

## ***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 #1**

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Bunzel et al., present a multi-proxy data record from the Maldives (equatorial Indian Ocean) over the last 200 kyr. The integrated evaluation of proxy records suggest a close linkage between the Indian monsoon oscillation, intermediate water circulation, productivity and sea-level changes on orbital time-scale in the Maldives Inner sea.

General comments: The paper of Bunzel et al., is an interesting contribution to our understanding of the climate of the equatorial Indian Ocean. I have two main concerns that I would like to see addressed before the paper can be published in *Climate of the Past*. First, the authors often refer to previous published works in their manuscript but the data are not presented on the Figures of the paper, making the comparison with these previous works very difficult. Second, the authors always discuss the periodicity in their different proxies or the timing of variability (example: variability in the precession band, phases of reduced northern hemisphere insolation. ...) but there is no statistics

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to confirm the significance of the periodicities and the exact timing (spectral analyses) that are discuss in the manuscript. This also hamper to compare with the variability found in other published records. I therefore recommend major revision for the current manuscript.

Below are my specific comments: 1) I don't find the title of the current manuscript really suitable. The title suggest that the Maldives record is mainly driven by "Indian monsoon dynamics" whereas the authors conclude that the record provide a close linkage between the Indian monsoon oscillation, intermediate water circulation, productivity and sea-level changes on orbital time-scale. Therefore, a title such as "A multi-proxy analysis of late Quaternary equatorial Indian ocean for the Maldives, Inner Sea" could be less confusing.

2) Lines 57 to 59. There is much more references of Arabian Sea works at the orbital and suborbital time scales (Clemens et al., 1996; Altabet et al., 2002; Clemens and Prell, 2003; Pichevin et al., 2007; Boning and Bard, 2009; Ziegler et al., 2010; Caley et al., 2011; Caley et al., 2013, Deplazes et al., 2013 are some examples).

3) Lines 179-180: "Local reservoir corrections were not applied". The authors should explain why they do not applied a correction. In general a correction of 400 years is applied in the tropics.

4) Lines 202-209: Oxygen concentration should be shown on figure 3 and not only on Figure 8.

5) Lines 217-221: The data of core M74/4-1143 are not shown on figure 4 making the comparison with core SO-236-052-4 impossible.

6) Lines 228-233 and 258-259: Are the Fe/Ca and Si/Ca good proxies for Aeolian dust? The results could be compared to dust data from site ODP722 (Clemens et al., 1996). This is important to discuss the provenance of the dust and the interpretation of the Fe/Ca and Si/Ca that stays speculative in the discussion (lines 258-268). Also, previous

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study in the Arabian Sea used rather the changes in the Ti/Al ratio of the sediments as indicator for grain size and thus wind speed, since Titanium is concentrated in heavy minerals in the coarser size fraction (Reichart et al., 1997; Ziegler et al., 2010 CP).

7) Line 278: "Fe/Ca record lacks significant variability on the precession band". Statistical analyses are necessary (spectral analyses) to confirm this point.

8) Lines 336-337: "While the benthic foraminiferal fauna preliminary show changes on glacial-interglacial time scale, the TOC content and Ba/Ca ratio are characterized by additional variability in the precessional band." Again, statistical analyses are necessary (spectral analyses) to confirm this point.

9) Lines 342-347: "Elevated TOC and Ba/Ca ratios at site SO-236-052 during phases of reduced northern hemisphere summer insolation suggest a direct influence of the Indian winter monsoon on productivity and related organic matter fluxes of the Maldives Inner Sea during the past 200 ka, which is consistent with the present-day situation (de Vos et al. 2014). The close link between the winter monsoon intensity and surface water productivity in the study area is confirmed by the difference between the  $\delta^{13}\text{C}$  values of the epipelagic *G. ruber* (Gr) and the epibenthic *C. mabahethi* (Cm) (Figs. 3, 8)." Again, statistical analyses are necessary with a phase analyse. Also, what could be the role of the IEW and ENSO mentioned in the introduction part?

10) Lines 372-376: "term trends of similar magnitude have been recorded from sites bathed by the Antarctic Intermediate Water mass (AAIW) in the southwestern Pacific Ocean (Pahnke and Zahn, 2005; Elmore et al., 2015; Ronge et al., 2015). The general resemblance of the various epibenthic  $\delta^{13}\text{C}$  records suggests a 375 significant role of AAIW in ventilation of bathyal environments of the Maldives Inner Sea, which is consistent with the modern oceanographic situation (You, 1998)." It would be good to add the data of the previous work mentioned on the Figure of the manuscript for a direct comparison.

11) Lines 385-386: "The reconstructed O<sub>2</sub> record reveals precessional changes be-

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tween oxic and low oxic conditions during northern hemisphere insolation maxima and minima, respectively". Statistical analyses are necessary (spectral analyses) to confirm this point.

12) Lines 406-412: "Agulhas leakage". I do not understand this paragraph and the link with the Agulhas leakage. If the authors want to demonstrate a link between their record and the Indian monsoon they can compare directly with published monsoon records. Also, the forcing of the Agulhas leakage at terminations is driven by the subtropical front migration and is not directly link to the Indian monsoon (Peeters et al., 2004). For the IEW impact, a statistical analyze with the phase relationship (spectral analyses) will help the interpretation of the record.

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