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Climate of the Past Discussions

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Interactive comment

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

Dorothea Bunzel et al.

dorothea.bunzel@uni-hamburg.de

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Response to Referee#2

We acknowledge the detailed comments and suggestions by the reviewer, which helped to improve our manuscript considerably. In order to improve the statistical evaluation of our results we have performed Blackman-Tukey spectral analyses for selected proxy records. In addition, we have created new figures for BT power spectra and the comparison of own and published stable carbon isotope records. Below we respond to all comments raised by the reviewer.

With kind regards, Dorothea Bunzel

Overall, I think the analysis of the data and its presentation could be improved. Some

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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). I am also missing clear common thread and objective. This starts already in the abstract. It's starts of 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)

Response: Thank you for your suggestions. In the revised version we will include results from Blackman-Tukey spectral analysis in order to evaluate the variability in the precession band. For a proper statistical evaluation of long-term variations (i.e., in the excentricity band) our time series is too short. Our conclusions on the general glacial-interglacial variability are therefore still based on the graphical correlation among proxy records. We will rewrite the abstract putting our study in a general context and starting with the main objectives.

Further comments:

1) 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.

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Response: According to the existing data and also supported by our new record, the OMZ in the Northern Indian Ocean is controlled by both changes in ventilation of intermediate (central OMZ; Das et al., in press 2017) and deep-water masses (deep OMZ; see Schmiedl and Leuschner, 2005; Ziegler et al., 2010), and by regional oxygen consumption responding to upwelling-driven high surface productivity (e.g., Reichart et al., 1998; Das et al., in press 2017). Our reconstruction corroborates this conclusion as it shows the general presence of an OMZ, which is also preconditioned by the biogeochemical processes in the Arabian Sea. The observation of generally reduced oxygen concentrations during glacial boundary conditions (particularly during MIS 2-4) reflects the ventilation signal of southern-derived intermediate water although with a probable regional signal of winter-monsoon-induced enhanced organic matter fluxes and oxygen consumption. The combination of the different factors (intermediate water circulation, summer and winter monsoon influence in different regions) will be emphasized and the discussion clarified in the revised manuscript.

2) Line 64: studies in stead of studied?

Response: All identified misspellings will be corrected in the revised manuscript.

3) 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.

Response: We agree that the role of sea level and dust fluxes is underrepresented in the introduction. Our proxy records highlight both changes in sea level and dust fluxes as relevant factors for sedimentation and marine ecosystem dynamics in the Maldives, Inner Sea. Therefore, we will enhance the introduction chapter including a new paragraph providing background information on the influence of these parameters in the wider study area.

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4) line170: “. . .are based on. . .”

Response: It will be corrected.

5) line 173: ‘was estimated to assess..”

Response: It will be corrected.

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

Response: See response to referee#1 comment #3.

7) 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.

Response: Thank you for this suggestion. We have checked the bromine XRF counts in relation to the measured TOC values. Both data records reflect the same pronounced glacial-interglacial pattern with high values during glacial periods, but also reveal additional variability in the precession band over the studied time period. In the revised manuscript we will therefore include the Br XRF counts and will also evaluate it statistically (Blackman-Tukey spectral analysis). Both Br and Ba/Ca records show the same trend and both indicate marine productivity (Ziegler et al., 2008, 2009), but the Ba counts are comparatively low and therefore we will use the Br record instead of the Ba/Ca record in the revised manuscript.

8) 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.

Response: We agree since we cannot exclude potential changes in carbonate production. Therefore, we will include Ti/Al and Fe/Al instead of Fe/Ca as aeolian dust proxies

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(see also answer to referee#1, comment #6). We will however still display and discuss the Si/Ca record since it reflects the availability of siliciclastic grains in the sediment for test construction of agglutinating foraminiferal species.

9) line 278: at the precessional band

Response: It will be corrected.

10) 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

Response: For a statistically more robust evaluation of the full orbital variability (including the long-wave components) in our data series we would need a time series, which is considerably longer than 200 ka. Nevertheless, graphical comparison of our data series reveals pronounced glacial-to-interglacial changes suggesting a link to eccentricity-driven environmental changes. Our conclusions are also in line with dust flux reconstructions from the Arabian Sea (e.g., Clemens et al., 1996) which show striking changes on the glacial-to-interglacial timescale (in the eccentricity band) suggesting a close link to environmental changes and associated dust availability on the northern borderlands. While the eccentricity component appears dominant in the dust proxies, variability in the precession band seems to be considerably lower (as indicated by spectral analyses). These results will be considered and discussed in the revised version.

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

Response: In the revised version of figure 7 we will display Br XRF counts (see comment above) and TOC as indicators for productivity together with the difference of the summer and winter insolation at 30°N, which enables a graphical correlation of the mentioned proxy records and insolation. This comparison reveals coherent glacial-

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to-interglacial changes in the TOC and Br record (also in the Ba/Ca record) with elevated values during glacial stages MIS 6 and MIS 2-4. For a statistical evaluation of the relation between insolation and the different proxy records we have performed a Blackman-Tukey spectral analyses and we will present the power spectra in a new figure.

12) 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.

Response: We are convinced that sea-level changes exert a strong impact on sedimentation processes and paleoenvironmental conditions of the Maldives Inner Sea. This is clearly reflected by the composition of the benthic foraminiferal fauna (e.g. assemblage 1, meroplanktonic taxa) and other parameters, such as the Sr/Ca ratio and grain size etc. We therefore do not want to omit this process from the main objectives. Instead, we will provide a bit more background on the relation between sea-level and Maldives paleoenvironments in the introduction chapter (see also comment above).

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

Response: We agree with this suggestion. We will create an additional figure for the epibenthic stable carbon isotope records, which will facilitate comparison of our data with published data (Pahnke and Zahn, 2005; Elmore et al., 2015; Ronge et al., 2015). See also our answer to referee#1, comment #10).

14) 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)

Response: The present OMZ of the northwestern Indian Ocean extends from the northern Arabian Sea into the tropical Indian Ocean (Reid, 2003) reflecting the re-

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duced ventilation of intermediate water masses (due to its remote position) and the biogeochemical processes related to monsoon-induced organic matter fluxes and decomposition. We therefore assume that the OMZ variability in the Maldives Inner Sea is influenced by the overall strength and lateral expansion of the Arabian Sea OMZ, but it is additionally controlled by the ventilation of southern-derived oxygen-rich intermediate waters (AAIW) and by local monsoon-related organic matter fluxes and oxygen consumption. The general resemblance of our epibenthic stable carbon isotope record with comparable records from other areas indicates an ocean-wide link of intermediate water ventilation. On the other hand, the significant variability of our new oxygen reconstruction from the Maldives Inner Sea in the precession band and its resemblance with the reconstruction from the Arabian Sea suggests an additional influence of monsoon-driven biogeochemical processes. We will clarify the text accordingly.

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

Response: see also comment above. The oxygen concentrations in the northwestern Indian Ocean display a gradient with very low values in the northern Arabian Sea and increasing values to the South. This gradient illustrates a clear relation to the monsoon-related biogeochemical processes in the Arabian Sea, but is also a reflection of the remote position of the Arabian Sea in terms of intermediate water ventilation. Nevertheless, a monsoon-induced strengthening of the OMZ in the Arabian Sea (as during MIS 3) will results in an increase of the north-south oxygen gradient in the entire northwestern Indian Ocean, which should then also be detected in the Maldives Inner Sea (although at a lower amplitude).

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

Response: We have performed Blackman-Tukey spectral analyses and will present the results in the revised manuscript. See also answer to referee#1, comment #7.

17) 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)?

Response: This is a good question, but we do not yet have a simple explanation for it. Obviously, glacial conditions during MIS 6 were different from MIS 2-4 (Dansgaard et al., 1993); the latter was characterized by relatively lower sea-level and more intense glacial boundary conditions. Previous studies showed similar patterns, with certain benthic foraminiferal assemblages occurring both during glacial and interglacial periods, e.g. in the Red Sea (Badawi et al., 2005). At the Maldives Inner Sea glacial-to-interglacial changes in food fluxes were likely not extreme and therefore ecological thresholds for certain species and faunas may not have always been passed during glacial-interglacial transitions. A detailed inspection of assemblage 2 (C. mabahethi-fauna) actually displays faunal differences between their occurrences in MIS 1 and MIS 6 although C. mabahethi is the dominant taxon in both intervals.

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