



Interactive comment on “Changes in productivity and intermediate circulation in the northern Indian Ocean since the last deglaciation: new insights from benthic foraminiferal Cd / Ca records and benthic assemblage analyses” by Ruifang Ma et al.

Anonymous Referee #1

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Review of manuscript “Changes in productivity and intermediate circulation in the northern Indian Ocean since the last deglaciation: new insights from benthic foraminiferal Cd/Ca records and benthic assemblage analyses” by Ma et al.. This manuscript presents data from sediment cores from the northern Indian Ocean (Arabian Sea and Bay of Bengal), comprising geochemical time series generated on benthic foraminifera as well as census data for planktic and benthic foraminifera. Based on these data, the authors deduce changes in monsoon driven changes in productivity

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mainly dominating the various records during the Holocene and changes in intermediate water chemistry during the deglaciation. In principle the authors present interesting data and some of the interpretations appear justified. There are, however, a number minor and more major issues, preventing recommending publication as is at this stage. These are some of the issues:

a) The biggest issue is related to the lack of constituency of interpreting the Cdw records. In lines 450-453 the authors claim that the Cdw values during the deglaciation are lower than during the Holocene. First, this statement is only correct if longer term averages are considered. On short time scales (which need to be considered, given that this is a chapter on millennial scale change), the youngest Cdw data in core MD77-191 (2-1.5 KaBP) are comparable to YD and HS1 values. Up to this point a big effort has gone into establishing Cdw as reflecting productivity variations at the sea surface and the related flux of organic carbon. Now the focus shifts to bottom water ventilation changes being recorded. If general water ventilation would play a role in setting the recorded Cdw values, this has to apply to the Holocene too and would therefore need to be considered there too. Interestingly, the authors do involve water ventilation during the Holocene in relation to the carbon isotope and census data, but not very much in relation to the Cdw records. Also, if the general interpretation for the Holocene section is used, why is there no change in the Cdw record around 16-16.5 KaBP? During this time, high *G. bulloides* concentrations (highest in the entire MD77-191 record) in the same core are shown in figure 5. High concentrations of *G. bulloides* strongly support the notion of enhanced productivity, as the authors themselves assume in case of the Holocene changes *G. bulloides* concentrations. Around 16-16.5 KaBP the high *G. bulloides* concentrations are not reflected in the Cdw data. This would suggest that the Cdw water are not very reflective of surface productivity changes, casting doubts on parts of the Holocene storyline. This would need to be addressed in a revised version, not only in this section but in large parts of the manuscript.

b) Also, in line 329 (and thereafter), the authors, for the first time, mention NADW,

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claiming that this water mass would dominate during the early Holocene at site MD 77-191. How does this claim compare to the modern water mass distribution in the area? Is it not true that most of the deepwaters in the Indian Ocean are mixes of various endmembers, of which NADW is just one? The only place original (largely unmixed) NADW occurs is off the southeast coast of Africa, with the northward propagation blocked by the Davie Ridge (although there is some discussion in relation to a potential northward spillover occurring). In order to substantiate their argument, a) the hydrography section needs improvement and b) there needs to be a more in-depth explanation how (even contributions) of a deepwater mass, currently occurring below ~2km in in the Mozambique channel, affect sediment cores at true intermediate water depth. The latter changes affect the discussion of the entire Holocene record.

c) There is some inconsistency regarding the description (interpretation?) of the habitat of the various benthic foraminifera species used in the study. In lines 141 and 142, the authors state that *C. pachyderma* is an epifaunal species. In contrast, in 289 and 290 they state that it is a shallow infaunal species. This needs to be clarified and consistently used throughout the manuscript.

d) At times the description of results/findings is too generic. As an example in lines 364 and following, a number of comparisons are made regarding the similarity of records. Generally, on longer time scales, yes there is some similarity. It should be pointed out though that there are also substantial differences at the millennial scale. This is particularly relevant for the comparison between *Corg* and *H. elegans* Cdw records. This needs a better wording.

e) (minor point) Figure 6 needs a better embedding/explanation in the manuscript. Some of the records are neither explained in the main text nor in the figure caption.

Overall, there are some useful data in this manuscript. The discussion of the data and subsequent interpretation lacks maturity at this stage and requires improvement. A moderate to major revision is required.

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