Review of *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.

The authors present benthic foraminiferal assemblage records and Cd/Ca data from the western Arabian Sea and Bay of Bengal (BoB) to investigate surface primary productivity (PP) and intermediate water mass variability in the context of the last deglaciation and Holocene climatic evolution. The authors find that Cd/Ca is primarily controlled by PP during the Holocene which mirrors monsoonal intensity. Notably, a strong monsoon is inferred to suppress PP in both areas due to enhanced run-off which increases upper ocean stratification in the BoB and reduced Ekman-upwelling off India as a result of decreased wind stress. During the deglaciation, the authors infer a dominance of water mass changes in driving the Cd/Ca signal, showing an enhanced advection of AAIW into the northern Indian Ocean during YD and HS1.

In general, the data and its interpretation appear mostly sound and in line with existing concepts about the paleoceanography of the Arabian Sea and the BoB as well as the influence of the monsoon on the PP in these areas. In this respect I find it noteworthy that the data (i) supports the presumed E-W dipole between strong upwelling off Oman and weak upwelling off India, and (ii) that maximum monsoon-induced run-off in the BoB apparently suppresses PP due to strong stratification, despite riverine nutrient input should enhance plankton blooms on surface level. The authors mention stratification as an explanation rather briefly, however, I would encourage the authors to devote one or two more sentences on this issue (see also my detailed comment).

I can also follow the arguments for the inferred intrusion of AAIW into the northern Indian Ocean during HS1 and YD, which agrees with the well-documented enhanced northward protrusion of this water mass in the Atlantic Ocean. However, I am not convinced by the way the authors come to this conclusion, which is based on the claimed mismatch between increasing *G. bulloides* abundances and decreasing Cdw estimates during YD and HS1. As depicted in the figure below, both records essentially follow the same trend, also bearing in mind that the resolution of Cdw is relatively low during YD and HS1. Hence, the proposed anti-correlation between *G. bulloides* abundances and Cdw seems to be an overstatement.

Irrespective of this problem, the good match of Cdw and d13C with AAIW reference records make it reasonable to assume that the Cdw values in deed capture water mass variability between HS1 and YD (Fig. 7). Hence, the interpretation at the end seems correct, but it is more likely that the relatively modest increase in PP during the YD/HS1 appear to have had a negligible influence of the Cdw. Only if PP is really high (such as in the mid-Holocene) Cdw is dominated by PP, as also indicated by the very high values > 1.0. The authors are somewhat over-confident regarding the use of Cd/Ca as a water mass tracer in the such potentially highly productive areas and might consider toning down their argumentation.

While the manuscript is well written, the Figures might benefit from rearrangement to make the discussion more easier to follow (cf. detailed comments below).

Given some moderate revisions I support publication of this study which represent an important contribution to our understanding of the deglacial evolution of the Indian Ocean.

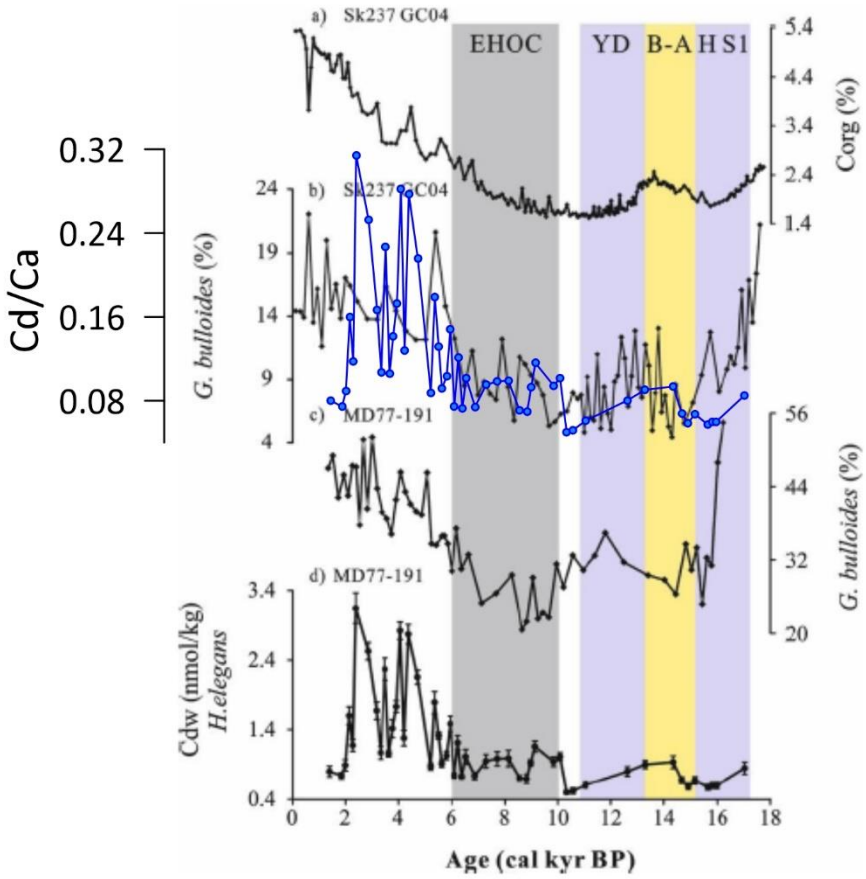

Figure 5 with Cd/Ca from MD77-191 in blue, illustrating that Cd/Ca follows *G. bulloides*-abundances (even more in the more high-resolution data of adjacent core GC04).

Detailed comments

Line 78: The motivation for the study is rather weak (essentially: we know little about the paleoproductivity of the BoB). It would be good to more explicitly state why we should care about this issue.

L. 85: "estimate past changes in the nutrient content, since the last deglaciation, over the last 17 kyr BP." The last part is redundant.

l. 100: "of the planktonic…"

l. 101 (and elsewhere): avoid using "." as multiplicator

L. 109: "Arabian High Salinity Waters" (all capitals)

L. 118: Neither Fig.1 nor S1 show salinity.

L. 131: "northern intermediate …" (no capitals)

Section 3: Please also include the statistical methods used in the study in the Methods chapter. Which program did you use to perform the PCA? Did you use a correlation or variance/covariance matrix?

Section 3.1.: Regarding the design of the study, I wonder why the authors decide to use four different species, when *H. elegans* is available as a well-documented, faithful recorder of bottom water Cd/Ca. What was the rationale to use the three calcitic species, especially as they include infaunal dwellers which are naturally not the best suited for detecting bottom water fluctuations?

L. 204-205: you might omit "over the last deglaciation"; add an "a" before "significant decrease"

L. 228 etc., regarding the PCA results: You show 2 PCs which explain 61% of the total variance. What's about the other PCs, how much variance to they explain and what was the rationale to limit the investigations to those two PCs? When the authors discuss the loadings of the individual PCs, they assign particular species with a very low loading to individual assemblages. PC1 for example is very much dominated by *B. aculeata* (+0.84 loading); the denoted loadings of -0.07 and less for *B. manginata, C. wuellerstorfi, G. subglobosa* (Table 1) appear to be rather insignificant. The same applies for PC2 which has high loadings of +0.42 and -0.62 for *S. bulloides* and *H. elegans*, respectively; I doubt that e.g. *G. soldanii* with a loading of 0.07 has a significant relevance to PC 2. Please reconsider the discussion of the PC 1 and PC 2 accordingly. You might also consider providing a bi-plot for PC 1 and PC 2 as an extra figure.

L. 249: "aragonite": change into "argonitic"

L. 262: The paragraphs discussing Cdw repeat in large parts what have been written about the Cd/Ca ratio in the Results chapter. Please avoid such duplication. The same also applies for Figures 2 and 4. Figure 2 might be moved into the supplement.

L. 264: please add the modern Cdw to figure 4 for reference.

Figures 2 and 4: The y-axes should have a common scale to enable direct comparison of the individual records. As shown, the Cd/Ca and Cdw records of Globobulimina spp. appear to show large fluctuations, however, compared to the other species these fluctuations are of rather minor importance (as stated in the text).

L. 286: the core tops values mentioned here should be shown in the respective Figure for reference.

L. 300-303: The final statement that *H. elegans* provides the most reliable Cd/Ca (or Cdw) data is not really surprising. As stated in the earlier comment I would appreciate if the authors could provide more arguments what they wanted to test/proof with including the other three species.

L. 307 etc.: I think the presentation of the Assemblage data could be improved: 1) as stated earlier the presentation of the PCA results is not totally convincing; 2) why do the authors start with Assemblage 3 not in the numerical order? 3) If they use the Assemblages as environmental indicator they could plot the abundance of the respective Assemblages instead of individual foraminifera species in the results figure; they could also assign specific environmental parameters to each Assemblage in the Figures (e.g. Assemblage 1 = more/less productivity or oxygenation), which would help to more concisely convey the message of the study.

L. 351: better write "Assemblage 1" instead of "fauna 1"

L. 353. “depleted *Globigerina bulloides* abundances” – replace by “low *G. bulloides* abundances”

L. 370: please refer more often to the respective Figures.

L. 398: a decreasing Cdw trend between 5.2 and 2.4 cal kyr BP is not evident for me, the values are constantly very high during this time period.

L. 405-406: the relation of stratification and PP should be discussed in more detail (see my general remark).

L. 431-432: The authors refer to summer insolation – pleas show it in an appropriate figure (e.g. Fig. 5)

L. 453: The reference to Figure 7: should’t it be rather Fig. 6?

L. 457-458: “Thus, we do not expect that surface productivity played an important role during the last deglaciation.” This statement is odd, as it has been discussed at great length that PP is influencing Cd/Ca. The following “In addition,…” does also not fit as the following sentence does not support the above notion of PP playing an unimportant role.

L. 462 etc: I am not convinced by the statement that increasing *G. bulloides* abundances during HS1 and YD are in conflict with both, Corg and Cdw records. With regards to Corg I agree that it declines opposite to the trend in *G. bulloides*, however, Corg does not only depend on PP but also on preservation, and potentially sedimentation rate (one way to check the influence of sediment accumulation would be to compute Corg accumulation rates). However, Cdw rather follows *G. bulloides* abundances, at least it is not anti-correlated, as one might infer from the text.

L. 482-487: the sentence is too long and complicated, please rephrase.

L. 496: “increase” instead of “icrease”

L. 504: “the entire biological factory was related to reduced monsoon intensity” – see my general comment: this statement need more justification. The presence/absence of *G. bulloides* might well be influenced by stratification and surface water freshening, but does this apply to other primary producers as well?

General remarks to the figures:

It helps the reader if the authors state next to the core name where the core is located (e.g. within the BoB)

Fig. 5: you might add here the benthic d13C records of MD77-191 and -176 used to discussed water mass variability (cf. Fig. S3)

S1: please add the variable + unit to the color shading on the right side of the maps.