

Interactive comment on “A multi-proxy analysis of late Quaternary Indian monsoon dynamics for the Maldives, Inner Sea” by Dorothea Bunzel et al.

Anonymous Referee #2

Received and published: 17 May 2017

I am reviewing here a manuscript by Bundel et al. on a multi-proxy analysis of a sediment core from the Maldives region. The data is interpreted in context of dust input, sea-level changes and oceanographic changes over the last 200.000 years.

Overall I think that this study and data can be eventually suitable for publication in Climate of the Past. It touches upon topics that have been published previously in the journal and the datasets will be of interest to the paleoceanographic community that works in the Indian Ocean during the Late Pleistocene.

Overall, I think the analysis of the data and its presentation could be improved. Some of the statements in the discussion (e.g. correlation of certain proxies with insolation or other proxy records, glacial-interglacial cycles) is often not supported by statistics or suitable figures (see comments below).

[Printer-friendly version](#)

[Discussion paper](#)



I am also missing clear common thread and objective. This starts already in the abstract. It starts with a paragraph that basically says : “We measured a lot of stuff on a sediment core in the Maldives region. . .and then we interpreted the data..”.

I think it would be much more appealing if the manuscript would start with the context of the study, the main research question or problem or a hypothesis. Then they should list their approach (multi-proxy approach)

Further comments:

I think the paragraph from line 57 to line 66 could be improved. This paragraph contains a controversy in the interpretation of past OMZ variability in the Arabian Sea and its relation with summer monsoon variability. Is a strong OMZ linked to increased productivity (monsoon driven upwelling) or reduced ventilation (lower oxygen conc in southern sourced intermediate waters). The study by Bundel et al could inform this debate by providing a record of oxygen concentration from further South. There are some records (e.g. Ziegler et al., 2010, Climate of the Past) that show that a deep (most extended) OMZ occurs during glacial periods. While productivity maxima in the Arabian Sea, occur during interglacials. The new data by Bundel et al., could help to explain this observation by proving constraints of the Arabian Sea intermediate water ventilation from the South.

Line 64: studies in stead of studied?

line 77 -82: This section lists the main objectives of the study. Its strange that the objectives 1 and 2 mention suddenly, dust flux and sea-level, while the entire introduction does not mention either of the two. I would focus on objective 3 and mention the subjects that deal with 1 and 2 in the discussion without putting to much emphasis on it.

line170: “. . .are based on. . .”

line 173: ‘was estimated to assess..’

[Printer-friendly version](#)[Discussion paper](#)

line 179-180: Why was a local reservoir age not applied?

line 224: Given that the authors did XRF scanning, they should also have Bromine data. Bromine has been used successfully in several studies in the Indian Ocean as organic matter indicator (Caley et al., 2013, QSR, Ziegler et al, 2008, G3). The authors could do the same to get a high-resolution organic matter record and get a better idea of short term variability in TOC.

line 258-260: What about the possibility that Fe/Ca and Si/Ca reflect changes in carbonate production / preservation? Maybe the dust input has been constant through time? See also related comments by the other reviewer. I fully agree with him.

line 278: at the precessional band

line 282: There are several studies that suggest that late Pleistocene quasi-100 kyr cycles are not driven by eccentricity, but instead are a response to skipped precession and/or obliquity cycles

line 342-345: I don't see a correlation of TOC or Ba with summer insolation. This should be demonstrated in a figure.

section 4.2: This section seems not to be very important in the context of the whole manuscript. I would therefore again suggest to omit the sea-level topic from the list of main objectives.

line 370-376: show the comparison with other datasets also in the figures otherwise the reader cannot judge your arguments

line 388-390: This sentence seems to contain a contradiction. Is the Maldives OMZ controlled by expansion of the Arabian Sea OMZ are controlled by the ventilation of southern sourced waters. (I would think it is the latter)

line 396-401: I would argue the other way around. Low oxygen conc in intermediate waters in the Maldives area preconditioned the waters that ventilate the Arabian Sea.

[Printer-friendly version](#)[Discussion paper](#)

So a deep Arabian Sea OMZ has its root in the central Indian Ocean (and is thus not exclusively controlled by monsoon variability).

line 428: demonstrate cyclicity through spectral analysis (see also comment by other reviewer, fully agree)

Figure 6: Why is assemblage 2 abundant in the glacial MIS 6 and the Holocene? (Why is assemblage 1 abundant in 5, but absent in the Holocene)?

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2017-54, 2017.

CPD

Interactive
comment

Printer-friendly version

Discussion paper

