rate is a factor affecting the accumulation of authigenic U (Figure 4), more extended explanation is necessary with figures. For instance, it is helpful to show 14C dating levels and tie points of each core. Which size of reservoir age was applied? Which 14C calibration equation was used? The magnetic susceptibility (MagSus) records of PS2609-1 and PS260606 were tuned to LR04-stack. What is the hypothesis to relate MagSus to the benthic d18O stack? Were XRF data (Fe, Si, Ti, Ca) used to correlate between PS2609-1 and PS260606? The authors also used alignment of XRF Ti intensity and Ca/Ti intensity ratio of PS2606-6 with the EPICA Dome C dust record. What is the size of age offset based on the tuning to LR04 and to EPICA Dome C dust record? Concerning core PS2603-3, MagSus, XRF data (which elements?) and biogenic silica were graphically aligned to the LR04 reference curve. Did the authors assume that the changes are synchronous? Why? Overall, what is the size of uncertainty of age model of each core?

- Estimation of authigenic uranium (aU) concentration

aU is estimated assuming a constant 238U/232Th that is variable with sites. Even if generally consistent aU trend is observed for the study cores on glacial/interglacial timescale, absolute aU is relatively small, often less than 3ppm except core DCR-1PC. Moreover, detrital U contribution might have changed on glacial/interglacial timescales. It will be useful to present figures comparing 238U/232Th activity ratio with aU concentration profile of each core to demonstrate potential influence of detrital 238U/232Th activity ratio on aU variability.

At last, this study used different analytical procedures to obtain the same parameter (aU, Mn/Ti and bSi) for the different cores. The consistency of the results is mentioned but it is not shown how the comparison was realized: some selected common samples were analyzed or common standards were regularly measured? Some more detail will strengthen the manuscript.

I recommend to accept this manuscript after major revision.

Minor / specific comments

Abstract last sentence (lines 23-24), "These records highlight… insufficiently documented role the southern Indian Ocean played in the air-sea partitioning of CO2 on glacial-interglacial timescales". It is unclear how this statement is extracted from the results obtained in this study.

Line 25, "exogenic carbon cycle". Please define this term.

Lines 26 and 41, “Sigman and Boyle, 2000”. The reference is missing in the reference list.

Line 67, “underrepresented Indian sector of the Southern Ocean”. It will be helpful to add the state of art about bottom water oxygenation state in the Indian sector to clarify unsolved issues. Such description will better define the objective of the present study.

Lines 71-84, “2.1 Core locations and material”. Add the description of the present-day water masses occupying the core locations.

Line 88, “*neogloboquadrina*” should be “*Neogloboquadrina*”.

Line 135, about Mn and Ti measurements. To avoid any confusion, indicate from the beginning, XRF scanning or ICP-MS measurement realized for different cores. Also, it is necessary to mention that Mn/Ti record was not obtained for core PS2603-3.

Line 184, “millennial-scale oscillations’. What is the temporal resolution of aU record? Considering the possibility of aU remobilization, is it appropriate to treat millennial-scale variability, in particular for the interval of low sedimentation rate such as MIS 5 (Figure 4a)?

Lines 189-191, “detritic values”. Mn/Ti variability of core DCR-1PC is estimated by XRF intensity ratios that are not converted to concentration. How did the authors know the background level corresponds to detrital values?

Lines 194-197, about glacial-interglacial trend of bSi for core DCR-1PC. Caution should be paid because the expected glacial high bSi value is not observed for MIS 2.

Line 207. Add “aU” between “Sedimentary” and “concentrations”.

Line 209, “a pronounced increase in sedimentary aU concentration during MIS 4”. This sentence should be revised because the description is true for PS2609-1 but not for PS2606-6 that shows a modest increase (Figure 3b).

Lines 213-214, “The highest aU…a gradual increase from about 30 ka, peaking during the LGM”. Core PS2603-3 does not show the described trend because no clear peak is identified (Figure 3d). Please revise the text.

Line 228. Delete “which seems to higher CO2 levels during MIS 5”. This is result section, thus premature to compare with pCO2 record.

Line 233. Add “inside of the sediments” after “at the sediment-water interface”.

Line 237, “the proxies broadly agree”. What does this sentence mean? The proxies follow an expected trend? If so, what is the hypothesis to expect some trend?

Line 240. Add “of core DCR-1PC” after “values”. It is unclear why the observed Mn/Ti trend can be treated as “a regional increase in carbon export and sequestration”.

Lines 292-293, “broadly similar to the SAZ record”. I don’t see the similarity because the SAZ core (DCR-1PC) is characterized by aU maximum during MIS 3 that is totally absent for the SAZ cores.

Line 293, “noisy Mn/Ti signal”. In general, the authors did not provide temporal resolution of different parameters for different cores. The mentioned “noisy signal” of COR-1bPC was possibly related to high-resolution XRF scanning.

Lines 302-303, “COR-1bPC was closest to the most vigorous upwelling location”. Is this statement enough robust? The bSi concentration of COR-1bPC is high but comparable with bSi at PS2606-6 considering different temporal resolution.

Line 306, “alterative” should be “alternative”.

Lines 327-333, about the deep or bottom water masses. This part should be placed in section 2.1. The present-day water masses (AABW, upper CDW and lower CDW) should be shown in Figure 1b. How did the author distinguish the water masses? Using a T-S plot?

Line 340. Add “and in pore water” after “interface”.

Line 372. Delete “XRF peak” since some Mn/Ti data were obtained using ICP-MS.

Numbering of the figures and the table should be corrected since the number always contains “2”.

Figure 1. (a) right panel. “AAZ” should be replaced by “AZ”. Show the position of transect indicated Figure 1b. (b) Indicate the present-day water masses.

Figures 2 and 3. Combine the two figures like Figure 4 to facilitate comparison between all study cores and avoid presenting atmospheric CO2 and dD twice. Indicate the latitude and water depth of each core.